Cristina Pallí i Monguilod

Entangled laboratories: Liminal practices in science

(spine text) Entangled laboratories: Liminal practices in science

A tots els àngels que em sostenen...
To all the angels that sustain me...

...entre els quals cal mencionar, per estricte ordre d'arribada a la meva vida, mare i pare, els meus avis, mes germanes i en Michael.
...among which I must mention, in a strict order of arrival to my life, mother and father, grandparents, sisters and Michael.

I després, com no podria ser d'altra manera, a l'IBF i a l'estol d'amics que acull. Amb la seva generosa hospitalitat m'han ensenyat de quantes maneres diferents es pot imaginar la pertinença.
And then, as it could not be otherwise, to the IBF and the swarm of friends that it shelters. With their generous hospitality they have taught me of how many different ways one can imagine belonging.

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Donant gràcies –ich bedanke mich

> "I find that the process of thought is rather like a large, unitary, fortuitous moment of being carried away, which is broken down into little squalls and flurries which have no particular relation to each other but which all come together in a greater overriding movement"
>
> (Serres, 1995, p. 33).

No hauria de ser difícil escriure aquesta secció a tall de reconeixement envers totes aquelles persones que han contribuït, d'una manera o altra, a fer possible aquesta tesi en la forma final que ara teniu a les vostres mans. I tanmateix ho està resultant una mica, com si no pogués acabar, ni tan sols de pensament, de configurar una llista ja de per si de límits difosos. I com que això de marcar límits no ho porto massa bé (i aquesta tesi n'és la prova empírica), no puc fer altra cosa que intentar deixar constància de l'embolicada constel·lació de connexions amb les que estic, per lligams de reciprocitat i amistat, endeutada; connexions que entreteixeixen presències òbvies, presència s més amagades i fins i tot absències. Això ens portarà a temps, llocs i noms que des de segons quina perspectiva poden semblar molt allunyats i crear fins i tot una sensació de desproporció. La rèplica, però, si és que en cal, és molt senzilla: aquesta perspectiva no és la meva.

Una de les maneres més fàcils de començar agraïments és donant gràcies per les beques i col·laboracions que han fet possible el meu trajecte de vegades errant. Les beques –*munera sorti, munera deis*- m'han donat el nostre pa de cada dia, regalant-me un temps i un espai que no podré agrair mai del tot. Les beques directament rellevants per a la tesi han estat tres: la beca FPI (1997FI 00420), lligada a un projecte i treball col·lectiu del meu departament també becat pel Ministerio de Educación y Ciencia (nº PB94-1521); una beca Erasmus-Sòcrates durant el doctorat que em va permetre una llarga estada a Gran Bretanya; i una beca atorgada per La Caixa juntament amb l'organisme alemany DAAD.

Gràcies a la primera d'elles vaig poder entrar a formar part de la Unitat de Psicologia Social del Departament de Psicologia Social i de la Salut de la Universitat Autònoma de Barcelona. Becària durant quatre anys, aquesta és la pertinença més definitòria de totes les que m'articulen des del punt de vista professional, la que m'ha constituït com el que sóc: una "psicòloga social" que fa equilibris amb un peu a cada una de les dues parts de l'etiqueta –i que, per acabar-ho d'arreglar treballa en un territori ambigu fruit de l'encreuament de vés a saber què.

Institucionalment ja no tinc cap altre lligam amb el departament que no sigui el d'estudiant de doctorat, lligam que estic a punt ja de consumir. Però sovint haig de fer un esforç per recordar-ho. Quan em pregunten d'on sóc, continuo contestant que del departament de psicologia social de Barcelona, i en certa manera no dic cap mentida si és cert que 'qui som' està íntimament relacionat amb 'd'on venim' (i 'con quien andamos', si fem cas als refranys). Aquesta pertinença em constitueix de forma tan radical que sense ella no em puc pensar i és per tant just i necessari que doni les gràcies a totes i cada una de les persones que m'han fet companyia i de les quals he après més del que elles imaginen. A aquesta llista haig d'afegir la Isabel Crespo i en José Luís Lalueza. La mirada que l'etnografia va desplegar hauria estat ben diferent sense els anys de treball plegats i tot el que vaig aprendre d'ells.

Canviem de país i de llengua: ens n'anem a Reading (Gran Bretanya) per dos anys. First I must thank the Department of Psychology of the University of Reading for welcoming so warmly. During these two years I got to know many people, all of which gave me more than I could have expected. There are plenty of things I feel grateful for, but the word 'hospitality' concentrates many of them –shelter, ideas, conversations and homes. The dimensions of Rex & Wendy's hospitality were so incommensurable and extended that nothing I say will prise enough their generosity. I consider myself privileged to have been, if only for a while, one of their extensions.

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Through them, I got to know people who populate my memories in such an intensive way that I still catch myself in internal dialogue with them from time to time. Maybe this is why I tend to forget how long it is –too long in fact- since I last saw them. Risking partiality, I must mention some: Lindsey, Steve, Marcia, Paul, Rose and Nick –Nick in particular: neither my work in Reading nor my-self would have reached the shore without his company. They all have made possible a spreading of my homes, belongings and horizons, agreeable encounters that extend possibilities of being. The list of hosts must be extended with Rolland and Joanna, Jamie and Arabella and the Vineyard, including dogs, horses and woods. The extend to which my thoughts are theirs will become clear for those who read this thesis.

In the last years, the Institut für Soziologie from the Ludwig-Maximilian Universität in Munich has given me the institutional support necessary to bring this work to good term. In particular I must thank the group around Prof. Ulrich Beck for all their help and ideas.

Aquesta tesi analitza la vida d'un institut de recerca de la Universitat Autònoma, l'IBF (que ara s'anomena IBB (Institut de Biologia i Biomedicina). Parlar de 'generosa hospitalitat' per referir-me a l'IBF està més que justificat. Aquest institut i tots els seus membres ens han regalat més temps, atenció, esforç, dedicació, paciència i confiança del que ens esperàvem, durant i després del treball de camp, incloent les correccions adients a posteriori. I això tothom, des dels caps de grup, passant pels seniors i postdocs, fins als becaris –especialment els becaris, que no només van aguantar estoicament la meva ignorància i preguntes, sinó que fins i tot van trobar temps per mostrar curiositat per la nostra feina. A tots ells els dec molt, per coses que apareixen a la tesi, i per coses que no. Cal mencionar molt especialment al llavors director del centre, en Francesc Xavier Avilés, que des d'un principi va donar suport a la nostra proposta i va fer molt per facilitar-nos la tasca, amb una confiança que li hem agraït de debò.

No afegiré més perquè, en el fons, la tesi sencera mostra no només la meva admiració i agraïment, sinó que és testimoni de com els nostres mesos junts van donar lloc a un entramat complicat de relacions que ens va vincular íntimament –un *entanglement*. Aquesta paraula suggereix justament aquestes relacions embrollades –la paraula castellana és més bonica: *enmarañadas*. Haver estat a l'IBF significa entrar a ser part de la seva xarxa de relacions, que atrapen de forma seductora fins i tot als psicòlegs socials que hi treuen el nas. Per dir-ho més planer, haver estat a l'IBF deixa marca. Certament, amb un simple 'gràcies' no n'hi acaba d'haver prou.

Un dia, tot fullejant un *Archipiélago*, vaig trobar un retall d'una entrevista a l'historiador Ginzburg, on es comparaven les disciplines intel·lectuals a un port de mar, d'on la gent entra i surt creuant-se amb persones, objectes i formes de saber. L'entrevistador contestà a Ginzburg el següent:

> "Hemos de admitir que es ésta, la del puerto de mar, una imagen muy bella porque da idea de libertad y de tránsito intelectual. Pero para concederse esa libertad se necesitan buenos maestros, alguien que tutele con mano firme. ¿Cuáles fueron los suyos?"
>
> (Entrevista con Ginzburg, 2001, p. 94-5).

Doncs bé, dos dels meus mestres han estat alhora els meus tutors de tesi, en Miquel Domènech i en Tomás Ibáñez, dels quals, combinats, n'he obtingut la barreja perfecte de llibertat i mà ferma. Qualsevol persona que conegui la meva trajectòria i el tema de la tesi s'adonarà, sense que sigui necessària molta perspicàcia, que aquest treball no podria haver estat fet a qualsevol lloc: porta la marca de la casa, i en això en Tomás hi té una gran responsabilitat. Quan encara a la carrera jo llegia els seus llibres, sempre me n'impressionava la sensació d'obertura teòrica. Aquesta mateixa obertura l'ha mostrada deixant-me buscar sense restriccions ni condicions, acceptant l'itinerari teòric vacil·lant d'algú que encara vaga, i no tant el d'arribada o el de qui ja ha trobat. I el mateix puc dir d'en Miquel. Haig d'agrair a ambdós tutors la llibertat que m'han donat de cercar i cercar creant més preguntes que respostes.

A més de tot això, que no és poc, a en Miquel també li haig de donar les gràcies pels intents de posar mà ferma en la dosi necessària que necessita una persona que oscil·la entre la tendència a perdre's entre mil camins seductors i la tossuderia d'incloure-ho tot. Especialment pels seus intents d'acotar sense imposar, així com el seu acompanyament en unes discussions que han fet madurar les idees de manera diferent a com ho haurien fet soles. Així doncs, bona part de l'ordre i coherència de la tesi és cosa seva. A més de tutor en Miquel és membre, juntament amb en Francisco Javier Tirado, de l'equip d'etnografia que va dur a terme el treball de camp. Als dos haig d'agrair les idees, les discussions a tres, el suport i seva paciència.

La meva família ha acompanyat, suportat, recolzat, patit, estimat i odiat aquest treball al mateix ritme en què ho feia jo. Han fet tot i més per fer-me la tortura més lleu i el plaer més gran: ma mare ha fet de mare, mon pare de pare i mes germanes de germanes amb una dedicació i delicadesa que cal no donar per suposat i agrair. També ells s'han s'han tret un bon pes de sobre... Els meus avis, que mai no han acabat d'entendre massa bé què carai és una tesi fins que no l'han poguda tocar, es van posar les mans al cap i van festejar-ne el final en una reacció espontània que per a mi va compensar, si no tots, ben segur que molts dels patiments. Amb aquella ingenuïtat fresca que els caracteritza, estan feliços perquè creuen que finalment la seva néta podrà deixar d'estudiar... i guanyar-se un bon jornal. Jo no m'he vist amb cor de desenganyar-los... L'Elisabeth també ha contribuït: els regals, el bon menjar i les taules ben parades han estat la seva manera de donar un suport que he necessitat.

Hi ha alguna gent que també s'hauria alegrat molt de veure aquest final, però la vida va decidir, en un canvi d'última hora imprevit i un xic traïdor, que els tocava encarregar-se d'altres coses. (Tiu Lluís, Rex, Joe, ma?: *um beijo*).

Ara em toca la dedicatòria difícil: com donar les gràcies a algú que és tan present en aquestes planes, tant part del procés, que quasi s'hi confon? How to thank Michael? It does not matter how well I arrange these sentences to describe the way in which he has been there for me, I will always produce a scanty praise. He listened to my arguments, doubts and extravagances with a patience that verges on the impossible, giving support to what he considered good questions and challenging absurd ones nicely but without compasion. He has been a company to my histerias, bad mood and insomnia without reproach. He has done what I would have never dared to ask, what nobody else would have done. He has taught me, in a way I cannot forget, what it means to be there for the other, to belong to the other: *Miteinandersein*.

Darrere d'aquestes planes hi ha un estol d'amics i amigues, que no anomenaré en part perquè espero que no els calgui, en part perquè repetiria molts dels noms mencionats abans. Gent que van sentir una sensació d'alleugeriment compartit quan es va imprimir l'última plana d'aquesta tesi. Tots vosaltres m'heu recolzat exactament de la manera en què jo ho necessitava, i això ni té preu ni es paga. Perquè tal com em va ensenyar fa temps algú –oh, *auctor donorum*, *largitor*, viatge sense retorn que en mi has obert límits- en l'amistat no hi ha comerç just, sempre quedem a deure una miqueta, per tenir excuses per poder endeutar-nos més i més i més, i embolicar-nos més i més i més. Altres que ens lliguen en *entanglements* i *attachments* que ens constitueixen i ens fan habitar els límits. Perquè ara puc contestar, després de tant de temps i tantes preguntes vostres, que és de límits que prova de parlar aquesta tesi. De límits entesos no com una barrera o un final, sinó com un moment de metamorfosi on les emocions i les passions ens mouen per constituir-nos en regals dels uns pels altres.

"En el alba, la lejanía se erizó de pirámides y de torres. Insoportablemente soñé con un exiguo y nítido laberinto: en el centro había un cántaro; mis manos casi lo tocaban, mis ojos lo veían, pero tan intrincadas y perplejas eran las curvas que yo sabía que iba a morir antes de alcanzarlo"[1].

(José Luís Borges, *El inmortal).*

"Las palabras, desgraciadamente, no protestan"[2]
(Ramón Valdés)

"And here Alice began to get rather sleepy, and went on saying to herself, in a dreamy sort of way, "Do cats eat bats? Do cats eat bats?" and sometimes, "Do bats eat cats?" for, you see, as she couldn't answer either question, it didn't much matter which way she put it"
(Lewis Carrol, *Alice in Wonderland*, p. 5).

"Passa moltes vegades, que cansa més el que no es fa, el repòs és haver-ho fet"[3]

(José Saramago, *La mort de Ricardo Reis*).

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[1] At the dawn, remoteness bristled with pyramids and towers. Unbearably I dreamt about an exiguous and sharp labyrinth: in the centre there was a pitcher; my hands almost touched it, my eyes saw it, but so intricate and perplexed were its curves that I knew I would die before reaching it" (José Luís Borges, El inmortal).

[2] "Unfortunately, words do not complain" (Ramón Valdés).

[3] "It is often the case that it is more tiring what is left undone; it is a relief once it is done" (Saramago, *La mort de Ricardo Reis*).

Impossible missions: "Alice expelled from Wonderland" [1]

> "In general it seems to me that the most potent point of departure for solving the puzzles of local culture is the point where the locals actually 'see' your presence. Only there can the anthropological encounter turn into an exchange between subjects"
>
> (Hastrup, 1990, p. 259-60).

> The impersonal nameless person that results when the 'I' is destroyed, erased; a subject outside identification, always out of place and without function, just "a word too much ... 'He', the fictive distance between 'I' and 'I', that makes it impossible to address to myself, since I am never the same"
>
> (Blanchot, in Herrera, 2001, p. 54, our translation).

Constituted as a half-psychologist, half-ethnographer in a department of social psychology, she started her Ph.D. program already knowing her destiny-destination: as a member of an ethnography group, she would have the privilege to participate in a laboratory study. Which meant that some months after the end of her degree, and with shamefully little knowledge in anthropology or STS, she was sent 'for a mission'. Not to the exotic lands of the other, but "chez soi" (Augé, 1998), not very far away, just some hundreds of meters away, to a research institute of her own university –but far enough to enter into relations with another community, and to realise this was *an-other* community. It was precisely at this moment of contact when our ethnographer realised the weight of her belonging: being a social psychologist did not define her completely, but it nevertheless did it strongly – tradition, expectancies and prejudices, to put it in terms of Gadamerian (1996) baggage... Her psycho-ethnographic self could not be severed from the conversation between her 'I' (already co-constituted by previous 'me') and the generalised other that one of her communities of belonging represented (Mead, 1934).

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[1] "Alicia expulsada al país de las maravillas" (Bunbury).

She soon realised that her self did not feel quite at home in this new community –or maybe this is not the right way to put it: maybe we should say that her self did not seem to come into line with others... In this new community, selves had not only different knowledge and engaged in different working practices, but were also accountable to different demands and projects. Which ones? Well, this was precisely her mission: understanding, participating, observing, analysing, feeling... in order to catch a glimpse of what 'being' meant in that laboratory. This was hard work and our ethnographer started immediately: a pen, a notebook, a smile. Rather than a fly on the wall (Law, 1994), she was an annoying fly bothering behind the ears of poor scientists working in the lab: "what is this?", "why are you doing that?", "what is this for?" Look, ask, hear, smell, feel, touch, chat and poke about.

From time to time she wondered what she was doing there, what she was doing this for. Not out of love for this community, was she? Not yet at least. Out of social responsibility? Perhaps; after all this is what one would expect from social sciences. But surely out of work duty: in order to come back and report to the community of origin –and, of course, publish... or perish! If her mission had been secret, she could have been a spy. But it wasn't –at least, she did not feel it was: nobody was in war. Of course her team was aware of the colonisation that any ethnography implies, but they thought of themselves as ambassadors of peace and contact. And this is also how scientists offered to interpret their visit at first: they said collaboration was a great value for them and that they were more than happy to contribute to their research.

"What did you say you want from us?"

If one studies proteins, the chances that the protein turns to you and asks you with a smile "hey, wait a sec, could you tell me why you are studying me?" are rather low. This is one of the added problems one has if the object of study speaks –although this

obvious point took a while to hit home among social psychologists[2]. Probably, any human collective being studied would ask such a thing -one can imagine several generations of amused Nuer asking themselves jokingly what on earth all these people wanted (and want) from them! Then, just imagine what happens when those studied are researchers -used to being the ones posing questions, formulating hypothesis and getting answers- are visited and studied by people who also claim to be researchers -that is, by a group of people who could be their colleagues. If somebody thinks that, under these circumstances she or he could have blocked the flood of questions, this somebody has not been in the IBF. After all, as Augé warns (1998, p. 74), this is one of the most dangerous forms of ethnocentrism: to attribute all reflection to the self[3].

It is not surprising, then, that one of the first questions that scientists posed to our psychologists was 'why?': "why science?" And in particular, "why us?" They answered that they were interested in knowing how science is produced in a Spanish context, including everything: the techniques scientists were using, the organisation of the laboratories, their ways of funding, publications, contacts with other laboratories... And they told the truth; they had no hidden intentions, not at all, but they could not specify their objectives more concretely simply because they hoped that the research itself would show them interesting paths to follow. One of the characteristics of ethnography is that it allows for circular hypothesis, that is, hypothesis which emerge along the research and which mould one's performance *while* performing –hence, circular. Apparently, this plain formulation did not convince them much, for they kept on asking the same in several occasions, but at

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[2] See an account of this process in van Dijk (2003), Ibáñez (2003) and other contributions in Íñiguez (2003). Names in such accounts usually include, among others, Antaki (1994), Billy, (1987), Bruner, 1990, Edwards (1997), Edwards & Potter (1992), Parker (1992), Potter & Wetherell (1987), Wetherell & Potter (1992). See also Bourdieu (1982).

[3] "What, then, do the ethnologist and his or her subjects of observation have in common? The answer is, they are both looking at each other. In this nothing radically distinguishes the ethnologist's situation from that of being face to face with a radically foreign group; the ethnologist is also the object of successive identifications made by the observed which have to do with the place he or she takes or that is assigned to him or her on the research site, and with the position he or she occupies (or is imagined to occupy) in society at large. The reciprocal process of identification –and this is surely what makes chez soi ethnology so original- is carried out by means of criteria and references that are in part shared by the ethnologist and those he or she is studying" (Augé, 1998, p. 41-42).

least it was enough to keep the situation more or less under control for a while, and for some months it seemed that our psychologists could manage without having to make their objectives more explicit.

If this short and unconvincing answer was enough to block their questioning for a while, their scepticism nevertheless forced our young psychologist to pose these very same questions to herself: why was she studying science? She had learnt two different answers: 1) science is a set of practices which has very important effects on society (the soft version would talk of 'influences', the more radical of 'constitutive effects'), and therefore, it is necessary to understand how science works and changes if one wants to understand society. 2) To this argument, one can add that science has become a rhetoric of truth with dramatic power effects, and that one needs to analyse science to be able to 'doubt' (deconstruct) its privileged position. Constituted as she was by the constructionist traditions in her department, we could say that her own position combined elements of both answers. The first allowed her to be convinced of the necessity to understand how science works, the second conferred on her work a kind of 'political legitimation', an ethical justification: it is only good to study science and fight power relations.

Due to circumstances which will in short become clear, there came a moment where she was asked to tell them more about her position regarding science, and she felt that, to be honest with them, she had to make these two ideas mentioned above explicit. They could understand and quite agree with the first, and they laughed a lot about the second. They considered (and still do) their position to be far less advantageous than she implied: they think they are not enough valued by the government and society; they experience continuously scarcity of resources to carry out their research; they resent the small percentage of public budget dedicated to science, as well as the little relevant role that education and research are given during voting campaigns, etc. Many of them have miserable salaries, experience labour precariousness, lack of proper full-time jobs, and the impossibility of the university to

secure their future or offer alternatives... They were completely astonished to hear her talking of science and power.

On attempting to justify herself, she discovered that being taught some arguments on science by teachers, or writing an essay on science for colleagues sharing one's opinion, was a universe apart from discussing and defending these same arguments convincingly in front of sceptical others- especially if these others happened to be scientists! These difficulties partly proved that she did not have things that clear after all. How do you explain certain things to those who do not share a minimal common frame? "Hey, Psycho, what do you mean that scientific knowledge is socially constructed?" Where do you start? Where do you support yourself? How do you explain what you mean by "things could be otherwise" to those who have been trained to "discover how things really are"?

Much as she agreed with the general argument about science and power effects, she was not able to express clearly the connection between science subjection effects and the particular experiments carried out by 'her' Ph.D. students at the bench on potato proteins! She heard herself repeating all the time the intrinsic link between science, public policy, and money. She must have been so clumsy with her argumentation that eventually one of them shouted, "You are only worried about money, aren't you?" she burst out laughing: of course not, but somehow, she was not able to convey anything else.

Needless to say, the fact that she was not very skilful when trying to explain what they meant by power relationships does not mean they were not all enmeshed in them: scientific discourse remains one of the most powerful nowadays and critical examination is needed. But she nevertheless had to recognise the perversity of believing one knows "the identity of science" (Stengers, 2000): scientists rejected what they felt as a projection of her pre-conceptions onto the field, exercising a kind of violence, already convinced (pre-judiced, some might say) of the intrinsic link

between science and power. Hence, their protests. A sense of ridicule invaded her, and she decided not to bring the issue back until she could argue it better.

But, above all, her uncomfortableness was fruit of the dichotomic definition of relations that her own assumptions were creating: a divide between 'scientist on the side of power' on the one hand, and 'emancipated and emancipating nonscientists (social scientists?) willing to show that things could be otherwise' on the other. Quite an aggressive confrontation whereby one side felt entitled to reproach and rebuke the other for 'not being critical enough'. Scientists appeared either as the wicked people trying to accumulate privileges and prestige, or as the Lego pieces (subjects) through which 'Science' (either as discourse or as institution) establishes its hegemony. She felt this was too much of a bellicose approach, maybe quite useful to separate and reinforce identities (natural sciences vs. social sciences, for instance?), but perhaps not that good as a tool for social change (Gergen & Gergen, 2004) -and not necessarily good for an ethnographic relationship either. An approach assuming two sides –different, distant and impermeable to each other; a self and an other. But did this categorisation hold?

It seemed to her that studying science left them in a problematic situation, in which the distinction and relation subject/object, or observer/observed was, to say the least, complicated: they studied an activity to which it is not yet clear whether they belong or not. Are they studying it from an exterior position, since it is clear, of course, that psychology is not scientific? Or are they analysing it from inside, since, as everybody knows, psychology is a real science? When they emphasise the constructed character of scientific knowledge, does this mean that 'natural scientists' are as little scientific as they are, or that they are as good scientists as biologists are? They come back, again and again, to the relationship between natural and social sciences, between 'we' and 'they', between self and other.

Being moved around

When they started the ethnography, the categorisation 'we' / 'they' was very present. This would have been a good moment to follow one of these advices of the politics of identity: 'make your position explicit': "tell me, who are you? From what position are you talking? To what effects? How will you acknowledge power relationships?"... Two groups trying to establish a dialogue: biologists and psychologists, natural sciences vs. social sciences engaging in conversation to get to know each other.

However, after a while, it became quite clear that certain categorisations arose irritation among both, some members of the IBF and ourselves: "You keep on talking about 'science' –they told her- but there isn't such a thing as 'science'. There is physics, there is biology, there is chemistry, there is biochemistry... and each of these has a particular way of conceiving problems and solutions. Sometimes we do not understand each other". They were complaining against a certain amount of simplification and homogenisation involved in the psychologists' attempt at 'representation of the other'. The other side of the coin, which they did not seem to be so aware of, was the homogenisation of the 'social sciences' they were also constructing in order to be able to discuss with the psychologists. The result of this categorisation and homogenisation was the constitution of a field of discussion with two sides, the natural and social sciences, which resulted in a battlefield where positions were difficult to move.

Nevertheless, in spite of those moments of fixity, when she could feel that positions were parapets, there were many other moments where one could feel movement. Indeed, this clear-cut division and the appearance of solidity of their positions started trembling with the relationship, and categorisations started to melt playfully (in a way that at the time she could feel rather than reason; it took her a while to be able to articulate it in words), and positions changed so strongly that it would have been rather difficult for her to state 'what her position was'. The argument is not that relationship eliminated categorisations definitely, because this argument would forget

that categorisations can also be a way of relating (Bhavbnani, 1999). Depending on the type of relationship created, categorisations came and went, creating different effects (Star & Bowker, 1997). But the fact that categorisation did not melt away for ever, but had a more strategic role, does not diminish the evidence that in many moments, the relationship involved a fluidity of positions, a movement that it is difficult to grasp with the simple notion of categorisation.

After a while it became obvious that these two groups were not homogeneous. The group of 'the scientists', which turned into 'the biologists', diversified into 'the biologists, the biochemists, the chemists'. The group of the psychologists went from being a trio of 'social scientists' to being 'psychologists' (she very much doubted that social psychology ever gained a status of reality there...). But this division of initially homogeneous groups did not challenge the categorisation in terms of 'we'/'they'. The first disruption of this clear-cut division took place when members of the IBF tried to classify the psychologists according to their hierarchy. Scientists' groups have a pyramidal structure that strongly distinguishes between Ph.D. students, post-docs, seniors and bosses, and they tried to apply this categorisation to the other group. They immediately saw that one of the psychologists was a Ph.D. student, and the rest not (even though they could not quite make sense of the whole structure: the absence of a clear pyramid disoriented them).

Once this division was established, it had the effect of creating affinities among those perceived as 'equal'. For our young psychologist it was easier 'to pass as a member' in the group of Ph.D. students than with seniors or bosses, whereas for her teammates, bonds distributed the other way round. As Hammersley & Atkinson put it, "to establish over-rapport with one group sometimes means not t do it with others" (1983, p. 98). It would nevertheless be naive to think that these affinities emerged spontaneously, just because there was a 'perceived similarity'. Partly, perhaps. But also because these attachments were worked upon and used to create strategic links –in spite of good relations with people, this was an ethnography, and they were working. The relevance of this categorisation, though, is that it somehow

managed to cut across the first 'we' / 'they' categorisation: some characteristics were perceived as uniting her more to the group of Ph.D. students (regardless of the discipline) than to her own group of social psychologists.

Still, in other occasions, conversations could articulate another kind of position and affinity: that of gender. She was often positioned as female, either by others or by herself, sometimes in spite of her resistance, sometimes with her complicity. Thus, when some of the women of the lab were telling her of their problems (working and raising children, of the kind of jokes they had to bear, or of the self-demands they had to impose upon themselves to be perceived as 'committed' with their work) they were counting on a kind of complicity from her part –a kind of 'you already know what I mean'. In other occasions, some bad-taste jokes about women were working the differentiation man-woman, and created a split and tense atmosphere -with a trick: the moment she got offended, she was closing the possibility of escaping categorisation accepting 'being a woman'. In moments like this, the gender categorisation became relevant, and was able to articulate itself over other divisions [4].

There was, however, a very strong categorisation dominating the relations throughout the research. A categorisation which was, so to speak, a kind of Damocles sword hanging over their heads: science-nonscience. Or, to put it more explicitly, the question of whether the psychologists were or not scientists like members of the IBF. Whereas at the very beginning scientists of the IBF took them in as researchers, the former were still not very sure about the latter's status as scientists. The psychologists' performance, arguments and way of working did not help much to persuade them, so they felt the need to elucidate whether psychologists were "real scientists" or not: were they one of them? The answer to this question came neither

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[4] It was easy to feel offended by these jokes, but the possibility of avoiding offence was open. Jokes were not performing exclusion directly, they were rather a kind of tests of resistance to check what were you able to endure so as to become part of the group. If you were not offended, you could challenge gender categories and associated assumptions, making space for a certain ambiguity (you looked like a woman, but you proved 'strength' and did not react as such). If you got offended you could not show 'high resistance', therefore, you were weak, reacted exactly as a woman would, not good enough to be one of them. Ambiguity was gone, categorisation clear, disposal immediate (Latimer, 1997).

immediately nor definitely. Rather, it remained an open question which put psychologists under suspicion, evaluation, scrutiny –which means that scientists suspected, evaluated and scrutinised.

Their first investigations went in the direction of finding out whether psychology was a science, with questions such as 'what are you doing?', 'what for?', 'how do you contribute to science and its progressive knowledge?', 'how productive are you?', 'how much do you publish?', 'where?', 'do you have scientific journals with referees?' Sometimes our psychologists could avoid questions, sometimes not, but for our Psycho they were always an on-going challenge to their own position, which forced her to re-think again and again the relationship of her discipline to science, as well as her own. One could say, then, that scientists were psychologists to accountability –and that, instead of simply ignoring this demand, she felt herself interpellated (Law, 2002a). With each questioning they were (re)constituted as good or bad researchers; as scientists welcome among other scientists, as naive non-scientists, as non-scientists feigning belonging exposed by real scientists... They were constituted as 'one of us' or 'one of them'. Constituted and 'deconstituted' as a person, as a researcher. If this was a dance of identities and ontologies (Cussins, 1996), she felt definitely 'danced around'...

There was a moment in which this omnipresent categorisation (science vs. nonscience) came back with strength, almost with violence. As it happened, thanks to a friend of a friend of a friend, members of a group of the IBF got to know that these psychologists came from a department partially inhabited by a swarm of sacrilegious social constructionists that, allegedly, were going around claiming that science was the same as astrology, or that there was no difference between scientific knowledge and tribal traditions. They were offering hospitality to the enemy! After hearing this, some members of the IBF jumped to her neck! Convinced that they had finally discovered their 'true and hidden motives' –something like disparage science-, they confronted her: "finally, Psycho, we've caught you, now we know what you are up to".

She tried to deny the charge, of course, but at the same time she did not want to hide the fact that most of STS perspectives on science did put science's authority into parenthesis. Her attempts were in vain[5]. They discussed for a while about such topics, but it is definitely difficult to argue with people who defend a position proper of a nineteenth-century epistemology, without knowing it, and who think that being biologists gives them a privileged insight into what science is and what principles science follows. Especially, if the person they argue against is not able to articulate a mastering voice... After some days, they stopped face-to-face confrontations on the topic, and relations recovered their levels of cordiality.

Nevertheless, a couple of people told her that in her absence debates kept on taking place: "you don't know what you have provoked! We have spent the whole morning arguing and discussing about what science is and is not!"; "we have spent our lunch trying to decide whether psychology uses the scientific method or not!" Such discussions went on for some days, until they got tired of them –some even told her they could not bear the thought of discussing about science again!- and polemics progressively died away. After a while, nobody talked of the incident any more, with the exception of some occasional joke about spells and tribes... Apparently, the IBF went back to normal –work must be done, and inflamed discussions at coffee breaks are not a great help. However... how do you go back to normal if you are the poor psycho-ethnographer causing this mess, if your leg is pulled at your back and you feel as a complete incompetent? She was even given a new nickname: from Psycho to Psycho-killer...

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[5] After the first discussion she was tired, exhausted, in rage and confused. But this was just one of several. From that moment onwards, jokes were the order of the day: "Psycho, we need your help: our computer went crazy, and we'd like you to put a spell on it!". "Psycho, dance an African dance for us, please, to see if you exorcise it! Or, better, shouldn't we go and carry out the experiment in Cambodia?" You can surely complete the list. First, convinced these were just jokes, she simply smiled; after their insistence, jokes stopped being funny; finally, she started to suspect they were no jokes at all... To make it worse, their discussion had spread to other laboratories, and their alleged bad intentions of discrediting science were on every one's mouth. She had no option but to talk to some of the people (especially some bosses) and try to solve the misunderstanding.

This incident brought about a sudden redistribution of positions. A previous categorisation which until that moment had remained open -suspended, so to speak, fell on them as the sky must have fallen on the Gauls' heads, making other categorisations lose currency: she was not a Ph.D. student carrying out research, assimilated to their figure of the 'hands' (see below), but was once more turned into a psychologist, at best a social scientist under suspicion. In short, psychologists became definitely one of 'them'. Needless to say, the opinion of the majority seemed to be that they did not quite pass as 'real scientists'. And she still did not know whether to be glad or sad about it...

Coming back home?

Our ethnographer is tired. Tired of suffering questionings, of being called to accountability, of not having solid ground upon which to stand. Oh, those little earthquakes! But above all, tired of not knowing what to answer. Not necessary to others, but to herself. She is tired of not knowing how to quieten down all her doubts and questions, how to allay her fears, her insecurities; not knowing how to stop this inner quivering, this sudden inkling that some of her presuppositions are not helpful. What to do now with this melting of positions into Serresian pre-positions (Serres & Latour, 1995)? What to do with all these things she had not expected to encounter?

She had entered the lab quite convinced of what she knew and of what she would find: social constructions in the shape of genes; powerful discourses of reality and discovery; scientific concepts and results encapsulated and transported in narratives (Knorr-Cetina, 1999), shaped by polemics and debates; authority arguments legitimising scientists 'on the name of science', rhetoric of truth (Ibáñez, 1995) hiding the necessary negotiation involving results and discoveries, objectivist, impersonal rhetoric typical of scientific papers (Mulkay, 1985, 1991), frustrated replications (Collins, 1981; Mulkay & Gilbert, 1986), interpretative repertoires (Gilbert & Mulkay, 1984; Potter & Wetherell, 1987) which eliminate from the reign

of science that which does not fit with accepted standards, such as para-science (Collins & Pinch, 1982), etc.

She had expected to find all this, and this she has found –no problem! But she has encountered more, which she cannot quite apprehend with her 'tools', so she has to look for more... On the move, so to speak. But of course this theoretical journey does not precisely contribute to her creating an image of competence; actually, she never manages to give the impression of knowing where she is heading. In contrast to Ph.D. students of the IBF, so sure of standing on solid ground, so self-assertive in their opinions, our ethnographer can never feel certainty –still searching, no knowledge to take as a secure point of departure. Which translates cruelly like this: in the IBF, the definition of a good Ph.D. student does not leave room for performances like hers.

She can feel she is disorienting them: they cannot believe that a Ph.D. student knows so little about her thesis or topics as she does. The shadow of a doubt is perceptible in their eyes: is she really a good Ph.D. student? Is she really a good researcher? Is she really a good scientist? This doubt is not theirs alone. She herself starts hesitating seriously about her capacity to perform as a good researcher, gaining a new awareness of the dimensions of her ignorance. Poor thing! Completely in the dark regarding whether she belongs to science or not, whether she wants to belong or not, her own identity as social psychology is disintegrating by the moment under the weight of the questioning. Is this ethnography taking in water everywhere? Can her own lack of competence grant her this belonging at least, or not even that?

The more insecure, the more she admires the security sheltering Ph.D. students in the lab. Would it not be wonderful to feel this certainty, to share all this beauty? Our ethnographer is sometimes blended by the beauty of everything she observes. She is under a spell[6]: she goes home while protean glimmerings sparkle under her eyelids,

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[6] "Indeed, it does not matter the transmitted content if it is transmitted in ugliness; only the latter will remain and the content will vanish, giving way to violence; if we give birth in beauty, transmission will work, the content will remain and this fine demand, on propagating itself, will allow everybody to live around it. This is what I understand as spell" (Serres, 1994, p. 15-6, our translation).

which she shakes off with a sharp movement of her head; and every time she wants to wash her trousers, she first needs to empty by handfuls the pockets full with adenines. In the jewel case she keeps the memory of the plastic gloves she once wore while defrosting test tubes; and she loses her heart when she can walk among loops, alphas and betas dancing like puppets on the theatre-like screen of the computer. Every time she steps into the lab she hears a voice she remembers having heard before, years ago; a voice she once imagined addressed to her. And what if...?

From time to time, though, our ethnographer must return to her community of origin. Searching for shelter, for a split second of calm, running away from all these questions. But, surprisingly, on coming back 'home' questions do not diminish, but increase in number and intensity. She is not alone in her interrogations, she can recognise 'her' biologists in her voice -the voice of her half-adoptive community talking as an almost generalised other of reference: "is this your home, Psycho? Is this what you do? And what does 'being' here mean? Come on, Psycho, answer to us. Is your work rigorous? What is your method? What are you doing for science? And for society? What is this knowledge useful for? Well, are you actually producing knowledge? Where are your publications? And your hypothesis? What do you mean you are not quite sure?" A pause, she needs a pause. Our psycho-ethnographer gasps for fresh air.

Talking with several communities simultaneously, she does not know to whom she must answer. She tries to resist being dazzled by the now slightly less mysterious securities of the lab, but still cannot help but feel undermined by her own insecurities. More things do not fit. If she belongs to the first psycho-community, why should it bother her –as it does- that she is not considered worthy of the second? Why is she upset when people in her department talk of 'her' biologists with frivolity?: "Tell them that...". "Tell *them*? And what if 'them' is also 'me'?" If she imagined coming back home, she now realises her membership work has just started. Has this house ever been her home? Trying to go back without finding the path, the stability of the landscape blurs: some of the places, so familiar before, become unrecognisable now.

Less and less convinced of which community defines her agency, she wanders in a dangerous in-between: agent of the psycho-community of origin or double agent of the biocommunity of adoption? Who sends her? Where is she coming from, where is she going to? Where does she belong?

While drifting, she clutches at the only things that sustain her, at the only belongings she can call hers, her field notebook and her pen.

Part I: Opening paths

> "That every beginning is difficult holds in all sciences"
> (Karl Marx, p. 295).

> "Yo les diría que en realidad no hay una sola pregunta que les sirva de fundamento, sino muchas y que en gran parte surgen en el momento mismo de la propia investigación. Porque, en efecto, quien parte de una sola pregunta se arriesga muy a menudo a hallar una respuesta previsible"[1]
> (Interview with Ginzburg, 2001, p. 96).

1. "Once upon a time": introducing introductions

It is never easy to write the first sentence of a text; it is difficult both to conceive (what sentence can gather and convey the exact amount of interest and intrigue - enough at least to inform and attract the reader?); and to write down (the first step of a colonisation of otherness with no way back). The first fear can only be dealt with by writing, so instead of searching for the perfect sentence (search which will probably bring us to a halt), I will risk a simple, first sentence to set us in motion:

"Once upon a time, there was a laboratory".

Done. Granted, the sentence is not so attention-grabbing as "It was a dark and stormy night" (see Curt, 1994), but it might help us avoid the conundrum in which Schutz's (anti)hero is caught: getting stuck in an endless one-sentence novel. One problem less, with the added plus that this banal sentence helps us overcome the second fear: too late for regrets. It is the belief of these pages that there is never a desert beach, a space outside colonisation. We are always already crossed and constituted by relations, colonised by absences which are present in us. In this sense, the appropriation performed by the text you have in your hands is less of a sacrilege than

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[1] "I would tell you that actually there is no single question that works as grounds [for my books] but many, which emerge during the very research. Because, indeed, who departs from an only question often risks finding a foreseeable answer" (Interview with Ginzburg, 2001, p. 96).

it seems. It is not an invasion of an alien space, an other far away, but an acknowledgment of the entanglement in which and through which people populating these pages and I have been mutually constituted. In this sense, it is as much an act of power as one of surrender –of 'giving in'.

"Once upon a time there was a laboratory". This text presents an ethnography of a research laboratory in biology and biomedicine. As such, it looks for shelter within, and hopes to contribute to, the tradition of Studies of Science and Technology (STS), in particular, to the so called "laboratory studies". Which means that this thesis undertakes the impossible mission of trying to say something new about a field that has been exhaustively harvested. At the same time, on dealing with technoscience[2] it also attaches to an emergent concern within social psychology (Domènech, 1998; Domènech & Brown, 1999; Domènech & Brown, 2001a; Domènech, Íñiguez, Pallí & Tirado, 2000; Íñiguez & Pallí, 2002). This is the simultaneously protective and paralysing context that signifies our contribution, which, as it will be clear, kneads an heterogeneous dough, mixing field notes and memories with a social psychological look, anthropological tools, literature in STS and a pinch of philosophy.

Whilst the aim of the research *as a team* was pretty clear, I entered the field without knowing on what particular topic I wanted to write my personal project, and this became one of my urgent tasks during fieldwork: to find the topic of my research project and thesis. What to study, without reiterating actants, cyborgs, constructions, modes of ordering, failed replications, and interpretative repertoires? I was in the dark[3], not knowing what to do but to grope: observe, ask and write. In moments like this, though, one is confronted with the charmingly unforeseeable character of life. While I was suffering seeing the days go by without my reaching a decision, certain events were already opening up a direction for me to follow; or, better, a direction I

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[2] From now on I will write 'science' instead of the longer 't echnoscience', even though with this we do not want to imply the inextricability of science and technology.

[3] I am aware of being quite careless by using this expression, that suggests an identification between knowledge and light and ignorance and darkness. Such links have radical consequences when thinking issues of blindness, for instance. On such topics, see Schillmeier, 2004.

had no choice but to follow. Relationships established with members of the IBF created such intensive experiences, that I felt strongly compelled to reflect on them.

It is no secret that ethnography confronts one with something other than oneself: the whole point is trying to approach and report about a group from an alien perspective (or 'self-alienated', if the author does belong to the group studied). In my case, this task provoked a series of reactions that brought me to pay attention to the ethnographic relation itself. Much as I tried to analyse the data in a more STS-style, I kept on going back to the relational aspects, and, in the end, I had to accept that I would not be able to approach the analysis of our data without trying to make sense of what had happened between us previously. Thus, after fieldwork -and making use of the freedom my department had given me to take up a long stay in Reading (UK)- I put by side momentarily (with a certain bad consciousness) our field notebooks, just to surrender to the compelling urge of writing about the ethnographic journey. This topic emerged so intensively and so spontaneously that I never felt as the steersman of the ship, but rather a shipwrecked person on a small raft sailing adrift.

The end result of this give-in to the siren chants took the shape of my research project (see Pallí, 2000), a questioning of the very division self-other which is supposed to inform ethnographic encounters. These pages articulated reflections on the (re)presentation of otherness, its relation to power and identity, the precariousness and on-going character of belongings and communities, the existence of an in-between space were relations are still being constituted, and all the monsters begotten by ambiguities and ambivalences. All this was written and structured so that form was also a part of content. I did not want to unify the narrative into a single voice, but tried to produce a feeling of the multiplicity of Cristinas and trajectories and relationships which had been and were constitutive of the experience. In short, if this was self-reflection, it should not be understood simply as thinking about oneself to be critical and aware of one's mistakes, one's position, one's power effects[4], but rather,

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[4] For a debate on reflexivity, its virtues and its excesses, see for instance, Ashmore (1989), Ibáñez (1994), Latour (1988a), Lynch (1982) & Woolgar (1988b).

as taking seriously as material for analysis the metamorphosis that relationships involve.

Once the ethnographic experience had been more or less analysed, I was ready to recover and work on the data collected in the several field notes. But... on what? The long round-about had not solved my problem. However, since data was crying out for (rightly deserved) attention, I took my field notes and started to work, reading and interrogating data, until some themes emerged out of this process. I have no recollection of deciding 'I am going to study this'; rather, it almost felt as if data was speaking up, presenting certain topics we could not dismiss. Of course, we know that data is never 'out there', speaking by itself, but are 'seen' and 'read' by eyes which have already been shaped by theory, tradition and personal concerns. Nevertheless, topics of study kept on appearing with a dynamic on their own, posing their own questions, so to speak, apparently without much relation to the topic of the previous project -which was not necessarily bad[5].

More problematic was the fact that the different topics appeared as quite unrelated with each other, without any main concern animating them. It was not a problem of scarcity of ideas, on the contrary, but these ideas simply seemed to resist my ordering requests. I was lost again among bits and pieces! If I had been unable for two or three years to answer the damned question "what are you writing the thesis on?", it did not seem that my current work would make things easier, the problem being that I did not know whether I could stand several years of further drifting... Meanwhile I kept on working, what else could I do, apart from cursing?

One day I talked to Lupicinio Íñiguez who, after reading my first project, suggested that the notion of 'limit' could unify the variety of topics and perspectives of those crazy pages. Even though saying that my project was about limits set an unwanted

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[5] A simple extension of the project would not do, for the form in which it was written made it almost impossible: the mixture of languages, and especially the way of uniting all the topics in a hybrid, non-linear narrative did not seem adequate any more for the Ph.D. thesis. Not only for the inconvenience of writing a piece of work which cannot be read by many, but also, and more to the point, because the freshness of the experiment would have been lost if I had simply repeated it.

boundary around it, it became clear that it was a comfortable way of thinking and talking about it. Indeed, when I myself looked at my project again, I could see 'limits and boundaries' at work everywhere: every story, and even the way stories were told, could be regarded as attempts to think about liminal concepts. At some point, a suspicion grew in me: what if my thesis was also about limits?

Since I quite liked the idea (and especially, since it produced an alibi to defend myself when attacked by the cruel question 'what are you writing your thesis on?', irreflexively posed by people who otherwise think of themselves as nice), I decided to adopt it, first without much conviction though, let's say it all, but later without dislike. From that moment onwards, when asked about my topic of research I would half-stutter, half murmur 'the thesis is on boundaries and limits'. Whether I liked it or not, these notions were becoming very trendy within academia, and this is probably why such an answer let me get out of the quandary without much problem. People used to observe me from behind half-closed eyes and say 'oh, I see'.

Funnily enough, though, I came to feel truly that this was an honest answer, even though the material I was producing seemed to contradict me –proteins, hybrids with one foot in science and another in politics, deterritorialised angels virtualising science... This was at first slightly bothering: was I inadvertently lying? Claiming to work in a topic disconnected from my real research? Was I simply confused, creating a bad alliance between what I would have liked to do and what I felt obliged to do? However, I did not consider the relation between these two trends (what I was doing and what I thought I was doing) as one of mutual interference; they did not seem to be exactly a disturbance for each other, although they were clearly not working shoulder to shoulder. It was as if the explicit themes were inspired or invigorated by implicit concerns –a kind of hidden motor, or a heart injecting blood, to swap metaphors. As if what I was not working was informing what I was working.

One day I decided that, if I had been working with limits in mind, then my elaboration of the data must have something to do with them. So, I started looking for

them in the already written pages. It did not take me long to find lots, as if the empirical chapters had already been dealing with ways of conceiving and working limits and boundaries all along. With the benefit of hindsight, it seems obvious that my concern with limits had already led my interests when analysing data; but it is not less obvious that data and the whole experience had led my interests towards limits. I will not waste neither your time nor mine trying to resolve the unsolvable and quite irrelevant question of what was first.

What is nevertheless interesting, I believe, is to keep in mind the particular relation out of which both my theoretical concerns on liminality and the empirical elaboration of data emerged: neither the same nor other; connected, but not quite. This is why readers should not be surprised (which does not mean they should not be disappointed) if this thesis leaves them an ambivalent aftertaste, as if something did not quite match or add up, as if the thesis could not produce a smooth mixture of all the concerns informing it, the initial split or gap still lingering behind connections. As if the answer "on limits" was too vague to articulate all the presences and absences inspiring these pages. As if, in short, I had never quite known what this thesis is about. Just in case, you'd better not ask me the damned question...

2. Making our preparations

The process of preparation and realisation of an ethnography –from the moment one starts to toy with the idea, to the final writing of results- is quite long; long enough not to inflict it upon the reader in its totality, and to force the writer to decide what to include and what to cut out. These decisions must be confronted from the very beginning: when should the narration start? Is the reader interested in knowing all the preparations of the study? All the work of searching for bibliography, planning and making contacts? Probably not. A couple of scenes will suffice –a couple of short cuts. First: the thesis you are reading would not exist were it not for the funded project on Spanish science held by my department at the time I entered, directed by

Professor Tomás Ibáñez[6]. This project consisted of several sub-projects, one of which was an ethnography in a laboratory, led by Miquel Domènech and Francisco Tirado. There had not been any laboratory study within Spanish contexts, and the idea sounded attractive. This thesis presents some of the fruits born by it.

Second flash: We chose as 'object of study' the then called Institut de Biologia Fonamental (Institute for Basic Biology), IBF from now on. Why this institution in particular? Many reasons were involved in our choice of the IBF as a suitable laboratory to be observed, some more pragmatic than others[7]. This laboratory is part of our own university, and we considered it interesting to give it visibility. Moreover, it was quite convenient, for the IBF was within a stone's throw from our department (which would save money and time). One more reason was that one of us was acquainted with a researcher of the IBF, a felicitous circumstance supposed to help us being accepted. It seemed, then, that the IBF was our best candidate. Accepted unanimously. Would it accept us in turn?

At that point, I lost track of the process. One of us, Miquel Domènech, made some phone calls, moved some contacts, managed to organise a first meeting. Cristina the Ph.D. student had nothing to say or do in this part of the negotiations. I re-entered the process when our first 'informal contact' was already organised. A meeting in the IBF. As agreed, one day we left our department and walked for some minutes until reaching the building that, everything going fine, would offer us shelter for some time. We approached it with a mixture of emotion, curiosity, respect and apprehension.

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[6] Research project nº PB94-1521 financed by the Dirección General de Enseñanza Superior DGES (Ministry of Science and Education), *Analysis of the psychosocial factors which influence the diffusion and projection of Spanish scientific activity.* Participants were Miquel Domènech, Joel Feliu, Ana Garay, Tomás Ibáñez, Lupicinio Íñiguez, Luz Mª Martínez, Cristina Pallí, Margot Pujal, Joan Pujol, Francisco Tirado, Félix Vázquez.

[7] The time is gone -one hopes- when people had to fancy-dress their pragmatic choices with the appearance of disinterested methodological matters. Today, to phrase it better, many already admit that pragmatic decisions are also and already methodological.

We had some minutes left before the meeting, so we took a short walk around the building to size it up, trying to gage what we would have to test ourselves against. A kind of 'reconnaissance flight': grey, concrete, few windows, land without building next to it. "Don't you think it is rather smallish?" –asked one of us. I guess he had in mind other labs described in the literature; indeed, compared to Daresbury (Law, 1994), La Jolla in California (Latour & Woolgar, 1979; Latour, 1987), labs in Berkeley and Heidelberg (Knorr-Cetina, 1981, 1999), or Traweek's (1988) national labs such as Stanford Linear Accelerator, the IBF looked rather smallish. But for me, with a fabulous conception of a laboratory (literally fabulous: a small room full with colourful, smoky, bubbling fluids spilling all over), this building was huge. I have this image engraved in my memory: standing on the road below it, I had to lift my head to encompass it all with my look: All this? Did we really have to study all this?!

The next scene shows us sitting around a table in the meeting room of the IBF. In front of us, four men hiding badly the curiosity and amusement with which they were listening to our comments –I suppose they were also trying to get the measure of these three people, with whom they would have to spend some time if we were finally accepted. We sat face to face, the physical distribution contributing to our split into 'we' and 'they' -mutual examination, then, feeling vulnerable to their inquisitive look. We tried to look honest, interested but not threatening, kind and intelligent at the same time (but judging from their badly hidden smiles, I'm not quite sure we succeeded). They wanted us to tell more about our plans and intentions: why on earth did three psychologists want to study a centre of biological research? What kind of methodology would we use? How could we, social scientists, understand the research of natural sciences? How could we understand their work without knowing biology? How long and how often would we need to come? How much attention would we require? (Which translates as "how much would we bother?"). What would we do with the information obtained?

While answering all their doubts, we negotiated the conditions of our work. Importantly, we required a couple of Virgil to accompany us in our route through hell

(concealed laughter again), so as to introduce us to people and procedures plus free access to spaces, documents, papers, meetings, etc. We assured them we would not do covered research, but would ask directly every time we wanted to know something; and that if at any moment there was something for us not to see, not to hear, not to know (or not to repeat aloud), we would also respect this. We emphasised that we were interested in watching how Spanish science is done, that is, how *good* Spanish science is done, and that we had no intention to look for gossip, mistakes or failures. Above all, we had no intention to put the IBF in a compromising situation. On the contrary, we would do all it was in our hands to contribute to the prestige of the centre. Relief about this point, smiles became wider. Once reassured, they emphasised the importance of collaborating among colleagues of the same university, and accepted our deal.

We were in. We were in and trapped: no way out, a whole ethnography to do! And now what? Mixed feelings: one the one hand, lots of excitement: people looked so nice, their experiments so interesting, and the ethnography so challenging! On the other, fear: would I be able to? Could I cope with everything while doing my Ph.D.? This mixture of feelings would not abandon me during the whole ethnography, not a single moment. Nicely trapped. You must be thinking that I am a bit theatrical now: after all, this is just a research! But then, maybe you don't know how dangerous ethnography is. Escape is not so easy as it seems; an ethnography hunts you, it catches you; it forces you to face an other with which you become enmeshed. Contamination, the other gets in you. Working with gloves is of no help here, there is no protection to get out of it immune –and if you do, bad sign... You maybe don't realise it while you are busy working in a hurry, taking notes before you forget what you have just seen; but then, some when, there comes the end, the last day of fieldwork. There is a morning in which you wake up and think "Oh, jolly good. Today I do not need to go to the IBF anymore". But then, you don't quite know how, you find yourself there once more, saying bye-bye to people again, more kisses, more hugs ("hey, Psycho, are you still there?") Postponing the moment of your exit. ("But it is not for good, I'll come back, sure"). And that day you realise you are done for...

3. In the field

After receiving their permission to visit the laboratory regularly, we started our fieldwork. At the very beginning, visits were rather short –say, a couple of hours-, to ease the assimilation of new information. The number of hours spent per visit increased regularly, and by the end of the fieldwork, I used to spend morning and afternoon there. The progressive increase was for us the best way to become acquainted with the new context and new practices without feeling exhaustion, despair or being completely overwhelmed. It soon became clear that we were able to obtain more information if we stayed longer observing all the processes taking place one day, than if we were present more often but for shorter periods of time. Longer stays are easier to assimilate to their 'natural day': members of the IBF tend to arrive in the morning, most of them reasonably early, have lunch at university and work until late afternoon or evening. Since experiments at the bench take time, one can only understand them if one witnesses its progress, and not if one catches short snatches without continuity.

Although ideally we should have visited the IBF every day, as (most of) its members (mostly) do, our simultaneous work in our department made it impossible for us. The frequency of our visits stabilised itself around three days per week -or two, depending on our availability- alternating the weekdays (one week we went Tuesday and Thursday, another Tuesday and Wednesday, another Monday, Wednesday, and Friday, etc.). At the very beginning we went to the IBF the three of us together, then in groups of two, and finally, once our levels of trust and self-confidence were higher, alone. Each member of the group took his or her own field notes. Recorded collective discussions ensued.

As we will later on see, researchers of the IBF have a strong rhetoric of vocation, with which they quite often justify the need to spend many hours a day in the lab, and especially, to go to the IBF at unexpected hours and unusual days. They tell stories of

people 'caught' at work at night, at weekends, at Christmas and during summer holidays. In a way, researchers who limit their working day to eight hours, always within reasonable hours, and respect weekends and holidays, are labelled as having a low level of commitment. I included these 'extra-moments' in our observation: I stayed some days until late and went to the laboratory during holidays. My presence there was acknowledged, and helped me to perform 'one of us-ness'. I did so partly to get to know how work during holidays develops, and partly, I suspect, to make sure I was seen there, as a way to secure my 'levels of commitment': the difference between trying to pass as 'native' and 'going native' is sometimes rather thin...

We used to spend most of our time observing what they were doing. When they were very busy or needed to be very concentrated, we would remain silent or leave them alone, so as not to bother them. When relaxed, either we would ask them to tell us what they were doing -and why- or they themselves would offer explanations spontaneously. Most of the researchers had the ability to explain their tasks easily enough for us to understand them in spite of our little specialised knowledge. At least in experimental laboratories, researchers are used to teaching, helping each other and instructing beginners; in this sense, they could easily fit our presence and 'demands' into their habits and routines. Which also means that our presence there was not as disruptive as it might seem. After my having spent months in one laboratory asking about procedures and techniques, one day one post-doctoral student heard me make a comment about psychology, and suddenly cried out "oh, now I understand: you are one of the psychologist! I thought you were a Ph.D. student asking for help!" This anecdote illustrates how normal teaching situations are for them, as we will later see.

During fieldwork I wrote down in my red notebook at the end of every visit everything I could remember. I made use of a second, smaller notebook, where I could take fast notes, write down some crucial information I should not forget, or capture important literal expressions. Since I had some spare moments occasionally to write notes in the library, at first I used to carry both notebooks around. After a while, though, I could detect some half-scared, half-curious looks thrown at the big,

red one[8], and I thought it sensible, given our promises of confidentiality, to leave the diary notebook at home, and use only the small one when necessary. When writing at home, I made a particular effort to reproduce comments with the same words used by people. But needless to say, I cannot claim that they are literal (surely the reader does not attribute the same credibility to my memory than to a recorder). Unfortunately, as it is easy to understand, people felt intimidated if I wrote down or recorded all they said in front of them, so I sacrificed accuracy for an ease of communication. Information and insights extracted from my notebook have been melted in the text without further indication. However, when I reproduce particular comments made by people, I leave them in quotation marks, without reference. (It does not make sense to give a reference of the diary, since this is not a source that can be consulted or contrasted).

What we did record were interviews conducted at the end of the fieldwork with their consent and participation. With the exception of one person, the rest accepted immediately. The head and seniors of the groups were interviewed individually. A group interview per observed group was chosen as the best option to gather the opinion of all Ph.D. students; besides, collective discussion proved productive. The exception was two Ph.D. students who were interviewed individually because they were abroad the day of the interview with their colleagues. In one case, a Ph.D. student who had participated in a group interview came to us afterwards and asked us whether it would be possible to add some more comments but in private, to which we agreed. This person did not object to being recorded, simply to being heard by the rest of the group. We also carried out interviews with the secretary team and the two janitors individually. Interview extracts will appear in quotation marks and accompanied by an indication of the number of interview, type (individual/group) and page number of interview they are extracted from. Quotes have been slightly modified to facilitate comprehension.

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[8] I had promised that nobody would have access to my notebook, over which I had to keep a certain surveillance. This gave rise to a certain mystery as well as mockery around it.

Very few interviews were led by the three of us, most of them by two of us, and some by one of us. With one exception, we were quite satisfied with the interviews: the atmosphere was relaxed, people seemed to feel comfortable enough to participate and interrupt each other in vivid discussions, they felt confident enough to express opinions about everything we asked, even when their opinions expressed some criticism about some people or dynamic of the IBF. Needless to say, we granted confidentiality (not only during the interviews, but also during fieldwork). We had promised we would not reveal who said what, that is, quotations would be anonymous *and* we would not tell other members of the IBF what was said. They were offered the possibility to interrupt the registration in case they wanted to mention something that they did not wish to be recorded; only in two occasions in two different interviews were we asked to do so.

Next to participant observation and the interviews, we also gathered some documents from the field –leaflets, maps, photocopies, references and papers published by them. To interpret some of these documents we required help too, papers were rarely understandable without their explanations. We also had authorisation to record with a video camera. We used the camera in a couple of situations, to record the way they used equipment, their explanations of some techniques, as well as empty spaces and their progressive colonisation by a new group (I will let you imagine their faces and comments when they caught us recording empty spaces...). Nevertheless, the use of the camera did not fit so naturally as taking notes. Both, people recorded and especially I, seemed to be quite bothered by the presence of this device. Had I had enough patience and time, we could have explored more possibilities with the camera, but I must admit that, at that time and given the amount of information to handle, the efforts that a more intensive use of the camera would have demanded seemed not worth it. This is not necessarily what I think today, though. Ethnographies share that much with life: one wishes one could start them anew with the experience one has on finishing...

The IBF was constituted by several groups. It soon became clear to us that it was completely impossible to observe practices in all of them. We needed to select some of the processes which interested us, and dismiss the rest, although it was not clear to us on what grounds we should do the selection. In the end, our proximity to the director of the Institute decided. He was the person closer to us –the one who took more care of us, so to speak- and the first in offering entrance to his group. This meant that we started our analysis observing those laboratories which were more intimately connected to him: the laboratory of Protein Engineering and the laboratory of Biocomp uting.

Later on we were invited by Ph.D. students and seniors to enter the laboratory of Molecular Biology. Members of this last group kindly pointed to us we would miss a lot if we stayed only in one laboratory ("how come you don't visit us?"), and they offered us help. This was one of the biggest groups in the IBF, together with the director's. In spite of the amiability of its members, many of whom spent time telling us about their techniques and research, we did not stayed so long in this lab observing practices as in others. On the second half of the ethnography, after a kind invitation by one head (who reminded us we had promised to visit them but still had not done it), we entered another laboratory, that of Applied Molecular Biology. Again, we felt warmly accepted there, and spent several weeks watching their bench work.

Parallel to the analysis of these labs, I visited from time to time a fifth one, at their own request ("are we not worth a visit of the psychologists or what?"). We had not planned to visit this lab in particular, for their practices were quite distant from the rest of the techniques we had been learning; they were the only ones who did not recreate a world within the lab, but tried to bring the lab to the world (or rather, to turn the world into a lab -Latour, 1999; Stengers, 2000). If I was to repeat the ethnography today, (especially after having read Stengers), I would certainly pay more attention to this group than I did then. However, at the time the group was not going through one of their best moments, and observation there would not have been easy (a member was about to leave the group, another was doing a visit abroad, the

other spent most of his time teaching; the heads of the group were never there physically). Even though I never did a systematic study of this laboratory, I dropped in from time to time to talk a little bit with some of its researchers, with whom I was in very good terms, too. I learnt from them a different perspective, a different circuit of relations within the IBF, a different vision on the centre, stories told from a less heroic point of view, not only by them but also in other groups.

Protein Engineering, Biocomputing, Molecular Biology, Applied Molecular Biology[9]. These were the four laboratories which we explored more intensely. They all share a characteristic which could be worrying, and it is at least worth mentioning: these four laboratories were amongst the biggest, led by three of the most powerful heads of the IBF (the director and vice-director of the centre among them). Even though they did not need to coincide in their strategies and affinities, they constituted a strong sector of the IBF, the sector which more strongly determined the direction in which the centre should move. But we know that other heads would have wished a different equilibrium of forces within the IBF, heads who had other ideas about how some things in the IBF could be done.

Every centre has divisions and disharmony at some points and moments. If I mention this split among the heads in the IBF is not to provide food for gossip, but just to warn of the partiality of our perspective on the IBF. The vision of the institution we deliver is strongly marked by the information received by members (heads and otherwise) belonging to these strong laboratories, just for the simple reason that we have gathered most of the information from them. We had contacts with members of other groups, too; out of the laboratories remaining, we had a very good relationship with the head of one team, and with the seniors and Ph.D. students of another. Their opinions have been picked up basically through interviews and informal conversations and I do not need to say that they have been given exactly the same weight as the opinions of other members. There is something I strongly regret,

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[9] The lab of protein engineering was also known as 'Enzymology'. Even though the second lab was called "computing biology laboratory", in what follows we will use short denominations such as 'biocomputing lab', or 'bioinformatics lab', as members of the IBF do.

though: we did not establish contact with the head of still another group whose presence was quite visible. In short, even though several voices were picked up, not all of the different visions of the IBF were given a space. And this is a partiality which must be kept in mind.

I cannot emphasise enough how warmly accepted we were by members of the IBF. They dedicated time and attention to us, and answered all our questions with patience and understanding. I also think that, at moments, they had fun with us: our research was quite an event for them, and I know that they told friends and ex-members of the laboratory that they were being studied by psychologists. I kept on meeting people visiting the IBF who suddenly asked me, "oh, so you are the psychologists, aren't you?" We triggered off lots of curiosity and rumours, and, in a way, it was a sign of distinction to have the psychologists interested for what they were doing. People complained if we did not observe them, and cordially invited us to join them.

But at other moments our presence there was something to hide or at least ignore; when some authority from the Catalan government or some invited speaker from some important laboratory came to visit the laboratory, they were never told about our research. Whereas for many members of the IBF we were a source of amusement, for the heads, when it came to serious stuff, we had to remain a non-presence -which was totally understandable, of course. I also know that at some moments we were a nuisance; we tried not to bother them in bad moments, but I suppose that one unavoidably disturbs the normal course of things. They also knew they could tell us "now is not a good moment, we'd better talk later" -and as a matter of fact, they did it when necessary.

We also got to know, because nobody tried to keep it secret anyway, that one of the heads opposed our staying at the centre; he said it openly to other heads before our arrival and his attitude did not change after meeting us (actually, we were never properly introduced, nobody dared! This is how we noticed that something was wrong). In spite of his disagreement, he respected the majority in favour of our

research, and tolerated us there without ever being unkind. He knew we were studying his groups too, but never made a move to stop us. I cannot say I do not understand his reasons for not wanting us there. He thought that a) our research was quite pointless, b) we would make his people lose time, c) we could endanger the confidentiality of some of the results.

I tried my best to reassure him that he should not be worried about the third reason, since we had no intentions to communicate the content of their tasks to others; I could not 'deactivate' the second reason, it was obvious that, on asking doubts, we distracted researchers from their tasks, but we tried to distribute our 'nuisance' equally among people and labs, not to bother the same person for too long, try to pose the questions in the most appropriate moments, waiting for people to have breaks or cigarettes to ask some questions, etc. I considered the question about the value of our research to be none of his business, so I did not try to justify us in that point. Even though I appreciated him not being hypocritical to us, and I am grateful to him for having accepted our presence not only in the IBF but also in his groups, I regret having missed his collaboration, for everybody told us that he was quite an interesting and theoretically well-read man. Unfortunately, he never gave us the chance to check that out for ourselves.

The rest of the heads received us cordially. None of them made it difficult for us to observe and interview their people, and some offered us explanations and help. However, with the exception of one head who worked at a table in his laboratory, we were never invited (and we did not insist) to their offices. Our ethnography is therefore very limited to those practices which happened in open spaces or behind open doors: corridors, labs and the library.

Unlike other populations, which have grown used to the continuous presence of anthropologists –for instance ¡Kung people in Africa (Shostak, 1981); the Ticuanas in the Columbian Amazon (Pastrana, 2002); some settlements of Gypsies among us (Crespo, 1998, 2001)- the group of people we wanted to study had never received

such a visit. In fact, they had only a vague idea of what an anthropologist was -let alone what social psychology was- and had not heard much of ethnographies. This was of course an advantage, they did not have fixed expectancies regarding how our work should be carried out, or the kind of relationship or transactions we would have to establish –we were very fortunate, if a joke is permitted, not to have to thank our informants with money or tobacco! Which is not to say, though, that they did not have expectancies about us at all.

Most of them were holding two different preconceptions, which, in spite of being contradictory, they could simultaneously sustain. The first one was that, being psychologists, our function was to detect individual and personality characteristics (or disorders!). Accordingly, and especially at the beginning, they kept on asking whether we thought they were crazy, or what conclusions had we reached about their personalities. Nevertheless, since they all noticed that we did not go around asking personal questions, but were on the contrary dedicating most of our efforts to their work, they gradually abandoned the idea that we were there looking for pathologies or exceptional characters (although I have my doubts they really got rid of this idea completely).

The second expectancy was more harmful for us: they thought we were there in order to detect their mistakes, errors, biases, their deviation from good science. They never quite believed us when we said that we were interested in how science (that is, good science) normally works, and they remained suspicious that we were hiding some shameful or dangerous motive for our study. If you add to this the uncertainty as to who would read or have access to the collected information, the setting is ripe for the emergence of the figure of the 'spy' or the 'tell-tale'.

I know what you are thinking: of course we told them this was not the case! I told them, and told them and told them again, and I promised and swore. But this is exactly what I would say if 'betrayal' was part of my plans, isn't it? How do you convince them of the contrary? Granted, after a while, they surely realised that I was

not mentioning or repeating to others information which I learned, and I would like to think that eventually they felt secure. All of which, I hope, helped to weaken their belief on our 'hidden motives and missions'. And indeed, many of them ended up confiding us delicate information, which they would not have done if they had not been sure of the confidentiality of the data.

Nevertheless, I also know that in some occasions I was concealed what they considered 'dangerous information'. I sometimes caught the hesitating tones of those who in the last moment decided not to tell me something (which I already knew through others, for instance). Or the hidden signs done on my back (no, don't tell her this!) which I could read in the eyes of the interlocutor before me. And even though this lack of complete trust was somehow disheartening, I have to admit I understand it. I would probably have done the same. After all, who was I? A suspicious social psychology (whatever this is), quite an incompetent Ph.D. to make it better (who could not even explain what her thesis was about), accompanied by two more social psychologist whose status was never very clear (the lack of an explicit hierarchical structure disoriented them), whose thesis supervisor was a member of the team of the vice chancellor!! So, who could blame them?

In any case, I must keep what I promised: no real names will be mentioned; compromising information will not be written. To prevent problems or inconveniences, the description of the characters has been altered and deformed in order to make recognition as difficult as possible. When we narrate some concrete anecdote or event, we have normally respected the status and the gender of the characters intervening, because we thought they were important factors the reader needed to know. However, when we talk in general of the figures populating laboratories, without referring to actual situations, we will keep, like Hess (1992), a generic feminine - not so much out of conviction that this is the best strategy, but because it is a solution which does not make reading arduous for the reader[10]. Given

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[10] Other formulas proved both disorienting and quite a nuisance, such as "she or he does this so that others can help her or him"; "s/he would do it". I even tried characterising some figures with the same

the strong gender stereotypes associated with the figure of the scientist, we thought it better not to use a generic masculine.

Gender

And now that we touch on gender, some clarifications may be needed. There is no doubt that gender plays a role in science. After so many years of research, we know that there are several layers affected by gender[11] -the constitution of subjectivity being probably one of the most impacting of them. Nevertheless, gender, as such, will not be directly discussed in this thesis. Which is not to say that gender did have no influence on the ethnography. On the contrary, I firmly believe that the whole process, including data, was strongly marked by my position as a young, female Ph.D. student. (Which means that when considering power relations, one has to take into account both factors, age and gender, plus, to put it in statistical terms, the interaction between them).

Whilst there is no denying of how drastically gender participated in the constitution of our relations (as well as how gender was constituted through relations), it has been too elusive for me to analyse and make this process explicit. Partly because one cannot reconstruct the useful fiction of being the (partially external) observer (Stengers, 2000, ch. 8) as easily as with other issues: one's being is completely caught in what one is trying to make intelligible. To express it in Heideggerian terms, gender is, simultaneously, ontically close to and ontologically far from, our being: "what we can get clear about is only what is least pervasive and embodied. Heidegger

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generic gender respectively, as referring to all 'Ph.D. students' and 'seniors' as 'he' and 'post-docs' and 'heads' as 'she'.

[11] "Over the last fifteen years, these words [gender and science] have come to be used as an umbrella covering three distinct (even if related) efforts: a) studies of women in science; b) analyses of scientific claims about gender; and c) studies of the roles that cultural norms of gender have played in the historical development of science" (Keller, 1998, p. 17). Some works have put in evidence quite obvious effects, such as, for instance, the number of women, in comparison to men, who decide to choose a scientific degree first and a scientific profession later[11] and actually manage to become a scientist (Conefrey, 1997). Or how preconceptions shape scientific knowledge, reproducing gender stereotypes. More subtle, it has been shown how values and epistemological attitudes within science are gendered (Haraway, 1981, 1985; Harding, 1986; Keller, 1984; Longino, 1990).

has the sense that the more important some aspect of our understanding of being is, the less we can get at it" (Dreyfus, 1991, p. 22).

Indeed, what looked from the point of view of 'gender roles' or 'equality of opportunities' looks like a reasonably neutral situation can reveal itself as quite a complicated assemblage enmeshed with knowledge, power and self, to use Deleuze's (1997) tri-partite description of Foucault's concerns. Indeed, whereas I could not claim to have been 'excluded' or 'discriminated' as a women, I nevertheless felt trapped sometimes, constituted within certain margins difficult to challenge. Simultaneously, I could often recognise how I myself was contributing to my own constitution as a gendered being, sometimes to my advantage, but not always. (And this is, definitely, another reason why gender is so difficult to approach...). A decent treatment of these issues would require more efforts (both in terms of time and literature) than available for me at present, and this is why it will have to wait for a better occasion –lest I risk simplification. I can only make clear that my inability to approach explicitly these issues should be taken as the best proof of their overwhelming presence.

What I did explore was the existence of gender discriminations. I searched for confidentiality and complicity with women; I asked implicitly, I asked explicitly, but people were very reluctant to admit differences within the IBF attributable to differences between men and women. In this sense, gender did not dye their ethos as intensively as reported by Traweek (1988) among particle physicists. This does not mean that women did not feel discriminated or marked as 'women', but, according to women's most frequent answer, not more than in the rest of spheres of their lives. None of them ever admitted to being discriminated for being a female scientist, even though they felt so for being *working women*; the obligation to perform at work without their results being altered by 'family affairs' (marriage, house, children) was bitterly resented.

There was a women who explained to me how she had to come to work to the laboratory in the early morning so that she could be back at home at 8:00 AM to get her children ready to go to school (while suffering the ironic accusing comments of colleagues that could only see her arriving too late in the morning and leaving early in the evening). Men were also affected by difficulties in carrying simultaneously the weight of work and family. However, stories differed slightly; for instance, some heads and seniors were said to endanger their marriages by staying until too late in the laboratory. In that case, the tension seemed to be experienced on the personal sphere, whereas for women the weight was more noticeable in the professional one[12]. Due to obvious reasons of age and family responsibilities, these situations affected more seniors and heads than Ph.D. students.

Rumour had it that one of the heads preferred to choose female members for his group, whereas another was said to be quite misogynous. However, both groups had members of both genders; women belonging to this last group denied any kind of pressure, mistreatment or discrimination, on the contrary: even one of the seniors was a woman (which was not the case in the group allegedly preferring women...). There was one situation, though, where gender was said to play an important role: promotion within academy. It was said to be more difficult to reach the position of professor if you were a woman than if you were a man. Whilst I know I do not have to make efforts to convince my readers that this is so, I hope they will understand that the topic of discrimination within academia was beyond the reach of our study.

What should not have been beyond, however, was the fact that the division separating influential heads from not so influential heads coincided with gender divisions –or bluntly put: the first were men, the latter women. Some of the female heads had avoided confrontation, and had even decreased their physical presence in the building. Others tried to do their best to assure their positions in the laboratory, and be sure their voices were taken into consideration. To imply that this distribution is random would be as ludicrous as offensive. Nevertheless, due to the causes I have

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[12] For an attempt to observe how people negotiate differences between these two spheres, while

mentioned before (lack of time, bigger dedication to lower levels of the hierarchy, plus a certain degree of disorientation and negligence) this work will unfortunately not touch this issue.

Pronouns

The reader of these pages may have noticed two oscillations by now. I have been using the pronouns 'we' and 'I', and I have been using present and past tenses. This requires some comment. Let us start for the first. The choice of personal pronouns has been related to epistemological positions –in particular, to the need to make the trace of subjectivity more or less visible (e.g., Woolgar, 1988a, ch.5): whereas natural sciences have tended towards an impersonal, neutral, impartial 'we', to refer to the scientific community and erase the presence of the particular, embodied knower, *some* streams within social sciences have emphasised the presence of the 'I', in an attempt to make visible the subject, the indivisibility of the object and the subject, the impact of the let's call it "social baggage" the author enters analysis with (in either version: cultural prejudices a là Gadamer, prejudices a là individual social psychology -attitudes, values, culture, biases, gendered perspective, particular locations, subject positions, etc...). However, I will not refer to this debate, and I hope that at the end of this work it will be quite clear what position I share. Here I want to call attention towards a different topic: the collective nature of knowledge.

The work you have in your hands is, partly, an individual work. This thesis has been written and signed by one person, and only one person is directly under scrutiny now: I will be made responsible, in fair justice, for mistakes and incoherence. Nevertheless, any attempt to present this work as *just* my individual work would not only be a conspicuous lie, but an inconsiderate lack of respect towards all those whose work has been consumed and swallowed in my route. I have already mentioned many people who must be thanked for their help and support. But now I am talking of a more intimate entanglement: there are some people who are so enmeshed with this

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constructing them simultaneously in relation to gender, see Capdevila & Pallí, 1998.

thesis, that one could say these pages are theirs as much as they are mine. For a start, all the members of the IBF are not only talked about, but in-corporated in a radical sense here. They are being constituted by my perspective-dependent account, but they also constitute these pages, and constituting me in a sense I will try to clarify later on. These pages, then, are their belonging as much as mine (whether they feel happy about it or not is another issue...).

Members of my department also own this text. Cristina, the social psychologist trying to make herself a place within academia is a partial product of my department: arguments, ideas, discussions, problems, insights... this is so obvious that it should not need much comment. In special, two names need to be mentioned: Miquel Domènech and Francisco Tirado, responsible for the ethnographic team carrying out the research which will be described next. Their ideas, support and patience, generously given in a range of situations from group discussions to coffee pauses, are the material out of which the arguments of this thesis grow. Regardless of whether they agree with everything I say, and after insisting in being made the solely responsible for incoherence, I must also make explicit that this is, in a strict sense, their work too. Hence, the 'we' (this time, very concrete and tangible) with which the 'I' intermingles.

The Times

In this ethnography, several moments are woven, intertwined; whilst this is probably the case in every ethnography, the long time span through which its interpretation expands make these pages rich in temporalities - "*temperies*: wise and prudent mixture of times; time to love, time to die, time to sow, time to reap" (Duque, 2000, p. 98, our translation). Not in vane the etymology of the word time takes us back to mixture, medley, miscellaneous state (Serres, 1994, p. 95). Which means that not only times, but also memories are woven together in a folded narrative that constructs a past, a present and perhaps a future (Vázquez, 2001). Reflections during fieldwork

draw on a kind of memory of direct experience and 'fresh data' which constitute a different texture than my analysis in Reading, based on an interpretation of the memory constructed by field notes, feelings and recalled events -whose mixture, in turn, cannot be the same than the memory that sustains me in Munich, where other reflections, experiences join the pack, together with the memory of my previous memories... Time is a tissue whose density varies and wraps the narration in different ways. I do not have time to go deeper into this issue (you see, after so much time, there are topics which will remain untouched), but it is only fair that one points to this characteristic, for it is definitely playing an important role in the way interpretations are constituted.

There is, however, another issue related to time I would like to discuss. One day, while re-reading some of the pages I had already written on the IBF and its groups, a doubt assailed me: I realised I had been writing in present tense: "The IBF *is*; this group *works* like this; the structure of the laboratory *consists* of". This may appear normal, but it is not that obvious; can I talk in present of the institution I am portraying with data gathered in 1997? Since we left, many things have changed in the IBF, and they do not exist any more as I experienced them. Time went by, and some things followed along. For a start, the name: we carried out ethnography in the IBF, now the IBB stands in its place (Institute for Biology and Biomedicine). Only a new name? I doubt it: the change of name responds to some well-thought reason, a change of strategy, a change of collective spirit or identity.

More examples: many of the Ph.D. students I met have already left the institution, and this means that more than half of the population I met and interviewed is gone. The organisation of some groups may have changed, and I run the risk of explaining the functioning of structures which do not exist anymore. Some of the hesitations and ambiguities provoked by the emergence of new groups and new ways of working may have been solved (or not) during this time; what were novelties then, are not novelties anymore, and it is possible (but not to be assumed) that ambiguities have blurred, so I may be describing organisational ambiguities which have been

smoothed. Last but not least, the director of the IBF we met, whose scientific career was intrinsically united to the centre, is not director anymore.

And still, despite all these good reasons to portray the IBF in the past, I did in present. I had not deliberated about it, I did not feel the present tense was 'a choice' -not a conscious one at least: I started writing, and verbs came out spontaneously in present. That a (non)choice is spontaneous does not mean it is trivial, though. Actually, the choice of tense may reflect particular epistemological positions, and indeed, this question has stimulated some debate within anthropology. In particular, the use of the present tense known as 'ethnographic present' –a kind of 'historical present' which describes the object of study in present- has come in for some criticism. The next paragraph is a good summary:

> "The anthropological discourse has been marked by an extensive use of what is known as the 'ethnographic present'. It implies the use of the present tense as the dominant mode of representing the others. The use of tense has been seriously criticized as reflecting a particular relationship of observation and distancing to the object (Fabian 1983: 86). It has been described as a vague and essentially atemporal moment (Stocking 1983: 86), reflecting the ahistoric or synchronic pretense of anthropology (Crapanzano 1986:51)" (Hastrup, 1990, p. 14).

To start with, the use of the ethnographic present might be interpreted as an unwarranted assumption about the stability of the object of study: what one tells of the object was valid at the time of observation, it is valid at the time of description and presentation, and it will remain valid afterwards. Therefore, it "unduly magnifies the claim of a statement to general validity" (Fabian, 1983, p. 1980). But Fabian's strongest critique, extensively exposed in his book *Time and the Other* (1983)[13], is

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[13] This author denounces how, influenced by evolutionism, we have constructed a naturalised notion of Time which has allowed to translate cultural 'difference' into 'distance' –both temporal and spatial: if we are in the present –as we feel ourselves to be-, if we are constituted by the forces of the progress, then, the temporally distant other must be in the past; we are at the top of the arrow of time, and we look back just to realise how far back others still are. Fabian contends that the way in which this distance between self and other has found expression is through the use of the "ethnographic present".

that this tense places the other almost out of time, in an eternal present which is actually creating a division between the self or analyser, and the other or analysed; between our 'lively present' (a present that progresses) and the other's reifying present (a solidified present which actually symbolises the backwardness of the primitive). Thus, anthropology often performs a denial of coevalness:

> "By that I mean a persistent and systematic tendency to place the referent(s) of anthropology in a Time other than the present of the producer of anthropological discourse" (ibid, p. 31, original emphasis removed).

The use of this present denies the links between the object and the author by constituting the object at a distance, creating a division between object and subject which preserves the idea of an exotic other[14]:

> "This synchronic suspension effectively textualizes the other, and gives the sense of a reality not in temporal flux, not in the same ambiguous moving *historical* present that includes and situates the other, the ethnographer, and the reader" (Clifford, 1986, p. 111).

However, as Fabian himself admits, it is not a matter of banning the use of present[15]. A simple change of tense will not do, if we do not keep an eye on implicit assumptions accompanying it. Especially, because the alternative to the ethnographic present –a past, even an autobiographic past- is not less problematic. Instead of fighting a representational notion (Law & Hetherington, 1998), the use of the past tense may even make it stronger, since it reveals a desire of achieving a correspondence between our 'object' and our description of the object: 'if the object had not changed, we could use the present; since it changes, the present is not appropriate any more'. In this view, language still needs to adapt to the object it

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[14] The perversity of such device is not reduced when it is disguised by courtesy and rhetoric of respect (Rosaldo, 1986).

[15] Moreover, I would say, that in the case of my study, there is little danger of enclosing 'my subjects' in a remote past. I have to report about scientists, and as every body knows, science, is future–granted, also a present strongly rooted in tradition, but looking straight and projecting itself like an arrow into the future. This is the myth of progress, of which science is a strong motor, the huge divide which differentiates us from 'them', the primitive savages. This is why I suspect that if, by using the present, somebody is in danger of being locked in a remote past is rather 'we', social psychologists...

describes, we are still looking for a correspondence between our narration and an external object –if our IBF is gone, let us use past.

I had another problem with past tense. Half convinced about critiques of the present, I tried to change a couple of pages into past. The result was quite catastrophic: I kept swapping to present, without even realising it. The result was a weird badly written mixture with which I could not identify at all. This puzzled me for a while: whilst the awareness of change did not barred me from writing in present, there was no way I could bring myself to talking of the ethnography as a past event. How could I write sentences such as "the IBF *was* a very important institution in Spain"? I could not help but having the feeling of killing my object of study. This may sound intriguing if one takes into account that fieldwork took place in 1997; data talks about past –in fact, even then the data I gathered one day was already past when looked from the perspective of the very next day, of the very next minute. Certainly, my experiences were past, but I felt the object to be present.

This is a relation between present and past common to ethnography (Fabian, 1983). One experiences the present as strongly constituted by past, since, as von Weizsäcker says, "(t)hat which is past is stored in facts. Facts are the possibilities of the appearance of that which is past" (von Weizsäcker, in Fabian, p. 88).[16] Present, then, is an accumulation of past –a past that does not make sense if it is not read from the present. Thus, if past is here, it is never the case that the author travels to the past to apprehend an object; rather, all the author has to do is unfold its present baggage of past –memory. Thus, the present of the object of study is constructed through the writer's past (ibid, p. 88): the knowledge out of which the object is constituted is linked to the author's autobiographical past. And this does not diminish the factual nature of the ethnography, to the contrary: it is through memory –and not from the

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[16] If present includes past in the shape of experiences, it also includes future in the shape of expectancies; thus, the present is a transit, a travelling between past and future (Kosseleck, in Martínez, 2003), from the space created by the past to the always receding horizon of the future: the present is the intersection in the here and now of a past that projects us into the future. This is how we can understand von Weizsäcker's claim that the present is the one-ness of time –not so much because the present encompasses all the times, but rather because the union of times creates a present.

immediacy of present- that ethnography becomes a fact, 'the presently factual'. Only if we oppose 'subjectivity' to 'facts' does the evidence of their biographical nature force us to abandon the notion of 'facts'. But if we accept that facts are radically implicated with past memory (as well as with future projects), then we can still retain their possibility as a useful and real fiction (Fabian, 1983; Latour, 1999; Stengers, 2000).

Now, if the present's object is linked to the author's past, it is no wonder that there is a tendency in anthropology to write in the present: when we work our memories, we do not reconstruct a past, but elaborate a present, our object's present. As Strathern, quoting Hastrup, puts it:

> "Her specific point is that fieldwork experience is memory before it becomes text, and the author's engagement is always with his or her present memories –the past is not past. "The dialogue was 'then', but the discourse is 'now'. There is no choice of tense" (Hastrup, n.d.:28)" (Strathern, 1991, p. 48).

This is, I think, why it was so difficult for me to talk of the IBF in the past. When I think about the IBF, I am never returning to the past, but recreating a present with a mixture of memories, interview transcriptions, field notes and expectations. Data is memory, and precisely because it is memory, it is already present. (Actually, the past that is not in our present memories is not even past, it is no-thing). Every time I read my notes, every time I write about the IBF, I bring *that* IBF, *my*[17] IBF back to life, back to present. My work gives life to an institution that has not died, but comes to live again and again every time I write about it. To have to talk of the IBF in the past would be a very contradictory act: I would be killing my object (declaring it past) exactly at the same moment in which I am giving birth to it. And given the intimate connection between subject and object, identities and times are implicated in complicated ways: much as the IBF is part of my past, it is also part of my present, of

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[17] Ethnographers are often chastised for the use of possessive demonstratives in relation to those studied –my community, my ethnography, my IBF. However, there is a sense in which this possessive is meaningful, since it acknowledges the partial act of creation that every ethnography (read, every production of knowledge) supposes, and the involvement between object and subject in this production. Of course, one should not forget the 'partiality' of the argument (Strathern, 1991).

my identity as a social-psychologist-half ethnographer. If I was forced to talk of the IBF in the past, I would also be talking of myself in the past, contributing to the death of my own persona –no wonder that one feels emotionally unprepared to commit such an act!

In this thesis I write about people I have very present. I can hear their recorded voices and jokes, and read what they did and said. This happened some years ago, but my re-membering and re-calling takes place now. I am continuously in conversation with characters which I live as present and alive, and not as people-gone-by. Simultaneously, I know that the characters I will present can be completely equated neither to the persons I met then, nor to the persons they have become now. I am talking of, and to, figures which exist and do not exist at the same time –inhabiting an in-between world, I cannot help the feeling of being in conversation with a world of ghosts... To make things more complicated, I am painfully aware that these people have neither the same means of recollection of the ethnography I have, nor the same motivation. I do not know how often they remember that a team of social psychologists was among them and tried to understand and narrate their lives –not very often, I guess. For them, I lie rather in the past –or better, I am quite absent from their present. And if I know myself to be past for people who inhabit my present: who is now the ghost?

The ethnographer as a liminal presence: transported to a dubious, ambiguous present of whose existence one is never sure –a world of memories that one re-lives as present, re-members or ensembles as present. This is why Hastrup still thinks that this verbal tense "must be redeemed as the discursive instance of anthropology" (Hastrup, n.d.:29; in Strathern, 1991, p. 48), for "it is a necessary construction of time, because only the present tense preserves the reality of *anthropological* knowledge" (Hastrup, 1995, p. 14). What is more, the narrative constructed by the ethnographer is of a different kind from the stories that members of the community formulate of themselves. Members' accounts are immersed in history, the anthropologist's not. Of course the other is immersed in history –in "histories of their own" and in "global

history"- but this does not prevent fieldwork from being placed in a liminal space that seems to "suspend history for both parties" (ibid, p. 20).

The present is the tense that reflects better this suspension, stemming not from distance, but from the shared time that fieldwork involves: "Using the present tense is to speak from the centre of another time-space, which existed only at that fleeting instant when the ethnographer impressed herself upon the world of the others –and changed it" (ibid, p.21):

> "Although part of the anthropologist's life-history and also representing a moment in the course of local history, the experience of the fieldwork as such is outside history (as a particular temporal mode). It is so strongly marked by liminality that the ordinary succession of events is suspended (cf. Turnbull 1990). (...) They also create history, but it is a kind of history that is but a fleeting moment and cannot be spoken about in ordinary historical categories. Hence the ethnographic present. The tense reflects the reality of fieldwork" (ibid, p. 20).

As Fabian reminded us, this shared time is what makes possible the establishment of a relationship–and this, for Fabian, does not mean two different entities meeting each other, but a moment of reconstitution, where self and other mutually affect each other. Hastrup will radicalise this moment, with contact leading to the blurring of the very distinction between self and other. However, since she places this encounter in a liminal phase, this blurring does not prevent us from realising the duality of the ethnographic experience, the still resilient experience indicating that two levels remain –one, the ethnographic field, in which we perform union; another, that of writing, where we recreate from a distance. Or if we express this duality in another way, we have the world created by stories told by members of the community, and the world created by the stories of the ethnographer:

> "In short, the reason for the present tense is located in the dual nature of anthropological practice of fieldwork and writing, or presence and re-creation. The reality of the cultures that we write depends on a particular narrative

construction, a discursive present; the realities of other people, of course, have histories that are retold in local language. It is not for anthropology, however, to recast biographies and social histories in full, or for that matter to retell local stories. That is far more convincingly done by those who live them. The hallmark of anthropology is to experience the force of detail in practical life and to recast it in a theoretical mode that transcends it. Life has to be recreated in a separated language in order to be comprehended" (Hastrup, 1995, p. 21-2).

As Strathern reads Hastrup's proposals, hers are another way of questioning the representational relation between narrative and object. Ethnographical writing presents a narration which is no longer tied to the empirical; somehow, it constructs a narrative which is neither in the past, nor in the present, but 'out of time'. The present tense, in this sense, manages to convey better this detachment from the empirical, and it roots us in the discourse that creates the ethnography. Ethnographic narrations are written from this liminal space, out of time, and according to Hastrup, they are never attempts to represent but to "re-enact" or "evocate" another world (ibid, p. 21). As she clearly puts it: "Once we have realized that anthropology is not about replacing one discourse by another, or about representation or translation, we may return to a consideration of the anthropological practice as a creative process –of *presenting* ethnography. Even if the object of study must be historicized in all sorts of ways, the choice of tense is right; what would the point of anthropology be if its truth had already gone at the moment of writing" (ibid, p. 24).

4. Mapping

And now, as any introduction worth of its name should do, we will present a short advance of what the reader can find in this work. The present chapter has still two more sections. In the first one I will offer some reflection on the ethnographic tale with which we have welcomed the reader around the dichotomy self-other, in relation

between the ethnographer and the community that offers her hospitality. To do so I will mix several sources in a dangerous balance, without softening the tension of putting together authors who do not try to say the same. The point I try to weave is that the mentioned dichotomy is not a sign pointing at a relation between two entities 'a self' and 'an other', but an arrow: a vector of movement, of exile, of expropriation. Or, differently put, a movement that makes evident the presence of the outside in us that we cannot but apprehend as an absence.

The second will approach this apparent need that the scientific community has to exclude annoying others beyond its borders thanks to accusations of lack of scientific character. While this type of exclusions happen quite commonly with identities based on differentiation, I will look for the specificity of this mechanism in science in the work of Isabelle Stengers. We will accept some of her risky proposals so as to take ethnographic lessons seriously: to let the door open to the possibility that our 'objects of study' disagree about our way to represent them.

After this, we move to chapter 2. As expected, it will offer a description of the IBF, so as to provide the reader with some bearings to find his or her way through this institution, its members, its groups and ways of working: what, when, where, who, how many and how much. We will also summarise the developing structure of a scientific group, that usually adopts a pyramidal shape, as well as its anchoring at university. This point will reveal crucial: the functioning of the IBF cannot be fully understood unless it is situated in this broader context: Spanish university and Spanish science. This is why present changes in the latter, especially lack of resources and a strong decrease in the growth of departments, have such strong impact upon our laboratories, and unsettle their ways of organising themselves. One can say, without exaggeration, that the future supposes a challenge for scientific groups, that have to explore and experiment new ways of adapting to resources. Likewise, this new situation will alter not only the sequence and nature of the scientific career new Ph.D. students can envisage, but also the type of attachment heads and new members feel to

each other and to the group. This chapter, then, draws the scenery where the rest of the ethnography dwells.

The next three chapters (3, 4 & 5) contain the nut of the empirical work. The first of them will approach the institutional character of science, but from a mixed perspective: a first part will deal with the topic of scientific reproduction, a second with the relations between science and other institutions. Science has been institutionalised in Western society to such an extent that nobody questions its raison d'être –for us, science has always been with us, we can only think ourselves with science. It may thus be interesting to inquire how scientists imagine this continuous being there, and to this aim we will explore their ideas of how scientists come into being, so that science reproduces itself through the reproduction of its members. Next to their linear, almost developmental accounts on how they learn and become expert scientists thanks to the scaffolding of the group and the sustaining activity of the head, we will present a different one –a narration that will attempt to frame their notions of reproduction using some anthropological tools such as notions of 'flow' and 'substance'. This is an account that comes close to, and takes distance from, their own conceptualisations; we offer a non-native account that articulates how they fathom their being together, but that also allows us to extend analysis in a way that theirs do not.

For instance, our 'description' will help us make place for the coexistence of an individualistic and a collectivistic perspective in the bosom of science: an understanding of the group as an entity that works as one, while being constituted by many, through the particular attachment produced by the head of the group. This imagined articulation presents a particular relation between singularity and plurality that will also be discussed. In the second part of the chapter we will put our own account to the test, to see whether it can shed light upon the way they conceptualise the relation between science and other institutions –in particular the fit between science and politics. Since in many occasions members of the IBF claim that these are two independent domains with occasional interferences, we could oppose this native

vision to the one defended by scholars in STS -the by-now famous seemless web articulating heterogeneous actants in a single assemblage. But instead of creating such a clear cut between these two positions, we will try to show that scientists' perceptions of the relation between science and politics are quite ambiguous, leaving also space for a perception of the indivisibility of knowledge and nurture.

In chapter 4 our concern will be the process through which scientists construct a common universe. If, as STS has shown, reality is a contextual scientific achievement, and not the solid ground on which science rests, we still have to grasp how scientists manage to articulate their local accomplishments and contribute to their collective endeavour. To produce a tentative answer to this interrogation, we will enter three different labs where different protein techniques are carried out. As we will see, even though they claim to be working with the same protein, the different nature of results obtained in each laboratory make this identity problematic –that is, identity is turned into a problem. And this not only for STS scholars, but also for scientists themselves: the protein as a single object is not a given, but emerges as a project that requires coordination among scientists, instruments and practices. The chapter will illustrate all this coordination work or 'weaving'. Questions about the singularity or multiplicity of the protein will be examined, and a way to imagine the protein's being as neither one nor many will be presented.

Chapter 5 will deal with several tensions that cross the IBF and its knowledge production, such as those between national science and international science, market and gift economy, exchange and collaboration, territorialisation and deterritorialisation, virtuality and actuality, stability and mobility. These tensions will be examined not to create antagonisms or reify dichotomies, but to show how the IBF is constituted precisely in the creative, moving field defined by all of them, as a complicated assemblage that brings together parts which do not quite fit. To describe the work of constitution of such an assemblage, this chapter will inquire into the notions of exchange, mediation and movement, as well as into their bonding characteristics. If the two previous chapters had imagine assemblages articulating

proteins, instruments and scientists on the one hand, and heads, groups, science and politics on the other, this chapter will describe assemblages extending through mediation to constitute international science –to constitute the IBF as a member of international science in its own right. Thus, the chapter will challenge associations between periphery and marginality by showing how a modest player can magnify itself through connection to global networks.

At last, the doubled conclusion will try to connect commonalities and differences, as well as to blend the different concerns that are elaborated in the whole work. This will be done twice and differently; first, theoretically, linking thought with thought; second, while narrating one of the main celebration of the IBF –the Christmas bash, a kind of ritual party that symbolises and performs being and belonging in the IBF.

As announced, then, we present now a reflection on (ethnographic) relationships in section II, followed by an exploration of scientists' boundary work in section III.

Part II. Moving relations

> "Es necesario el 'corage' de mantener abierto el
> sentimiento como sede de la posibilidad de pensar y
> querer"[18]
>
> (Félix Duque, *Los Humores de Heidegger*)

1. Ordering others

> "Daydreaming like this, sitting among the pretenders,
> [Telemaco] saw Athena, and went to the hall, hurt that a
> stranger was kept for so long waiting at the door and
> told her these swiftly words: "Be blessed, oh, foreigner.
> You will be our friend and after eating you will tell us
> what you need"
>
> (Homer, *Odyssey*).

The use of the expression 'being other' - popularised by authors such as de Beauvoire
and Lacan, and related to concerns about the construction of 'gender', race and
ethnicity, as well as the construction of the object of study in anthropology (Clifford
& Marcus, 1986) and cultural studies (Said, 1978)- has progressively spread to refer
to situations where some collectives are deprived of autonomy, marginalised and
submitted to domination. In other words, to talk of 'others' entails talking of power
relations among groups (Apfelbaum, 1989). Processes of 'othering' have been
denounced either by the very excluded groups, or by groups in more privileged
situations willing to contribute to the end of such inequalities and injustice, and
strategies in this direction have been seen as attempts at "representing the Other"[19]
(Wilkinson & Kitzinger, 1996).

This expression should call our attention to two different aspects of the process. On
the one hand, to the political character of *speaking for* a collective and promoting its

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[18] "it is necessary to have the courage (*Mut*) of keeping open the feeling as a site of the possibility to
think and love" (Félix Duque, *Los humores de Heidegger*).

[19] The somehow essentialising move of these strategies, as we will discuss, is sometimes betrayed by
the 'respectful' but pompous capital letter of the Other, as criticised by Lee & Stenner (1997).

participation in the common forum, in the determination of its own life conditions[20].
On the other, the constructive nature of *speaking about* a collective, the fixing
moment involved in the production of knowledge about an other; indeed, the
production of knowledge has consequences on the being of objects of study, it
constitutes them, or, as Curt (1994) would say, they are 'knowledged into being'
(Curt, 1994; Ibáñez, 1994; Sampson, 1993; Clifford & Marcus, 1986; Parker, 1992;
Potter, 1997; Wilkinson & Kitzinger, 1996) –which may also translate as knowledged
into destruction (Baudrillard, 1979).

This dual character means that there is no way to speak for without speaking about
(Callon, 1986; Latour, 1987; Lee, 1994; Michaels, 1996). No way to represent
(*Vertretung*) without re-presenting (*Darstellung*) (Spivak, 1987). Moments of
presentation are also moments of construction: there are no others, but othering
(Wilkinson & Kitzinger, 1996). Stories which order and disorder others (Latour,
1991; Law, 1994).

The union of speaking for and speaking about constructs a powerful, effective,
authorised discourse which confers legitimacy one the speaker's position (Lee, 1994):
the other and the person talking on behalf of the other need to be seen as speaking in
one voice[21]. Then the person becomes a transparent intermediary (and not a
translating mediator –Latour, 1993) and a legitimised authority (and not an imposing
power, Barnes, 1986). However, precisely this articulation between speaking for and
speaking about is questioned by voices that problematise acts of representation. One
of the first problems pointed to is the danger of simplification and homogenisation.

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[20] This is, for instance, the underlying project in contributions by community psychology (Lane, 1996;
Lane & Sawaia, 1991; Martín, 1988; Montero, 1984, 1994, 1998; Sánchez-Vidal, 1991; Serrano-
García & Rosario-Collao, 1992).
[21] When a physician talks about neutrons, her or his credibility is based on an implicit –a political
strategy: what the physician says the neutron does is exactly what the neutron does; the scientist
presents him or herself as a representative of the neutron, as a translator of the voice of nature: what
they say about nature is what nature itself would say if it could talk to us (Latour, 1992). This pretence
is not far away from the concerns we have been discussing here: social scientists also attempt, in
certain situations, to present themselves as the best representative of an other: this is why they can
claim to be 'speaking for'.

This problem is accentuated by the belief that the speaker needs to share an identity with those spoken for: it has been argued that one needs to be a woman, for instance, to be able to talk on behalf of women. Whereas there is something very reasonable in this proposal, difficulties start when 'women' is not a homogeneous collective, but is disaggregated by lines of ethnicity, social class, sexuality. Thus, some lesbian black women may challenge the legitimacy of accommodated white heterosexual women to talk on their behalf. For some years such claims became frequent and necessary, to break the fiction of unity and show the dangers of reification hidden behind the word 'woman'. Once this work had been done, some women have defended the possibility to work with this category as a fiction that allows women to unite politically (Burman, 1996), and to avoid a separation into collectives of single individuals (Alcoff, 1994). To make it more explicit yet, some have claimed that representation should part company with a politics of identity, where only like can talk about/for like, and work to ground it on solidarities. Solidarities which allow ironical juxtapositions of heterogeneous partners (Haraway, 1991).

Attempts to fight power relations involved in representation have made an appeal to dialogue and formulas that recognise the deep dependence of the 'I' upon the 'you' (Sampson, 1993). Nevertheless, in spite of good intentions, an awareness of dialogism and insistence upon conversation does not solve all problems (see Wilkinson & Kitzinger (1996) for a collective discussion of such topics). We cannot forget that dialogues are still embedded in power relations and are not unmediated (Spivak, 1986, 1990). Moreover, why do we always think that a disfavoured group is always willing to enter into conversation with the majority? Sometimes they are too full with rage and bitterness, anguish and fear so as to want to dialogue (Strathern, in Rabinow, 1986). Or anger, and not dialogue, may be the connector invoked to create unions[22] (Lorde, 1984). Neither should we assume that all collectives have 'their own

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[22] "My anger is a response to racist attitudes and to the actions and presumptions that arise out of those attitudes. If our dealings with other women reflect those attitudes, then my anger and your attendant fears are spotlights that can be used for growth in the same way I have used learning to express anger for my growth. But for corrective surgery, not guilt... (A)nger expressed and translated into action in the service of our vision and our future is a liberating and strengthening act of clarification, for it is in

voice' –sometimes minorities are so subjugated that they reproduce discourses of the majority; or they voluntarily adapt them either strategically to 'fight from within', either to survive –and who can blame them? Some groups have fear to speak; others resist a strategy which mimics attitudes of the majority. Domination may even destroy identities: some groups do not know who they are, what they want, how to articulate their desires (Atwood, 1969; Morrison, 1987, 1992 [23]).

Still, others denounce that any attempt to let the other speak is already an act of omnipotence and colonisation: we should shut up and let the other speak for themselves (bell hooks[24]). Yet, this strategy is still problematic, for it does not touch on the asymmetry of resources which makes it quite difficult for this 'other' to speak even if we do not talk. Not everything is a single matter of voices... To represent the other is not simply to talk for another, but also to compromise in practices that challenge the actual distribution of power. The problem with silence is that we are not only responsible for what we say, but also for what we leave unsaid.

Until now, we have talked of some of the problems and contradictions which supervene when one thinks of the relationship between self and other. These reflections, though, seem to precede the relationship between self and other, as if they were posed by a good researcher, in one of these so-much-called-for reflexive moves, to avoid side effects. These thoughts involve attempts to clarify one's position, the perspective from where one talks, as if one wanted to enter the relationship as clean as possible. Ethics, dialogue and communication between already-made islands, and

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the painful process of this translation that we identify who are our allies with whom we have grave differences, and who are our genuine enemies" (p. 172, 174).

[23] "(...) anybody white could take your whole self for anything that came to mind. Not just work, kill, or maim you, but dirty you. Dirty you so bad you couldn't like yourself anymore. Dirty you so bad you forgot who you were and couldn't think it up" (Morrison, 1987, p. 251).

[24] "I am waiting for them to stop talking about the 'Other', to stop even describing how important it is to be able to speak about difference. It is not just important what we speak about, but how and why we speak... Often this speech about the 'Other' annihilates, erases: 'no need to hear your voice when I can talk about you better than you can speak about yourself. No need to hear your voice. Only tell me about your pain. I want to know your story. And then I will tell it back to you in a new way. Tell it back to you in such a way that it has become mine, my own. Re-writing you, I write myself anew. I am still author, authority. I am still the colonizer, the speaking subject, and you are now at the center of my talk" (1990, p. 151-2, in Fine, 1994, p. 70).

the search for the pure beach from where to speak without dirtying ourselves: show me your reflexivity (I reflex? I reflect?), tell me: where are you talking from? Where do you stand? Are you talking from within or from without? Above, below, right or left? What are your assumptions? Have you made your position explicit?

Nevertheless, and albeit good intentions, such thoughts trying to detect previous 'positions' implicitly deny relationships, which involve moving positions: "Relations are, in fact, ways of moving from place to place, or of wandering" (Serres & Latour, 1995, p. 103). Is there not a strong danger of reification in all this talk of selves and others? Are we so sure of what counts as 'self' and what counts as 'other'? Are the limits between selves and others so clear-cut as these debates assume? How can we tell where we are, if others will make us move? If the ethics of position is not useful, should we try one of motion? (Lee, 1998[25]; Crespo, Lalueza & Pallí, 2002). Can we think from relations, instead of doing it from reifications? Of processes instead of things or categories? It is not a matter of simply inverting the value of the dichotomies –of dreaming of redeeming others and evil selves, this would scarcely change the landscape. Once we admit our participation in the construction of the relation self-other, is there any way to disrupt the process of othering (Fine, 1994; Lalueza, Crespo, Pallí & Luque, 2002)?

2. Self-other: the third

If under the dichotomy 'self-other' anthropology has discussed about the relation between cultures –namely that of the ethnographer and that of those observed- the particular position or sequence of positions of the ethnographer is directly informed by how one understands this contact (and, of course, the other way round). In any

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[25] "(N)o ethical position is stable or complete enough to provide the assurances of good faith and responsibility which we habitually look for in our 'ethics'. (...) Since agency is not a property, there is no 'authentic' place from which to speak of oneself or at which to achieve ethical adequacy when speaking on behalf of others. (...) In some circumstances it is vital that we do not respond to the very powerful calls to position that we might hear, and vital that we restrain ourselves from making such calls on others" (Lee, 1998, p. 474, 475).

case, the task of the ethnographer participates directly of the tension self-other. On the one hand, and with rare exceptions, the ethnographer knows she is a member of a different collective than that being observed; on the other, she must work to become 'one of them'. This is, as Velasco & Díaz de Rada (1997), following Geertz, suggest, a rather ironic position:

> "The irony of trying to resemble another, while, simultaneously, talking of this other as somebody different. The irony to seek identification with those we represent from the distance... Fieldwork shows the seriousness of a conversion, but the irony to have achieved it by pure learning. A fiction, an art... but not falsity" (Velasco & Díaz de Rada, 1997, p. 28, our translation).

A fiction, Clifford (1998) also agrees, "an enabling fiction of reciprocal encounter" (p. 80). Moreover, attempts to 'become one of them' are also ambiguous through and through: one tries to pass as a member, albeit knowing this is impossible –how can one become fully a member when one knows this new belonging has an already-fixed expiring date? At the same time, however, there are moments in which one cannot help but feeling one of them, truly living one's fiction. This double positioning, half inside, half outside, is what constitutes the ethnographer as a figure in a liminal state (Munro, 1999; Turner, 1994), a marginal native (Hammersley & Atkinson, 1983, p. 100; Freilich, quoted in Velasco & Díaz de la Rada, 1997, p. 28):

> "the ethnographer must be intellectually poised between 'familiarity' and 'strangeness', while socially he or she is poised between 'stranger' and 'friend' (Powdermaker, 1966; Everhart, 1977)" (Hammersly & Atkinson, 1983, p. 100).

An ambivalence which is noticed by natives, who are often left with the doubt regarding how much they can really trust the ethnographer, and lived by the ethnographer herself, left with "a sense of 'betrayal', or at least of divided loyalties. Even a kind of schizophrenia" (ibid. p. 102). And, for certain, insecurity –the uncertainty involved in not having your shoulders covered by 'your own' community, in inhabiting a community which questions you continuously; the instability of a

world that is lived with a precariousness which does not let you feel at home: "These feelings should be managed for what they are. They are not necessarily something to be avoided, or to be replaced by more congenial sensations of comfort. The comfortable sense of being 'at home' is a danger signal. (...) There must always remain some part held back, some social and intellectual 'distance'. For it is in the 'space' created by this distance the analytic work of the ethnographer gets done" (ibid. p. 102).

We could say, then, that the ethnographer is quite an ambivalent figure on the boundary. Despite not being a complete foreigner, she is even less a full member; rather, she is locked in the ambiguity of the stranger[26] (Bauman, 1991, Beck, 1998; Schütz, 1972; Simmel, 1992)[27] and shows, among other things, the amount of work that being a full member involves. In other words, she makes evident that, as Munro (1999) puts it, our affiliation or belonging is always partial and incomplete –that we can never belong totally. This figure is placed in a space where we all, from time to time, inhabit: the in-between space (Bhabha, 1994) of the as-yet-unlabelled (Star, 1991), the Borderlands (Anzaldúa, 1987) that we can glimpse in some moments, in some situations, in some relations, live the precariousness and instability of belongings, selves, identities and houses; moments when we can, even if for a split moment, open ourselves to the experience of marginality.

Try to be a young, female Ph-D student in psychology among male consolidated scientists; it is not much, but it gives you a taste of monstrosity... Enough to perceive affinities with all those excluded you had thought so distant... all of them being figures which can help us destabilise limits –of houses, identities, selves, communities, nations (without forgetting the acritical uses such figures are often put

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[26] "The stranger's pain is due to the fact that, if he goes too far away from his own origin he loses his identity. Therefore, if he becomes too acquainted with the new environment where he has emigrated, he loses its origin but if he does not become acquainted he loses his relationship to the place. The stranger is constantly in the oscillation of losing either sociality or identity, either relation or origin. But since being a stranger is not something that 'happens' to someone, but it is our normal condition, we all find ourselves constantly in this dance between two poles in irreconcilable tension" (Galimberti, 1996, p. 64-5, our translation).

to use, denounced by Kaplan (1995)): the exiled (Carter, Donald & Squires, 1993); the Jews (Grosz, 1993); the Palestinans (Bowman, 1993); the Diaspora (Hall, 1994); the Black Atlantic (Gilroy, 1993); nomads (Deleuze & Guattari, 1988; Braidotti, 1994); homeless (Wardhaugh, 1999); immigrants (Bhabha, 1994), the traveller (Clifford, 1997; Augé, 1998); surrealism (Clifford, 1988), cyborgs, Frankensteins, onco-mice and other vamps (Haraway, 1991, 1997). Figures that call our attention to the interstices of the in-between space.

But if ethnography takes place neither here nor there, but in the liminal space, then the ethnographer, placed in this third space, becomes also a third figure:

> "In fieldwork, the representative of the third culture emerges as a third person character. The ethnographer is neither you nor I, but assumes a strangely unknown position as 'she' or 'he'. *She* is *my* reflection in the mirror, the subjectified object ... Also in her own culture, or in a parallel culture, the ethnographer –as the representative of the third culture- will inevitably live and work in the third person. As such she is a friend to the locals and a stranger to herself. This is a truly privileged position for ethnographic work. It is not solely a matter of both participating (assuming the role of *you*) and observing (keeping *my* professional aims intact), but also, and more importantly, of letting go of both and living, feeling, and experiencing from the position of the third person" (Hastrup, 1990, p. 267-8, original paragraphing altered).

This is not a logic of 'either/or', neither one of 'and/and', closer perhaps to a 'neither/nor'. Thus, during fieldwork a new identity emerges, a third position which corresponds neither to one's culture nor to the other's. It is a self[28] that is both I and not I; during the fieldwork, it is a position with which the ethnographer identifies – but ironically, with tension: the ethnographer knows the constitution of this identity is

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[27] "I would like to look familiar objects (including objects of research) with an eye that defamiliarises them: the anthropologist's or simply the stranger's" (Ginzburg, 2001, p. 92, our translation).
[28] The idea that fieldwork involves the creation of persona has been expressed by comparing the first to a process of socialisation (Velasco & Díaz de Rada, 1997, p. 26).

part of one's job, and therefore, attachment has something cynical and instrumental. Attachment is uneasy: she is me and not me at the same time –a partial connection. However partial, though, this is definitely a connection; otherwise, it is difficult to understand the sadness of the departure –the tear, the sorrow that Evans-Pritchard diagnosed as a symptom of a good ethnography. A departure that is half a death –I will never be again the psycho-killer, a half-psychologist trying to pass as a biologist. When one leaves the fieldwork, one senses one is also leaving one-self:

> "The sorrow also emanates from the anthropologist saying goodbye to the 'she' with whom she had become so familiar at last. On parting we leave an experience behind which is the ultimate sign of the semantic creativity of anthropology, that is, the reflection of a subject, who is a mirror-image of no-one, but who will for ever 'exist' as a language-shadow in the discourse upon our friends" (Hastrup, 1990, p. 268).

This picture is simply making perceptible a transformation that is not a privilege of ethnography, but common to membership work. The intentional and instrumental setting –this is our job- allows an observation of a process that often goes unnoticed. If becoming a member is a process of constitution, efforts to belong –to pass as a member- proper of fieldwork evokes a relation between 'truth' and 'the subject' slightly different from how this relation is usually imagined in science:

> "it seems to me that the Modern Age of the history of truth starts the moment when knowledge, and only knowledge, allows access to truth, that is, the moment when the philosopher or the scientist or simply the one looking for the truth is able to recognise knowledge in himself through his acts of knowledge exclusively, without being asked anything else, without his subject's being having to be modified or altered" (Foucault, 1987, p. 40-1, our translation).

This contrasts with the way in which this relation was conceptualised among the Ancient Greeks, where the latter was to be understood as a spiritual one –one had to suffer a conversion, a transformation, understood not in the Christian sense as a trans-

subjectivation (a renunciation of one self to be constituted in another world some when), but a self-subjectivation[29]. The person could not have a direct access to truth; instead, one had to constitute oneself (had to constitute in a self) as a subject with access to truth. This meant working upon oneself, applying to oneself some technologies that would enable one's self-constitution as a subject:

> "We will therefore designate as spirituality the set of searches, practices and experiences among which we find purifications, asceticism, renunciations, conversions of the look, modifications of existence that constitute -not for knowledge but for the subject, for the very being of the subject- the price to pay in order to have access to truth" (Foucault, 1987, p. 38, our translation).

Ethnography, and the type of sensibility it requires, is a far cry from the constitution of subjectivity in Ancient Greece, but if I bring both ideas together is because it seems to me that fieldwork recreates a situation in which the self must be subject of ontological changes as a way to produce truth. Not a cognitive act but a form of life (Rose, 1990, p. 18). In ethnography, like in the period studied by Foucault, not only does the subject act upon truth, but truth also acts upon the subject (ibid, p. 41)[30]. Thus, fieldwork requires a particular type of work effectuated upon the self –partly by others[31], partly by oneself- to enable the emergence of a partial self able to access the truth, out of the processes of constitution that the ethnographer undergoes: "To become what one has never been, this is, it seems to me, one of the elements and

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[29] Foucault's argument is complex and fine with details which distinguish between different phases – the work upon oneself was differently defined in Plato times from stoic times, the first being still linked to a knowledge of oneself and a care of others, whereas in the I and II centuries care of oneself becomes independent. Here we will not be concerned with the specificities of the argument, but will focus on the effects that Greeks imagined upon the subject.

[30] Lévi-Strauss' construction of the ethnographer as a marginal figure which needs to impose an exile upon himself or herself is a case in point (as well as the similarities he perceived between ethnographic work and the path of Buda as a type of renunciation to the self). (Misgivings are likely: I am not saying that ethnography is of a spiritual nature. Yet, it may share with other tasks –spiritual ones among them- this work upon oneself, this continuous monitoring of one's transformations in order to extract truth out of it).

[31] Of course, ethnography is never a lonely process: transformations upon one's self are only rarely voluntary. Mediators are continuously needed, exactly as among the Greeks: "The previous problem is the relationship with the other, the other as mediator. The other is vital in the practice upon one's self so that the shape of this practice reaches its object, that is, the self" (1994a, p. 57, our translation). See Foucault (1990) for an analysis of the figure of the master or teacher, differently conceptualised in the first period, the stoic period and among Christianity.

fundamental topics of this practice upon oneself (ibid, p. 54, our translation). Ethnography shares with Ancient Greeks that much: it recognises and uses to its advantage the ethopoietic character of knowledge.

3. Journeys...

> "Everything starts with a journey, (...) in any case moral and almost always also physical, even if it is a matter of studying groups of the same society to which the researcher belongs. This journey involves crossing the cultural difference, those allegedly existing boundaries between one's own society and the society object of study"
>
> (Velasco & Díaz de la Rada, 1997, p. 28, our translation).

The metaphor of the journey has impregnated ethnography and fieldwork since long[32]. A journey moving us from similarity to difference, from proximity to distance, from familiarity to strangeness –a journey in the search for the other[33]. Indeed, ethnography has always been indebted to the tension between 'we' and 'they', in which we have to learn to move within a double horizon, "[the horizon] that impedes to perceive distance as proximity, and the one that prevents us from perceiving immediacy as distance" (Velasco & Díaz de la Rada, p. 29, our translation). This awareness has brought a new challenge: can we actually leave? Do we ever arrive? Are we prisoners our language games? Can we set aside our way of being to grasp another? Can 'we' understand 'them'? How to recognise difference without reifying it? How to acknowledge otherness without taking limits for granted,

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[32] Ethnographic experience has been understood with several metaphors –socialisation, a relationship master-pupil, initiatory journey, surrealist collage... But the journey has been a privileged one. Actually, as Pratt (1986) explains, the emergence of ethnography as a narrative cannot be separated from narrative of journeys, commerce and colonialism (see also Rose, 1990).

[33] Not only ethnography has used this image. Intellectual trajectories are prone to be read in similar ways: "...the successive landscapes that I crossed during a bit longer than half a century. I make use of the term 'landscapes' because it is not precisely my work that I would like to resume or present here, but rather the problems which I encountered while trying to construct myself" (Ricoeur, 2001, p. 31, our translation). See also Ginzburg's comparisons between history and a maritime harbour, a place from which depart and come back meeting different people (2001, p. 94).

and how to approach 'chez soi' without taking too much for granted? Are we unconnected islands colliding in cultural shocks every time encounters happen (Stolcke, 1995; Augé, 1998[34])?

These questions somehow assume that contact with otherness and its interpretation are a cognitive matter, and that instances that constitute us, such as language and culture, are not only limits we can never overcome but also limitations or constrictions that enclose us. And then, encounters are necessarily problematic. But in the thought of Gadamer we find a different way of conceptualising interpretation which could help us overcome such a limiting conception of contact, given the hermeneutical character of ethnography (Crapanzano, 1986). Importantly, he will avoid conceptualising 'encounters' as the meeting of two already-existing beings; rather, in a type of translation of Heidegger's being-in-the-world, Gadamer proposes the encounter of interpretation as an event.

As it is well known, this author offers us an understanding of interpretation as a moment of becoming affected by the you of a text or tradition –an encounter between self and otherness as a fusion of horizons. But this fusion of horizons is not a melting into each other's position, as if two subjectivities became one empathically[35]. This is a mutual being-affected that takes place because being is in and through language, and the other poses a question that interpellates us. This question addresses us already from a place and with a direction –with a sense, like an arrow directed to our-selves[36]. When we enter into relation with something other than ourselves, we make ourselves vulnerable to the questions of the other, for the question opens, splits, breaks,

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[34] "Nothing could be more disastrous, intellectually and politically, than vague and lazy references to the so-called multicultural society –that is, to cultures conceived of as closed and complete totalities, objects, according to the circumstances, of respect, boycott, or exclusion". (Augé, 1998, p. 52).

[35] Taylor (1985) also reminds us that understanding does not mean agreeing. Actually, only those who understand each other can disagree: "the dispute is at fever pitch just because each side can fully understand the other" (p. 37).

[36] The question as an arrow is not an image that Gadamer uses in exclusivity: "Foucault, so can be now read the title of the paper that Habermas dedicated to him, left an arrow nailed in the heart of the present. And, as Velázquez took care of following hyperbolically his master Pacheco's advice by making the place of the king leave the painting of the Meninas, so keeps Foucault's arrow pointing to us from this text. Will we be able to allow it to injure us?" (Jácome, 1994).

introduces "a rupture in the asked being" (Gadamer, 1996, p. 439): affected –altered in our being.

Relations, then, (and not only ethnographic ones) alter us, configure us in new forms: they act upon us. Or, to put it in other words, we become affected, moved by the e-motio. We do not leave an encounter as we entered it: each new connection alters our particular configuration in a way which expands or constricts our power to act -our conatus, as Spinoza would put it (Brown & Stenner, 2001). Affects, then, would be modifications of our particular essence or unfolding in the world through encounters with other entities –a kind of Heideggerian modulation, a passionate vibration that attunes us with others (Duque, 2001, 2002)[37]. This transformation can be understood as this movement without changing place (Deleuze & Guattari, 1988), or, as Bachelard put it, a change "through intensity of being" (1994, p. 193). There are moments in which the contact with something other brings about an intensity which attunes intimate and external space, and then the difference between interior and exterior is disturbed: we feel neither inside nor outside, expansion without growing big, an extension of the self through transformation without movement –a metamorphosis or change in our nature (ibid, p. 206).

To comprehend, then, entails an assumption of the situation of an other, not in a cognitive sense, but making place for something different in one's being, to take charge of it in one's being, in the unfolding of its possibilities. Not encompassing everything as if we pretended a victory in a battlefield; and not even voluntarily, as if by an act of generous will, but as an unexpected event that gives birth to an intimate intermingling of radical consequences: a mutual becoming, more an expropriation

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[37] In spite of differences between Spinoza and Heidegger, in these two authors we can also recognise some similarities. For a start, as we have already mentioned, they both oppose an intrapyschic, individual notion of affection, in order to conceptualise feelings, emotions and passions as exterior forces that move and alter us *ontologically*. Thus, they are not a side product, the emotional tinge of our thinking or acting, an irrelevant excrescence produced by our practices, but are constitutive of our way of being. Likewise, whereas none of the authors would reduce knowing to feeling, both recognise their intimate connection –either knowing results out of being affected, either sentiment is the ground for a further knowing. Moreover, both authors can make place for affectivity because they first understand things and beings as enmeshed with the world in a process of sustained engagement with

than an appropriation (Duque, 2002, p.101). This is why such encounters cannot be understood by analysing the relata, for the relationship creates a newness which constitutes relata anew. Whatever self and other were before encounter, they emerge as such in the relation itself. An extension of each partner through the other: a becoming other. Thus, interpretation changes us ontologically: it gives us new possibilities of being.

We have moved from affects and passions to 'possibilities of being'. This Heideggerian movement, as Duque (2002) reminds us, is not gratuitous, but finds a justification in the origin of the German word for possibility: *Möglichkeit*, related to *mögen* (love, like) and *Vermögen* (faculty or capacity *and* fortune). This knot of meanings revealing the sentimental nature of possibility, which in Latin remains hidden, reminds us that if we can do something is because we have been given a possibility, a capacity, a faculty –that is, something which is consigned or given to us by something other than ourselves: a present. And this consignment makes us able, which does not only mean to do something, but to unfold possibilities. To give, in this case, is not to give things, but to give possibilities, that is, to give a capacity that makes something other possible. Being and passion, then, are intimately linked. To be, to love: to give, to free potential of creation to let something other be. One exists if one lets something other be, an engagement that affects while enabling something other to exist. To give-in; not a surrender to the other, but a surrender so that the other can be. Love or the Gift, *die Gabe*: not to give oneself to the other, but an exhortation for otherness to be.

But in interpretation we make another experience: that of our finitude (Gasché, 1999). That is, the awareness that if we can find ourselves in the other, this is never a move that brings us to completion, but that shows our radical openness. The experience or affection out of contact with otherness is a becoming, a process of making of ourselves that opens us to other experiences and reveals that our existence is intrinsically incomplete (Gombrowicz, 1997). This is the radical consequence of our

the world. And it is this intimate entanglement what allows to understand why a change in the relation

historicity –incomplete, we never 'are', but 'become': we make ourselves as a project that never reaches totality or completion. Thus, the true experience involved in comprehension is that of our own limits –and the impossibility to transcend them -our historicity, our position, our culture, our present. It is precisely these limits –our culture, tradition, prejudices- that constitute our horizon, from which we can project ourselves and experience otherness. Thus, our limits are not a prison[38] we need to abandon, but conditions of possibility of our existence, what constitutes us: we are our limits.

Once we conceptualise contact as a transformation, and not as a going beyond limits, we make space for an awareness of liminality, of all those transformations which happen on the boundary. This fusion of horizons, then, comes close to what has been called third space, liminal space (Bhabha, 1994) or "white space, where distance abolishes its reach thanks to the bond" (Serres, 1995a, p. 31). The space created by relation:

> "(W)hen a brave swimmer crosses a wide river or a strait lashed by the wind, the itinerary of his journey divides into three parts. During the time in which he does not lose sight of the bank/shore of departure or in which he discovers the one of arrival, he still inhabits either his dwelling of origin or the goal of his desires; in other words, French here or Japanese there. Yet, in the middle of its journey there comes a moment, decisive and pathetic, in which he -being at the same distance from both banks/shores, on crossing during a more or less period a big, neutral or white strip- does not belong either to one or the other anymore, and he may even belong to both at the same time. Unsettled, suspended, as in balance while moving, he recognises an unexplored space, absent in all maps and that no atlas or traveller has ever described. (...) An

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of forces in which we take part entails an affection of our being –an alteration.

[38] This is equally valid for language. Gadamer will claim that being, in as much as it can be known, is language. Existence, in as much as it is meaningful to us, is not thinkable beyond language. Nevertheless, he also warns (1992) that language should not be thought as a substitute of Reason, a new totality or absolute: neither with reason nor with language "should one mistake universality with totality. (...) language is a universal but not a closed totality" (p. 69-70, our translation).

undifferentiated centre, through which we all go in order to approach all"
(Serres, 1995a, p. 26-7, our translation).

This is, precisely, the space in which ethnography 'takes place' -the space that
ethnography crosses, while being crossed by it. The ethnographic relation is a fiction
which keeps us in the gap between self and other, and does not allow ethnography to
take place "neither 'here' nor 'there'. It is everywhere, being actually a third culture
in any cross-cultural dialogue, sown with discrepancies (Hastrup, 1990). If an
ethnographer mediates between traditions, it also creates a third moment that brings a
relation to the world that was inc luded in none of the two traditions. This is a
productive, creative trip, thanks to which she does not simply 'discover' what was
already there all along, neither repeats what she already knew before departing:
"Ethnography is neither subjective nor objective. It is interpretive, mediating two
worlds through a third" (Agar, 1985, p. 19).

4. ...without returns

Awareness of this affectedness intrinsic to relation disturbs the metaphor of the
return-journey when applied to ethnographic relations. We have suggested before that
an ethnography shares nature with processes of self-constitution such as those
analysed by Foucault among the Greek. They have often been conceptualised as a
journey, where the 'I' appears "in the horizon as a target of an uncertain and
eventually circular trajectory that is the dangerous trajectory of life" (Foucault, 1987,
p. 88, our translation). This process is usually expressed through a navigation
metaphor: the captain uses techniques to find the right trajectory, the one that will
bring the ship back to the harbour through dangers and adventures. And this view has
also been a topos of our culture, often adopted by ethnographers themselves, that
understand fieldwork as a journey where the 'I' suffers vicissitudes, just to come back
save and sound to one's community of origin. The ethnographer tries to invest herself

with a culture just to divest of it afterwards. A kind of reversible socialisation (Velasco & Díaz de Rada, 1997, p. 30).

This is, however, where I take issue with this metaphor. If we take seriously the kind of affection that relation involves, the type of metamorphosis or alteration that encounters entail, then we can understand how it is that we never step twice in the same place: one never returns, the circle is never closed. As we have suggested following Hastrup, the ethnographer creates a 'she' as a kind of extension of herself in order to reach a new position/new belonging. This 'she', that is neither self nor other to her previous 'I', remains partially attached to her: 'she' is she and not she at the same time. This process is indeed a transformation: changes in one position (the 'she' of the field) will influence the other drastically (the 'she' previous to relation), and therefore, the figure of the ethnographer cannot be shaken off without consequences. When fieldwork is finished the ethnographer does not simply retreat into the old self, for this self has already been altered in a process of becoming other. Thus, the circle is never closed: there is always a small deviation that stops us from remaining the same; we may move, but we never reach a centre or close into identity: the circle expands into a spiral.

The same affectation is evident for those that conceive of ethnography as a particular kind of translation (Asad, 1986)[39]. Since there is no immediate communication between cultures, some have understood the process of making sense of otherness as one of translation –an attempt to enter into relationship with the 'you of the text' (Gadamer, 1996; Shotter, 1989). Even so, Benjamin warns: "there is always an element that does not lend itself to translation" (p. 75). It is never possible to translate everything, for there is always what Bhabha (1994) calls "a seed of otherness" which prevents total translation. This notwithstanding, this untranslatable element does not stop communication, on the contrary, it creates the gap that makes possible its bridging: "translation thus ultimately serves the purpose of expressing the central

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[39] Even though this image is based on a conceptualisation of culture as text (see Geertz, 1973), which has allowed a particular easy-to-justify apprehension of otherness, the notion of translation is not necessary limited to textuality (Asad, 1986).

reciprocal relationship between languages" (p. 72). Through translation, connections are made.

This means that translating is never an attempt at reproducing identity, for "no translation would be possible if in its ultimate essence it strove for likeness to the original" (Benjamin, 1975, p. 73). Rather, to create connections the translator's language/culture must be modified: a transformation so as to render a new meaning, a new relationship. For, as Asad says (1986), it is our world, not the other's world the one which is put to the test with translations:

> "Our translations, even the best ones, proceed from a wrong premise. They want to turn Hindi, Greek, English into German instead of turning German into Hindi, Greek, English. Our translators have far greater reverence for the usage of their own language than for the spirit of the foreign words... The basic error of the translator is that he preserves the state in which his own language happens to be instead of allowing his language to be powerfully affected by the foreign tongue" (Pannwitz, in Benjamin, 1975, p. 80-1).

It is this otherness -this nucleus of intranslatability- that is responsible for the difference, for a repetition which is never the same, the refrain (Brown & Capdevila, 1997), provoking further displacement, movement, relation, saving us from "the hell of the same" (Baudrillard, 1993). Maybe we cannot abandon language, but we can let ourselves be constituted by other languages, so that even language can be experienced liminally. Once language is affected by something other, it does not result in affirmation any more, but is kept open and in suspense. We experience language liminally: not even our native language is natural (Spivak, 1990, p. 38)[40]. One becomes a stranger in one's language -not mixing languages or speaking a foreign language, but sculpting a foreign language within one's mother tongue[1], inventing a minor use for the major language (Deleuze, 1994a). A language that makes us stutter, mumble and confront silence (Deleuze, 1994a; Blanchot, 2001b): a dwelling in the

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[40] "I'm devoted to my native language, but I cannot think it as natural, because, to an extent, one is never natural... one is never at home" (Spivak, 1990, p. 38).

limit of language. Not outside language, but in the outside of language, where words keep silent.

This transformation translates into a questioning of our homes. Home, usually identified as a haven, often equated to self and security (see Bachelard, 1994), is imagined as a kind of protective container that separates us from the outside. Belonging: my containers and me. Inside: my skinned body, my home, my land, my nation, bounded territories to protect from invasion. Possession, order, meaning, discourse; the master is at watch. And outside? Barbarians, foreigners and strangers, monsters, deviants and other teratologies, disorder, meaningless and inarticulacy. Tales already told it all: the house is warm and safe, the woods are cold and dangerous –full with unreliable wolves. We walk and experience space very differently depending on the regime of proximity and distance: a protective, expected geometry of visual distances vs. a disturbing, unpredictable tactile topology of proximities –*habiter* for the house with its stable corridors and rooms, *hanter* for the wood that surrounds us: "In geometry I inhabit, topology haunts me" (Serres, 1994, p. 70).

But encounters with otherness disturb this distribution –disturb the 'happy phenomenology of home' (Sibley, 1998; Wardhaugh, 1999; Webster, 1999) . As Strathern (1991) suggests, one never comes back as one left, one never adapts to one's culture in the same way. The ethnographer usually gains a different perspective upon one's own society -not independence but a partial detachment. She may come back in order to discover that she does not quite recognise her abode any more – something is different, as if she could notice a different presence. Something weird happens to our house, that in spite of its existence, it is absent to us: strangely present: "suddenly, the home turns into another world" (Bhabha, 1994, p. 10).

Moments of encounter when "we become emplaced in our own displacement, secured in our own insecurity" (Brown, 1998, p. 11). A feeling of disorientation–when the protective geometry starts dancing like bushes shacked by gusts of wind; when the

outside and the inside confuse into each other (Coates, 1991; Huet, 1993). When the two geographies cross we can feel the insecurity of the woods inside home – unhomeliness[41]. After all, Serres warns, "the house, we have known it from the incipit, inhabits itself –or haunts?- in a tree" (ibid, p. 71), and it is there that this crossing of geographies becomes possible. There, and... in enchanted houses (who said ghosts do not exist?). The intuition of a presence within, of the impossibility of boundaries[42].

Ethnography produces a traveller that remains a traveller when coming home, which is another way to say that the ethnographer never returns. The journey is one-way.

5. Exits and exiles: exceeding oneself

> "It is part of the anthropological condition to be alone among strangers. In the field and in the research process, one is permanently in a kind of exile"
>
> (Hastrup, 1990, p. 128).

> "...the *Myth of the ethnographer's radical exile*. Its main mythemas or bundles of episodes are the following: the uprooting from one's own society as incitement to anthropologic vocation, the ethnographic practice as systematic technique of uprooting, fieldwork as the transformation of one's own spirit into a laboratory

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[41] Bhabha (1994) transforms the German adjective *unheimlich* into the noun unhomeliness. Whereas in German the word means 'uncanny', Bhabha's play links this feeling not to homelessness, but to the state of unhomeness (p. 9). One is at home without feeling sheltered by it, something from the outside is in it, our home is nevertheless absent for us, it turns into something else.

[42] This awareness can be bound to fear. You are sitting in your living room, but can't help turning round over your shoulder, almost without daring, in case you find what you should not. Whatever you are scared of has not remained outside -does not come from outside. This is a direct challenge to the happy phenomenology of home. For we know that at home much can go wrong: "While the home can have these positive symbolic qualities, it also provides the context for violence, child abuse, depression and other forms of mental illness" (Sibley, 1995, p. 94). Or the place of those who do the dirty work for others (Webster, 1998). Let alone colonisation relations between colonies and metropolis (Ching-Lian Low, 1993). Home can be precisely the place where one feels homeless, the place one is afraid of coming back to (Anzaldúa, 1987). "What is missing from 'the house as haven' thesis is a recognition of the polar tensions surrounding the use of domestic space, tensions which become a part of the problem of domination within families. They derive from the ambiguity of boundaries which some people have difficulty in resolving. Oppositions like inside/outside, clean/dirty, tidy/untidy are essential features of the dwelling and its sub-territories, but they are not stable or fixed" (Sibley, 1998, p. 94).

> of an 'other' symbolism and the simultaneous disclosure that if
> 'l'autre est un je' is because 'je est un autre', the consequent
> discovery of the subject's inanity and, finally, the postulate of
> ethnography as a way of renunciation which culminates in the
> rediscovery of Buddha's message"
>
> (Aranzadi, 1996, p. 83, our translation).

If being has been characterised by this openness, this exit of one-self, this movement without return to a centre, then it is not so surprising that ethnography, experience of encounters with otherness per excellence, has been understood through the image of the exile. The use of the figure of the exiled to symbolise other situations than political exile encounters a fair resistance. Kaplan, for instance, criticises how in (post)modernist[43] discourses 'figures of displacement' have lost their origin linked to historical, economical or political conditions leading to displacement, and have become abstract, generalised figures with metaphorical power, whose collective nature has often been ignored, in favour of an image of the (romanticised) individual traveller. And yet, without denying the historical specificity of the exile, there may still be some characteristics of this displacement that sheds light upon the condition of the human existence –in particular, its liminal character (Agamben, 1996[44]; Borgna, 1996; Galimberti, 1996[45]; Hastrup, 1990; Lévi-Strauss, 1992).

This liminality will be located by Jean-Luc Nancy's (1996) in the 'ex'. Rejecting etymologies that link the word to land or ground (*ex solum*) he prefers to trace origins to e*x* plus the root *e'*, shared by several words such as *ambulare* and *exulare* - meaning 'to go'. Thus, exile would be the action of the *exul*, the person who goes, who leaves, who departs not towards a place, but who departs absolutely. Exile is a

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[43] Kaplan acknowledges both continuities and discontinuities between modernist and postmodernist discourses. Still, she positions herself within postmodernism as a political standpoint that allows her a critique of modernist tropes (such as the tourist, the traveller, the exiled, the nomad).

[44] "exile is not, then, a marginal juridical-political relation, but the figure that human life adopts in a state of exception, it is the figure of life in its unmediated and original relation with sovereign power. This is why it is neither right nor punishment, it is neither inside nor outside of the juridical order and it constitutes a threshold of indifference between the external and the internal, between exclusion and inclusion" (Agamben, 1996, p. 48, our translation).

[45] "Borgna... has told us in a radical form that exile is a human condition, in the sense that if it is true that each of us is a bearer of masks, if it is true that each of us shelters an unknown and therefore a stranger, and if in this stranger we can find our *autós* or authentic self, then we are exiled in substance and very frequently" (Galimberti, 1996, p. 61, our translation).

movement of departure that has already started but must never end. Thus, the exile is pure negativity, as long as one resists the temptation to cancel it in a movement of re-appropriation, as, for instance, with the hope and conclusion of a possible return, as understood in the Hellenic, Christian tradition.

Nancy proposes to focus our attention to the 'ex', to this movement towards the outside: what if this is the same movement required by the *ex*istence? "Existence as exile, but not as a movement out of something proper, of one's own, to which one can or cannot come back: instead, an exile that would be the very constitution of existence, and therefore, reciprocally, an existence which would be the consistence of exile" (ibid, p. 37, our translation). He does not suggest that we conceptualise an exiled existence or an existentialist exile. The point is more simple and more challenging –to perceive *ex* as the property of life, a property of estrangement. It is not a matter of going in exile, but to *become* exile. Being as aperture and exit, self as a movement towards outside. This is not the eternal 'dis-appropriation' that makes it impossible for us to conceive of our self: rather, it is the radical attempt to think of the exile as our property: "it is necessary that the relation with oneself takes place, that it has its place, and this place must be thought of as exile" ... " 'I' only takes place 'after' the exit, after 'ex', if one can put it like this. However, there is no 'after': 'ex' is contemporary to the 'I' as such" (ibid, p. 38).

A complete dis-appropriation if we think of property as things possessed (Cacciari, 1996) –a deterritorialisation, if one wants, of land and possessions. But also a radical belonging or proper relation to one's place, in the sense that one reterritorialises in this constant movement, in the movement of the outside, a reterritorialisation on deterritorialisation itself (Deleuze & Guattari, 1988, p. 381). It is not that the self welcomes home the other; rather, the self can only hope for a home that emerges as an expropriation, an exit of oneself towards the other. This is why, paradoxically, the 'ex' may be a condition for hospitality: Latin languages remind ourselves better of the intimate connection between *hospes* and *hostis*, between offering hospitality

(*hospedar*) and being a stranger (*ser extranjero)* (Cacciari, 1996). Which translates as the awareness of our intimate dependence from others.

"The question of the Other in this fatal universe is the question of hospitality" (Baudrillard, 1993). And you, off whose hospitality do you live?[46] Next to whom are you living (Serres, 1982)? With whom are you in debt? Who is your home?

6. Arrows of the outside

> "entonces sentí que definitivamente me separaba, me exilaba de mí mismo, y una multitud penetró impetuosamente en mí"[47]
>
> (Vila-Matas, *Recuerdos inventados*).

> "The phenomenology of the poetic imagination allows us to explore the being of man considered as the being of a *surface*, of the surface that separates the region of the same from the region of the other (...) Then, on the surface of being, in that region where being *wants* to be both visible and hidden, the movements of opening and closing are so numerous, so frequently inverted, and so charged with hesitation, that we could conclude on the following formula: man is half-open being"
>
> (Bachelard, 1994, p. 222).

If existence is movement towards the outside, an openness that challenges closed interiors, then one is forced to admit the absence within, the nothing-ness of the subject, the emptiness at its heart, the otherness that already inhabits us. Or, differently put, that we are nothing but folds –fold upon fold upon fold (Deleuze, 1989; Serres, 1995a). We are made of absences as much as of presences (Mansfield, 2001): constituted by that which we are not, have never been and can probably not

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[46] "The host is not a prey, for he offers and continues to give. Not a prey, but a host. The other one is not a predator, but a parasite. Would you say that the mother's breast is the child's prey. It is more or less the child's home. But this relation is of the simplest sort; there is none simpler or easier: it always goes in the same direction. The same one is the host; the same one takes and eats; there is no change of direction. This is true of all beings. Of lice and men" (Serres, 1982, p. 7).

be; constituted by what we do and what we leave undone[48], as if this scary revelation of the presence of the outside could only be intuited as absence (Foucault, 1988; Blanchot, 2001a). An absence to which already Bachelard, in his own way, pointed when he described being as a spiral:

> "Thus, the spiraled being who, from outside, appears to be a well-invested center, will never reach his center. The being of man is an unsettled being which all expression unsettles. (...) [Being] does not stand out, it is not *bordered* by nothingness: one is never sure of finding it, or of finding it solid, when one approaches a center of being. And if we want to determine man's being, we are never sure of being closer to ourselves if we 'withdraw' into ourselves, if we move toward the center of the spiral; for often it is in the heart of being that being is errancy. Sometimes, it is in being outside itself that being tests consistencies. Sometimes, too, it is closed in, as it were, on the outside" (ibid, p. 214, 215).

Otherness, then, refers to a not-quite experienced outside that inhabits us. Otherness is here "an infinite fold that concerns us before any 'I', any decision, any potency" (Ruiz de Samaniego, 2001, p. 25, our translation). A "power-other", to put it in Blanchotian terms, a 'field of force', a movement of becoming[49] that reaffirms by withdrawing itself in this movement; "an affirmation of the inexhaustible negation" that emerges as an indefinite oscillation without oppositions or conciliations, but the "swaying of an indifference" that never resolves in an affirmation or a negation (Blanchot, 2001a, p. 85, our translation).

A field of force that destroys the possibility of the original subject. Not because the subject disappears, rather because the subject is disappearance, a new way of being (Gabilondo, 2001): force as a demand that expresses the name while disappropriating it, turning it into other: a destroyed and lost I, that does not belong to itself (Ruiz de

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[47] "Then I felt that I definitely separated from myself, I became exiled from myself, and a multitude penetrated impetuously in me" (Vila-matas, *Recuerdos inventados*).

[48] Maybe this is why we can feel nostalgia for something we have never had, for something we never were and know we could not have been.

[49] Not alterity, but alteration, alienation, dispersion, disappearance, absence.

Samaniego, 2001). An absence, absenting, not as a 'going away', but as an impossible way of remaining, that preserves something spectral, an "experiencing that one is not where one is, without for all that being somewhere else" (Gabilondo, 2001, p. 68):

> "this is the place of metamorphosis, where the radical intimacy and the outside of all presence conjugate. The place of the infinite sheltered as relation: eternal in-between" (Ruiz de Samaniego, 2001, p. 25).

Thus, to talk of encounters with otherness is not to refer to a crash between two entities, a 'self' and an 'other' that meet to enter into an exchange that leaves them unaltered. Rather, it is a way to signal a movement of affection, of folding, of becoming other. An arrow or vector that moves a fictional self always expropriated from an inexistent centre towards an always-receding exiled horizon; the pointing to the impossibility of a deadly closeness that would asphyxiate the self, the evidence that we are always already constituted in and by the very movement towards the exterior. A no-return journey towards the unforeseeable sustained by hospitality.

Part III. Accepting provocations

> "In order to lull research, and intelligence on passing, there is nothing better than a category"
> (Serres, 1995a, p. 59).

> "To suspend judgment, to recognize that what one presents as the object of this judgment does not respond to categories (is it useful to remember the strict relation between categories and indictment?) and therefore does not authorize any judgment"
> (Stengers, 1997, p.111).

1. A fly on the wall (revealing community constructions)

For many, the figure of the ethnographer should be an invisible one: seeing without being seen, hearing without being heard. As if the ethnographer had to embody all the epistemological demands of neutrality, the analyser should wander around without being noticed, without having an impact upon those studied –as if it was a telescope watching stars, a microscope spying the intimacy of bacterial life, an unnoticed neighbour peering through a hole. For good or for bad, though, dreams of neutrality and purity were murdered long ago, and the story with which we were opening this work testifies to this impossibility: our fly got too close to people's noses and risked a couple of smashes...

And we were a nuisance indeed: we posed more than an interpretative problem –or, better, we posed an interpretative problem that, as we know after Heidegger and Gadamer, is also an ontological one. Who and what were we? Were we scientists or just pretenders? (Or something in between, which is tantamount to the latter). Had they accepted that we were 'scientists', this decision would have had a dramatic impact upon their identity: if science had given shelter to performances such as ours (sacrilege!), they would have had to change their idea of science and... of themselves!

Not to have their identity altered involved our exclusion: by making us 'other' (non-scientists) they could assure the stability of their self-understanding.

This claim brings to the fore the link between belonging and identity (Garfinkel, 1967; Mead, 1934), showing how the construction of selves and others is involved in boundary work (Douglas, 1967; Munro, 1995, 1998, 1999). Through our definition as 'other', the border they imagine between science and non-science was preserved. Thus, the distribution of identities as 'one of us' or 'one of them' works to keep borders and divisions, to position and move people, to perform inclusions and exclusions. It is not that something is excluded because it is other, but it becomes other precisely in the process of exclusion. What counts as 'self' or 'other', then, is not settled, but an effect of negotiations and impositions, changing every time borders of collectives are redrawn. The ambiguity and openness of the relation is solidified, so to speak, into categories. Differently stated, they are effects of relations, effects of ordering processes (Law, 1994) –boundary work: ordering others and othering orders (Pallí, 2001).

2. Limiting communities

Maybe at the very beginning of my ethnography I had imagined myself crossing borders: I entered a biology laboratory looking for the scientific community, which I imagined full with scientists. This is somehow an implicit in many ethnographies. The ethnographer is supposed to 'enter' a community which is pre-existent, in order to learn what kind of life and ways of being this community allows. One assumes one has to cross, if only symbolically, some kind of limit: one leaves partially one's own collective in order to join temporarily another one. One has to cross a distance, undertake a journey. Once 'on the other side', the ethnographer will try to learn what kind of knowledge, beliefs, values, culture, etc., members of the second community hold –just to come back home to tell about it to the first community of membership.

In this context one can understand better why the members of the community with which the ethnographer establishes contact are called 'doorkeepers'...

However, while searching for the secret of the community –its identity, the core set of conditions which assure belonging- its natural character appears completely compromised. If we take seriously what our 'being moved around' taught us, we will accept that it is precisely all this interpretative work in which members engage that contributes to the existence of the very community (discussions about what it is to be a member of the community, who qualifies as such, calling others to accountability). This closes a reflexive circle (Garfinkel, 1967): while somebody tries to show she is a competent member of a collective, she is contributing to bring this collective into existence; at the same time, to participate in this construction is what defines us as members. The possibility of working on what it is to be a member, and who (or what) is entitled to this belonging is one of the tools people has available to construct, reproduce or challenge belonging and affiliation. The work to create the world constitutes us –as people who constitute a world which constitutes them while they constitute it[50].

Thus, looked at it from this perspective, rather than considering the community as a pre-existing entity (which then must be explained resorting to other exterior forces such as the market, class struggle, common cultural inheritance, etc.), we'd better look at all the processes, intimately linked to a particular context, which happen and construct a particular situation[51]. A community is not an already finished entity to which we ascribe and are ascribed, but collectives which, like any other, are unstable, precarious, incomplete and partial, always on the making (Abbot, 1995).

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[50] Apologies for the sentence, but bizarre expression is, in this particular case, not my fault: put the blame on ethnomethodology.

[51] It is important to remember that these practices have an 'in situ' organisational character; they are intimately related with the context, not only in the sense that they happen in a place and in a time, but also because they themselves are constitutive of this spacio-temporal context. Put in a different way, they have an indexical character. We do not find, as in Goffman, practices which take a meaning depending on the situation, but the very situation is (re)produced through these practices. The context provides no frame, but it is constitutive and constituted. Moreover, they are never finished but remain open, needing actualisation in concrete actions, in concrete situations they are always an endless, ongoing, contingent accomplishment.

This work at the border -all this Garfinkelian work of construction of groups to secure belongings- has consequences for the way we think of limits –limits which allegedly delimit and define communities. As soon as we accept, in line with ethnomethodology, that there are no unmediated, pre-existent communities independent of our practices, but that communities are brought to existence by the on-going praxis of its members (who work to become members), then the very idea of stable limit separating communities becomes a much less plausible reality. It is not simply a matter of saying that borders are arbitrary, constructed and not natural: if communities are always on the making, this means that their limits are still to be determined, under negotiation, being made and remade too –in short, effects.

The closer we come to the limit, the more the limit blurs; the more we observe those practices carried out 'between' –in our case, between groups- the less stable differences become. This contact zone is never a resting one: the balance of difference and similitude is renewed continuously, changing or not, depending on the kind of work performed. Limits can change shapes; differences may become stronger or weaker; new separations or unions may appear; new groups define or blur, its members coming in or out of focus. Limits which are reconstituted through relationships –being the very heart of discussion, negotiation and often even imposition.

Far away from being the 'natural fruit' of difference, boundaries involve work of creation and maintenance. Which help us understand why people spend so much time, in so many and varied occasions, to discuss what makes us similar or different, what unites us or separates us. Of course, this does not grant that limits respond to strategic configurations –there are plenty of unexpected, side effects in a relationship, some more perverse than others. Inasmuch as we try to position ourselves and position others, we are also positioned (and moved) by them. We find, then, "boundary work" (Gieryn, 1983), a work of creation and recreation of limits, of management of identities and belongings. We find, in other words, "labour of

division: production, consumption and disposal of differences for belonging and exclusion" (Hetherington & Munro, 1997; Munro, 1997), the work of construction of distances and proximities justifying identities and belongings. But not natural differences, pre-existing differences which remain the same throughout history, but differences which are continuously worked upon moment by moment. Not differences and identities, but acts of differentiation and of identification (Latour, 1988b, p. 169).

3. Science: the community beyond fiction?

> "Ever since Galileo constituted the reference for what we now call 'modern sciences' –a power to which another power, that of the church, would eventually give way- the question 'Is it scientific?' has become the decisive question, the question that arouses passions and provokes invention, the question on which the raison d'être of the sciences apparently depends"
>
> (Stengers, 2000, p. 74).

We could say, then, that when we arrived at the IBF, we triggered off some boundary work, interpretative work to decide our membership: "you are not scientists, you do not belong". One could be tempted to think that our presence among scientists opened up a debate that contaminated research –we created an artificial situation which would have never taken place if we had not been there. We produced too much noise –an interference. But after Serres (1982 –who draws on information theory), we know that noise is precisely the ground that makes communication possible, calling for interesting effects. Thus, instead of kicking ourselves for intruders, we may do as Hammersley & Atkinson (1983, p. 14-5) suggest[52], and take the reaction to our disturbance as an index of how this particular scientific community reacts to nuisance.

Such situations are not that abnormal; accusations of not being 'scientific enough' were the order of the day. If a member of the laboratory wanted to put into question the theoretical approach of another laboratory, she only needed to imply that this approach was not 'scientific enough'. If a new method challenged many of the usually accepted ideas in a field, this method was labelled as 'artificial', 'entelechy', or 'non scientific'. If one wanted to put a mate's vocation into question, one could accuse this person of not feeling science strong enough (taking care of kids or working part time could always be taken as a proof of lack of real scientific vocation). If the behaviour of a boss was regarded as being more based on formality and social relations than on knowledge, accusations of her being more of a politician than a scientist would ensue.

In other words, the questioning of belo nging was not focused only on us, but it took place within the community as an intrinsic mechanism, so to speak, as a device to work continuously on the boundary -which facts, which projects, what questions, what tasks, what instruments and techniques, wha t theories, what persons will deserve membership? Like a fractal phenomenon, no matter at one level one looks, the question 'is it scientific' is always at work with effects of inclusion and exclusion. That is, with effects upon the collective constitution of the collective, or, to call things by its name, with political effects.

Whilst such mechanisms of boundary work are generally spread in a society that conceives of group relations in a competitive way (Ibáñez, 1996), consequences are quite important when the collective in question has the prestige of science, associated with truth: exclusion equals delegitimation, and scientific 'objectivity, 'neutrality' and 'truth' are opposed to the lay person's 'illusion', 'ideology' and 'opinion'. It is to combat unequal effects of exclusion and subjection that some scholars within social sciences have attempted to show that science is an activity like many others, without a 'differential trait' that justifies either power abuses or privileges. In other words, the

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[52] "We cannot avoid having an effect on the social p henomena we study. Instead of treating reactivity merely as a source of bias, we can exploit it. How people respond to the presence of the researcher may be as informative as how they react to other situations (Hammersley & Atkinson, 1983, p. 14-5).

political preoccupation of such social approaches has been to equate scientists to lay people in the life of the city.

Nevertheless, Isabelle Stengers, a Belgian philosopher trained in life sciences, has wondered whether the strategy adopted on behalf of this concern does not impede us to perceive and analyse specificities. Does one need to ignore differences between activities in order to attack exclusion? Does this strategy not strike back, in that it makes it impossible for us to realise the novelty that science brings to society and therefore to analyse it properly in order to understand *and* resist if necessary? She will propose a way to accept the singularity of science without for all that granting it a privilege standpoint –neither in society nor in the production of knowledge.

To do so she will ask once more 'what is this thing called science?' (Chalmers, 1982), and will bring us back/forward to an interesting discussion of a demarcation criteria partially invented anew. This may at first sound surprising. After many philosophers' efforts throughout a century, we had to accept that no epistemological criteria was able to give a proper answer –or, as Latour (2000) jokingly suggests, a popper answer- to the division between science and nonscience. Stengers will suggest that we have never found an epistemological answer because this question is not of the order of epistemology, and hence, she will look somewhere else.

She will suggest –to cut short a complex argument that she unfolds in a whole book- that what distinguishes science from other ways of producing knowledge is not a new type of reason but a new *use* of reason[53]: scientific knowledge is produced about something which participates in the creation of knowledge about itself. This is clearly

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[53] Instead of turning to the Greeks, Stengers looks for the invention of modern sciences in Galileo's work on the accelerated motion of heavy bodies (what became the "discovery of the laws of motion"). The reader may produce a sigh: Galileo once more? Is not Galileo the great mythical scientist, incarnating all the idealisations of science? Can this man be our point of departure for a different understanding of the sciences? Nevertheless, Stengers thinks it is almost a matter of 'bad taste' to make one's point with those scientists whose theories have been refuted: one vents on the losers. Interestingly, then, Stengers softens a frequent critique on STS studies (Law, 1991; Star, 1991), that of focusing too much on big characters –for instance, Pasteur (Latour, 1986). Moreover, since Galileo's ideas have not been refuted, it poses a bigger challenge for the analyst, who does not have the benefit of distance at hand as when studying, say, phrenology.

illustrated (but not only) by the experimental setting[54], that allows scientists to withdraw so as to let another agency act: in order to be able to claim something about a bacteria, biologists put bacteria to perform (while constructing bacteria as beings able to react to their questions). But this principle can be expanded to situations in which bacteriologists analyse a lake, psychologists let themselves be interpellated by depression, philosophers inquire into the secret of sexuality, sociologists interrogate social problems, etc.

In all these cases that which is analysed must have a way to alter the knowledge produced about it –with mediations, of course, but with a clear contribution. The invention of modern sciences, then, is "the invention of the power to confer on things the power of conferring on the experimenter the power of speaking in their name" (Stengers, 1997, p. 165). Knowledge is produced neither by one only actor (a lonely thinker) nor by two actors (a discussion among colleagues), but it is constructed with the participation of a third party –the object of study- which has something to say[55].

Having into account that a scientific object is not always a person, one might prefer the expression 'to inform' instead of 'to speak' or 'to say', but the point remains: this third party is expected to contribute with new information, to participate, to take part

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[54] The invention of the modern sciences, as it is related by Stengers, gives experimentation a privileged role. However, she herself will try to downplay this importance. First, to take up a proper experiment is not enough to consider something as scientific –consensus and power are still involved. A good example in point is parapsychology. It does not matter how hard they try, and how many 'scientific rules' parapsychologists comply with, they will never be considered as a science, because the rest of scientific disciplines –especially those neighbouring it- have *already* decided that parapsychology is not a science. But if this is so, it is partly because disciplines such as parapsychology have never been able to show that they have been constituted through an event. Second, maybe the event came of the hands of the experiment, but this means neither that non-experimental sciences are not real sciences, nor that experiments will be able, on their own, to recreate an event.

[55] On analysing science as a vocation, Max Weber (1992) claimed that scientists are purely and simply at the service of the *cause*. What cause, though? Here I cannot resist the connection with one of those etymologies that Duque makes available: the Latin word *causa* originated the Catalan/Spanish word *cosa* (thing, in English. *Causa* not in the sense of causality (cause, the originator of an effect) but in the juridical and politic sense: a collective discussion. A similar origin is found in the English thing and the German *Ding*: assembly of knights with vote and voice. At the service of the cause, at the service of the thing. If this etymology suggests collective discussion (of a community of people with vote to intervene in decisions), it also makes it clear that this is a discussion about something which is put under trial. Science is not simply a collective discussion of two parts (let alone one of chivalry, pace Merton), but these discussions must always involve a third party –something other than scientists themselves, be it a phenomenon, a thing, a proposition, a relation or a person.

in, to be a part of, the constitution of knowledge. Notice we do not say here that the object must "speak on its own", if by this we mean that it is a pre-existent entity that is simply introduced into the game without mediations so that it can have its voice heard: scientists constitute or create witnesses, they do not simply 'discover' them. However, it must be constructed in a way that it still has leeway to perform and answer, and precisely this leeway will be tested by colleagues, under risk of being accused of being a 'mere artefact'. Whereby the difference between 'fact' and 'artefact' is never one of 'spontaneous without human intervention' vs. 'elaborated by humans', as Lynch (1985a) has shown. Scientists assume from the start that they must invent, but they must do it so that the object in its turn can 'inform' and make a difference.

If I write a poem about a cow, I do not expect the cow to have much to say about it: she would not object, and neither would somebody in its name. Thus, if the poet says that "la vaca és cega"[56], nobody would ever think of opposing that actually, as a matter of fact, the cow is not quite blind, it has simply lost a 30% of vision in the right eye. This absurd comment (absurd, says Duque (1995), in the Greek sense, *atopon*, without place, out of place) would not add much to the type of knowledge and insight over the world of cows and its relation with humans we acquire through the poetic analogy (which informs us more about the world of people than that of cows, by the way).

This is not to deny that through poetry knowledge can be produced. Nevertheless, there is a difference between this and how a biologist would create knowledge about the cow, for now, if a scientist describes the vision system of a cow, the cow itself has quite a lot to say about it. Even though the cow does not speak and it can never offer 'its truth', it can, 'as it is', participate in the production of knowledge about itself, contradicting those scientific claims which do not agree with her. A squad of scientists, all claiming to speak on behalf of the poor thing, will take the cow's

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[56] "The cow is blind". A verse of a famous Catalan poem by Joan Maragall.

reactions into account so as to accept or reject their claims: the animal will be given a chance to object[57].

Let me hasten to insist that this demand of including explicitly and systematically a third party in the process of production of knowledge is not a necessary condition to produce knowledge. There are ple nty of other ways, and our culture as well as others have developed several. To return to the aforementioned example, both types of knowledge about cows are valuable, but they will be more or less appropriate depending on the use I want to put it to. If I want to reflect about the course of life, its ephemeral and vulnerable character (while hinting on passing at nationalist concerns), I would probably not ask a scientist. If I want to understand or improve the cow's sight, I would probably not ask a poet. Knowledge is not 'adequate' or not abstractly and out of context. As Jean-Luc Gautero (2003) writes, "Am I closer to a restaurant when I am two kilometres away from it down the road or when an impassable ten-metre deep grave separates us? This depends on whether I just want to see it or a I wish to sit and eat" (p. 68, our translation)[58].

But that there is no difference in value does not mean that its process of constitution does not differ. Thus, scientific knowledge has as a particular characteristic that it is produced out of a self-constraint: scientists attribute their constructions the power to constrict their freedom to describe the world 'as they want' in order to describe the world as it forces them to. Thus, scientists create third parties as actors in a discussion that, despite speaking only through scientists, take part in the creation and solution of a problem (if other scientists confer enough reliability on them). This is, then, the great achievement of science: to have found a way to include in our human

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[57] Science, through a chain of mediations, has managed to re-conquer "something one believed to have been lost: the power to make nature speak, that is, the power of assessing the difference between 'its' reasons and those of the fictions so easily created about it" (Stengers, 2000, p. 81). Nature is now an actor on its own.

[58] Likewise, it does not mean that poetry cannot be used, with modifications, to produce scientific knowledge; or that they are incompatible knowledge. Nor that scientific knowledge lacks poetic character. Stengers herself defines the experimentalist as a hybrid between poet and judge. Actually there is a upsurge of proposals to bring both practices in connection. See Glykos (2003) for some references of this intermingle.

discussions the agency of something other. And this is the achievement that any type of science should respect[59].

4. The event

With the invention of modern sciences, an event has entered the world. The notion of the event, which Stengers borrows from the philosophy of Whitehead and Deleuze, supposes the emergence of some novelty in an unforeseeable way. A relevant occurrence which is nevertheless contingent, not necessary: it happened, but it could as well not have happened, and it is neither predictable nor reproducible. But once it has taken place, it conditions facts coming after it, of which it will become a constitutive part. The event is the 'terrain of invention'. In our case, this event are the modern sciences, the emergence of a new way of arguing, a new of making facts themselves talk, while taking this setting not as an artefact, but as a true (mediated) communication of the world.

The complication here is that the power to submit one's construction to a third party is an achievement that responds to the logic of the event, which means that, no matter how relevant it is, cannot be provoked or reproduced, repeated by non-creative imitation. An event is like a gift: it is something given, which cannot be forced into happening, for its arrival escapes human agency. In some sciences, this event has occurred, and it is in these fields that one can talk of the wondering/rejoicing of science. There are other fields where the event is still to come, but this does not mean that they are not able to do good science, as long as they keep on looking for risky constructions, and risky relationships between humans and non-humans. These sciences just have to look for the establishment of other possible relationships between subject and object.

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[59] To run the risk means to construct an object while simultaneously conferring on it the possibility to unfold its being, to let be its possibilities of being. As if, next to the drive for control and domination, science also left space for Heidegger's passionate love. And this is not the plead for a new reason, a new science that would oppose the science criticised by feminism, postcolonialism and cultural studies, for example, but an acknowledgment of other images that science can produce in its bosom.

And still, some disciplines, attracted by the success of (experimental) sciences, have tried to gain scientific status through the wrong path: in the submission to a narrow definition of a method, in the experimentation through the control of independent variables, in the simple accumulation of data, in the simplification of relationships or of one's object in order to formalise them, in the obsession to stick to a particular aseptic rationality, etc. These answers are all misleading, for they confuse the event with the methodological justifications with which this event has been justified *a posteriori*. In their attempts to become recognised as science, many disciplines have turned into poor imitations of the so-called hard, experimental sciences, killing their creativity and engaging in nonsense practices.

These sciences actually condemn themselves to being nothing but pale reflections of science, since by trying to imitate others, they diminish the possibility to let an event happen –they mutilate themselves. And they do on constructing an object which has no other choice than behaving in the way the scientist has pre-established. Thus, they cancel the object's power to put them in risk, and without accepting the risk of being contradicted by the object, there is no possibility of good science. For now that we know that objects are invented there is not enough with claiming that every construction is the same. There are inventions which construct with constraints, and there are inventions which construct as an actor, that is, letting still place for the object to 'speak'.

Stengers derives several consequences from the emergence of the event, two of which are of interest for us now. The first, in line with present STS studies, will not be of epistemologists' liking (and probably neither what some scientists would like to hear): nothing seems to separate science from nonscience, apart from its claim 'not to be a fiction'. This is why epistemologists' task was never successful. We do not find a different logic, a different way of thinking, some particular method or rational procedure which grants scientific character. Nothing, in short, to grant the repetition of an event.

However, this does not mean that science is a task like any other (and this is the part which will not delight social constructionists), for an event really happened: a new use of reason and devices was created, enabling a third party to enter our collective discussions. The fact that one cannot describe how one got there, or how to replicate or reproduce this event, does not mean that the event did not take place. It simply means, following Deleuze's (1994) distinction, that the event is not of the order of the grounding [*fondement*], but of founding [*foundation*][60].

According to Stengers, it is this foundation which sustains scientists' certainty: they know that such events occur. Nothing grants a new happening; they only know they need to keep on working if they want to have any chance at all of a new event happening. In front of the questionings and challenges to science, they silence sceptics with their "yes, we can". 'Do you think it is not possible to discover the secret of life? You are wrong, we can – so do not lose your time philosophising about its possibility'. If it is feasible, they construct it, they engineer it into being. This is why the border between science and nonscience is of the order of the practical ("here, we can"), and not of the normative. In any case, the question "is this scientific?" has no settled answer, it is open for *in situ* negotiation[61]

There is a second consequence to be derived from Stengers' historical account on the invention of modern science. Scientific knowledge is a fiction[62], in the sense that it is a human elaboration. Nevertheless it is a type of fiction which, thanks to its capacity to create a mediated third voice, enrols enough power to 'silence rivals', to make a difference regarding other fiction claims. Paradoxically, then, and already from its invention, science magnifies the creative power of fiction to negate it so as to distance itself from fictions:

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[60] "The foundation concerns the soil: it shows how something is established on this soil, how it occupies and possesses it; whereas the ground comes rather from the sky, it goes from the summit to its foundations, and measures the possessor and the soil against one another according to a title of ownership" (Deleuze, 1994, p. 79).

[61] Even though once something is admitted into the community, this admission will be legitimised in its 'truly and thorough scientific character', as if this was not precisely what had been in question all along.

> "it is the obviousness of this power of fiction that constitutes not only the "terrain of invention" for modern science, but also the means by which it will stabilize itself so as to better detach itself from it. Wherever a 'new use of reason' is produced –and this is how I propose to identify the singularity of the modern sciences- it will imply and affirm the inability of reason alone to vanquish the power of fiction" (Stengers, 2000, p. 80, original emphasis).

Since their identity stands upon the claim 'not to be a fiction like others', sciences will define themselves through differentiation from any other type of knowledge. With this move, science's identity was created in opposition to what was still regarded as submitted to the power of fiction –storytelling, myths, arts, literature, philosophy. This is why the very demarcation between science and non-science is consubstantial with science, part of its identity since its very birth: science cannot but understand itself as 'other' to other knowledge. To put it in other words, both modern science and its politics were defined at the same time –differentiation of what is scientific from what is only fiction, plus the subsequent exclusion of the latter. A particular politics which constitutes 'science' as a singularity and silences those who may put it into question. A particular politics, moreover, which denies its political character.

It is at this point that scientists cannot escape the paradox: their claim of being independent of politics is a highly political claim. Their fervent defence of their autonomy, their defence of their exclusive right to talk 'in the names of science' –all this is nothing but the political discussion of who can enter the discussion. No wonder, then, that the questioning of the difference between science and nonscience is so controversial, for actually, what is being discussed is who has a saying in scientific matters. Should science and all its decisions be left exclusively in the hands of scientists, or do other members of society also have a saying? This is why the questioning of the autonomy is automatically a matter of conflict –of political conflict.

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[62] Therefore, the use of 'fiction' here detaches from 'false' and comes closer to the creative meaning

5. Science/Politics

It is part of our Western common sense that science mixed with politics produces bad science –scientific knowledge should be a bastion of neutrality. In an essay in Pandora's Box, Latour convincingly shows how this division was created already among the Greeks, precisely at a time in which another invention was being begotten, that of politics. Indeed, as Jean-Pierre Vernant suggests, Greece was the place, not where democracy as we know it was invented, but where the different ways through which a group can organise and constitute themselves were explicitly problematised and publicly discussed (in Stengers, 2000, p. 61). Precisely public discussion itself was the object of a debate that took several shapes. One of these discussions referred to human nature: should man be conceived as a 'political animal' or as a 'rational animal'? Behind this question, a battle was taking place: Socrates and Plato on the rationalist side argued against the political practices of the sophists.

As a result of this confrontation, a conceptual division was created between 'opinion' (politics) and 'logos' (reason), together with the subordination of the first to the second (Stengers, 2000). Latour shows this with a trenchant (and slightly spiteful) analysis of the dialogue between Socrates and Callicles (Latour, 1999). Thus, the difference between Reason and Power (or Right and Might) was constructed as an opposition that has conditioned how we conceptualise science *and* how we conceptualise politics as disconnected activities. This division has been reproduced and 'perfected' thereafter by struggles between Hobbes and Boyle, and later by Rousseau (and nowadays by particular contributions to the infamously famous Science Wars[63]).

The point is, though, that this particular solution was, and it still is, a device to erase public discussion, legitimising instead only the intervention of 'experts' in the decisions on knowledge. Politics is related with the organization of the common life

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with which Anderson (1996) talks of 'imagined communities'.
[63] For an exploration of heterogeneous positions on intellectual warfare, see contributions in Labinger & Collins (2001) and Jurdant (2003).

of humans (praxis); science, as fabrication of facts, is related to poiesis, concerned with things and utilitarian ends. This distinction also reproduces a division between those who are entitled to speak on behalf of science ('experts' in the aforementioned debate), and those who do not (lay people), between those people who can know the 'how', and those who can only play to guess hypothetical 'why's, the lay people:

> "The laws of 'nature', which have, in our world, announced their accessible character, express the fact that, in a new mode, the modern sciences have taken up Plato's old project: to create a relation to the truth in the name of which the Sophist could be chased from the city" (Stengers, 2000, p. 164).

However, both Latour and Stengers nicely show how science has actually united again what Aristotle distinguished, for every construction of fact links humans in a new way. This is one of the most striking characteristics of science, which once and again is made invisible by distinguishing it sharply from politics: the intermingle and entanglement between the world of things and the world of humans, between facts and history. Or rather, their mutual constitution. Indeed, the invention of modern sciences introduced an important change in the way to solve collective discussions: scientific discussions are not settled by scientists alone; the discussion is joined by the phenomenon of study (be it human or nonhuman) which will be recognised as 'authorised' to settle a debate, that is, "to 'make a difference' between the different possible arguments of scientists. If a fact proves resistant enough, it will become a constitutive part of science from that moment onwards. Of science, and through it, of society, since this fact has been attributed the power to intervene in our discussions. To express it differently, scientists 'socialise' entities which did not belong to society (Latour, 1999, p. 259), but that once they enter it, have constitutive effects upon it.

Think, for instance, of what has happened with genetics. There are many ways in which we Westerns have understood ourselves in the past, and there are many ways in which we could have understood ourselves in the past, in the present and in the future. But from all these possibilities one has been actualised with the invention of the gene, and it does not seem to become weaker by the day precisely, it does not

matter how much we stubbornly repeat that it could have been otherwise. Indeed, it could; scientists could have found another way to create a construction that would not be objected by the world (even though not any construction would have been appropriate). However, from the moment science invented genes and accepted them as constituents of our community, it is a heroic, almost impossible task to know oneself a human and not to see ourselves as gene-carriers.

Science, then, is an endeavour that invents and delivers new entities that redefine the limits of what we understand as community, fictions that propose new bonds of interest and meaning between artefacts and humans. A scientific proposition presents us with a fact that aspires to be recognised as a member of our collective, and with each scientific fiction which is constructed and accepted as a fact, our collective changes. A new collective of which the new scientific invention is a member, and whose introduction re-distributes identities and roles within the collective. Science brings about new ways of rethinking our 'being together' and, looked at it like this, it is a highly political endeavour:

> "Far from taking us away from the agora, ... [science] redefines political order as that which brings together stars, prions, cows, heavens, and people, the task being to turn this collective into a 'cosmos' instead of an 'unruly shambles'" (Latour, 1999, p. 261).

This evidence does not force us to conclude that there is nothing else in science than politics, or that science is reducible to might. This move would still be indebted to a dichotomic picture created by the debate between Socrates and Callicles –either knowledge is different from power, or knowledge is equated to power. But it helps us reject the opposition reason/knowledge/science vs. illusion/opinion/politics, on noticing that these poles emerged simultaneously out of a same problem as interdependent terms which mutually construct each other. And once we reject this division, we can reject the exclusionary politics that excludes nonscientists from collective discussions about the destiny of scientific inventions.

Differently put, the scientific event has made a difference, but it does not signify it. We can acknowledge the new use of reason that has been created without for all that accepting disempowerment. To accept the creative power of sciences does not mean that we have to submit to it: we are still free to negotiate and discuss what consequences the event should have. We should have the opportunity to discuss publicly what we should do with those inventions (for instance, up to what point we want to allow genes to define us, in what circumstances, with what limits, etc). This is the type of public discussions that are called for with pleads for cosmopolitics[64] (Callon, 2003; Latour, 1999; Rodríguez, Tirado & Domènech, 2001; Stengers, 2000).

This thesis is not the place to develop the notion of cosmopolitics. Nevertheless it is interesting to mention briefly the possibility to disturb the exclusionary politics of science in an attempt to show the political character of such demarcation criteria –of boundary work. Especially because the relationship between science and politics is quite visible in scientific discourses, and, as we will see, will appear later on with a different shape. It is interesting as well because the authors mentioned allow such a questioning without reducing science to politics. Instead, science and politics appear as activities that are different and similar, sustaining each other but never reducible to each other –extending each other's possibilities.

The acceptance of the ideas presented in this third section do not always have consequences regarding the boundaries of science and politics –it also has consequences for the way in which we conceptualise our relation with our objects of study. To this argument I will now turn.

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[64] This is a path that rejects the dichotomic choice between a pure science that pleads for its autonomy and a cynic view situates science always on the side of power. It looks for ways to recognise both, science's singularity and its vulnerability to power. Thus, for instance, public discussions in hybrid forum, would try to elaborate collectively a new regime that acknowledges the capacity of science to imagine and create new beings and introduce them in our society, with a political debate to make tolerable the type of collectives thus created. Thus, Callon advocates for new collective experiments and social reengineering. Stengers creates the notion of cosmopolitics, Latour talks of Parliament of things and politics of nature, etc.

6. Demarcation revisited: Stengers' Shibboleth

There is an important consequence of defining science as she has: the differentiation between object (researched) and subject (researcher) is brought into play. Indeed, a scientist must manage to produce an object distinct enough so as to contradict her. Only so does it make sense to put it to the test: it does not matter how constructed the object is, it must be so created that it has a chance to *object*.

Stengers' proposal is provocative. We are used to identify the traditional scientific perspective with the claim that object and subject are different, and critical approaches to science have attacked this postulate emphasising their inextricability. Is this not a step backwards? However, she is not suggesting that object and subject are distinct as an a priori condition to do good science –actually, her use of the notion of 'invention' prescribes such a reading. On the contrary, this distinction can be assumed as an effect of good science. If we want to produce science, the scientist must run the risk of being proved wrong, and this requires the construction of a distance between subject and object. In this sense, the *risk* involved in this distancing is a requirement to talk of scientific knowledge[65]:

> "[Stengers'] equation is simple, although very hard to carry out: no risk, no good construction, no invention, thus no good science and no good politics either" (Latour, 2000, p. xix).

This proposal is a translation of Popper's falsationism (and of his negative notion of truth), in that it keeps the ethical stake to which a scientist must submit. However, the advantage of her demarcation criteria is that it is highly relational. It does not make sense to ask if a particular object can be approached scientifically, nor if science is a privilege of concrete disciplines. Neither experimental approaches can gain the status

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[65] In this point, she perceives similarities between her position and Sandra Harding's, who also keeps the distinction between object and subject, while changing the meaning usually attributed to this distinction: the distinction "is recognized not as a right, but as a vector of risk, an operator of 'decentring'. It does not attribute to the subject the right to know an object, but to the object the power (to be constructed) to put the subject to the test" (Stengers, 2000, p. 134).

of science, nor is psychoanalysis automatically dismissed from it. It all depends on the particular relation that objects and subjects are able to invent.

These reflections are not abstract: they have strong practical consequences for research, including that carried out by social psychologists among scientists. I dare say that they are even more important for us, since living beings do not remain indifferent to the questions posed by the scientist, they can interpret these demands from their point of view, and therefore, can make themselves exist in relation to it. In other words, what for the subject (scientist) are conditions for the production of *knowledge* are simultaneously conditions for the production of the existence of the 'object' (which, for other perverse reasons, is usually called 'subject'). The 'object', then, looks and asks back and reacts to the inquiry. Differently put, the object she is dealing with learns or changes ontologically with the questioning, and therefore cannot work as authority.

In this situation, the scientist is an author running a *pathetic risk:* the scientist must accept the responsibility of posing a question, for each question has not only epistemological effects, but also ontological. This responsibility will send her more questions in return: 'Who are you to be asking me this question?' 'Who am I to be asking you this question?' This entanglement does not mean that there is a dissolution between object and subject; for her science is always a question of "inventing about", and not of "inventing with" (which also justifies that science cannot be reduced to politics). Rather, it means that we must think even harder to achieve this differentiation in a way that preserves risk and inventiveness, instead of reproducing a relationship of forces; a way that does not mutilate our object of study. Because one should not forget that in science, an ethical question is always also a technical question:

> "Rather than a strictly ethical question, this is much more a question of what Félix Guattari has called a 'new aesthetic paradigm', where *aesthetic* designates first of all a production of existence that concerns one's *capacity to feel*: the capacity to be affected by the world, not in a mode of subjected

interaction, but rather in a double creation of meaning, of oneself and the world" (ibid, p. 148).

If some of these ideas are not new (critical social psychology has somehow been built upon the awareness of the ethic involvement between psychology and its 'subjects'), her insistence on the differentiation between object and subject poses interesting questions when we apply these insights to our own ethnography. It seems clear, even more so after reading our tales, that members of the IBF are 'objects' able to answer back 'who are you to study us? What do you want from us? And we are emplaced by these interrogations. How do we construct our objects? What risks are we willing to run?

7. Representing scientists

When discussing the suitability of attempts at representing others, debates usually involve 'others' very politically correct (Michaels, 1997[66]); that is, others almost everybody agrees one has to 'respect', 'represent' and 'speak for' with accuracy, trying to 'transmit' faithfully their voice. This is especially the case when the study aims at understanding how a group of people conceive of life, their world construction –whether we follow this aim for the knowledge's sake, or to help this group being understood by society. '*When we talk of them, we also talk on behalf of them: if they themselves talked, they would say the same*'. However, in the case of scientists, this motivation is not that clear. Rather, we seem to be quite keen on telling a different story from the one told by natives themselves: the act of ventriloquism that representation incorporates is here revealed lewdly: we find one of those situations in which the ambiguous and mobile union of speaking for, and speaking about, allows us strategic sleights of hand (Lee, 1994).

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[66] See his critique and Condor's (1997) to the way Sampson (1993) uses 'women' and 'black' people to strengthen his own identity.

Such discrepancy is usually justified as an emancipating strategy: it may be politically important to provide the readers with a different version than the one held by natives in order to disrupt power relationships. Thus, ethnographies among doctors have often aimed at a challenging of medicine's authority in the name of patients' empowerment; ethnographies of the police have tried to put in evidence the repressive logic of institutions; ethnographies in private companies has shown and analysed subjection strategies at the womb of organisations, etc. Works in STS tends to fall in this second case. Researchers usually assume that scientists are a collective which do not need empowerment –they are usually perceived as too powerful: the world's view of this collective has become not only their reality, but our reality, Western civilisation's reality; to put it crudely, their reality coincides with the 'state of Nature'. Hence, the importance of STS' *leit* motive: to show that science is an activity like any other, subjected to exactly the same kind of constraints as any other social activity is supposed to show that 'things could be otherwise'. This would be the first step to 'question' or 'doubt' science.

But wait, because I hear the sound of teeth. The parasite seems to be at work (Serres, 1982), and somebody is having an ordeal. An on-going feast where the guest eats without saying thank you... I will now ask you to turn back to the opening quasi-tail, in which our young ethnographer felt half-threatened, half-seduced by a community that felt in turn threatened by her approach. Or do you think it is chance that they called her Psycho-killer? With a stroke of genius of which they must have been unaware, they said through this nickname all they had to say: "you are feeding on us". Binary oppositions, when they are used to justify confrontations, hide the relational context in which identities are produced (Gergen, 1991, 1994) –even those elaborated in ethnographies. Intellectual warfare. After all, it is no news that social scientists have created their identities in relation to those of natural sciences –either imitating them, either trying to gain a distance (Domènech, 1998; Latour, 1991). And the emergence of the Science and Technology Studies is no exception: we consume scientists and grow big: new fields of study, new training courses, new departments: a

colonising move, a consumption of the other, a feast with which to feed our identities (Pallí, 2001).

What happens if, in order to show that things could be otherwise, we have fallen into a battle of categories –we social scientists vs. they natural scientists- and have placed ambivalence on one side? Of course, it made sense, on wanting to study science, to travel in search for the exotic other (Woolgar, 1988b), to labs of the so-called 'hard' and 'exact' sciences, given its repercussions in society. But confess: did this choice not betray a mixture of admiration and resentment? At least, a certain amount of bellicosity –let us attack the dominator- and once the antagonism is in place, distribution of virtues and sins follows. We social scientists know that sciences (by which we often mean natural sciences) are intrinsically linked to Power, Capitalism or Male domination –unlike us, feminine social sciences, dolphins and other delicate natures (Mulkay, 1991). Thus, some approaches to science have had the tendency to assume they know what the identity of science is and what mechanisms science uses, while projecting homogeneity (be it power, consensus, ideology, politics, patriarchy, domination). In other words, they have taken the position of the judge, the gaze of power (Stengers, 2000)[67].

The problem with this movement, warns Stengers (1997, 2000)[68], is that, when one explores the world 'in the name of social sciences', one tries to find the social object everywhere, and may lose other aspects of one's object of study (Schillmeier, 2003, 2004). There is nothing wrong in sentences such as "science is social", "science is linked to capitalism", "science has power effects", as long as these sentences do not go accompanied by 'only'. Only social, only power, only money, only politics... This is a reductive, imperialistic move. When social sciences decide that science is not a virgin, unknown and out of reach territory any more, but a legitimate terrain to conquer; when social sciences decide that science is a continent to colonise (Lee &

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[67] "The gaze that sees the same, the undecidable, where those he is observing have as their raison d'être to create difference, is the gaze of power" (Stengers, 2000, p. 74).
[68] In this point, her critique coincides fully with that of a symmetrical perspective (Domènech & Tirado; 1998; Latour, 1987, Law, 1987).

Brown, 1994), or an other to consume (Pallí, 2001), then, what else than 'society' (consensus, power, social opinions, ideology, prejudices, stereotypes, social influence, interpretative repertoires) can they find in their explorations?

No wonder this approach horrifies scientists, who resent the amputation of their object of study, which they know more complex[69]. Of course, the degree of offence expressed by scientists has sometimes been interpreted as a proof of the acuteness of the social interpretation –that is, as a reassurance that 'we' have hit where it most hurts, and very rarely as a problem in our interpretation. The tension created by the creation of an (ethnographic) account that is so emphatically contradicted by the indigenous members of the community has often been disguised under the sign of irony. However, these categorisations and dichotomic definitions end up in reifications. Not that categorisations are problematic per se, but they are if they are used to cut the flow of relations, to fix and imprison others' identities –and let us face it, they are put to this use quite frequently.

Members of the IBF were curious: why us? Why do you study us? And, as I have explained, they were not particularly impressed by the answer. They felt the weight of my reductive approach. In their exaltation, they used counter-examples and questions that are somehow familiar for social scientist researching upon science: gravity laws ruling falling objects everywhere on earth, comparisons between scientific knowledge and sorcery, between Western medicine and other cultures' healing systems ("would you not take antibiotics if you had an infection?" "Would you not operate if you had cancer?"), and challenges to try to disobey nature's laws (if there is no reality, why don't you jump out of the window?). These are the "death and furniture" type of arguments (Edwards, Ashmore & Potter, 1992) that we have sometime or another had to hear.

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[69] Not only scientists themselves. Somebody like Kuhn, who did so much to bring to awareness the role of the social and political in science, resented a certain 'excess' of the social in approaches to science: "If *Structure* is a contribution to the sociology of science, then the core of that contribution has been missed or else denied by many of those who trace their own work to it. My concerns... have indeed been sociological, but they have also and inseparably been cognitive or epistemic... What is it

In front of these reactions, which nevertheless reveal misunderstanding, we had several options. One, to ignore them and even laugh at them –what a naïveté!- with a fine irony[70]. Two, to interpret that scientists complained because their privileged position as source of power and legitimacy was being challenged. Or three, to consider why else could they be so offended, and whether this reason could have something to do with us. To choose the third is to recognise the situation as an ethical one (Stengers, 2003) –making clear the link between ethics and ethos: to recognise that it forces as to put into question "the way in which one defines oneself in relation to others, while defining them as mates, preys, depredators, people to manipulate or to seduce, people to laugh with or to laugh about" (Stengers, 2003, p. 279). Is there something in our approach that provokes such a scandal? If so, is this 'something' worth defending? Do we put differences to work for war or for peace?

Now, we can construct an object such that it corroborates all we already thought before we stepped into fieldwork. We put our categories and identities to work, and do not leave space for their self-definitions: we hold them so tight that we scarcely leave space between our position and that of 'our subjects'. The object just says 'yes' (and when it says 'no', we laugh stronger and interpret it as a 'yes'). We leave fieldwork as we entered it. Or we decide to run some risk, put our construction to the test, and accept a potential 'no' as precisely this, a potential 'no'.

What are they protesting against? Can this work for us as a 'no'? What has their 'no' to do with me?

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about what scientists do, I have been asking, that makes their output *knowledge*?" (Kuhn, T.S., quoted in Keller, 1998, p. 18)

[70] See Woolgar (1983) on the uses of irony; see also Stengers (2000).

8. And now what?

Does it make sense, then, for social sciences to study science? The answer, to my opinion, can only be affirmative, as long as we avoid the reduction of the sciences to the same, whatever same is defined; as long as we avoid judgment –the power to decide what things are (Stengers, 2000); as long as we are able to turn, as Stengers suggests, the principle of symmetry against ourselves: to force us not to decide who is the victorious and who the defeated. As long as we are able to join Stengers' reading of Leibniz' constraint: "philosophy should not have as its ideal the 'reversal of established sentiments. (...) not to collide with established sentiments, so as to try to open them to what their established identity led them to refuse, combat, misunderstand" (Stengers, 2000, p. 15). This does not mean one cannot disagree, or one cannot hold a different vision of a community than that of its members. Of course one can. However, the question still remains whether an offensive account that claims to know better creates the possibilities to discuss publicly the issue, or whether such an attitude –pretension to know what science truly is- does not only give birth to a war. Offensive narrations close debate, other attitudes may open new possibilities of discussion.

In order to avoid a confrontation strategy, while not swallowing native descriptions as a true account, we will attempt to articulate a way of 'talking science' that leaves battlefield by side. This work is neither an account that presents science as the truth, nor an anti-truth device for ideological exposure. These pages articulate a particular description of science that enables an understanding from a particular perspective. Native accounts are not taken for granted, but we do not explicitly aim at contradicting them. Rather, I will try to make a space for them, either accompanying indigenous narratives at some points or problematising them at others.

Maybe a good strategy is not to forget our ambivalent position regarding the sciences –half in, half out. If ours has always been a love-hate relationship, with its contradictions, ambivalences and ambiguities, ethnography cannot but make them

present in all its intensity. This is a conundrum that undermines us if we try to hide it, but also a situation full with creative possibilities, as Spivak clearly pointed when she said that "it is especially important to choose as an object of critique something which we love, or which we cannot not desire, cannot not wish to inhabit, however much we wish also to change it" (Spivak, 1996, p. 7). Partially in, partially out, we remain nevertheless connected to some practices that we can explore from the interstices. Neither a communion of the same nor a war of differences, what type of relation can we imagine? In what ways can we move each other?

1. Description of the IBF

As an undergraduate I attended a course in qualitative methods, ethnography among them. I was quite mesmerised –it looked so exotic, so exciting, though I could not suspect then that a couple of years later I would have the privilege to engage in one. Among some of the advice the lecturer Lupicinio Íñiguez gave us, I remember recommendations to be accurate and to write down every sensation I gathered from the fieldwork, especially first impressions. One never knows what our first impressions may be revealing to us during analysis[1].

It was with this command in mind, and my eyes wide open, that I stepped for the first time into the research centre that will become the main character of this thesis. And, if I have to believe my field notes, I summarised this first impression with the following words: "Autònoma aesthetics, this place has the aesthetics of the university". A bit disappointed by this first thought –I had expected a more spectacular impact - I simply wrote it down, obediently, in my notebook. It was not until much later that the importance of this statement would strike me again, for indeed, the fact that this centre belonged to a university has been a determinant throughout its history –for better and for worse: people working in the laboratory depend on the university; scientists are also teachers in continuous contact with students; lab budgets are determined by the university; decisions affecting research are conditioned by administrators, and so on. The IBF is somehow constituted by the creative tension between scientific laboratories and the bureaucratic machine of the university, in a typical dual structure proper of the academic world (Strathern, 1999). But before we can discuss some of the practices and processes shaping this research centre, some introduction is required.

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[1] The lecturer accompanied his advice with the following anecdote. One of the persons in charge of an ethnography in an old people's home wrote down, as his first impression: "here it smells like wax". This sentence was the hint to suspect that, in spite of formal claims to the contrary, the nuns running the institution were exercising pressure on the residents to take part in catholic rituals.

At the time we were there, the IBF was composed of eight research teams, each with its own laboratory. Certainly, it was not a centre of the big dimensions which we customarily find in the STS literature, but neither was it small in Spanish terms, and it has always enjoyed a good reputation within and beyond Spanish borders. Founded in 1970 to promote research unimpaired by disciplinary borders, the IBF hosted research groups of Enzimology, Molecular Biology, Microbiology, Applied Microbiology, Biocomputing, Immunology, Cellular Immunology and Neurochemistry, as well as some 'Services' for the university and wider scientific community: the service of cultures, the service of sequencing and modeling of proteins, and a small but very well equipped library where one can find the specialised journals of several fields. These services will be described later on with more detail.

Originally part of the Sant Pau Hospital in Barcelona, the IBF found a stable home in the campus of the Universitat Autònoma de Barcelona some years after, where it remained ever since. This setting gives it a particular geographical position; with advantages in the sense that it is in intimate contact with the university and its other research teams, industrial parks and institutions but also with disadvantages because the institute remains far away from other laboratories and institutions in the area, in spite of being well connected through trains, roads, highways with Barcelona and other cities.

The centre is located in a two-story building on a hill of the university campus that isolates it from the other buildings. The Medicine and Science faculties are its immediate neighbours, at about a five-minute walk up and down the hill respectively. The physical aspect of the IBF is not very different from the rest of the University. Despite the shy attempts by grass and bushes to colour the campus with life, concrete walls, cheap floor tiles and grey colours predominate everywhere, matching with the functionalist (bunker-like) aesthetics of a university built at the end of the sixties (1968) in the final stages of the Franco dictatorship. From outside, then, the IBF does not stand out, it could well be a faculty, and visitors first realises that this is not the case when they find laboratories instead of classrooms.

The first floor is subdivided in two spaces differentiated by a door. After crossing the entrance door, visitors find themselves in a not particularly nice hall, a mixed space that results in a combination of reception and administrative uses, giving one the wrong impression that the centre does not receive many visitors. In this undefined space one finds tables and chairs in one corner; a photocopy machine in the other; the door to the secretary's office, from where a person can easily watch over the entrance to the building and especially to the director's office; the entrance to a very well resourced library; a meeting room for discussions and presentations; and toilets. At the end of this hall, a grey door, always open, leads to the laboratory section: a corridor with labs on each side. The floor downstairs is similar in structure containing laboratories and offices [2], as well as a pantry. Every laboratory is equipped with a variety of instruments, tools, machines and computers.

A hasty inspection of the building could overlook one of the most important pieces of equipment: *the* coffee machine. Around it people from all laboratories meet, relax and chat, as well as take a coffee before, during and after important experiments. Next to it many discussions take place, problems are discussed, parties are organized, proposals of collaboration are offered, any kind of information is exchanged... In a very literal sense, the coffee machine creates a space allowing a heterogeneous circulation which gives consistency to the collective spirit of the IBF: scientists from different laboratories get to know each other and exchange their points of view there, and this includes heads. This is one of the places where the small societies constituting each individual laboratory articulate to produce 'the IBF'. If the distribution in laboratories performs division (one group per room), this coffee machine weaves unity.

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[2] One of the laboratories of this floor was empty when we visited the centre, which was informally called, not without irony, the room of the 'potent group'. This was a room which was supposed to host a new, strong group, and which remained empty for a long time, waiting for the proper candidate to arrive. By the time we were there, a new group joined the centre, occupying this free room. Thus, we could witness (and register with the camera, which contributed to our credibility: "the psychologists

The IBF, as we have already emphasised, belongs to the Universitat Autònoma de Barcelona (UAB). However, ever since its creation, it was conceived as quite special and privileged an entity, able to trascend the proper organisation of the rest of the university. Adscribed directly to the university and not to a particular department, the IBF was the first interfaculty and multidisciplinar centre of the UAB, born as an answer to the demands of contemporary scientific research, an attempt to imitate the free research institutes depending from the central government, the CSIC (The Spanish Research Council)[3]. Multidisciplinarity is a key word in the descriptions that members of the IBF give of themselves. And this means, for example, that if a scientists has a problem in a topic which is not his or her speciality, there is surely somebody in this institute who can solve it: "all we have to do is cross the corridor" (EI01-10). Or as somebody else put it, "in the IBF there is a very high concentration of knowledge, techniques and know-how" (EI13-09). Another advantage of joining so many different groups together is the possibility of sharing resources –which is compulsary, according to the statutes of the centre. Every group which joins the centre must put its machinery and equipment to the service of the rest of the members.

Interdepartmental and multidisciplinary as it is, organically the IBF hangs directly from the vice-chancellorship. It is governed by a Plenary and an Executive Councils. The Director, Vice-director and heads of the Research and Teaching Commissions are democratically elected every two years. The institute has juridical personality, *as if* it was a department on its own. But the 'as if' is important here: unlike departments, the IBF cannot have members adscribed directly to the centre and it does not have the capacity to contract members. Thus, it cannot have its own researchers, something which will have drastic consequences in the constitution, membership, live and atmosphere of the centre. The staff of the IBF, then, is not paid by the institution itself but by the university. And research staff must be adscribed to

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are recording an empty room!") how a laboratory and its space were created anew. When we left the IBF, this group was already starting to carry out research.

[3] Laboratories and institutes adscribed to the CSIC appered often as a reference point for many members of the IBF; either as a model to follow, or, less frequently, as a model from which to take distance. In any case, the IBF is linked to the CSIC from its conception.

another department in order to be able to remain in the IBF (with the exception of Ph.D. students, who belong, albeit temporary, only to the IBF). As the director of the centre puts it, the IBF is a scientific institute which, in order to exist and survive, must "fit" into the environment of the university (EI01-13).

Thus, all the heads of the research groups, including the director and the vice-director of the IBF are members of other departments; the post they occupy belongs to these departments, and are paid by them. In other words, they all belong to the university, and need to be either lecturers or professors. They must decide, with the consent of their departments, what percentage of their dedication will be devoted to the institute. Thus, for example, the head of a research group may dedicate 50% of her time to the IBF and the rest 50% to the department. Other heads may decide, with the aproval of the department, to devote the 100% of their time to the IBF. Independently of the time they dedicate to the IBF, these lecturers and professors still need to attend their duties as members of the department, that is, they still need to attend meetings, participate in discussions, and, above all, teach. Teaching takes a big part of their time.

The budget necessary for the maintenance of research, administrative expenses and the services is given by the university. However, the university does not fund the research, and each of the groups needs to find funding on their own. For instance, through a) schoolarships from the university, the local government and the central government and the UE; b) collaborations and contracts with private companies. This circumstance imprints an important characteristic on the organisation. Groups coexisting in the IBF do *not* compete with each other for resources. This does not mean that one cannot find unequalities among them, battles about the distribution of the budget and investment of money, suspicacies and reproaches which heads direct to each other. As any organisation, the IBF experiences tension in its bosom. But, in principle, the number, size and prosperity of one group does not impinge harmfully on the others. On the contrary, the more blooming one group is, the more advantatges may bring to the whole centre –funding, equipment and Ph.D. students.

All in all, the IBF is one of the strongest research centres in the Spanish landscape. Very few centres have available as much infrastructure, equipment and staff as the IBF, and even less produce such an amount of both research of good quality and Ph.D. students. With a good reputation inland, it is also renowned abroad: some Ph.D. students told us that the mere mention of their heads' name can open doors for them to do postdocs or collaborations in foreign laboratories. This international projection is actively promoted by the centre, which aims at producing competitive results and publishing internationally. As the director of the centre expresses suscintly but convincingly, "we are and want to be present at a European level" (EI01-17). Nevertheless, the development of this centre is held up by several serious hindrances. The first one -probably all Spanish scientists would agree- is the lack of economical investment by the government in science, lower than in other European countries (such as France, Germany, the Netherlands or Denmark). Competition at European level is difficult under such conditions. This notwithstanding, results are quite impressive, as the director of the centre admits:

> "I think that we can be satisfied in the field of molecular biology, biotechnology and biochemistry, because we have reached a level comparable to other Europen countries, and we do it with resources which, with some exceptions, are globally inferior. Especially in staff, in high qualified technicians" (EI01-17).

Another hindrance, which combines with the previous one, is scarcity of space. The IBF would need to increase the number of groups and members in order to expand (it would have to increase, as they say, its "critical mass" (EI01-16): such an increase would bring more members, more resources and budget, more production –and, of course, as the director is fast in pointing, more headaches... However, expansion is partly hindered by lack of space, which in turn hinders growth[4], in a circle quite difficult to break.

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[4] Of course, they have already thought of the possibility of constructing another building in order to expand the centre. But this means a lot of money –land, building, new equipment, etc... And one should not forget that the IBF has no money on its own, it depends on the University, which means

2. Members

Administratively, members of the IBF are divided into research staff (scientists) and Service and Administrative staff (from now on, "P.A.S." or "Personal d'Administració i Serveis"[5]). We will describe each category apart, since they have different profiles, tasks and problems.

2.1 P.A.S.

If the university is the institution where researchers and teachers exercise their functions, this institution cannot work properly unless there are other figures seeing about administration and bureaucracy. These figures are called, in general, P.A.S. and under this label we can find three different categories: secretaries (subdivided in turn in 'administrative' and 'auxiliary'), librarians, maintenance technicians and high-qualified technicians (TAQ). The P.A.S. are either civil servants or have a contractual relationship with the university. Unlike researchers, they have a fix timetable, from 8:00 to 16:00 or 9:00 to 17:00, and they need to clock in when they arrive and when they leave –with the exception of some TAQs. They depend on the administrative organs of the Faculty of Medicine -in particular on comissions such as "Gestió Acadèmica" (academic management) and "Administració de Centre"[6] (centre administration)- with the exception of the librarian, who depends on the Faculty of Sciences. All these figures, nevertheless, are at the same time subordinated to the

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that such expensive projects need to be aproved, funding possibilities need to be gathered. Expansion is not easy. Nevertheless, they tried it once, although the project was not successful unfortunately. Taking advantage of some unconstructed terrains next to the IBF, they had planned the construction of a new building next to the centre. In this new building they would have invited the laboratory of biology of the CSIC, the CID, with the aim of creating a huge, hybrid institute between the UAB and the CSIC. Due to a combination of several reasons –in which the distance between the UAB and Barcelona played a part- the project had to be abandoned.

[5] "Administration and Services Staff".

[6] Some occasional administration questions (such as leaves and permissions) may be solved by the secretaries of the IBF themselves.

director and vice-director of the centre, who are in charge of defining and controlling their tasks.

In the IBF, the process of attributing tasks to the P.A.S. seems to be quite implicit. It looks as if it was assumed that P.A.S. already knows what to do, and what is expected from them. As one of the maintenance technicians said, "you do what you see needs to be done, what nobody else does" (EI06-03). In a similar way, when new Ph.D. students enter the IBF, they are only told informally and partially what they can expect from the maintenance technicians or technicians. This lack of explicit information sometimes occasions some problems -problems of indefinition, which may be quite harmless, but also some frustration (either in those who expect more from the P.A.S., or in those P.A.S. who are demanded too much).

2.1.1 Secretariat

An institution such as the IBF could not work properly without a strong team of secretaries. The team has two and a half secretaries and a librarian. Yes, you've read it right, two and a half secretaries. The puzzle is solved like this, this half position is absorbed by the librarian, who is supposed to dedicate 50% of the time to secretary tasks and to the library - although the library usually requires the totality of her dedication.

The tasks of the secretary team are difficult to delimit, but when one presses the point, they summarise their own tasks as "providing researchers with the necessary administrative support, so that they can have all they need, in time, and at a good price" (EG06-01). This may involve activities so varied such as ordering material, prepare bills, fill in bureaucratic forms, tape-writing curricula, projects and other papers that the research teams may need to send; organising meetings and conferences, etc. Moreover, there are other tasks which aim at giving support to the institution, like promoting some of the services of the centre within the university,

providing others with information about facilities in the IBF (or the IBF with information about others, such as government or university regulation). In short, as the secretaries put it, "the function of the researchers is to carry out research; ours is solving all the nonsense stuff so that they can do their task in peace" (EG06-19).

It was said that secretaries do not want to stay for too long in the IBF and that after some years they all apply for other departments. This information seems difficult to understand, since everybody, secretaries included, insisted that the work atmosphere in the IBF was very good. After asking them directly, secretaries gave us a plausible explanation for this high index of rotation. It seems that work in the IBF is not properly recognised by the bureaucratic machinery: the salary is too low for the amount of responsibility and work it involves and possibilities of promotion are rather low. All these conditions sometimes cannot make up for the good atmosphere of the working place.

2.1.2 Maintenance technicians

One of the first things we learnt when we enter the laboratory was that one of the maintenance technicians, one of the veteran members of the institute, is crucial for the good running of the centre. We had only been there once or twice, and people would already say to us: "Oh, if you want to know how the IBF works, you need to talk to him". "Have you already talked to our technician? You must!" "If you are around for a while, you will sure realise how much we depend on him. You'll hear people calling for him continuously". You can guess our curiosity.

Every single thing they told us about this technician happened to be true: he is everywhere, he helps everybody, many laboratories depend on his work, and they all call and need him all day long. We even saw notes hanging at the doors of laboratories with messages such as "we need you, come along when you have a minute, please!" Thanks to his efficiency and responsibility, he has gained the respect

and appreciation of the rest of the members of the IBF. So much so –they say- that, were he to leave the centre, the IBF would never be the same. "We would have to close the IBF... I don't think the heads would let him go –we were told- surely they would try to keep him". (EI04-24).

The centre has two maintenance technicians to give support to the laboratories, specialised in different aspects. The first one is in charge of the technical assistance for all the laboratories –he takes care and repares (when possible) all the non-specialised machines and equipment of the whole centre, of infrastructure (if new equipment is needed, he obtains information about prices and offers in the market, and visits professional fairs when organized), and of the whole building (electricity, gas, heating). He is also in charge of both biological and physical security issues, for which he needs to attend seminars and new courses periodically: he collects, stores and disposes of all toxical and radioactive fallout from the whole centre[7], and makes sure that security devices are in a good state. Apart from his dedication, he has some abilities which have made him famous: he is able to fix almost everything. What is more, he is able to create any gadget and utensil with his own hands and a few pieces. Thus, if you have an emergency, he is the man you need. He is said to have saved experiments running the risk to be damaged because of the failing of a machine, to have created refrigerators, to have fixed pipettes. The second maintenance technician is qualified in animal treatment (an euphemistic label which encompasses puncture, bleeding, sterilisation and inoculation). For instance, he takes up blood analysis, develops anti-bodies in the blood of animals, which needs to be recovered afterwards when the animal is killed. He is also in charge of cleaning and sterilising tools and material from the whole centre. He also needs to know how the equipment works, in order to be able to inform new Ph.D. students. Like the first technician, he applies for courses and seminars to update his knowledge.

Serious matters are mainly discussed together with the director and the vice-director, whereas on an every-day basis they are more in touch with the vice-director. Whereas

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[7] Until a specialised company comes to pick them up, or they loose their contaminating character.

nobody controls the way they organise their time and dedication, they do have to be sensitive towards, and adapt to, the rhythms of the different laboratories. When members of the IBF needs something from them, they can go and talk directly to them, regardless of the hierarchy, since they say that they do not care "who orders them a task, a Ph.D. student, a doctor, a professor, the head of a department... Work is work anyway" (EI17-6). As they work in the same space as the rest of the researchers, and any of the eight heads or of the sixty-two Ph.D.-students may need them and call them, they are continuously under control: "this is the worse kind of control: seventy people looking for you continuously" (EI17-14).

The profile of a maintenance technician is quite specific for the task to be done, that is, it is a profile very suitable for the IBF, and less adaptable to other environments. Maybe this is one of the reasons why maintenance technicians have remained so stable there. But apart from the specificity of the profile, another important reason for this stability is the satisfaction that maintenance technicians express regarding their position and responsibility within the IBF.

2.1.3 TAQs and the services

Some research teams are in charge of what is called 'a service': they offer some 'service' or 'task' the beneficiary of which is the whole research community[8] of the university, that is, they perform a task -for instance, they run a piece of equipment or carry out an experimental process- whose results will be delivered to the group that entrusted the task in the first place. A group cannot decide on its own to become a 'service': this is a qualification conferred on the group by the university vice-chancellor (on several grounds: depending on whether this task is perceived as necessary for the community, or profitable, for instance). The creation of a service also involves the creation of a new post for a TAQ or 'high qualified technician' that

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[8] Any member of it may require this service, usually for free, or in some cases, only after paying a low fee to cover the expenses, when the task involves expensive material. (Researchers of other universities may still apply for the service, although they pay a higher price).

will lead the service. This figure, contracted and paid by the university, is not supposed to research but to provide a more specialised support for others to research –its profile, then, corresponds to a person with a degree, not necessarily with a Ph.D. degree, although to be a doctor would not be an impediment either.

At the moment we were there, the IBF had three TAQs, leading three different services:

1) Service of cellular culture

2) Service of proteomics

3) Service of bioinformatics

These three became two for strategic and bureaucratic reasons: the service of cell culture, and the service of protein sequencing and modelling. It was considered better to have two strong services than three not that strong; in this way one can exercise more pressure at the Vice-chancellor office, in order to ask for the transformation of a temporal vacancy into a permanent one. For the sake of clarity, we will nevertheless describe each of the three separately.

Service of cell cultures

When one does empirical research, one needs raw material to research upon –in other words, one needs proteins, genes, numbers or bacteria, for instance. Obtaining this raw material is not easy, and it usually requires quite a difficult, time-costing procedure. Laboratories may decide to produce their own materials, or, alternatively, order this elaboration to a TAQ. This is exactly the task of the Service of cell cultures: the cultivation and maintenance of cultures of cells, as well as the development of any technique needed to supply a laboratory with the cells or bacteria they need. The TAQ or 'technical director' of the service is directly under the instructions of the director and vice-director, who is also the scientific director of this service. Whereas tasks related to culture and maintenance of cells are rather

repetitive, the development of new techniques may give occasion for publication, as long as this does not interfere with the functioning of the service.

Service of proteomics

This service, as its name reveals, sequences the chain of amino acids composing a protein, in order to identify the protein and its expected behaviour. The TAQ is in charge of a complex, delicate machine: the protein sequenciator. Since the functioning of the machine requires expensive reagents and maintenance, customers have to pay a low fee. With this money they first buy these reagents, and second, save some money in store for the next reparation –as it will inevitably come. (Making profit is not allowed). Customers from outside the university (from all over Spain, mostly from the rest of Catalonia) pay a higher price for the sequencing of their proteins. Sometimes, when the price is too high, customers propose alternative ways of paying: for instance, they might offer the IBF group to participate in their research and co-sign the resulting publication. Thus, the service of protein sequencing is also a very interesting device to create contacts. Not unlike the previous service, this one is clearly overworked –even with a waiting queue. Despite the long lists, members of the IBF have always preference.

Service of bioinformatics

This service has to do with computers and information. Here we do not encounter samples of proteins or genes travelling from one laboratory to another. The product offered is information, in different shapes: database, programs, computer modelling, and theoretical predictions. To do this, the laboratory requires sophisticated computers[9], used not only for the service, but also as equipment for a biocomputing

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[9] Given the importance of this equipment, it is surprising to know that these machines are quite vulnerable to the inclemency of the weather. They have been struck (and destroyed) by lightings twice!!

laboratory. The value of these machines is reflected in the fact that this is the only laboratory which remains locked at night and weekends, like personal offices.

The service is threefold. First, they offer the 'data bank' service, led by the TAQ, who is in charge of creating and actualising a database of proteins, and keeping the computers in order. Any person of the university is allowed to have access to this database: when required, the TAQ opens an account for the solicitor, who then can use a program to check information. When people do not know how to use the program, they can also contact the technician. The service also offers programs to treat biomolecules. In this case, customers either go personally to the IBF to be taught how to use the graphic programs, or, more commonly, if the customer does not have experience with computers, the TAQ does the work himself. The third service is modelling. This task requires knowledge and expertise, and must be done by the TAQ, who tries to capitalise the work by producing a joint publication out of it. Members of the community do not need to pay at all –in this case, they do not even have to pay a low fee, since no material is needed.

2.2 Research staff

The more numerous group in the IBF are researchers, all of which are regarded as 'scientists' [10]. Organised in research groups (one group per laboratory), they can be classified in four categories, coincident with the four hierarchic levels of a pyramidal group: PhD students, postdocs, seniors and heads. Abilities, knowledge and responsibility distribute differently in each of them, as we will now present in more detail.

2.2.1 Ph.D. students

When I first entered the laboratory, something stroked me: the whole IBF was full with students going up and down. I found this surprising, probably expecting for the sake of stereotypes a building full with men (and a couple of women, for the sake of tokenism) with white coats, working seriously and in silence... Instead of this, I met a swarm of students, just like me, chatting and going in a rush from one place to another –and ignoring us completely, by the way. They still did not know that we were "the psychologists". Full with curiosity, I turned to one of our 'door keepers', and asked him what all those students were doing there. "Students? What students?" –he answered to my astonishment. Completely puzzled, I rubbed my eyes and had a second look. The students were still there, just before us. How could he not see them? "I mean, all those" –I insisted, while pointing to the host of students. "Aaah!" –he replied, finally making sense of my apparently nonsensical question– "you mean those! They aren't students, they are 'becaris'!"

The literal translation of 'becari' is 'grant holder': a student who has taken up doctoral studies, and who, moreover, has received a grant from some institution to work full time in the thesis. A 'grant holder' cannot be simply equated to a 'Ph.D. student', the first label is more restricting, since only some Ph.D. students manage to get a grant. Still, to ease translation in the rest of the work we will use the second term to refer to this figure[11]. In any case, this anecdote made me think. Maybe if I had not been a Ph.D. student myself, this incident would have passed completely unobserved. But it did not, as something made no sense to me: I was a grant-holder *and* I considered myself to be a student!! So, how could he say that a grant-holder was *not* a student? Difference seemed to lie in the following: much as the Ph.D. student is seen as a person in process of learning, this person is not conceptualised as

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[10] Even though P.A.S. think of themselves as contributing to science, none of them regarded themselves as 'scientists'.

[11] Moreover, both 'becaris' and Ph.D. students are conceptualised as qualitatively different from degree students. As a technician said: "Here there are no students, but Ph.D. students. They have already finished their degree and take up their own research with autonomy: they know what they want

a 'student that acquires knowledge', but as a 'scientists who develops' –a *scientist* in progress. To put it in developmental terms (and as we will see later on that this choice is not gratuitous), a Ph.D. student is a scientist in its childhood, and not a mature student. Already incorporated in the 'production chain' of the laboratory, it is a full-right figure of the research group, and a very essential one for that.

Recruitment

If the Ph.D. student is no student any more, this means that the transition has been done some when else. Indeed, the heads of a research group usually try to recruit future members when they are still degree students at the faculty –usually during the last year and the one before last. In this way, they acquire skills while finishing the degree, so that when they enter the lab to work on the thesis, they are almost ready to carry out productive research. Heads themselves may engage in general 'recruitment campaigns', or, as a senior put it, a "task of co-option" in their classes. Some recruitments are a bit more personalised, as when the heads or one of the teachers in charge of practical demonstrations spot out a potentially good student –either due to their good curriculum or ability working at the bench: "we try to make these cases 'sign up'" (EI11-02)[12].

This period of apprenticeship allows a mutual observation too. The student must decide whether she likes the team and the topic of research and is skilful enough. Equally, the head must detect whether she is an adequate candidate to become member of their team. If both sides are satisfied, the head of the team will suggest that the student applies for a grant, and remain with them in order to do a Ph.D. thesis. We know now that from all those young students who visited for a period of time the IBF, only some will be interested in staying –those with vocation or interest.

and what they need. In the IBF they have a maturity and a serenity that you don't find in degree students" (EI17-07).

[12] In some other cases, some degree students who already know they want to work in research-or those who have curiosity to know what research is like, or even want to gain experience- may take the initiative and knock on the door of some head. Others do it because... it is the easiest option: "you have

Of those, only some will be welcomed to stay –those with ability and interest. And of those, only some will be able to stay -those who find financial support. Getting a grant (which usually requires a good academic curriculum) is for both heads and students the best (but not the only [13]) option, since it grants a relative temporal continuity (usually four years) and increases the odds that the Ph.D. student stays and finishes the thesis. To enrol and train a student implies a true investment; it takes time, money, efforts and patience. If a Ph.D. student leaves after a year due to economical insecurity, the laboratory has lost a lot.

Topic of research

Once in the IBF Ph.D. students start developing a personal project intimately related to the work carried out by the group. Ph.D. students enjoy a certain, limited degree of freedom to choose their research interests. While this does not mean that they are not in charge of decision of details, the choice of topic and orientation comes predetermined by the inclusion in a research group that already has a tradition of research within a line. This situation has advantages and disadvantages. The clearest disadvantage is lack of freedom: their research is much more controlled, directed, than that occurring in disciplines where Ph.D. students enjoy full freedom to choose topics. But this is counterbalanced by the increase of both time optimisation and feelings of security provided by collective work.

Growing as a scientist is not a lonely process, they feel accompanied. Students find a common and delimited field of bibliography the y need to be acquainted and fluent with. In some months, they have read and manage the most important knowledge in

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always studied, and then they offer you the possibility to do the Ph.D., and you go on doing the same" (EG03-10).

[13] The Central Government, the Autonomic Government or the University are the most frequent sources of funding. However, it would not be accurate to say that groups only accept students with good academic curriculum –some people without brilliant marks are bright and/or very skilful at research. The group may sometimes obtain public funding for a project which includes money for a grant; or find money from European sources to send the Ph.D. student to another laboratory; or it has some money in store to pay some Ph.D. students for a while. Money sometimes seemed to appear as if by magic...

the topic of interest. Updating is easier: since everybody works in similar topics[14], and given the strong specialisation of the group, one can be reasonably well informed of changes within one's field. The heads, postdocs and other members of the lab have also read the same material (at least, the most basic and classic texts, maybe not the most recent bibliography), which means that conversation is quite fluid. They can have discussions in which they argue their positions by using references to support themselves. Everything contributes to an increasing feeling of mastery; they feel "they know about the work they are doing".

2.2.2 Postdocs

Once students have finished the Ph.D. thesis, a new phase begins. They have already overcome the first serious 'obstacle', and can be considered as quite experimented scientists. They have proved able to design and take up experiments in a reliable way, be skilful enough and have 'good hands', and surmount the continuous problems one comes across while doing research. Now they can stop and look for a job outside university (mostly, in a private company), or they can decide to keep on working in research. If they choose the second option, they will become postdocs.

This label is ambivalent, though, since it refers to two different dimensions. In the strict sense, postdocs (postdoctoral student) are researchers who have already finished the Ph.D. thesis and receive a grant (independent of the source of funding) in order to start a research project, usually in a different laboratory from the one where they did the thesis. The term, though, is usually widened to include scientists developing

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[14] There are some deviations from the normal case, as when a Ph.D. student engages in a minority line of research within a group, or when s/he chooses a different topic (say, something more applied). Advance and progress tends to be more difficult in these cases: other colleagues have not read the same bibliography, have not done the same procedures or techniques, have not found the same problems; they cannot give much advice or be of help. The heads themselves are quite at a loss. This means that the Ph.D. student needs to make the way throughout the wood of doubts alone. These types of Ph.D. thesis take longer, and create much more frustration than others. Nevertheless, people in this situation talk of themselves as having acquired more expertise, as "having become inured", more self-sufficient and reflected. Whereas we are not here to judge what model is better, the way exceptional cases are told indicates that the opposite situation is more normative.

research after the thesis, regardless of where they receive their financing from. Thus, for instance, if after defending the thesis the ex-Ph.D. student is offered a contract in the university (as an associate teacher with a part-time contract so that she can remain in the laboratory), this person will also be considered a postdoc. We will later see that there are several ways of being paid as a 'postdoc', but, in principle, this label, as it is commonly used, refers more to a 'developmental stage' than to a mode of funding – not a Ph.D. student anymore, not yet a senior.

The level of knowledge, skill, confidence and competence of a postdoc is reasonably high. This figure has what they call 'fresh' or 'hot' knowledge: they manage the different experimental techniques, since they are still working at the bench, and have experience in how to solve experimental problems. All of which makes them an ideal figure to help the other Ph.D. students of the laboratory: indeed, when a Ph.D. student has doubts or problems at some point of the research, and these cannot be solved by a more experienced Ph.D. student, she will turn to a postdoc for help. If the postdoc obtains the salary through a grant, she will usually not teach; if the salary comes from the university, her time will be distributed between research and teaching.

The learning and developmental process goes on, and the person's abilities increase throughout time. There will be a moment when this person will not be considered a postdoc anymore, but a senior, the next stage in the development. Unlike the change from Ph.D. student to postdoc, which is marked by the viva of the thesis, there is no clear passage delimiting the border between a postdoc and a senior. The limit of this phase is rather blurred, the factors that finish it are more difficult to concretise, but there are some indexes. For instance, if the person obtains a post as a lecturer, she will be regarded as senior, and not as postdoc anymore. If the person has been working for long in a laboratory, she will achieve the category of a senior or 'old cat' –which means that the age, as an expression of years of experience, will also play a role in how the person is regarded. The older or established the group is, the more differentiated the figure of the postdoc and the senior are. The ambiguity regarding the temporal limit of this phase explains the discomfort experienced in those cases in

which self-perception does not coincide with other people's expectations -as when a 'young' senior feels insecure of how good she performs her function, feeling closer to postdocs than to older, more experienced seniors, while she is clearly considered a senior by her head and Ph.D. students (and expected to perform as such).

2.2.3 Seniors

Somehow, some when, the level of experience and the age of the person allow us to refer to her not as a postdoc anymore, but as a senior. A senior is somebody whose knowledge and skill are very high, a clear point of reference for the rest of the laboratory. This progressive transformation is accompanied by a change in the activities developed by seniors: they need to dedicate a lot of time to help and even direct the work of less experienced members of the laboratory. In concrete, our senior will spend time answering doubts of other Ph.D. students and other postdocs, and especially, planning, designing and supervising the experiments of Ph.D. students. Whilst the head makes 'big decisions' about their thesis (such as what direction research needs to take), everyday decisions and advices at the bench are usually in the hands of seniors. Whereas a Ph.D. student with doubts may ask for help to any postdoc if needed, they are usually assigned to a particular senior. Indeed, each senior takes care of some determinate Ph.D. students so that the latter know in whose hands they are (without this being an impediment to ask for help to whoever may be necessary). Postdocs with doubts will also look for the seniors for advice.

Not only do seniors take care of Ph.D. students, but they also assume partly the management of the laboratory, such as controlling the economical budget and the spending of the group or even authorising buying. In a way, seniors need to substitute the head, whose presence inside the lab is rare. Seniors have often never been explicitly asked to take on all these functions, they end up doing it, because "somebody has to" and it seems "only natural" that it is them, "you simply occupy the space and the role you are given" (EI05-05). This 'spontaneity' also applies to

their function of teaching and supervising other members of the lab. It is not seen as an imposed duty, but more a moral consequence of its position, a natural extension of their job. When asked whether their work of supervision is acknowledged, seniors reject this formulation: "acknowledgement is not the word, helping is part of my function; if I can train a new member, then perfect" (EI05-11).

If we take into account all the different tasks seniors carry out, we will understand why, quite often, becoming a senior involves abandoning one's own research at the bench. This is not always so: some seniors, especially if they do not have to teach, may still manage to find enough time so as to take up experimentations. But this is not common. Normally, and without quite realising it, seniors start a trajectory which will transform them from "hands" into "heads". Partly because "you don't find the time to research", mostly due to the time dedicated to teaching; partly because "production at the bench is already assumed by the 'inferior rank', the Ph.D. student" (EI11-07): "Since you cannot work because you have to prepare others, you become more 'director' and less 'hands'" (EI12-15). This transition and the abandonment of experimental work that implies are easier for some than for others, who experience it as a hard renunciation:

> "I regret having lost it. I used to enjoy experimentation at the bench, I am more 'hands' than 'head', and it helped me switch off, it was good for my mental health. But, above all, it generates a kind of 'personality crisis', that is, well, maybe with schemes that are a bit nonsense, related to passivity and... I don't know quite how to put it, how to group people together and all that. It is like saying 'I am neither one thing nor the other'. Anyway..." (EI11-09).

Seniors are usually quite busy. One can see them quite often speeding from one place to another, with or without the white coat, discussing with the Ph.D. students, now talking to the head, then answering the phone at their desk[15]. The pressure under

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[15] For this is a difference: seniors already have a private table. That is, not only do they have their place at the bench, but they also have some private space. This is, as we will see, a privilege in a building (actually, in a university) where space is gold, and indeed, it marks somehow the high position in the hierarchy of the group. This table is placed, in some cases, within the laboratory, and quite often outside, in an office next to it.

which seniors work varies a lot depending on their particular situation (basically, on who pays them and on whether they teach or not). But in general they all give the impression of being very busy –and some, clearly under 'stress'. The more seniors one group has, the more fluid help and consultation within the group are, and the more different sub-groups about different topics of research the group can have. And the more seniors, the less Ph.D. students per senior. Those seniors with teaching obligations and several Ph. D. Students in charge seem quite overwhelmed.

The senior is a figure in contact with both, Ph.D. students and postdocs on the one hand, and with heads on the other -a kind of connecting figure. When the group organisation works properly, the head of the laboratory deals directly with Ph.D. students only in rare occasions, where quite important matters need to be discussed; otherwise, most problems are dealt with simply between the Ph.D. student and the senior. Of course, the degree to which this is so is highly dependent on the amount of autonomy that the head grants a particular group and the degree up to which the head delegates to others. The latter covariate –or so they say- with the head's personality (what in the organisational literature one finds as style of leadership). In some laboratories, the head was said to be able to delegate, and give seniors a lot of autonomy, so that they could take many decisions without having to ask permission to the head. In others, the seniors were held a bit tighter, needing to inform and ask for consent for most of the decisions. Still, even in those laboratories under stronger control, the senior decides what decisions need to be approved and what decisions can be by-passed[16]:

> "We never have the last word in a decision. We discuss and negotiate, of
> course, and we even suggest and insist what direction should be taken, since
> we are the ones who know what Ph.D. students are really doing. But the
> definitive decision belongs to the head... you are subordinated. Every-day
> matters are different: I filter all the problems, they never get to the head... And

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[16] We once witnessed a case in which the head of the group protested because a relatively important decision concerning the research of a Ph.D. had been taken by the senior without the head having been informed or asked. This situation created a momentous tension: the head did not appreciate having been by-passed, and the senior resented the lack of confidence of the head. But the tension produced by this incident did not last long, and soon the calm reigned again on the laboratory.

then, there is this other more indefinite terrain, where you don't quite know whether it is necessary to meet with the head or not... criteria is not very clear. But in this respect, some heads are kind of strict" (EI12-05).

A similar ambiguity is to be found when it is time to sign Ph.D. students' masters and thesis. They usually have the head of the group as the formal supervisor but the truth is, though, that seniors have also a very important role in this supervision. This recognition needs to be negotiated with the head of the group. For instance, seniors may easily appear the director of the master. He may even appear as co-director of the thesis, but only in those cases in which the head of the group gives 'green light'. It is well known that some heads are more generous or less 'worried' about it than others. But in general, there is always scope for open talk and negotiations about it (which does not mean heads always say yes. As a matter of fact, they sometimes say no).

Another problematic aspect is that of publication. Even though they do not take up experiments themselves, seniors do not stop publishing. All those Ph.D. students and postdocs who in a way or another use some previous work of the senior, or work in the senior's area, or, more frequently, are helped and supervised by the senior, add the senior's name as an author of their papers. However, seniors sign neither as a first nor as a last author (which are, as we will see, the most valued positions). Seniors are eternal ' 'middle names': "This in-between status is an inconvenient... middle positions are always 'filling hands'" (EI12-17). This situation can sometimes be resolved, again, through negotiation with the head; then the head might allow a senior to sign a 'minor paper' in the last position.

Seniors cannot define their own line of research until they become head of a group. This usually means waiting several years, gathering experience and enlarging one's curriculum (while saving some ideas to test them, when the right time comes, with their own Ph.D. students). Finally, there will come a moment when seniors will prepare projects of their own, obtain some funding and some space to establish their

own group and start their own line of research. This change may be softer in some cases than others, but it usually happens gradually (the senior starts her own projects while still leading the head's ones). Sooner or later, though, the senior's projects will be too demanding and the split will be fulfilled[17]. Collaboration between the young head and her old head often takes place.

2.2.4 Heads

With this, we reach the highest position, that of the head of a research group. This change of levels (from senior to head) most usually involves also a change of scenery: from the laboratory to the office. Now we find a character enmeshed in papers, dealing with corrections of results and drafts of scientific papers, making contacts, reading bibliography so as to orient properly its research team, looking for interesting information for the group, controlling economic resources, sources and budgets.

Heads are in a difficult position. On the one hand, their theoretical knowledge is huge; strategically, they have a very good perspective on the state of the research in their field, and can argue very well why and in what direction their line of research must evolve. Moreover, they know all the journals of the field as well as the kind of issues in which each of the journals is interested. In order to maintain and increase all this knowledge, heads must keep on reading (strategic reading, they call it: abstract, introduction and conclusion). All this knowledge and capacity of direction is what makes them one of the most needed members of the group. On the other hand, however, heads cannot even remember when they worked at the bench for the last time: they are not updated, and therefore, have never put into practice some of the newest experimental techniques; slowly "they lose their hands". Thus, they can decide in what direction research should go, but not *how*: they can only help with

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[17] In some occasions, if the head of the group retires, the senior will assume leadership (hence the name of 'dolphin' which seniors are sometimes given).

broke strokes in the experimental work, which is not a problem, since this is what the rest of the members of their team are there for.

Heads dedicate most of their time to what they call 'management and politics': finding funding to carry out research; making interesting contacts with other laboratories in order to establish collaborations; organising and attending to conferences; discussing with private companies which could invest money in some of their products, research projects or patents; meeting with the vice-chancellor of the university to make some request, or quarrel with some of the bureaucratic procedures of the university; attending to several meetings, giving interviews to the media... plus teaching at the university! Heads cannot do it all, for, as people say, the day has only 24 hours. Whereas they are not irreplaceable at the bench, they are so for the activity of the group taken as a whole: without them, nothing could exist.

3. Structure of the group

The linear trajectory that a scientist is expected to follow in life is intimately connected with the structure of the group: the different stages of a scientist's career coincide with the different positions in the structure of the group. Thus, hierarchy is seen as an expression of maturity and expertise –which are assumed to be directly proportional[18]. At first, one starts at the bottom, and progressively, while becoming an experienced scientist, will climb positions until reaching the top. This structure takes the form of a pyramid: the head –one, and only one- at the top; below her, if possible, the seniors -one or, if possible, more than one; below, one or more postdocs; at the bottom, Ph.D. students –ideally, several, as many as the head and seniors can supervise.

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[18] See Traweek (1988) for a similar native developmental narrative, where experience and maturity go hand in hand, and in connection with hierarchy.

They conceive the pyramid as the natural expression of the development of scientists[19]. What is more, the pyramid is also seen as an expression of a healthy, normal development of the group. The stronger the group, the clearer and more stable will its pyramidal structure be, and difficulties to stabilise this pyramid will be read as problematic -a bad sign. Since, as we will see, the stabilisation of the pyramid depends on the income of resources, it is no surprise that this structure experiences 'deviations'. We will come back to this topic but first I would like to explore the organisational characteristics that a pyramidal structure confers on the group.

After Foucault and organisational studies, we are all well aware of the connections between the pyramid and regimes of visibility and power. The pyramid is the essence of a hierarchical relation: it leaves its members, especially those of lower levels exposed to visibility and accountability. As members of one group say, "we don't know how she manages to do it, but eventually our head gets to know everything". But not only does it grant the subjection of the bottom to the upper hierarchy, it also allows a distribution of control throughout the structure and other Ph.D. students and postdocs can be as strong a source of control as the head. Still, effects of a pyramidal structure go well beyond control, and we will now discuss some of the characteristics that this structure imprints. Later on we will argue that the way science is seen to flow –the way knowledge, techniques, money, papers, etc. travel- also participates in the emergence of such a pyramid.

3.1 Division of labour

Nowadays science has become too expensive an activity. A scientist alone could never afford to buy all the equipment needed to produce interesting and competing

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[19] I will not enter the Byzantine discussion of whether this trajectory has adapted to the hierarchy, or whether the pyramid had evolved as the best organisation to give scaffold to the maturation of the scientist. Probably a bit of both: the sequence and development constitutes the structure, and at the same time, this structure works as support for the development and sequence. It does not particularly matter, the case is that we see how the organisation of the group and their conceptions of the group match and constitute each other.

results. If there was ever one, the time of the lonely scientist is gone: being a scientist involves being part of and working in a group (Barnes, 1994; López, 1993; Sánchez Ron, 1993). We have already seen that the group establishes a particular division of labour: the head makes sure the group has everything it needs, the senior mediates and supervises, postdocs and Ph.D. students experiment and teach colleagues. The vocabulary they use to think of themselves, full with synecdoche, reflects this division. Ph.D. students are seen as, and called, 'the hands'; the head is "the head"[20]. Postdocs and seniors would be intermediary steps connecting and mediating both extremes[21]. To "do hands" means to acquire manual skill; to "have good hands" means to have ability. A senior, complaining of lack of time to test experimentally some of his ideas, once told us that his 'head' was working very fast, but that his 'hands' did not let him go further; another senior, who often claimed of himself to be more 'hands' than 'head', said that he was more 'homo faber' than 'homo sapiens' (EI11-26). He also claimed having accepted a Ph.D. student to supervise thinking that "I'll teach her, and she will be 'my hands', we'll work together" (EI11-11).

This division into 'heads' and 'hands' reflects the way practices are distributed throughout the pyramid –distribution which Ph.D. students will assume during their learning process. Part of the process is dedicated to impose one feeling: new members are not expected to think on their own. Or rather, not to offend people, I should rephrase it, and say that the Ph.D. student is not expected to make decisions. Needless to say, every single moment of the research involves thinking and taking up actions - but big decisions are left for others. The head, often together with the senior, will choose the topic of research, the particular task to be carried out and the procedures that Ph.D. students must follow. Every time Ph.D. students encounter difficulties, they are expected to ask for help to the senior, the postdoc or other colleagues. Stories

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[20] In English, Catalan and Spanish the polysemic word 'head' –what we have on our shoulders- is already the same as 'head' –leader. But in Spanish and Catalan we tend to use the word 'jefe' to refer to the leader. The context leaves no doubt that when they say 'head' and 'hands' they do not mean 'leader' but refer to the part of the body. Of course the comparison refers to a hierarchy, but not only: the head is the 'thinking part'.

[21] One could even think of 'hands' and 'head' as being qualities which each position shares in different proportion. Thus, Ph.D. students would have plenty of 'hands', heads would have a lot of 'head', the

and anecdotes told in the lab remind them (warn them) of how spontaneous initiatives taken without previous consultation usually end up in failure -a loss of time, resources and money. The message is clear: "ask, don't think. Above all, don't be too clever". As veteran Ph.D. students put it, they prefer beginners to ask too much rather than taking up actions without permission or help.

However, this division between heads and hands does not turn the Ph.D. student in a simple, passive 'subordinate' following orders and instructions. In case they consider that their degrees of freedom are unnecessarily restricted, Ph.D. students have some 'resistance' strategies at their disposal. For instance, let us assume that a Ph.D. student is completely convinced that the experiment should go in a different direction, and she knows that the head will not approve; or that she wants to try something new that can be successful, but knows that the head will never authorise the spending. Then, the Ph.D. student has a last resource: the submarine. The submarine is an experiment which is secretly carried out (the experiment happens under the surface, without being visible –hence, its name). Even though they all know of the existence of such procedures, they all make as if this was a secret topic not to be talked too openly about. When we asked information about it directly, they used to laugh and show embarrassment ("Do you want me to tell you about the submarine? Do you want them to hang me or what?!" (EI09-27)), but the impression we had is that they quite enjoy talking about it. (Actually, it seems that in the IBF they talk about it more than they practice it...). In any case, the submarine is considered a subversive strategy to undermine the distribution of decisions within a group.

If somebody decides to do a submarine, discretion is needed. The head, and if possible, also the senior, should know nothing about it. If the experiment fails, the Ph.D. student remains silent (the experiment-submarine remains submerged): out of sight, out of mind. If the experiment succeeds, then the submarine comes to the surface. The Ph.D. student cautiously informs the senior about it, trying either to diminish the importance of her disobedience, or, more frequently, emphasising the

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rest being combinations of both, having more of the first quality (postdocs), or more of the second

transcendence of the result found. The hope is, that in front of such a discovery, the head will not even realise (or at least, will not reproach) the fact that the Ph.D. student had taken a non-allowed course of action.

The real submarine, an experiment completely different to what the head expects[22], does not take place often in the IBF (which does not mean that it never happens!). One reason for this low occurrence –in comparison, for instance, with other research laboratories they know- is explained by many Ph.D. students with the argument that the heads in the IBF are lenient and give quite a lot of initiative to the Ph.D. students. As one Ph.D. student said, "if too many submarines are being carried out, there is something in the relationship between the Ph.D. students and the head that is not working properly" (EI09-28). It seems, then, that there are certain characteristics of the relationship between heads and Ph.D. students that influences the amount of hidden research happening in a laboratory. First, personal trust: the more open they can talk with each other about the research, the more freedom Ph.D. students have to say what they think and the less submarines. Second, trust in the Ph.D. students' competence, and their capacity to take initiative. Third, the style of leadership of the head also seems to be playing a role: the more the head is able to delegate decisions on the rest of the members of the group, the fewer submarines:

> "There are two extreme types of head of groups, to my opinion. One is the head that uses the hands of the subordinate, and not the head; the head plans the experiments, knows the directions, and orders the task to the subordinate. A second type of head is just the opposite: the head directs from a certain distance, and allows the subordinate to take his or her own decisions, who

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(seniors).

[22] A type of submarine of smaller importance and transcendence occurs more often: next to the official job of the Ph.D., s/he can take up other small attempts to see if something is interesting enough to be researched. Or two Ph.D. students may try to do collaboration together to see if they can create something new. But one cannot say that this is a completely hidden activity. On the contrary, it is known by the heads, who tolerate it without problem, as long as the official task given to the Ph.D. is handed in due time. This kind of submarine is almost institutionalised, and it is not exactly what they would call a 'real submarine'. Moreover, since the head of the group does not spend most of their time in the laboratory, and most of every-day steps of the research go on without them knowing everything, it is sometimes difficult to distinguish what is a submarine from what it is not. It goes without saying, of course, that if the submarine produces an interesting result, and it is finally published, the heads will also appear in the publication.

informs the head about incidences whenever it is convenient. The submarine happens under the first type of head. And it can even become a quarrel, with the head trying to settle 'here it's who thinks!' -and things like that..." (EI10-09-10).

The costs of such a submarine play also an important role. A high spending may deter Ph.D. students from doing submarines. Not only because of 'bad consciousness', but mostly, because this expenditure gives them away –the heads have to approve and sign orders of material. Even the heads seem to take this economical criteria into account in order to 'decide' whether to 'see' or 'not to see' the existence of a submarine. The heads may turn a blind eye to these clandestine activities if their expenses are not too high –as long, of course, and this is important, the work the heads want done is being done anyway.

3.2 Teaching and learning

When a student enters a laboratory, everything is still to be learnt. As they put it, "at the beginning you realise that you know nothing". Of course, they have the theoretical and practical knowledge acquired during the degree; but somehow, this is not enough to know how to act and move in a laboratory and in a scientific group[23]. If they want to do an experiment, they have no clue where tools, material and equipment are. They do not now how these equipment work. They do not know where to get their material from, or how to order it when they run out of it. They do not know what exactly it is meant by sentences of handbooks and protocols describing research (we all know the difference between reading the instruction for an electric set and knowing how to manage it). More dramatically, when they engage in

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[23] New knowledge needs to be made concrete, rooted in embodied practice. This means that rather than 'being told a theory', they receive knowledge in the shape of advice, papers, tips and tricks, supervision, stories, anecdotes and jokes... patience and time –'have you already tried it this way?', 'why don't you do it like that?', 'I did it so, and it did not work very well; at the end I had to do it like this'... Each member receives a mixture between explicit and implicit, tacit knowledge, a know-how

an experiment, they will painfully discover that the possibilities of making mistakes are higher than expected: mistakes are popping up everywhere. What is worse: mistakes are everywhere *and* they are difficult to detect, which means that when something fails, beginners do not even know where the problem was.

To overcome all these hindrance they have no solution but to ask: they ask everything, they ask all the time, because "there is always somebody with experience who has an answer to your problem" (EG03-23). This already suggests how crucial the help, teaching and support of more experienced Ph.D. students and postdocs are. Unless one has been there, it is difficult to believe the amount of cries for help and support which take place in such a laboratory: "where are the scissors?" "what on earth did I do wrong?" or "can you help me with this?" When asked about it, Ph.D. students remember their first months in the lab with hilarity, confessing their embarrassment for asking too much ("you can't but be a pain in the neck" (EG05-14), and their strategies, such as not to bother always the same person.

Since all Ph.D. students of a group work in the same room, without walls dividing them, the space distribution helps resolve doubts, just by asking, *in situ* and fast. Ph.D. students can observe each other's job, and often, while carrying out their own tasks, they can follow the procedures other Ph.D. students do. If one of them starts an unusual technique, she will normally give notice to others, so that those interested can also learn how to do it. Or, if others see that she starts it, they will spontaneously ask 'what are you doing there?'; if doubts arise, all will try to contribute to the solution. Any publicly displayed attempt at interpretation of data will be interpreted as an implicit invitation to take part in the event: as Knorr-Cetina found in her lab (1998), as soon as members of the laboratory realise somebody is reading data, they all approach to have a look and give their opinion. The fact that they do not even ask 'may I?' is a hint towards the radically collective nature of the production of knowledge in these laboratories.

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that configures them with the time into more and more experienced scientists. This knowledge is never

But help is not only spontaneous, but also quite organised. The head assigns newcomers to a more experimented Ph.D. student. From the moment of the delegation onwards, the veteran Ph.D. student will be responsible of one (or more) new members and their acts. This is not simply a way of talking: to have a beginner 'in your hands' does not only mean helping, but also keeping an eye that she does not break or spoil anything. Mistakes are not punished –they all have been beginners, and they all know of the torture of the first months, when everything which can go wrong does go wrong. But they can definitely be scolded for being thoughtless or for not following advice; if a new member inflicts damage out of 'disobedience' (i.e., create havoc in an expensive equipment) then, not only she but also the Ph.D. student in charge will be to blame. This is why a beginner who does not ask enough, and takes too much initiative bothers as much as a beginner who asks too much –and twice as dangerous: "people will remember more the fact that you have broken down a machine, than the fact that you have asked the same four times" (EG03- 47).

Teaching others takes time: not only does it involve helping when there is a doubt, but also supervising continuously. The head gives more experienced Ph.D. students instructions so as to what kind of tasks the beginner has to do, and they have to make sure the latter obtains all the necessary help, not only to perform the tasks, but also to learn how to do it, so that from that moment onwards the beginner can work autonomously (for instance, supervising may involve delegating an easy task to the beginner, and supervise its realisation). In short, supervising takes time, effort and patience. Given the complexity involved, it is no surprise if I say that taking care of beginners is quite disruptive for old Ph.D. students -to say the least, they do not advance so fast in their own research. Although heads sometimes try to present each arrival as a gain ("how lucky, you will now have somebody to help you!"), they all know that to be in charge of a beginner is hard work, and the arrival of new degree or Ph.D. students arises jokes and bets ("guess who will have to take charge of them!"). "You always try to escape the assignation" –they told us- "but it is useless. When the head comes and says you are in charge, then you are in charge" (EI04-15).

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a 'general knowledge', but it is always transmitted in the context of a particular research.

However, this anecdote gives a false impression; it does not show enough the warm welcome that every single new member receives, and the generous amount of time and patience that old members dedicate to the new ones. Ph.D. students experience from the very beginning that one never learns alone, but inserted into a matrix of collective activities, in a similar way in which traditional learning is achieved (Rogoff, 1990). Like an apprentice, they first watch, then start doing some parts of other people's experiments; later on, they carry out some procedures themselves, under the supervision of more knowledgeable people. Little by little, with help, advices and teaching from more experienced Ph.D. students, postdocs and seniors, beginners start gaining more autonomy, until they can practice alone. As they put it, they need to "do hands" to acquire the famous 'know-how'. A Ph.D. student in the last years is able to show quite a high level of competence and mastery of their issue of research –quite impressive, I must say.

3.3 Economy: distribution of resources, knowledge and decisions

As we will see, to have Ph.D. students is easier (or at least, it used to be easier) than to have postdocs or seniors. To have a senior per Ph.D. student is a proportion that no laboratory can afford. As we will explain, a group is happy enough if it manages to have one senior. In this sense, then, the pyramidal structure is also an economical structure: one or two seniors may suffice to supervise several Ph.D. students. Moreover, the fact that several Ph.D. students have the same senior means that this point of the structure facilitates internal communication. That is, improvements of one technique made by a Ph.D. student get to be known by others, not only because they share it directly, but also because the senior remembers what has been done by them all and can pass this knowledge to others in the future. Since the senior is a rather stable figure in the laboratory, it grants the preservation of knowledge of the group: the senior is a reservoir of collective memory.

The structure of the group is thought to provide support and supervision for new members while securing productivity at the same time. On the one hand, the horizontal dimension of the pyramid makes collaboration and help among equals possible[24]; on the other, the vertical dimension facilitates supervision and help between levels. In other words, it regulates flow of knowledge, of doubts and answers. And it does so whilst regulating disturbances: problems tend to be solved first at the lowest level possible, and only if they persist will one have to call on to higher levels of the structure. The more basic and petty problems of experimentation are usually solved by those who are closer to the person in need of help –and these are usually other more experienced Ph.D. students, or, if the laboratory has one, a postdoc. They will constitute their first guide in their first steps into experimentation. If they cannot solve the problem, the question will climb the hierarchy, and the senior or the head will have to be involved. But the rule is clear: the sooner (and lower in the hierarchy) problems can be solved, the better. Thus, the pyramidal structure offers a kind of organisational filter to protect higher ranks so that they are not disturb with every day bench problems and are instead free to engage in other, equally necessary tasks.

However, not all the problems can be solved at 'lower levels'–and some *should* not, if they involve serious decisions. In those cases, questions need to be brought up to the senior. Among those of the same rank, it is not as important to know whom to ask as when they have to cross levels; in principle, a Ph.D. student will tend to ask more questions to the postdocs and the senior of which one depends than to others. Heads are also asked for doubts, especially, those doubts which involve deciding what direction to take, how much costs they can assume[25]. In short, the pyramid works as a kind of two-directional funnel: upwards, it allows flows of doubts and new

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[24] Which does not mean that the pyramid is a 'collaborative structure' per se, or that it emerged to accomplish this aim, on the contrary. Collaboration is typical of the IBF, not necessarily of all labs. Many other laboratories are described as 'highly competitive'. Knorr-Cetina (1998) reports about very collectivist environments in science where pyramidal structures are absent.

[25] A Ph.D. student will only rarely ask a question to another head. If they need to ask information to another group, they will address themselves first to Ph.D. students, and, if they cannot solve the doubt, to a senior. If information needs to be obtained from a head of another group, a senior or the head of one's group may do it.

knowledge created at the bench, so that contributions of many arrive to few. Downwards, it distributes the knowledge, decisions and new theoretical contributions of few to many.

3.4 Publications

The pyramidal structure is also related to the system of publication, and it conditions the order of signature –which, in turn, is informative of the relative contributions of the names appearing in the paper. The first author is acknowledged as the one who did the experimental work; the last one as the head of the group, leading the particular research. And those in the middle are people who, directly or indirectly, are related to the paper, their contributions varying, but being more or less unspecified. Middle positions can be occupied by other Ph.D. students who have helped, by some postdoc and, especially, by the senior of whom this Ph.D. thesis 'hangs'.

As everybody who has tried it knows, to write a paper is not a simple matter of writing down results, but involves experience, a wide vision of the field, a sense of strategy in order to produce a good and 'marketable' argument, that can interest a particular journal –let alone a decent knowledge and use of English language. In short, one needs a carefully thought plan of action. This is why the more experience somebody has, the easier this person can write a publishable paper. This task is a stressful moment for most of Ph.D. students: "a very thorny issue" (EG02-48), "writing the paper is always the limiting step" (EG02-17). In some groups, Ph.D. students themselves are in charge of writing the papers, so that the senior and the head may invest their time in other activities –otherwise, if they were too busy, the functioning of the pyramid might be blocked. In other laboratories, they do it as well, but with the help of the senior. Some other groups decide to free Ph.D. students from this task: either the senior takes care of writing the papers, or the head of the group

writes the papers directly. Ph.D. students often discuss differences in strategies among heads –this is without doubt a hot issue in the IBF[26].

There is another important factor deciding the degree of implication a head may have in the writing of the paper: the head's amount of knowledge in the particular topic. Heads can participate in a paper even if they do not know all technical details (EI16-23), but they do need to be acquainted with the theoretical framework. The more technical a paper is, the less the head can contribute, whereas theoretical papers will require the head's help. In some exceptional cases, there are Ph.D. students working in topics different from those the head is expert in. Then heads are limited to do more general corrections, without being able to enter into details.

When Ph.D. students write a paper, seniors may read it and do some corrections. But it is especially the heads of the group who are in charge of correcting and changing the paper until they consider it suitable for publication. This process of correction may take quite long, depending on how many people need to correct the paper, how busy the head is, how much the Ph.D. students and the head know of the topic of the paper, etc. As Ph.D. students jokingly say, "a paper is touched by everybody" (EG02-48), so much so, that "in the end, I will not have much to do with my own paper" (EI09-31). The process of correction is so long, that sometimes papers are delayed more than desired. But in any case, once the paper is finished, the likelihood of it being accepted is quite high. Given their experience and contacts, the decision of what journal the group should send the paper to is taken by the heads.

But one of the most interesting phenomena occurring in relation to the papers is the negotiation, discussion and decision regarding who is entitled to sign a paper as author –whose paternity is it? They must first decide who has contributed to the paper: who has done the experimental work? Who has helped at the bench, either

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[26] Those students whose head writes the paper are quite satisfied with this option, for this facilitates their tasks a lot, and allows them to finish the thesis with several papers published. However, they are also aware of the drawback, especially for those Ph.D. students who aspire to academic life: they don't learn to do it on their own, and leaves them in a certain dependency of the head.

with some method, technique or supervision? Who has planned and directed the experiment? Whose is the line of work to which the paper contributes? In whose laboratory and with whose equipment has the experiment/paper been taken up? Whose theoretical knowledge has been enrolled? This reconstruction usually produces a kind of genealogy depending on levels of involvement (to decide who should be in or out). This system is an acknowledgment of the collective nature of both work and publication within a laboratory. With rare exceptions, there are no single-author papers: how could there be, if there is no single-author work. There are no lonely thinkers, no lonely experimenters, no lonely scientist. Production is collective [27].

Of course, this re-construction poses some problems: how to distinguish the continuous advice that one receives from all the colleagues when carrying out an experiment from the more substantial contributions of those who deserve the recognition of seeing their name added to the paper? Decisions as regards to who is in or out may create some tensions, if those left out feel unjustly excluded. In some occasions heads may add names to the list: who else, not directly traceable by the genealogy, should be added to the list? An example of such a situation is when the head decides that somebody –usually a Ph.D. or a postdoc who need to apply for a grant, or a senior who has to pass a public contest- needs to increase his or her curriculum. Whereas we expected complaints when accounting for this exception, Ph.D. students still considered it as something 'rational', even positive –for this meant that their head cares for them. 'Today you need it, tomorrow I may need it'.

Not only does this system recognise the collective nature of work, but it is also a way of optimising gain: if only the person working at the bench signed the paper, only one individual would profit from it -there would be no advantage or gain in working together as they do. With collective publication, on the contrary, they simultaneously

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[27] There is an interesting effect of this genealogy. Since participation in a work is not completely pre-established by hierarchy or group organisation, the distribution of benefits (the names to be attached to a paper) needs to be talked about every time -allowing to keep property claims open and negotiable at every time.

increase the capital of the group: more than one person can profit from one paper – and, especially, the head of the group can add all the papers of the group to her curriculum[28].

3.5 Matrix of sociality

After observing for several months the amount of dedication and patience that members of the group dedicate to help each other, a question came to my mind. What did they 'receive back' for all this help? Most of them seemed to be teaching 'for free', without obtaining anything in exchange. In one interview, I decided to pose the question directly to the Ph.D. students: 'how are you rewarded for your help?' My question was received with agitation, clearly bothered by the individualistic, exchange-like undertones. As if I had formulated a question of bad-taste, they reacted almost with indignation at the implication that their help could be interested. They offered another interpretation:

> "A: in your day they helped you, didn't they?
>
> B: It is not so much that this task has to be acknowledged… it is rather... when you entered you were helped, weren't you?
>
> C: Exactly!
>
> B: ...then you must help now.
>
> C: It is like paying a debt you have.
>
> D: I suppose it is like a cycle, I mean, now they help you...
>
> B: Of course, come on, I mean, please!
>
> D: ... and afterwards you will help somebody else" (EI03-24).

On the one hand, they feel they obtain disinterested help from colleagues, for free, so to speak. This support is taken for granted: the head assumes members of the group will help newcomers, Ph.D. themselves assume they have to do it, and the apprentice

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[28] This last effect is particularly important: since the head is in charge of applying for grants and projects she must also enjoy of a thick curriculum -if the curriculum of the head does not grow, this is bad news for the group.

discovers she can expect it. Helping others is normally not directly rewarded: "the only recognition I expect is the thanks of the new Ph.D. student" (EI03-25) -even though, from time to time, a grateful Ph.D. student may in the future return the favour by adding the name of the person who has most directly trained her in a paper. Thus, one could say that they perceive and receive the time and effort of others as a present or gift -one is helped for nothing.

Nevertheless, as this quotation indicates, they also point that the person who has received help is morally obliged to help others later on. When a new Ph.D. student enters a group, she experiences very strongly how much dependent on others she is. This creates a strong sense of 'owing' to others, a feeling that they are running up debts with colleagues. In other words, perhaps one is not paid for helping, but one helps in order to pay back the help received earlier. Helping others, then, is simultaneously both an interested and a disinterested act. This is no contradiction; since Mauss (1954) we know that disinterested presents are the expression of an ethos which imposes obligations on those who receive -such as reciprocating. Each Ph.D. student is indebted to previous generations, but she will have to pay back to others, to the next generation: she has the chance to dedicate so much time to newcomers as she received when being a newcomer too.

This type of gift exchange, what they name 'paying your debt' or 'completing a cycle' (you are helped, you will have to help some day) is morally tainted through and through: they must help, lest they are considered unworthy of membership. It weaves the sociality of the group and puts members in a reciprocity relation –duties, but also feelings- that ties them together:

> "In that laboratory [abroad] I learnt that you really must help others. I already felt it here, but there it was extra strong" (EI04-23).

However, the expression 'to be in debt' should not confuse us. This expression might bring us to a universe of individuals united by a pact in a contractual liaison, in which individual interests have to be negotiated with the interests of others (you help me, I

help you). Talk of contracts and debts could bring us to a too individualistic landscape. This is not, nevertheless, how they experience this link. The pyramidal group is lived as a scaffold of support and productivity; learning is a collective activity distributed throughout all the practices which sustain the whole structure. A matrix of sociality, where one develops, learns, finds one's way to become an expert, competent scientist. While a member makes its way, she also becomes aware of the collective nature of their enterprise. They do not see themselves as a simple collection of individual scientists working in the same space, but have a strong awareness of being a group of interdependent members, of being intertwined and linked by belonging, identity, morality, etc. However, their conception of the group will still leave room for an individuality in a way we will discuss later on.

4. Fit between the academic and the scientific development: dangerous deviations

What we have been describing here is the structure of a scientific group in its ideal shape: "a pyramid of four" (EI13-29). However, to achieve this structure is hard work, and only possible if there is a good anchorage of the laboratory into the academic organisation[29]. Remember that the IBF cannot contract staff, and can only attract those who already work for the university. Thus, each figure of the scientific

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[29] Let us remind ourselves of the structure of the Spanish University. If we start from up to bottom, we first find the Professor, followed by the Lecturer. These two figures are civil servants, that is, once they achieve their post, this belongs to them forever –thus, people are rooted into a university, fact which obviously makes mobility quite rare. In order to obtain a post as Lecturer and Professor, several things need to occur: a) the state/university needs to offer a post in open competition, which happens at different rhythms: when universities are expanding, relatively often; in recession times, very rarely; b) the person who wants to win this post needs to prove its validity in open competitions with other rivals.

　　　　Next to them, we find two more figures, (assistant and associated teachers) which are different from the previous ones at least in two aspects: a) they are linked to the university with contracts, and they have a limited temporal horizon; b) the salary is quite precarious. Therefore, this position involves more instability. Whereas one needs to have done a Ph.D. thesis for the first two posts, nothing prevents a person without thesis to occupy a post as assistant/associated teacher.

　　　　Below all these figures, and with a status which varies in each department, we find the Ph.D. students. They are paid through grants, given by the Central Government, the local government and the University. Thus, the dynamics in which they are involved is of a different kind. This does not mean

pyramid should be able to anchor in the university pyramid. It would not be appropriate to say that each figure of the research group corresponds to a figure within the academic hierarchy –if this was the case, we could even doubt the existence of two different pyramids. No, there is no strict and fix correspondence between them: the head of a group can be lecturer or professor; the senior can be an assistant teacher, an associated teacher, or a lecturer. A postdoc can be an assistant teacher, an associated teacher, or may have a grant; the Ph.D. student usually is a grant holder; and we do not find heads occupying the position of an assistant, or we do not find seniors with a two-year postdoc grant. This means that when the IBF can manage to achieve and stabilise the following equivalence, things are going quite well:

Ph.D. student	Grant-holder
'Postdoc'	Postdoc/Assistant/Associated/Grant-holder
Senior	Lecturer/Assistant
Head	Professor/Lecturer

Notice that when socio-economic conditions are buoyant, both pyramids follow a kind of progression, a developmental line of a sort. If there are no impediments, one expects to go from one position to the higher one. Ideally, both lines should evolve in parallel: academic development should accompany scientific growth. The correspondence between these two development lines works out pretty well when the university is in a situation of expansion, that is, when new posts as lecturers are created: many people find its way to a stable salary, because the scientific developmental line anchors or climbs up the academic pyramid. The two developments are in balance.

Small oscillations or disarrangements do not alter the balance between these two pyramids. Thus, if a Ph.D. student needs more than 4 years to finish the thesis –four

that the number of grants is unlimited, they are also offered in public contest, but they are not linked to

years is the average duration of the grants-, the heads might find a contract as assistant or associated, which allows them to pay the Ph.D. student a little longer. The figure of the postdoc is so ambivalent, that oscillations are normal. Ideally, the figure of the senior should correspond to that of the lecturer. In those cases in which the senior occupies a post as assistant or associated, it is usually with the expectation that sooner or later –but better sooner than later- the university will offer a free vacancy for which the senior can compete. The head of the group can be either a lecturer or a professor. Ideally again, a professor, but given the scarcity of posts as professors, it is frequent to find heads of research lines who are lecturers. This is especially common among young heads of group. In any case, all the deviations cited here are not very harmful, either for the functionality of the pyramid, or for the people involved in the pyramid.

Unfortunately, given the economic situation and the low investment of the state in science, the academic progression encounters hindrances nowadays that do not allow a developmental narrative to take hold of it: academic progression has stagnated because the number of posts offered by the government has been low; lower, in any case, than necessary to absorb all Ph.D. students willing to give their vocation a chance. Scientific career is impeded by lack of anchorage in the university.

The scarcity of posts as Professor seems to be not excessively dramatic. Granted, it can be very frustrating for people who have been teaching and researching for many years, not to achieve the recognition (and salary) they surely deserve. But this frustration usually has a weaker repercussion into people's life and economy than the lack of posts as lecturers. A whole generation has been retained in posts as assistant/associated teachers -that is, with miserable salaries and with the end of the contract always threatening in the horizon, knowing neither if there would be a real possibility of becoming lecturers, nor when this possibility would become truth.

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a post, which means that every year chances of getting grants are renewed.

These people are trapped; they do not have a sure progression into academic life and given their age and trajectory they would not have an easy way out of university (which they do not want anyway). If one adds to it the fact that many of these people have meanwhile become head of a household, with partners and kids, we can have quite an approximate idea of the suffering and drama of the situation. The feeling is always one of precariousness and instability; temporal horizons are ambivalent. All they can say is "I will stay at university until I can and am allowed to" (EI07-5).

The consequences of this scarcity of vacancies as lecturer is no doubt dramatic for the generation of people trapped in the permanent 'temporal' contract situation. Still, the harmful effects of this lack do not stop at that level; if they are stuck in the positions of assistant/associated, this means that these positions are blocked, and cannot be occupied by new generations of scientists. This involves that a) nowadays, when Ph.D. students start developing their thesis, they already know, from the very beginning, that they cannot count on remaining at the university, since their progression is cut; b) when they are lucky enough to get a contract as assistant, they know nevertheless that this short academic career has no future, and that after some years they will be forced to abandon anyway. Some students, after finishing the degree and discovering the lack of future possibilities, renounce before starting to engage in a carrier without future. This tendency can be noticed, for instance, in the decrease of the number of students wanting to apply for a grant in the IBF [30]. In short, they know their way is blocked. And they need to weight up several alternatives, and make decisions.

4.1 Effects on the scientific pyramidal structure

The lack of resources bars some groups from establishing the figures of the postdoc and the senior. Thus, when money fails, these two positions tend to collapse in one, (sometimes only the postdoc, sometimes only the senior). And even though the senior

is more 'profitable' in terms of socialisation and production, nobody will complain if they only have money to keep a postdoc; on the contrary, they will consider themselves very lucky. As we will see, only 'rich' and stable groups can afford having intermediate figures. Thus the 'potential' pyramid is sometimes reduced to a more realistic pyramid, with three different hierarchical levels –and this is, after all, quite an optimistic panorama: a pyramid of three is still a pyramid.

The importance of unfolding the pyramidal structure is revealed by its negative, that is, by the sometimes-devastating effects which suffer those groups that do not manage to stabilise this pyramid. These groups have only two levels: the head and the Ph.D. students –the limiting case being a couple 'head-single Ph.D. student'. In these cases, the head needs to take on board all these functions normally attributed to seniors and postdocs as well: the socialisation, learning and guidance of new members. The problem comes when old Ph.D. students leave the laboratory, once they finish their thesis –to occupy more stable positions somewhere else, or to go and work in private companies, to other universities, etc. Then the head needs to invest efforts to train new members again, starting from scratch every time; the group needs to be reconstituted again and again, without any structure to sustain all these efforts along time. Not only is this a very disheartening process, but it also alters the group production, much more affected by oscillations[31]. For a group, this means a dangerous proximity with a dead end, or a very limited progress of the work. In any case, even though work of quality[32] is still possible, competition with better groups in leading fields of research is almost impossible.

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[30] Of course, another motive for this decrease is not only the perception of 'no future', but the fact, they all complain, that the government has cut down the number of Ph.D. grants per year.

[31] However, generalisations are always dangerous. In the IBF there is a group of two levels (head and Ph.D. students), which is experiencing what is considered one of the most promising developments of the centre. Now, this group has several Ph.D. students, and, as we will see in another section, his head has slightly changed the strategy: instead of promoting a hierarchical development, has both promoted and kept within limits a horizontal growth.

[32] During our fieldwork we had the opportunity to attend to the viva of a two-member group -a head and a Ph.D. student. The thesis was highly praised by all the members of the tribunal, its quality emphasised.

To overcome this situation, it is crucial that the head can keep some of her Ph.D. students after they finish the thesis, so as to consolidate the pyramid. Nevertheless, this is not easy; these groups find themselves trapped in a 'vicious circle': in order to obtain funding for a project you need to prove you have enough staff to carry it out, but in order to be able to obtain and pay staff, you need funding for a project. This is a vicious circle (EI15-05; EI16-04):

> "This is a fish biting its tail, as the saying goes, 'money attracts money' – money, posts, whatever. This is a ball, it grows, whereas small groups run to seed" (EI15-05).

In any case, it seems that the creation of a pyramid involves a lot of work and personal wear from the head: the head must look for economical resources, contracts, grants, contacts, etc. As the heads put it, "one needs to make one's life complicated" (EI01-15), "one needs to sow -day in day out, during years- in order to harvest" (EI13-31), "one needs to go and search for advantages, for they will not come at your door" (EI05-12).

But deviations of the ideal pyramid do not come only 'by default', but also by excess. A group may have so many Ph.D. students, that the senior cannot deal with all of them –and the head, even less. Then non-desired effects start to appear, such as Ph.D. students who do not have enough help and end up working adrift; postdocs who must assume the role of seniors when they still feel immature for it; seniors completely overwhelmed and saturated by the demand that either the base or the vertex of the pyramid exercise; heads whose table is so full with papers that they cannot deliver in time. When the group gets bigger than its capacity to process information, then production diminishes, work accumulates, and data lose actuality and quality.

Apart from waiting until being big enough to compete with others, a young group has another alternative: to unite forces with other small, young groups, or even with bigger groups. Collaborations may not be strictly necessary to grow, but it is clear that they play an important role in the IBF. At one level or another, most of the

buoyant groups seem to have an interesting net of collaborations. Moreover, the stronger a group is, the more attractive it appears for other groups to do collaborations. It seems, then, that the creation of collaborations may be a strategy to grow, and a strategy to keep on being big. The pressure to achieve a level of quality and competitiveness leads often to collaborations. As one head put it "if you apply for a project alone, nobody will believe you can do it all on your own, without collaborations; it is impossible" (EI13-05). Once two groups come together to help each other, they can buy equipment sharing expenses and uses, share resources, ask for grants[33], create contacts and further collaborations with international teams or national corporations, attract contracts by private companies, etc. The ubiquitous presence of collaboration within Spanish science softens a little the image of wild competition among groups typical of science nowadays.

4.2 Up and downs

When a group does not manage to achieve a stable structure by consolidating people, then it experiences constant up and downs, due to the cyclic rhythm proper of a laboratory. Indeed, the production of a research group is never constant, but changes intensities. There are a couple of reasons for this cyclic rhythm. The first one is that the rhythm of a single researcher is also subject to a kind of cycle. Every time somebody starts a new topic of research, this person will need a while before publishing papers or producing good research. This is particularly so at the very beginning of one's scientific career, but not only: every new project takes some time to bear fruits. Reading the literature, preparing successfully a particular technique,

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[33] This is a normal strategy, for instance, when applying for funding; it is not unusual to unite efforts among several groups to increase the chances of getting a grant –when several groups join, they can claim to have more resources, more staff, more possibilities of competition and success. The disadvantage is, though, that the amount of money one applies for is also higher, and after distributing it among all the groups, the remaining budget for one group may not be much. Moreover, collaborations are not always easy to obtain. It is not as simple as knocking on the door of a laboratory, and offer a collaboration –there must be some kind of previous connection, such as knowing some of the heads of another laboratory: "collaborations are the result of a history" (EI16-05).

carrying out the research, obtaining and interpreting results, writing the paper for a journal, waiting for its publication... all these activities take time.

This individual rhythm of production interacts with the structure of the group; in particular, with the number of members of the group, their position and stability. If one laboratory has three Ph.D. students, all of which are first-year, the laboratory will not produce papers for a while; later, when all the Ph.D. students start publishing, the production will bloom. And when the Ph.D. students finish the thesis and abandon the laboratory, the production will go down again. Given this pattern, the ideal would be that a laboratory has a regular entrance of new Ph.D. students. In this way, while the new Ph.D. student still learns, the more experienced ones are already publishing; and when the latter leave the lab the new ones are already in a productive phase. A regular entrance of new members could, in theory, prevent the laboratory from having lows in production. As our reader will already suspect, this is not always possible. Maybe no Ph.D. student is interested in the research of the group. Or maybe the student has no possibilities of getting a grant, or the group has no project, or no funding, or the application of the group is given no priority... This is not always up to the head, and they tell it straightforwardly: "there are many factors which we do not control" (EI15-58).

If a group has managed to have several members in an intermediate position (especially seniors) with a certain degree of stability, the cyclic moments are less noticeable. Regardless of the entrance and exits of fluctuant members with grants, there always remain some permanent members –even if this stability is in turn quite unstable with temporary contracts. Needless to say, groups with a lot of people also experience these up and downs, but they are much less noticeable. The stronger and diversified the group is, the most likely it is that they manage to compensate up and downs and obtain a stable or even increasing production. In any case, even when a group goes through a low, the situation is never presented as dramatic. "It is normal" -they say. Laboratories experience a cyclic rhythm, and the situation may soon

change. Groups that are now hectic may in the future look empty, and those empty may soon recover.

Cyclic rhythms are not only to be found in every research group. When taking a temporal perspective, veteran members of the IBF can perceive 'cycles' and 'up and downs' in the activity of the IBF as a whole. If several groups, for whatever reason, happen to be having a 'low' at the same time, they drag the centre down, and its activity diminishes: "one can notice that there is less life, and there is less productivity... The IBF feels cycles, it goes by fits and starts" (EG05-33-4). The solution, suggests the director, would be to make the centre bigger. Bigger in every respect: number of groups, number of people, number of resources, and number of meters... If the centre had many groups, the lows of some of them would not alter the total production and atmosphere of the centre so strongly (EI01-16). There was agreement that, at the moment we were there, the IBF was not in a 'low', but in a 'downwards' face, after having had its summit a couple of years ago. "We are in a moment of acute inflection" (EG05-34). However, people did not seem worried at all, they took for granted that better times would come back.

5. Challenging future: facing uncertainty

5.1. How to build a group in instability

By now we should already be convinced that without a strong pyramidal structure, the growth and progression of the group is threatened. At least, most of the heads of the groups are quite convinced about this point, and insist that efforts should be directed to the increase and stabilisation of intermediary figures. However, we also know that political and socio-economical changes make the consolidation of structure quite an impossible mission, at least through the traditional way of anchoring the group in the university structure.

Heads, then, have had to contrive new strategies to build up and maintain the structure, and the aim of this section will be the discussion of three of them. Two of the strategies are thought up by powerful groups with 'a history' -that is, consolidated groups with renown and with some resources at hand- which will find new ways to unfold the pyramid. The third is taken up by a rather young group which is experimenting a promising development, trying to become strong without an expanded pyramid. Such changes in practices will also resonate with changes in ethos: new generations show differences as regard to previous ones, and this chapter will include introduction and discussion of some of these changes, as they are reported by members of the IBF and perceived by the analysts.

5.1.1 First strategy: contacts and contracts with private companies.

One laboratory of the IBF dedicates many efforts to biotechnology –for instance, to the creation of vaccines, and it is even said to be one of the best three laboratories of Spain working in this (EI05-18). A great part of this development in biotechnology has been done in collaboration with a particular private company. This is a clever collaboration, which has advantages for both sides.

The company is interested in research results/products, since it is cheaper to pay the university to work for them, than to develop products on its own (which would require a huge investment to construct a laboratory, buy equipment and pay salaries to scientists). The university is in turn interested in the company, as entrepreneurs are willing to give money to the university for research -money which translates into salaries, so that the group can contract people that under normal circumstances would not have a chance to enter university. With the added advantage that, unlike those who occupy a vacancy as assistant teacher, associated teacher or lecturer, those researchers contracted by companies do not have to teach. Whereas the former are little by little pushed away from the bench by first-degree teaching and Ph.D. supervision, the latter are paid to do nothing else than research at the bench. This is

one of the reasons why these types of contract are good regarded. Another clear advantage is that the company is quite generous with the economical resources it provides for the research, at least enough to research without restrictions.

Nevertheless, there are also disadvantages, the main one being that this is not a permanent job. Contracts are temporal, and must be renovated every three or four years -although there is no reason to suspect that the company will not renew the contracts if everything goes fine. Actually, until now the collaboration has been fruitful, and contracts have been renewed without problems –which might reduce the sensation of temporal precariousness. Indeed, people working for the company are aware of their temporary character, but at the same time enjoy a certain degree of stability –a kind of unstable stability, which is also a stable instability, so to speak... Another difficulty is that, obviously enough, the company gives the money so that researchers work in very specific issues, those in which the company is interested – namely, applied research[34] out of which profitable results are obtained. This means that carrying basic research is increasingly difficult for those working for a company[35].

The pressure to hand in some result is stronger among those who work for the company than among those conducting basic research (even though a couple of Ph.D. students doing basic research disagreed). At least, it seems that, if deadlines are not

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[34] They felt the need to explain to us the characteristics of applied research, for "(t)his is quite a difficult concept to transmit for those who are used to basic research. In basic research, we have enough with saying that "things work more or less that way". In applied research, you have to produce something precise, which works as you want every time it is used. There is not enough with saying that it has to be reproducible. All work is somehow reproducible. It has to work perfectly. (...) If you create a vaccine, and it is inoculated in thirty thousand cows, none of them can die, and none of them should become ill afterwards. This is a work with a lot of responsibility, since what you do needs to work in all situations, well, in a big range of situations, even when used by any other person" (EI05-16/7).

In this fragment we can observe quite a usual movement. Whereas usually basic research is considered to more pure, in the sense that its only motive is 'thirst of knowledge', in contrast to applied research, looking for applicability and profit, this person reverses this evaluation. In this extract, applied research is useful research, much more precise and committed than basic research – which, as it is described here, sounds like not a very accurate and responsible task. Such rhetorical inversions are quite common among those working in applied research.

[35] Difficult, but not impossible: they claim that sometimes, if one finishes the company's demands in time, there may be spaces where a kind of basic research, related to the applied one, is possible (EI05-

accomplished, consequences are more dramatic. Time pressure is not regarded as harmful by all. One of the seniors who leads the projects for the company considers it quite beneficial, for one learns to "be competitive" and work "under pressure". There is a point on which all the people interviewed agree, though: collaborating with companies hinders publication, for several reasons[36].

Maybe the most important one is the secrecy of the work, due to the patents. Indeed, the company is interested in producing something profitable and not in knowledge 'per se'. In case of obtaining this something, they want to patent and commercialise it. Therefore, it is very important that no information about the product is known before the product of a fruitful research has been preserved under the form of a patent. As it is clear, to publish the paper in a specialised magazine would not be the best way to keep the research secret –let alone that it is not allowed to create a patent of already-published information. Consequently, publication needs to be postponed after the patent.

This requires time: a whole year may be needed until the patent is completed, and this might be too long if there are other competing groups working in the same issue -any of them may publish the same results before them. The impossibility to publish may have important consequences. It may not be very transcendental if one has the expectancy to remain in the laboratory working for the company for many years, but it may be more bothering if the person working has the expectancy or possibility to work in basic research in the future.

Not only seniors and postdocs are contracted thanks to the mediation of the company, but also Ph.D. students, with grants given not by the state to do basic research, but by the company to develop applied results. This is the case with several Ph.D. students

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04). However, the major part of one's research, if one is paid by the company, is dedicated, naturally, to applied research.

[36] First, it is very difficult to publish applied research, most journals privilege basic research; second, their task is quite repetitive (for instance, once they have created a vaccine, they have to repeat the procedure in order to produce enough amount of it so that the company can commercialise it), which

of this lab –and even those who receive public grants may collaborate from time to time with the company. The result is that, little by little, the majority of the group is related to the company more or less directly. Unavoidably, this situation creates particular conditions in this laboratory. Perhaps the most radical effect that this laboratory has undergone since they collaborate with the company is the progressive change in the orientation of the research. Progressively the proportion of applied research in comparison to basic research increases. This is nothing negative per se, of course. However, members of the laboratory see a 'danger' lingering in this transformation: the laboratory runs the risk of becoming an extension of the company. And these are not our words, but theirs: "we may become a production plant of the company, doing mechanical work; this does not quite fit in a university laboratory" (EI07-15).

Working for a company may have another influence on the group: the lack of mobility of its members. Whereas it is not rare for members of other groups to spend some months abroad, working with other laboratories, or attending conferences, members of this group tend not to leave the lab. Neither the seniors nor the Ph.D. students go abroad –either for stays and postdocs, or for conferences and meetings. Whereas there are several explanations for this fact, one of them seems to be intimately linked to collaborations with private companies. The point is easy to understand: leaving the laboratory for a while in order to spend some time abroad is incompatible with the regular and deadline-oriented work for the company:

> "In this group we do not travel. (...) Here we work, we produce… but to go outside to learn is not a priority" (EG05-19).

And participating in conferences is not the best way to keep one's work secret:

> "One cannot do stupid things with the company; you cannot exchange information with others, since patents are involved" (EI09-19).

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does not help to publish. Third, the group priority is 'delivering' results to the company good and fast,

5.1.2 Second strategy: colonising services, hybridising TAQs

One head of the IBF, with the collaboration of other heads, has invested time, money and efforts to create two Services for the university community within the IBF. Each of these services brings with it, as we saw, a vacancy for a TAQ, a high-specialised technician. This profile does not correspond to a researcher, but to a figure of support for research –this person, in theory, is not supposed to be a scientist, that is, is not supposed to be motivated to develop one's knowledge and one's group, neither to do academic career. Here is where the head's strategy enters the game: he has contracted some researchers as TAQs! All of them would like to keep on researching and enter the university through a more standard way, but since they knew that progress within the university was barred, they accepted this alternative[37].

The trick is not only that people with a higher profile have been contracted for a job with less profile. The master move is that by accepting this job they have accepted the tasks proper of a TAQ *and* those of a senior. In this way, they have become a kind of concealed or undercover seniors. Nobody should misunderstand this. The case is *not* that these people are paid as TAQ but actually work as seniors. On the contrary, these people perform as both, TAQs *and* seniors at the same time. The deal, then is the following: they work as technicians and are paid as technicians, and if they have extra time they can develop their own line of research *and* supervise Ph.D. students of their groups.

The contract or position as TAQ, then, grants their partial, temporal stability. At first sight, to obtain a contract as TAQ is an attractive alternative: this gives them a salary that at the same time allows them to remain at the university, and be able to keep on researching. For the group this also brings the advantage of incorporating a senior and

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and not publishing, which also demands time.

[37] Of the three technicians, one was offered the job; another offered himself for the post when he learnt about this possibility; the third one applied for it when a friend told her the IBF was looking for a technician.

securing the pyramidal structure, when the university is not able to absorb more people any longer.

The balance TAQ-senior, even when it works efficiently, has some inconveniences, though. Firstly, the qualification: over-qualified people perform tasks which are below their capacities for a salary lower than their knowledge and experience would advice. Secondly, like many other seniors having to combine research and teaching, the hybrid TAQ/senior hardly finds time to carry out her own research at the bench, since spare time is devoted to directing Ph.D. students. Thirdly, they lack the freedom to develop their own ideas, and carry out the research they want. Moreover, the development of their scientific career is barred: they cannot create their own group; they cannot develop their own research. Even publications are somehow impaired, since they can never publish as first or last author. And sometimes, their position as a technician is an impediment for them to be formally recognised as directors of thesis. There may also be a degree of dissatisfaction regarding the topics one needs to work in. It is obviously not the same to work in one's topic of research, than having to do analysis or running programs for others. Nevertheless, the TAQs try to make their job as rewarding as possible, and they usually find ways to enjoy it, even if this means investing a bit more time per task than strictly necessary. Besides, when possible, they try to obtain not only 'satisfaction' and 'pleasure', but also other forms of rewards such as publications and collaborations.

Until here we have considered the ideal situation, as if the post as TAQ allowed the person to keep a balance and work as a senior for the research group at the same time (50%-50%, to put it bluntly). But what happens when this balance is not so easy...? The truth is that the service, involving deadlines and deliveries, usually absorbs most of their time –at the end of the day, this is what they are paid for. As a result, the supervision of the Ph.D. students and the engagement of the TAQ/senior in the everyday functioning of the laboratory are clearly affected. They attend the most urgent needs, but they could make much more if they had more time. "You have to stretch yourself like chewing gum" (EI12-10). We must add, however, that even

though Ph.D. students under supervision of TAQs can perceive their stress, they do not feel abandoned: "This person gives himself totally to us; if we need him now, he leaves the rest and helps us" (EI04-08). In short, to be a TAQ may be a blessing and also a curse, since this may mean very little research, and in any case, always under stress, overworked, and at the cost of personal non-remunerated time.

5.1.3 Third strategy: turning the vortex into the middle

We have seen how big groups have tried to construct and preserve a pyramidal structure. We have also seen that there are other groups which, in spite of efforts and time, have not managed to grow. The question now could be: what happens with new groups, which still have not become big? How do they manage to acquire this big structure? Or, maybe we should first ask: do they? The head of a young group which has been in the up-and-up will help us answer this question. This group has received regular funding since its creation, increased the number of Ph.D. students and produced several Ph.D. theses; its rhythm of publications has increased, and its main objective is to do the same with the quality of the journals. The head has already several contacts with foreign teams, with some of which he shares funding and projects, and some of his Ph.D. students have spent some time abroad, in other laboratories.

Nevertheless, and despite this striking trajectory, this group has what until now we have regarded as an underdeveloped hierarchical structure: a two-level structure, head on the one hand, Ph.D. students on the other. This simple structure means, as we have seen, that the group is vulnerable and exposed to cyclic rhythms of activity, to up-and-downs. The group will have moments where Ph.D. students will be in full activity, and other moments where Ph.D. students will only be starting (or not there yet), and publications will diminish. The head is very aware of this, and seems to accept it:

"I believe that in my case, given that I am the only staff of the group, the up-
and-downs will be noticeable. I am not able to keep an input and output regular
enough for them not to be noticeable" (EI16-10).

This acceptance may come as a surprise. But he knows that the possibility to contract
intermediary figures is barred: no chances of new lecturers or TAQ positions; Ph.D.
students, as well as postdoc grant holders will eventually leave once their task is
finished; contracts with private companies are temporary. In short, the head knows
that any entrance, welcome as it would be, would nevertheless remain a temporary
solution –as he says, "new members will come in, but they will also come out" (EI16-
10). He knows that he cannot base the strategy of the whole group on those
circumstantial possibilities, and has accepted that his pyramid is very unlikely to
grow vertically, at least not in the near future.

Then, what about growing horizontally –that is, increasing the number of Ph.D.
students? One could think that, if one wants to be bigger, one needs to produce more,
and that increasing the number of Ph.D. students could be the right step in this
direction. This strategy carries a risk, though: if a head has too many Ph.D. students
but no senior or postdoc, the head may be overwhelmed. Doubts cannot be properly
resolved, papers are not written in time, work accumulates at the head's desk, etc.
Production would not increase, but rather decrease. When one has so many Ph.D.
students, the only solution is to develop the group vertically –that is, to build
hierarchy. This is precisely what he cannot do, so he takes a different strategy: not to
grow and keep the horizontal dimension at the proper size: "now we should not grow,
but consolidate ourselves. I cannot process more results per time unit!" (EI16-28).

They cannot grow vertically; the head thinks it better not to grow horizontally. The
head makes the good out of two evils: he has created a way of working which
actively avoids hierarchy and has laid the basis for a collaborative work between he
and his Ph.D. students. The strategy he chooses is quite opposite to the expansive one
traditional groups have developed. If the normal pyramid tends to multiply steps

between the Ph.D. students and heads, so that problems are filtered and heads are freed to devote their time to 'management and politics', this new strategy turns the head into a crucial point for the every day working in the laboratory. Actually, as I will try to show, the functioning of this laboratory cannot be understood without his presence. In this case, everything -the number of Ph.D. students, the number of papers, the number of projects and topics which may be taken up, the number of contacts and relations- is limited by the capacity of the head. The group can go as far as the head can go.

This different orientation can be noticed in the way the group is led. The first visible sign is the head's desk. He has his office within the laboratory – in one corner, one can see a table, some drawers, shelves and files. No wall, no door, no secretary separates him from the benches and his Ph.D. students. This is a conscious strategy, so that when he is in the laboratory, he is continuously at reach (available for his Ph.D. students as well as for other Ph.D. students, maintenance technicians or technicians). Since he is present in the lab when members of the group work, the consultation of doubts is very agile; likewise, if she needs some data from them (for instance, when he writes papers), he only has to ask. And let us incidentally notice, although he does not say so, that with this strategy direct control on Ph.D. students' work is also improved.

Another striking difference between this young head and the rest is that he is probably the only who still does research at the bench (with his own hands!). This is extremely rare among heads, busy enough with the rest of activities management requires. He would probably not be able to do it either if he remained in the IBF, since once the head is enmeshed in the every day activities of the laboratory "it is difficult to find concentration; it is difficult to do an experiment if you are thinking you must write a letter to somebody" (EI16-19). His strategy, then, is to go abroad, invited by another laboratory for some collaboration. When one disappears for a while, leaving teaching, group and bureaucracy behind, one can concentrate again in research.

This requires strategic thinking, he says: there is no point in trying to start a research which one will not be able to finish in a short period of time, so one needs to plan one's choice accurately. The reward is being able to publish some paper as first author. Apart from curriculum and pleasure, to keep on researching has another advantage: one actualises one's knowledge and can follow Ph.D. students' work better. When a head does not work at the bench anymore, it is difficult to plan an experiment properly (involving decisions about the proper technique, the time needed, what is feasible or not). This is why seniors usually take up the planning of experiments. However, in a group without seniors as this one, the head needs to be in charge of this task too; subsequently, it is crucial that the head keeps up with experimentation. This is a clear illustration of the head's central role in such organisation. He knows it will be increasingly difficult to keep on researching: everybody has limits, and he knows that someday he will find his; but meanwhile...

Another characteristic of this group is the way they write papers. Whilst in other laboratories Ph.D. students write their own papers, this head prefers to write himself all the papers produced in his laboratory[38]. Of course, he can do this because he does not have so many Ph.D. students as other groups, but for him it is important to assume this responsibility:

> "I think it is the only reasonable strategy, the best for the group and for each individual. I think one develops in science in different stages, and in each stage there are some things to learn. Being a Ph.D. student is not the stage in which you have to learn to write papers. When you are doing your Ph.D. thesis you need to learn to think, to read, to interpret your results, to discuss, etc. After you have finished your thesis, then it is a good stage to learn how to write papers. I think it is ridiculous to make people waste their time writing papers –I would then read the paper and need to cross everything out. How can a person who has just finished the degree have a wide enough vision so as to write a paper!"? (EI16-22).

For the head, this is also a matter of speed and quality of production: he can write better and more efficiently than a student –which comes to the advantage of the curriculum of the group and of the individual Ph.D. students; he believes it is better for a student to finish the thesis with fifteen papers, than with one written by herself. Once more, his strategy places the figure of the head in a vital position, in charge of the last product of the laboratory. As he once told me, "a laboratory is like a factory: in the same way as others produce yoghurts, our output are papers and patents". Final delivery, rather than the amount of learning by Ph.D. students, is what counts. Which may have something to do with the expectancy that very few of his Ph.D. students, if any, will actually be able to remain in his group[39].

His different way of working is perceived by members of the IBF. He is said to be very 'accessible', in both directions, interacting with people at all levels. Moreover, his group is said to be "a synthesis", uniting the positive aspects of the functioning of different groups. There is only one aspect in which this head does not stand out in rankings: Ph.D. students of the centre perceive that other heads dedicate more efforts to find ways of expansion and extra-funding. These efforts are something they appreciate, because their future depends sometimes on how active their heads are (whether they help them to find contacts and networks opening possibilities of future jobs).

5.2 Young generations: exhausting possibilities

Until now, we have distinguished three groups of people in different conditions within the IBF. There is a group of people whose belonging is secured: they entered the world of research with all the chances on their side to anchor in the university and

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[38] Although the more experienced the Ph.D. student becomes, the more he 'is allowed' to contribute to the writing process.
[39] And as the head reminds us, neither is it sensible for students to waste their time in a lab doing a not very rewarding task as writing papers before they know whether they will be able to remain within academia. If they ever go to the private world, curriculum will be more praised than experience in writing papers.

develop an academic-scientific career. (Here we find heads mainly, and some seniors –or, put differently, professors and lecturers) Then there is a group of people who thought they could follow the same model, but who painfully discovered that their progression was suddenly cut. They found trapped in a difficult and precarious situation; too well prepared, experienced and vocational for the private company, they found the way into university barred (here we find many seniors and TAQs).

There is still a third group, though, constituted by all those Ph.D. students, postdocs and young seniors who are just starting their scientific careers. This third group has closed doors to enter university, but their advantage is that they are well aware of the practical impossibility of an academic career in Spain: they have known from the very beginning that their progression will come to a halt. In an interview, three Ph.D. students were asked whether they had ever thought of the possibility of becoming seniors in their group. After three seconds pause due to astonishment and several looks exchanged among themselves, this was their answer:

> "A: ha, ha, ha !
> B: ha, ha, ha!
> C: ha, ha, ha!
> *Note of the transcription: (general hilarity)*" (EG02-09)

This awareness, however, does not prevent them from feeling vocation, from trying to develop a research career, from calculating what next possible move could be the best to bring them closer to a future in which science is still a possible belonging. I would like to explore now some of the paths they consider as future possibilities, how they evaluate these options, and how this situation of uncertainty affects their attachment to the group and science in general.

5.2.1 Private company

One may wonder why, if chances to remain in academia are so bad, they do not consider abandoning the university. After all, in many countries private companies engage in some of the most competitive research. To 'jump' to the private sector would not necessary have to imply the abandonment of research. They do consider this possibility; as one of them put it, "this is an option that is always lurking in your mind" (EG03-10). However, this option is not free from problems. One of them is that too well prepared people are rather problematic for the industry. But perhaps the biggest is that jobs in the private Spanish enterprise very rarely involve real research, because there is almost no industry taking up research –private research is to be found mostly in foreign companies abroad. The little industry that may exist cannot absorb the number of specialised people that the university produces per year. All this considered, it is quite clear that, in most of the cases nowadays, the choice of going to a private company equates to giving up research –together with the personal satisfaction attached to it. This is why the transition to a private company is regarded as the last resource, and they will try to exhaust all other possibilities before taking such a drastic decision. But what other possibilities do they have? What are the means and perspectives they have at hand, in order to try and survive within science?

5.2.2 Becoming a technician

We met some Ph.D. students who cherished the hope of occupying a TAQ position to operate some piece of equipment after the end of their grant. In spite of disadvantages, they see in this their last hope to remain in the IBF -and in science. However, most of the Ph.D. students were quite opposed to this solution. To start with, they all know that there are very few vacancies as technician, and the possibility of getting one is very low:

"This may happen to one of us, or maybe to none of us. This is a circumstantial solution. Besides, it is stupid to consider this as a future for

you, because there are thousands of very good people queuing in front of you, waiting for the same opportunity" (EG02-10).

Nevertheless, the impossibility of obtaining these posts is not the only inconvenient. Many Ph.D. students consider that these vacancies are very harmful. They have seen the situation of their TAQs: too much work, badly acknowledged, badly paid, academic career blocked, temporal instability... This is for some "a very bad solution", and they claim that "under no circumstances would they accept such contracts" (EI02-10/1). More generally, this generation has seen how their seniors, already with families, were suffering under instability, lack of money and precariousness at many levels. And this image pushes them to try and be realistic about their situation. Everything points at a quite serious impossibility to work as a researcher in this country, both at the university and at the industry -though for different reason. There is, nevertheless, one last chance, that many consider seriously: to go abroad.

5.2.3 Going abroad

As it happens, research in other countries seems to be in a better situation. On the one hand, the governments of those countries invest more money in research; on the other, the structure of their universities seems to be less collapsed. Even private companies may offer real research positions –for instance, in a pharmaceutical one. This is why some scientists, who see no chances of developing their careers in Spain, consider the possibility of going abroad either temporally, to do a postdoc, or even to live and work there permanently –or, to call things by their name, they consider emigrating.

Let us discuss the first alternative. As it is known, a postdoc involves applying for a grant in order to continue research in another laboratory (other than the laboratory of origin) for some time (from one to three years usually). Whereas nothing impedes to

go to another Spanish laboratory, it is usually easier to obtain the grant to go abroad. In the past, a postdoc was quite often an 'intermediary step', a possibility to be paid while making time, waiting for a post at the university (available to open competition). Those were the years when a Ph.D. student knew he had a chance to apply for a position which was not available yet, but would be in some months or a couple of years. For the 'trapped generation', however, the postdoc had slightly changed its function: more than a temporary shelter, it was an obligatory point of passage, since it was difficult to obtain a post as a Lecturer without having been abroad.

But this situation has changed again for the youngest generations. Since the chances to get a post in Spain are rather law, to put it euphemistically, the question they face when considering a postdoc abroad is, 'what for?' Definitely not in order to come back and compete for a post. The only reason for a postdoc, then, is to be able to keep on researching[40]. The strategy is simply to exhaust every single grant and economical help in order to keep on doing research. After these years, however, the situation may have remained the same –if not worse: no vacancies at the university, no job in a private company. They may have no other option than to abandon research anyway, with an additional burden: our researcher now is not that young anymore, has no experience outside research, has not quoted in Spanish Public Health system[41].

In this case, future looks rather sombre. Those who want to persist may opt for a postdoc. Others consider this option too risky. The decision to go abroad for a postdoc often means evaluating whether they can endure research under high levels of instability, continuous mobility, economical precariousness, and difficulties to settle and create roots somewhere –being well aware that at the end all these efforts

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[40] They can research for two years, plus two more years in case they apply for a "recuperation grant" on coming back -a grant which, in theory, intends to 'recover brains for national science', that is, it is supposed to help the postdoc come back to her country again: it provides them with a salary in a shelter laboratory for some time, while they look for a more stable job or a vacancy somewhere else. However, the efficacy of these grants is put into question.

[41] This indignant condition has been partially corrected recently.

may be in vain. This is why some Ph.D. students refer to this choice as 'a prolongation of the agony' (EG05-07).

Let us consider now a more drastic solution: if the situation in Spain is really so bad, and if one's desire to be a researcher is so strong, then, why not moving to another country where one can find a job in science? What about emigrating? This choice is quite radical, not only in the sense that implies a change of context, but also in the sense that it may be quite a definite move. Many are not willing to do this sacrifice; others cannot, some are already married, some have children. Some simply do not want to leave. But some do choose to go abroad for a postdoc. Albeit being aware of the risks this decision involves, people choosing it consider it a better and more dignifying solution than suffering under the conditions in Spanish science. Some see it as the possibility to get to know other laboratories and different ways of working, a chance of leaving borders behind: "he used to say we had to remove frontiers from our head, that we had to change our mentality" (EG03-12). For others, the fact that researchers cannot remain in Spain is a 'national shame', and not an occasion to celebrate nomadism: "I think it is quite pathetic... Our country cannot absorb professionals whose training has already paid –professionals who are highly valued abroad" (EG03-12).

The uncertainty of one's future is sometimes too overwhelming. Quite often, Ph.D. students decide to let themselves be carried by circumstances. This usually means to let time go by and remain in science until they cannot further their career (EG03-10). The time of strong planning is gone. One needs to accept uncertainty and indefinition, if one wants to try and catch up with dreams -even if one follows the dreams while knowing that, sooner or later, one has to wake up:

> "I think one has to say, "we will see". Yes, because thinking you can devote yourself to research all your life is difficult, very difficult. But I don't see why we should renounce to it from the start. You must know it is very difficult, and that you will probably end up working in something else, but you can still try to keep on going, and see whether you can put up with precariousness, or you want to

have a better life. Either you go on applying for grants until you exhaust all legal possibilities -postdoc, incorporation grant, anything- or you say 'look, that's enough, I look for a job' -and then you still need to be lucky and find it. And that's it" (EG05-06).

5.3 Risky vocations

Previous studies have shown the presence of strong vocation narratives among scientists (see Law (1994) for an interesting treatment of vocation as a mode of ordering; see also Gilbert & Mulkay (1984) and Weber (1992). 'Vocation' (the call of science) is for many researchers an explanatory force that pushes them to become 'scientists'. The IBF is no exception: narratives of vocation are to be found everywhere. Since 'passion for one's work' is quite an inapprehensible notion, vocation translates usually into 'hours spent at work' –which makes heads and production rates very happy, of course. Narrative of vocation associates with devices of control and visibility. To perform vocation, one needs to be seen at work many hours, and if you don't, then maybe you are not as good a scientist as you think:

> "The best people in research are those who stay twelve hours a day in the lab –the best in the sense that they know more, work more, are more productive. (...) There are those who stay until middle in the night, and then you already know what kind of person they are; they never find the moment to stop. They are also the most brilliant people, due to vocation I guess, otherwise you don't stay in the lab until 12 am. You stand out because you do not have science so deep inside, you know? I think there are people who carry it deeper than others. Well, and the amount of hours you work also have something to do with it: the more you work, the more you can think about a particular problem. Therefore, there is a lot of people who assume that if you have a private life, you cannot work properly in research, because if your work demands that you stay until 2 AM, you must do it" (EI07-22).

Until here, the story is not new. However, for a long time stories of 'vocation' were mixed with stories of 'brotherhood'. We have already mentioned that in the IBF one can breath a particularly nice, relax, co-operative environment. This is so now, and it has been so since its origins: they say this collective spirit was even more intense some years ago. Researchers, out of pure vocation, used to spend the whole day in the lab together, including weekends and bank holidays, and then, after a long working day, would spend some free evenings and holidays together! And since they had such a great time together, without realising it, they kept on working longer. Thus, narratives of vocation and friendship weave and constitute each other. Now, though, they all agree the atmosphere has changed. Granted, the good environment remains, everybody thinks this is a privileged institution: both personal and work relationships are great. Likely, people still work a lot and dedicate many hours to their job. However, the collective spirit, as it was before, is somehow gone. And consequently, the mixture between the narrative of vocation and that of brotherhood is being split: relationships are very nice, but they are said not to have so much influence as before in the amount of hours people work in the lab.

Nevertheless, narrations about vocation are being disrupted in at least two interesting ways. The first is a counter-narrative about the right to enjoy a personal life next to the professional one; the second, about a new awareness of the fact that research is not only a vocation, but also a job. Both narratives intermingle. I cannot say that these two counter-narratives correspond to a particular group of people, but they were clearly not told by anybody –I heard these counter-narrations basically from many Ph.D. students and some young seniors, with a high proportion of women among them. I do not think it is fortuitous that these disruptions come from members of the groups with more insecurity and economical instability, who know that their chances to keep on researching are rather law.

The first type of counter-narrations, those challenging the excessive, all-encompassing character of work, try to emphasise the right to enjoy a personal life, and show resentment against colleagues which reinforce the oppression of 'vocation':

"I think the mistake is to consider that you cannot do good science if you don't work many hours. And I think we should say 'we don't tolerate this anymore'. But your own colleagues, if you leave too early, laugh and say 'oh, look, this person is leaving at the head's time!' (...) They have to see you around –arriving soon and leaving late, it doesn't matter whether you really work or not, but they have to see you there" (EI07-23).

At the same time, though, these types of accounts are somehow ambivalent, for they reveal a certain admiration for those they try to differentiate from. Indeed, in order to distance from the wider 'vocational position', they sometimes reinforce the division between people who work non-stop (out of vocation: they feel science) and those who do not love science so much, since they dedicate their time to other activities (which usually means those who attempt to have some kind of private life next to the professional). To reinforce their own position, they usually emphasise that having a personal life is not simply a matter of 'choice' but of health –physical, psychological and relational health. If one works too much and makes too many efforts in the name of science, one loses the chance to enjoy a 'normal life' with family, kids, friends, etc.

And in order to convince us of the maleficent power of vocation, these warnings are usually illustrated with a 'short illustrated catalogue of light perversions': people working in the IBF in the middle of the night; people working every single weekend of their lives; people working together and spending holidays together; people working at the weekend and going together to the cinema afterwards, just to meet next morning again in the IBF to keep on working... As the story goes, some years ago there was a whole generation of researchers that were singles, because they did not relate with anybody else than themselves. We were also told of mothers working in the early morning hours and then rushing home just in time to wake up the kids and bring them to school; of people whose children and wife have not seen them for days, and also of angry wives (no word about angry husbands, though). We were told of people postponing having a family, or even deciding not to have a family, so that

they can dedicate their lives to research. Of people leaving the family behind in order to go and do a postdoc, etc.

The second variation of a counter-narrative of vocation is subtle. Like the previous one, it emphasises that the danger of a very strong vocation is the blurring of the limit between a professional and a personal life -to the advantage of the professional. However -and here one finds the main difference regarding previous generations- this is not problematic because it is unhealthy and it prevents them to have a rewarding private life, but because it makes you give away too many hours of your power labour. Have a look at this almost entrepreneurial account:

> "It is too easy for us to start working fifteen instead of ten hours, go home to sleep and come back. This is quite dangerous... It comes a moment when you think this is the most normal thing in the world. ... They make you feel that this work is something yours, this is the problem, ha, ha, ha. (...) You have the wrong idea that you are working for yourself, that you are your own head... (...) and then you might think "I work for myself, there is nothing wrong with investing some more hours". But this is a problem..." (EI09-26/7).

This Ph.D. student comes close to say that the problem with feelings of vocation is that they may become a device for subjection: one forgets that science is not only a source of pleasure and passion but also a job one is paid for. If one works too much, one is underpaid, and allows exploitation. Furthermore, this counter-narrative does not lose sight of the fact that one needs to live, and decently, with the salary/grant one receives. One can work for free in particular occasions, but one should not do it systematically: if one cannot eat or secure the future, one should not remain in science:

> "Without a grant I would never have started my thesis, for I knew something: I wanted to gain money –well, I had to gain money, since I have this bad habit of eating three times a day" (EG02-05).

"One must have some basic needs covered, that is, you need to have a satisfactory social status, a satisfactory economical entrance, and then once this is sure, you may see what you find" (EG02-13).

"My grant expires this year, but I will not be finished yet. If my head gives me extra money, I stay; otherwise I will look for a job. I will not work for free to finish my Ph.D. thesis, though -you need to live first, and then comes research" (EG05-01).

"I will orientate my postdoc to create a profile to work in a private company, so I will have a job somewhere, even if it is in a foreign country" (EG02-16).

Of course, these people are not simply telling us that money is necessary to survive, that much we all know. These accounts remind us that science is their passion *and* their job –and that, therefore, they need to make their living from it: one needs to feed one's body and not only one's mind. The idea of remuneration appears clearly. A researcher works not only because of pleasure, but also for money, so as to survive decently. Implicitly, notions of calculability make their appearance: one can work up to a certain amount, since this is what the 'contract' establishes; but as soon as one works too much, one is giving hours away. Moreover, these comments are rather a "declaration of intent": not only do they affirm that to care for economical security is necessary, but also that it is a *legitimate* priority -even a more important priority than researching, if researching involves instability. They announce, maybe also to convince themselves, that, much as they love science, they are not willing to put up with precariousness, for they have already seen where this leads: to a whole generation with unstable, undervalued, insecure perspectives. And they reject this possible future[42].

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[42] This does not mean that they think that previous generations made a mistake. They recognise that the situation led them to a blind alley, and they did not have many other possibilities. But still, they do not want to end up there.

The contrast with previous generations can be better appreciated when compared with the story of some old seniors. Two of them had tried to work for a private company. After a while, they had had enough. From that time they tell stories about boring jobs, "cover" research departments as a marketing strategy, sordid economic matters, "white necks & ties", travels through the whole country doing demos and selling products... Some months in the company doing a not particularly rewarding job were enough to convince them to go back to university and take the risk to bet for research. Theirs are stories, then, which privilege personal satisfaction and real vocation above economical needs and stability:

> "After two months I quitted and came back to the university. This meant to reduce my salary to a third part, to renounce to an indefinite contract for a two-year grant, and to leave a job as a chief of research. Some people would kill me for this!" (EI12-03).

There is little doubt that the context in which the younger generations find themselves is different from the context of previous generations. The low chances of promotion are so conspicuous, that they all have been forced to think in advance about their situation, and interrogate themselves about their priorities. Those who under these circumstances still choose to play vocation are free to do it, but they do it at their own risk.

This awareness promotes a different attitude towards the group, too. Some years ago, when conditions made it still possible, a group (and its head) would take care of its members. Now the person knows the group cannot provide her with a future; both the head and Ph.D. students know that their relationship is restricted in time: the person works with and for the group, as long as the group can give her shelter; however, as soon as the group cannot "pay back", then the person will have to part and look for shelter somewhere else. In such a situation, strong attachments and fidelities would not be adaptive. The tinge is more individualist, so to say; at the end of the day, it is the individual who has to take care of him- or herself. Fidelity stops short after reciprocity. No wonder than they develop, somehow, a more pragmatic point of view,

a point of view which reveals a real compromise with their work, but a compromise which is, at the same time, flexible[43].

> "I am in science because I like it a lot, because I enjoy doing what I do, because I like the work environment, and in order to be able to eat. Therefore, if some day I cannot eat, I do not like science any more, or I do not like the atmosphere, then I will quit" (EI02-14/5).

One would be tempted to conclude that the attachment of this new generation to science is weaker than that of previous generations, as if they had less vocation. I think this would be too fast a conclusion. These Ph.D. students may vibrate exactly the same when producing a good result or a good paper, or when doing the every-day work of the lab, as the older generations did. If they had better conditions, that is, if they could, they all would like to go on researching. Some are even willing to leave the country in order to keep on researching, with all the sacrifices this involves. All of them dedicate a huge amount of hours per day, including holidays and weekends, to work and develop their research. This is why I think that the argument should not be about quantitative differences as much as about qualitative differences: their attachment is not weaker, but of a different nature.

As it could be expected, relations have also changed on the side of the heads. Heads know that the short-term and middle-term situation do not look optimistic, and nobody even dares to bet what the long-term future will bring. Under these circumstances, no head can guess the possible consequences of decisions: should a Ph.D. student take the risk to go and do a postdoc abroad, just to return older and find no job? Should the Ph.D. student abandon research just after the thesis and look for a position in a private company? Is it better to go directly abroad and fight for a position in a foreign university, or in foreign private company? Which option optimises chances to remain at university? And who dares choosing? Given the

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[43] An example of this new attitude is that they may engage in a postdoc, since they would like to continue research, but with the idea of training abilities to search for a job in a private company afterwards. Between being a hungry scientist or worker with a decent income, they know what they would choose.

uncertainty and risk of the situation, new heads know that they can neither provide their Ph.D. students with a future, nor offer a possible future strategy (as heads used to do years ago) and suggest possible steps. Rather, they feel it is a matter of morality to make that much clear: Ph.D. students' *professional* choices must be Ph.D. students' *personal* choices:

> "After one finishes the thesis, one has to decide whether one wants to go on with a research career. One has to take one's decisions here. I cannot do anything for them, neither do I have to (...) I mean, there is unpredictability regarding the future of somebody who goes abroad for a postdoc, and therefore, the person must decide on its own" (EI16-13,15).

It is probably not fortuitous that this new attitude is accompanied by a new way of talking about and conceptualising Ph.D. students. We have argued that Ph.D. students are usually not considered 'students', but 'scientists', the first figure of the scientific pyramid[44]. Accordingly, heads understand the present situation as one of "truncated development": Ph.D. students are scientists who find it impossible to develop all their potency. There is only one head who, in the interviews, refers to his Ph.D. students as 'students': the young head. He talks of them as if they were students who, after finishing the first degree, decided to take up a Ph.D. thesis in order to deepen its knowledge and training. What these people will take up afterwords is not the head's problem anymore. The relationship established during the Ph.D. thesis is a limited *contract* and does not oblige neither of the sides to a further relation afterwards:

> "I think that the relationship established between a person who does the Ph.D. thesis and the group is a relation in which both sides take a profit, both sides learn something, and learn from each other. This relationship ends when a person finishes the Ph.D. thesis. Do we want to establish a new *contract*? If possible, perfect, but this is already a new, different contract. Is there any other way in which we can remain together? Yes, perfect. If not, not" (EI16-16, our emphasis).

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[44] They use the word 'student' to describe the position of somebody who is doing the degree (even though when talking of themselves, Ph.D. students may say that they are studying –but not that they *are* 'students').

The new situation has one more effect: the decision is delegated to the Ph.D. student. Actually, they are forced to decide. If some years ago entering a group meant participating of a dynamic that brought the person forward and offered her biographical trajectories (Ph.D. student, assistant, lecturer...), now trajectories are more insecure: affiliations do not carry with them a proposal of life anymore, now one has to 'construct' or mount one's own biography (Beck, 1986). Young scientists need to choose what their next step will be and assume the consequences of these risky decisions. This is a 'do it yourself' biography, which forces people to a reflexivity, continuous looking at themselves to decide what to be next. In a context of uncertainty and insecurity, this 'being forced to decide' can be more a torture than liberation –a continuous looking for private solutions which is tantamount to a distribution of risks. Whereas we all have to deal with these new situations, some are more affected by it than others; heads have to lead their groups in uncertainty, but their position is not threatened in the way some seniors' or Ph.D. students' are. Here we find one of the new types of inequalities emerging in our present, the unequal distribution of the need to deal with insecurity and reflexivity.

This responsibility to choose does not imply more freedom, as the 'common sense' idea would have it, but quite a paradoxical consequence ensues: levels of standardisation increase. At the end of the day, the uncertainty of decisions make the person completely dependent on the labour market, on the variability of the situation, on the institutional possibilities available to her. What is presented as 'a personal decision' is actually the choice among several already institutionalised paths that are made possible by several institutions. It is not that one *can* choose; rather, one *has to* choose. With one drawback added: institutional solutions are sometimes too constrained to a national landscape. Thus, the 'trapped generation' has been caught in the structure (spider web?) of the Spanish university, an institution which can only offer solutions –late and with efforts- to some of them. The young generations, on the contrary, have already started opening their biographies to a wider context beyond national borders (Beck, 1999).

The other obvious effect of this new situation is a tendency towards individualisation: for the sake of survival, individuals are now compelled to make themselves the centre of their own life plans and conduct (Beck, 1986). If biography is individually planned and individually oriented, it is no wonder that success is read in personal terms –but so is failure. The present situation, with the impossibility to develop one's career and the amount of frustration it carries with, is lived personally as individual crises. But feelings of failure and self-blame are stronger felt in the 'trapped generation': some members of this generation managed to find a post, others did not; this difference, in most of the cases, had nothing to do with personal value, but with being in the right department at the right time. However, the evidence that some colleagues have achieved better situations than others leaves room for self-questioning and doubts. In contrast, given the blatant impossibility of promotion, the young generation starts to suspect that their situation has nothing to do with their particular performance.

With our comparison between the 'old style' and 'new styles' among heads and groups we are not trying to make an ethical point, as if one could decide which model is better–a model where the head tries to plan and provide for the future, or a model where the head does not want to assume responsibility in a situation of uncertainty. Each model seems to be more sensible in different contexts, and it is probably a mistake to think that this is a matter of 'leading styles' or 'personalities': previous generations had some resources at hand that are simply lacking nowadays. Our argument is analytical: this new way of conceiving the group creates a different relationship between each member (now, an individual) and the group: a more contractual relation, which matches better with these new narrations one can find in the laboratory: individual decisions, evaluation of costs, a diminishment of the identification with the group, context of uncertainty, temporal attachments.

This has been our presentation of the IBF and its world: groups, services, members. They all are our objects of study, our 'substrate', as they use to tell us in a nice mocking mood. Give or take some brushstrokes, this landscape should enable the readers to get their bearings. The rest of the chapters will elaborate upon it, mixing some more concrete theoretical concern with more empirical data. Whereas each chapter will deal with a different topic, and can be read independently, they are nevertheless connected in a way we will clarify in the conclusion.

Part I: Reproducing Science

> "Dr. Kelvin, may I have your attention and concentration for a moment. I do not intend to dictate any precise sequence of thought to you, for that would invalidate the experiment, but I do insist that you cease thinking of yourself, of me, our colleague Snow, or anybody else. Make an effort to eliminate any intrusion of individual personalities, and concentrate on the matter in hand. Earth and Solaris; the body of scientists considered as a single entity, although generations succeed each other and man as an individual has a limited span; our aspirations, and our perseverance in the attempt to establish an intellectual contact; the long historic march of humanity, our own certitude of furthering that advance, and our determination to renounce all personal feelings in order to accomplish our mission; the sacrifices that we are prepared to make, and the hardships we stand ready to overcome..."
>
> (Stanislaw Lem, *Solaris*).

> "...porque entonces yo aún no sabía que a pesar de crecer y por mucho que uno mire hacia el futuro, uno crece siempre hacia el pasado, en busca tal vez del primer deslumbramiento"[1]
>
> (Juan Marsé, *El Embrujo de Shanghai*).

1. Introduction: science as institution

Whatever science is, there is a claim difficult to contradict: science is what scientists do. If I observe scientists while they are working, I am supposed to be observing science. Not all the science in the world, not all the science in the history, but surely, a bit of science. Then, there does not seem to be much of a mystery there: science is a human, localised practice, transmitted from person to person, from group to group. Science is produced by people and in this sense people seem to encompass science. However, this is only one side of the experience, for, on the other hand, we all feel that science is also bigger than individual persons. If scientists make science, it is also

true that science makes scientists, transforming lay people into vocational experts. Moreover, science is an entity that existed in the past, and will exist in the future, before and after a particular scientist's life. In this sense, it stretches beyond a particular person, a particular people, a particular nation-state, a particular time: if we look back into *our* history (that is, Euro-American's[2]) we find the clarifying (illuminating, illustrating) light of science, and if we look into our uncertain future, science awaits us in our horizon[3].

This is a duality inherent to science: science is made by us, and science makes us. But before we fasten to produce judgements of incoherence or ambivalence, maybe we can recognise something familiar in this duplicity: is this not the way in which our culture imagines the relationship between society and individuals? Society as a product of human practices *and* society as that all-encompassing entity bigger than ourselves which constitutes us. Described like this, the notion of science parallels that of society, which in turn mirrors that of nature: distinct domains, self-regulating systems supposed to function according to laws of their own.

This conceptualisation emerged - if we adopt Strathern's (1992) account- at the end of the XIXth century, beginning of the XXth, strongly linked to the notion of 'nature', as it came to be conceived at that time. Nature, which until that moment had been conceptualised with social vocabulary (such as class, divisions and connections, rank and classification, as Darwin's genealogical trees exemplify) was progressively 'naturalised', that is, it started being referred to as "an autonomous field to be identified by its own internal regulation" (Strathern, 1992, p. 112). Once this notion of nature was in place, social thinkers had at their disposal a model to mould

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[1] "...because then I didn't know yet that despite growing, no matter how much one looks towards the future, one always grows towards the past, perhaps in search of the first dazzle" (Juan Marsé, *El Embrujo de Shangai*).

[2] For a debate on the multicultural nature of science, see a debate in Configurations by Harding (1994a, b), Cohen (1994), Farquhar (1994) and Kuriyama (1994).

[3] Perceptions of our future with science are nowadays tinged with other tonalities. Science is perceived not only as a source of solutions, but also as a cause of problems (i.e., biogenetics running riot), not to mention the awareness of the side effects that scientific interventions bring about. Moreover, public awareness of risks associated with science has lead to a 'scientification' of protests against science:

conceptions of social organisation: a new 'naturalised' notion of society was on its way. From then onwards, inverting the direction of previous loans, social notions were understood through concepts borrowed from nature -natural(ised) notions, so to speak: structure, organism, function.

This description of 'nature' and 'society' as self-regulating systems resonates also with the ideas on science that have opened this chapter, as if the latter, in its irreflexive use at least, participated of the conception of the former as formulated at the turn of the century[4], not free from organicism and evolutionary thinking. Both science and society can be fathomed as entities bigger than their elements (scientists and citizens), split into parts or subdivisions (functional differentiation) integrated into a whole (Science, Society). They evolve and grow in complexity as centuries go by, projecting themselves into the future, moving with and along time -riding on its arrow, so to speak.

However, that they bear more than one similitude does not mean that they are regarded as one and the same entity; on the contrary, 'nature', 'society' and 'science' remain independent domains. After all, for Merton, who expressed well the

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"People suspect the unsaid, add in the side effects and expect the worst" (Beck, 1992, p. 169. See chapter 7 for a more detailed exposition of these and other arguments).

[4] To claim such a parallelism sounds ridiculous if one thinks how people like Prigogine, Morin, Stengers or Serres fathom science. However, let us not forget that only a minority of scientists dedicate time to reflect on science. Most of them simply work. This came as a surprise to me, a psychology student stuffed and saturated during the degree with discussions about what science was, what the scientific method was, what was the right way to conceive hypothesis and experiments, what was the degree in which one could say that psychology was (or not) a science, etc. Members of the IBF had not had these types of subjects in the degree: they had learnt the meaning of experiments or hypothesis by doing, and discussions about what science was did not seem to be common. They are doing science, they are scientists, and this is all they need to know. To claim that they do not think or read much about what science is (or differently put, that reflexivity does not seem to be their main worry) does NOT mean that they do not read a lot about scientific themes and theories. This lack of reflexivity, however, does not disturb their job, and far away from my intentions to say that it diminishes in any way the standard of their professionalism. Of course, this lack of interest about reflexivity may have to do with the security of identities. Since they know they are science, they may not need to interrogate themselves about what science is: they are!! Whitehead put it so: "Science has never shaken off the impress of its origin in the historical revolt of the later Renaissance. It has remained predominantly an anti-rationalistic movement, based upon a naïve faith. What reasoning it has wanted, has been borrowed from mathematics which is a surviving relic of Greek rationalism, following the deductive method. Science repudiates philosophy. In other words, it has never cared to justify its faith or to explain its meanings: and has remained blandly indifferent to its refutation by Hume" (Whitehead, 1967, p. 16).

perception we are trying to articulate here, science was "a distinct and autonomous sphere"[5] (in Vinck, 1995, p. 24), and if it needed an ethos[6] to regulate scientists' behaviour, was not only to grant proper functioning, but also to protect itself from the corrosive effect of both, society and its ideologies on the one hand, and individual interests of scientists on the other.

The mention of this author at this point is not gratuitous. This long introduction tries quite clumsily to call our attention towards a characterisation of science that can be expressed with fewer words: science is an institution. This is, of course, no news; ever since science became an object of study has this dimension been relevant, and the pioneer work of Robert K. Merton (1942, 1973) is there to remind us. Once science is described as institution, analysts will dedicate efforts to study its mechanisms of regulation. This tradition (with other outstanding names such as Barber, Joseph Ben-David, Stephen & Jonathan Cole, Suzan Cozzens, Diana Crane, Ellis, Hagstrom, Derek J. de Solla Price, Arie Rip, Storer, Harriet Zuckerman, among others) has provided us with accounts on the process of institutionalisation of science (i.e., Barnes, 1994; Needham, 1956; Ben-David & Zloczower, 1962), its ethos and its institutional imperatives as well as its ambivalences and contradictions, its reward system and evaluation processes (Merton & Zuckerman, 1971), its age structure and its effects of it upon disciplines' cognitive structure (Merton & Zuckerman, 1972).

Such institutional accounts detect a source of tension in the difficult relation between science as a collective enterprise and the individual scientists sustaining it. This tension between the collective and the individual is typical of institutional analyses. Theoretical contributions on group processes tend to oscillate between two perspectives; either they see the totality (the whole bigger than its parts), or they see the parts (parts constituting a whole which is nothing but the juxtaposition of the work of many people together). Mary Douglas (1986) would be a representative of

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[5] Luhmman's universe of systems, environments and spheres is not foreign to this portrait.
[6] "The ethos of science is that affectively toned complex of values and norms which is held to be binding on the man of science" (Merton, 1942, p. 66-7).

the first position; Elster[7] (1990) would defend the second[8]. This tension is reproduced in science: for some, science needs to be explained without reducing it to psychological characteristics or individual actions of scientists; for others, science can be understood as the sum of scientists' efforts during centuries[9].

Despite feeling more affinity for the collective point of view, as it could be expected from us being social psychologists, we will suspend positioning. Our aim is not to build an argument either for one or the other, but to state the difficulty our tradition seems to have to unite both perceptions: either we see the collective or we see the sum of individuals. This incompatibility may have to do with our perception of number and dimension. Institutions encompass many entities (many people, techniques, devices, buildings, etc.) and therefore, they are perceived as big. Since they are big, we assume that a macro perspective is needed to approach institutions – and then we do not see individuals, which are small. And if we decide to observe individuals, we do not quite understand how to reach the institution.

That this is not necessarily so has been already discussed in the discipline (see Callon & Latour, 1981, and other contributions in Knorr & Cicourel, 1981). Big constructions may be big, but this does not mean that they must be approached with different theoretical equipment from the one appropriate for more local situations; neither does it suggest that we need to change context –stepping back to increase our scope, so to say. If science is an institution, this should be noticeable when we compare statistics of the number and age of scientists in different countries, when we

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[7] "I have been saying that institutions 'do' or 'intend' this or that, but strictly speaking this is nonsense. Only individuals can act and intend. If we think of institutions as individuals writ large and forget that institutions are made up of individuals with divergent interests, we can be led hopelessly astray. In particular, the chimeric notions of 'the popular will', 'the national interest' and 'social planning' owe their existence to this confusion" (Elster, 1990, p. 154).

[8] It is important to notice that this opposition is not one of 'cultural dopes' against creative beings. Even when looked at from a collective focus, institutions, as Mead specified, are sustained upon the creative activity of individuals, and do not reproduce automats that repeat without inventing. It is precisely the danger of this creativity that forces the institutional perspective upon science to imagine norms, rewards and implicit socialisation to pressure scientists to conform.

[9] Funnily enough, institutional accounts often end up taking the individual for granted. It is not by chance that Mertonian sociology of science is also called sociology of the scientists (Vinck, 1995).

study the process through which national budget came to include R & D, but as well when we observe 'humble' practices in a lab.

Which is another way to challenge what is big and what is small: complexity is not only found when we 'look up', but also when we look down toward detail (Law, 2002b). Strathern (1991), looking for inspiration in fractal thought, builds a similar argument. There are, according to Strathern, different perspectives to approach phenomena of study. First, one may want to distinguish analytical domains affecting a phenomenon, say, religious and economic factors, and then approach it from either a religious or an economic perspective (or alternate both). Alternatively, one may decide to take an instance of this phenomena –for example, a particular performance of a ritual-, and deepen one's analysis on this particular instance, trying to take on board all those relationships involved in such phenomena. Still another option is to renounce to the close, detailed analysis of such an instance, and compare this particular ritual with other rituals, while trying to yield a conceptualisation of socialisation processes.

Nevertheless, regardless of which approach one may choose, something is constant. On the one hand, and somewhat obviously, the more one expands the observations and analysis, the more information one obtains –one gains more insight on religious factors, or more insight in the sequences and meaning of a particular ritual, or more insight on the general factors involved in socialisation. On the other hand, however, the more information we gain, the more information we lose: intensifying research in one direction involves always a renunciation to pursue other directions. The more we know about religion factors, the less we explore economics; the more we understand this particular ritual, the more we miss the connections between this ritual and (not)similar ones; the more we compare rituals to understand the general process of socialisation, the more details of those concrete rituals become literally invisible to us. This is an unavoidable effect of the production of knowledge: in the same proportion in that knowledge is produced, loss of information occurs. Gain and loss go hand in hand.

In a way, then, proposes Strathern, one can consider that, regardless of the scale at which we place ourselves the end amount of information remains the same. The particularities of knowledge may be differently distributed, but the quantity of information is constant: what we gain here, we lose there, no matter what direction we follow. Complexity does not depend on the perspective. The amount of information of complexity increases neither when we go 'down' into studying particular cases in depth; nor when we go 'up' apparently widening our perspective to encompass more cases into a generalisation. She rejects commonly accepted ideas, such as that the more you focus, the more detail you have; or that the more complex the question, the more complex the answer; or that, what looked at from the distance may appear as simple, may be then revealed as complex when approaching it: "Magnitude provides a simple example. If one thing observed close to appears as perplexing as many things observed from afar, the perplexity itself remains" (1991, p. xv).

Since we find these arguments convincing, this chapter will approach some concerns which have traditionally been regarded as institutional, but without leaving the IBF: the reproduction of science and its relation to other domains or institutions. Let us tackle the first. As Mary Douglas put it, "(a) convention is institutionalised when, in reply to the question, "Why do you do it like this?" although the first answer may be framed in terms of mutual convenience, in response to further questioning the final answer refers to the way the planets are fixed in the sky or the way that plants or humans or animals naturally behave" (1986, p. 46-7). In other words, an institution is so naturalised, that we imagine it as having been there for ever, just like the stars and animals, accompanying us all along. Indeed, in our quest for mythological origins, we celebrate a double birth among the Greeks[10], allegedly, for some, the cradle of our civilisation and of our scientific look upon the world...

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[10] Scholars aware of dangers of essentialisation will prefer to celebrate anniversary around the XVI-XVII centuries, and will talk of 'scientific revolutions'... As Findlen (1998, p. 243) says, this notion "seems to have enjoyed more lives than the Cheshire cat and to have remained as enigmatic". She offers several references for an introduction to the different histories of this concept: Cohen (1994), Cunningham & Williams (1993); Lindberg & Westman (1990); Porter (1986); Shapin (1996). Within

It is not only that we imagine that science accompanies us –society and science progressing hand in hand; actually, science does not experience progress: science *is* progress itself, an intrinsic, inextricable factor of Western progress. Progress in a double sense, quantitatively –scientific data accumulate- and qualitatively –scientific knowledge increases in complexity. If science is the motor making society progress, progress is an inner motor in science, responding only to scientific reasons. In any case, the believed continuity of science poses an important question: if science seems intrinsic to our civilisation, how does science advance through time, from generation to generation?

Like any other institution, science reproduces itself by producing scientists which will (re)produce science in turn. And here is where the duality with which we started this introduction gains visibility: without scientists there would be no science, but without science there would be no scientists. Mead portrayed this image sharply; as he wrote, without institutions we could not talk of "mature persons":

> "In any case, without social institutions of some sort, without the organized social attitudes and activities by which social institutions are constituted, there could be no fully mature individual selves or personalities at all; for the individuals involved in the general social life-process of which social institutions are organized manifestations can develop and possess fully mature

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STS, as well as within history, the so-called scientific revolution (which took place in Early Modern Europe) has been often identified as cause for the emergence of 'modernity' (as if, as Daston (1998) complains, the Early Modern already had in embryonic form what later on would be unfolded). The revolutionary ideas brought about by science are supposed to have contributed importantly to 'modern' secularisation, commodity capitalism, the scientific experiment. For a challenge to this privileged role, see Daston (1998), who claims that "(t)he new natural order of the late seventeenth century was not forged out of the specific achievements of the Scientific Revolution, although it was forged with the consent and collaboration of naturalists" (p. 169). Its emergence would have more to do with theology and jurisprudence than with natural philosophy, natural history, and the mixed mathematical disciplines. In an other order of things, Latour has contributed to disputes about the emergence of modern science with accounts that challenge both materialist and culturalist histories, emphasising the role of new visualisation techniques (such as Italian linear perspective, the Dutch 'distance point' method, the invention of the print, the illustrated book) reconstituting several domains simultaneously (art, science, economy...): "The rationalization that took place during the so-called 'scientific revolution' is not of the mind, of the eye, of philosophy, but of the *sight*" (Latour, 1990, p. 27). See also Whitehead (1967). For a very interesting article that challenges assumed links between science, northern European countries and capitalism, see Álvarez-Uría (2001).

selves or personalities only in so far as each one of them reflects or prehends in his individual experience these organized social attitudes and activities which social institutions embody or represent" (Mead, 1934, p. 262).

If science continues is because it flows through time informing past, present and future generations of scientists.

Now, then, the important question is: how does a person participate of this domain, becoming a scientist? How does science enter a person to turn it into a scientist? We imagine Nature solving the problem of reproduction through 'maturing' or 'ripening'; society through 'socialisation'. In science, IBF members also imagine a process through which the naive newcomer becomes an expert scientist, what they describe as 'learning', a process whose conceptualisation does not lack similitude with the cousin processes of maturation and socialisation - not by chance has time and progression such an important role in their conceptualisation of the scientific career: to grow from a naive newcomer to an expert member entails an evolution. Thus, the next section will present an account of how IBF members explain the process of becoming a scientist. First we will show in some detail how an experimental scientist comes into being; afterwards we will present a peculiar deviation from this description: differences between experimentalists and bioinformatics are startling enough to hint at a different constitution.

This account will make it clear that we should not forget links between institution, discipline and the constitution of individuals –links already hinted at in the description provided by the last chapter. Practices located in a laboratory can be partly explained by elements of traditional learning (for instance, what Barbara Rogoff (1990) calls 'guided participation' in an informal context), which make the lab resemble a workshop, where teaching and creation take place parallel to how a craftsman would pass his or her abilities and knowledge to a pupil. But these practices can also be partially apprehended by emphasising the institutionalised disciplining process that scientists undergo, and many elements hint at it: the pyramidal structure, up-to-bottom supervision as well as subtle mechanisms of

control spread all over, the work upon the body embedded in the exercises together with the strict control and optimisation of time and movements, etc. Not surprisingly parallelisms between molecular labs and factories have not gone unnoticed[11] (see Knorr-Cetina, 1999). The institutional-disciplinary character of science probably explains both, the 'natural fit' between the university and the laboratory research, as well as the intimate connections between private companies, the army and science (Latour, 1987).

These thoughts will not be dismissed, but the reader will not find a Foucauldian analysis of the scientific institution in these pages –I warn early so as to spare disappointments. Rather, we will take another path. We will inquire into the tension, present in the whole IBF, between individualism and collectivism. Trying to preserve this tension without cancelling it, we will propose a model to understand scientists' being together. For this reconceptualisation we will first need to redescribe their developmental account into an anthropologic model of flow of substance, through which we will then consider the possibility to understand relations as extension, a notion that derives from Strathern's (1991) work and has been elaborated by Munro (1996). This notion will help us fathom a partial 'being together' which neither does resolve into integration or totality, nor dissolves into bits and pieces without connection.

The second part of the chapter will deal with different ways of conceptualising the relationship between science and other institutions. Classic institutional studies picture science and society as domains mirroring each other, but yet distinct; in relation but independent from each other. Whereas science needs society to provide it with resources, it must nevertheless fight for its proper space, guarding off meddling. Changes stemming from economics, society or politics are seen as "external influences" coming from other "domains", whose interference science must fight to

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[11] Even scientists themselves seem over concerned with 'productivity' –they talk of productive groups, productive scientists, of unproductive phases, and some of them have no problem establishing comparisons between their lab and a production plant: "like some produce yoghurts, we produce papers"!

protect its independence: each domain has its territory, so to speak. As an independent domain, science becomes more than a simple 'way of working' or 'solving problems': science becomes a belonging, a home (for our culture in general and for scientists in particular), a place one can be 'in', a place one can be 'out'. In the same way as we feel there are entities that can be placed within Nature (a tree, a tiger, a body), and others within Society (a policeman, a stereotype, a political demonstration), we can also place entities within Science: we need to engage in so many discussions about who has the right to be included in science, or has to be condemned to exclusion; either you are a scientist, or you are not[12].

As expected, in the IBF a narrative circulates which constructs 'politics', 'economy', 'bureaucracy' or 'the media' as different domains with which science must come in relation to obtain 'feeding resources'; in this sense, this way of making sense of their own practices to themselves assumes limits between institutions, that occasionally overlap. To this account one could oppose that elaborated by scholars within STS, who perceive a much more intimate link between these different practices, denying those limits that in certain occasions scientists are so prone to defend (e.g., Knorr-Cetina, 1981, 1982; Latour, 1987). But instead of creating such a clear cut between these two positions, we will try to show that scientists' perception of the relation between science and politics is quite ambiguous. Whereas the circulation of the narrative mentioned above purifies science and creates bounded spaces, their practices recognise in plenty of occasions the indivisibility of knowledge and nurture. Thus, we will put our model to the test to conceptualise the way in which they can imagine intimacies which do not resolve in identity. We will also consider the symbolic consequences ensuing from both perspectives (that created by purifying moments and the one acknowledging mixture), particularly as far as the figure of the head is concerned.

Let us proceed now by presenting their account on how new members become good, mature scientists.

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[12] We will later deal with our perception that many entities actually belong to more than one domain,

2. Constituting experimental scientists

Despite inner predispositions that incline somebody towards the observation and study of nature, life and its mechanisms, members of the IBF say that a person is not born a scientist, but becomes one, and they have plenty of stories and anecdotes illustrating this genesis: from the curiosity and vocation of a lay person to the knowledge and expertise of competent members of the scientific community. Such explanations need to account for this process of training and transformation, but also for the fact that not everybody wishes to become a scientist, and that not all of those who do can eventually become one. They distinguish three factors with a strong influence in whether and how a scientist forms: vocation or 'the call of science', some kind of innate manual skill ('to have good hands'), and the structuring power of the group ('teaching') –the first two being 'internal', the third, 'external'. Of these three factors, the last one is given relevance, partly because it is the only in which groups can intervene.

2.1. Vocation

Not every single person hears "the call of science"[13], only some feel vocation. This is a feeling which many of them recall from their youth, like a seed they have inside and that develops if taken care of. When asked about what brought them to science, many members of the IBF mention having experienced a kind of natural, inner, inherent feeling of attraction to and interest in the keys of life and of nature[14]:

   "The truth is that I don't quite know where my interest in biology comes from...

   I do remember that when I was a child I used to dream of manipulating

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relation that Strathern will describe as merographic.

[13] This is indeed the sense of 'vocation' or 'Berufung': one does not choose, one is chosen or called.

[14] I present here an idealised biography. Idealised not in the sense that it is false, I have taken bits and pieces of several biographical accounts, in order to constitute an illustration of what they call a 'natural' biography.

molecules, well, at that time I wanted to manipulate living beings –plants- to extract properties from them... I must have read some paper..." (EI01-01).

Pure contemplation of nature is not satisfying enough. Our scientist-in-project feels the impulse to explore and manipulate so as to understand nature better. Our embryonic scientist used to hunt butterflies, dissect frogs and play with chemistry games and microscopes (EI11-25). Later on she starts reading magazines on science – first popularisations, then more specialised magazines (EI01-01). Stories differ, of course, but they nevertheless share this: this interest and amusement for science makes its way through, and they will slowly but surely discover their vocation: it seems the most natural thing to do, if one wants to deepen the understanding of nature, to choose the path of science:

> "I think that the continuation of this interest, if you want to make a step further, involves an approach from within official science. My grandfather used to tell me a lot about animals and plants, but you can already guess how basic his level was. When I grew, I started catching and dissecting frogs and all these things. One birthday I received a game with a microscope for kids and solutions… I mean, I started showing interest since my childhood, for me biology was completely vocational" (EI11-26)[15].

And, finally, the person reaffirms her interest by choosing a degree in the field of science. Chemistry, Biology, Biochemistry, even Mathematics in some cases. The scientist that students bring inside discovers soon what her vocation is: research[16]. In order to explore this possibility, many undergraduate students join a laboratory group some hours after seminars (sometimes, instead of seminars!) to participate in some work in progress. This is how our scientist in potency arrives at the IBF and takes her first steps into science.

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[15] Notice how in this extract we also find a commonplace of scientific discourse: common sense, or the more traditional sense of peasants, is not enough to satisfy the thrust of knowledge of the scientists; science goes beyond folk knowledge.

[16] This inner and potential scientist can be glimpsed in this quote: "In my opinion, a student of science aspires to do research. If you end up in a private company or somewhere else because of money is another issue. But the initial aspiration is to do research in science" (EG05-06).

2.2. Intrinsic ability

However, vocation is not enough. One must also show a certain amount of skill. Indeed, they say that some people have skilful hands to do experimentation (golden hands); others have abstract minds to operate computers; the rest lack these abilities, and will have to abandon science. The only way to discriminate whether a person "has scientific skills in her" is to see her at work; this is why these periods of mutual test are so important both for students and for laboratory groups. Even though skill can be improved with practice, one needs a certain amount of intrinsic, inherent skill. To express it in their idiom, whereas during the first years newcomers need to *'make* hands', you also need to *'have* good hands' –indeed, some have better hands than others, and they have anecdotes to emphasise how some people possess this ability, whereas others do not:

> "It was clear he couldn't face the bench... really, he used to lose the protein –he himself would put it like this, 'the protein went for the weekend and has not come back!', ha, ha. Unbelievable things used to happen to him..." (EI11- 19).

> "As undergraduate I made some practical demonstrations, and I was very bad at it, very, very bad, ha, ha. I used to forget steps and all that stuff. I decided it was not my thing" (EG02-03).

Until now, then, we have seen how the good scientist is pushed forward by two internal sources: vocation and ability. They are an intrinsic component, a kind of seed the person already carries –vocation and the initial skill cannot be learnt or given (neither can they be inherited!!). Either you have it, or you don't. But not even this is enough to grant that a scientist comes into the world. One does not become a scientist only because one feels attraction to plants, plays with insects and frogs or reads a lot of scientific magazines. Not even because one has 'good hands'. These are just possibilities that need to be actualised, so to speak: the inner seed needs to be taken care of. This is the task of the group, that plays a key role in the scientist's training:

the newcomer undergoes a radical process of learning sustained by the scaffold of the group, a kind of socialisation through which the persona of the scientist develops.

2.3. Moulding of the group

The most important force turning a young student into a full scientist is to be found in the group: without the support offered by the group (an intense period of tutored practice, learning and supervision) no scientist would be constituted:

> "When you enter the laboratory, the truth is that you feel as if you had just left the nursery school... You realise that you know nothing, that you are... totally useless" (EG03-03).

The emphasis with which they explain their initial uselessness and incompetence points to a very radical transformation: they need to be constituted as scientists. If to be a 'persona' means being a competent member of a culture, to declare oneself as 'incompetent' amounts to expressing lack of personhood. The importance of this constitution, in particular, the crucial role of the group shaping the scientist, is recognised and emphasised by all, especially by beginners themselves. Like in socialisation, this process involves a double movement: science must 'enter apprentices' so that they can 'enter science'. That is, Ph.D. students must incorporate new knowledge, develop manual skills, and learn techniques, procedures and the handling of equipment. But not only: they must also learn how to act, see, smell, interpret graphics and results, argue, write and discuss. In short, the Ph.D. student has to acquire a new *persona*: eyes and nose of a scientist, hands of a scientist, thought of a scientist.

The borrowing of vocabulary and conceptions from the sphere of development – 'natural trajectory, development phases, maturation through time'- is a way of acknowledging the nature of the process: in the same way as society constitutes individuals, the group constitutes scientists. In a similar way in which socialisation is

the operator through which we have imagined the relationship between society and the individual in the twentieth century (Strathern, 1992[17]), they regard learning and group teaching as the operator between the individual and science. Only after undergoing this process of moulding by the group can the persona of the scientist emerge.

They need to be able to reproduce with exactitude some detailed procedures in the right sequence, attentively and embodying meticulously certain movements. Minuteness is important: as our Ph.D. students would say, you must know in which situations the reaction mixture should be shaken, in which it should be stirred[18]. They need to know how to handle particular equipment –even a simple pipette requires practice to perform skilfully the different movements it allows! The coordination of hand, eye and instrument involves a particular syntax, a real manoeuvre coordinating the body and the device. The amount of substance or water to add to a solution, the right time to do so, the right sequence of steps and movements or the intensity with which it is done may make the difference between success and failure. As Foucault has put it, "(i)n the correct use of the body, which makes possible a correct use of time, nothing must remain idle or useless: everything must be called upon to form the support of the act required" (1977, p. 152). Decomposed into different parts, different gestures and different behaviours, the body is moulded and manipulated so as to grant productivity. Everything in the lab seems to be oriented to increase the efficiency of their practices: advices on hand movements, positions, gestures and procedures; distribution of machines and bodies in the lab so as to optimise coordinated action. An economy of time and gestures. This is the exchange that the body of the beginner must indulge in: productivity for obedience, becoming a subjected, exercised, and docile body.

One of the aspects that stand out is time management. Procedures at the bench are sequenced so as to optimise time. For instance, a procedure that requires a solution to

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[17] See this work for an analysis of changes in this relation due to transformations of our concept of 'nature'.

[18] See also the report of scientist Alridge, H-10, p. 33 in Mulkay & Gilbert, 1986.

rest for several hours will be conducted in the evening, so that the solution can rest during the night, and the scientist does not need to wait the whole day for it (what they call a 'dead moment') –actually, the type of procedure I have now in mind is called 'overnight', indicating that this time organisation is not an individual preference, but a rather standardised suggestion. Meetings with heads and seniors will also be fitted in 'dead moments' –actually, the other way round: Ph.D. students will distribute their activities so that their meetings coincide with a 'death moment'. Experimental work is usually left for those moments of the day when the scientist is more 'active', leaving moments of tiredness (after lunch, for instance) to gather and photocopy bibliography. Moreover, a researcher very rarely engages in only one task at a time. They usually conduct several procedures simultaneously, so that during the 'dead moments' of one procedure they can carry out a second. Ph.D. students are often seen hurrying from one piece of equipment to another, contriving to do two tasks simultaneously; tips regarding what to combine with what pass by word of mouth from expert students to beginners; and if a researcher discovers a new way to optimise tasks and reduce the time of elaboration, she will let others know.

In short, time is linked to a "positive economy": "it poses the principle of a theoretically ever-growing use of time: exhaustion rather than use; it is a question of extracting, from time, ever more available moments and, from each moment, even more useful forces. .... to intensify the use of the slightest moment" (Foucault, 1977, p. 154). What at first look may seem a bit like a histrionic rhythm of work proves its effectiveness in the long run. By calculating "time expenditures" and adapting their behaviour so as to decrease them, they may save only some minutes at a time; however, when one sums up all these minutes at the end of the day, the gain is considerable. And this is important for them: to be fast may mean to be the first to discover something and, everything going well, to be the first in publishing this something (or, more prosaically, it may mean to finish your thesis before your grant expires). In most occasions, being fast is an advantage for the researcher rushing up and down; even when it is not, it is at least always beneficial for the whole group

(even though members of the group do not usually distinguish personal from group benefits).

For them, saving time is as much a rational decision as an embodied practice: "time penetrates the body and with it all meticulous controls of power" (ibid, p. 152). Beginners embody regulations such as the rhythm and sequence the activities during their progressive involvement in different exercises, graduated in terms of difficulty. (We know that beginners learn techniques next to more experienced members by practising -we have already seen that they are supervised and accompanied until they are able to carry out a technique on their own successfully). This progressive involvement enables progressing disciplining, through which productivity concerns are embodied. To such an extent that when they practice, they never feel they are obeying the impositions of an external authority (i.e., 'save up as much time as possible so that you produce more for the group'), but unfolding a programme, as if the action was controlled from within, one step after the other in a type of "anatomo-chronological schema of behaviour" (Foucault, 1977, p. 152). The body crossed – constituted- by power.

Vision

Maybe the most impressive process for a witness is the disciplining of seeing. Problems revolve about *how to interpret* what one sees, but especially, about what it is that one sees[19] (for instance, as in determining whether a bit of the image observed

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[19] Some authors have found it useful to classify the type of visual evidence (biologists) deal with, according to levels of elaboration. Thus, Amann & Knorr-Cetina (1990, p. 88) distinguish between:
a)   techniques of manual and instrumental enhancement, that is, operations to make unelaborated data visible –for instance, taking a picture of a gel or looking naked eye whether a reaction in a test tube is taking place;
b)   proper 'data', that is, the elaboration and interpretation of visual evidence into 'informative proofs'. An example would be the interpretational practices gathered around a DNA gel
c)   'evidence', which the data already interpreted included in scientific papers or presentations. Evidence is accompanied by (unclosed) interpretations, and have already suffered transformations, selections, simplifications.
According to these authors, it is the second type of visual evidence that creates more interpretational problems. Whereas the first and third types appear as "unproblematically readable", the 'data' of the

is figure or background). When Ph.D. students face the bench for the first time, they do not know how to see. Where more experimented members see proteins or genes, they only see amorphous gels, incomprehensible graphics or undistinguishable stains. It is not only that they need to learn how to interpret what the eyes register: the eyes themselves see differently. New students entering a laboratory need training to be able to perceive any kind of evidence as evidence. To inspect the course of a chemical reaction in a test tube may not be an activity which arouses a lot of interpretation among scientists, but it is nevertheless an action which requires to be able to distinguish colours and appearances of reactions, and a certain knowledge of what reactions are expected or not. To have a look at a gel in order to decide whether the gel came out properly and can be further interpreted or whether it has to be dismissed because the bands did not form properly will not arise much discussion, but it still demands that the scientist making this decision is able to recognise and distinguish bands in a gel, as well as the proper characteristics a gel should display. As Knorr-Cetina puts it, "(i)nspection requires visual know-how" (1999, p. 95).

I became aware of this not only through observation of the students' first days in the lab, but also through changes in my capacity of seeing and understanding what I saw. Whilst manual procedures are somehow intuitively understandable, even for somebody who does not know much biology (for instance, to understand that they are letting fluid run through the column to filter a protein), the capacity to see is not immediate. I remember staring at protein gels and seeing only a huge blue blot, although other Ph.D. students insisted that in this blue gel there were microbands, that is, accumulations of proteins of different sizes. It was not after some weeks, when I found myself agreeing with a Ph.D. student on how nice her gel results looked like, that I realised I was able to distinguish microbands!

The more sophisticated and elaborated results are, the more room for ambiguity their interpretation leaves. Doubts are never settled simply 'perceptually' or 'cognitively', if by this we mean that a single person 'distinguishes' the object or 'decides' or

second kind become gathering points, around which scientists discuss (offer and challenge)

'deduces' interpretations. As it has frequently been claimed, moments of 'visibility' are performed collectively and through talk. When a Ph.D. student approaches a gel to see if the procedure was successful and the result can be interpreted, this move is automatically perceived by others. Expectancy is aroused, and nobody wants to miss the result: either the very Ph.D. will say aloud whether she is satisfied with the result, or any other person will ask her "so, what did you get?". Any hint that somebody interprets a result works as a kind of signal for other scientists: they tend to leave what they are momentarily doing, and approach the place of interpretation, in order to intervene and contribute with comments, looks, supporting or challenging interpretations, etc. This is a moment of collective performance of 'seeing'.

Interpretations are immersed in language. But as Amann and Knorr-Cetina emphasise, they do not deploy only a semiotic character, but also a conversational one[20] (Amann & Knorr-Cetina, 1990). The interactive, dialogical structure of talk is "the machinery of seeing" (ibid., p. 92). Moreover, another type of action intervenes: pointing. Discussions about a piece of visual evidence always refer to the steps and techniques just performed. Put in other words, conversation concerns the object, is linked continuously to the image or visual evidence in point, as well as to the procedural steps the researcher followed to obtain the data (Amann & Knorr-Cetina, 1988, 1989; Knorr-Cetina, 1999; Lynch, 1988). It also tries to link evidence with the past, a past that is constructed as 'evident' and 'stable', so that legitimacy and credibility accrue to present results[21] (Woolgar, 1990). Thus, conversations tie procedures, theoretical knowledge, evidence, interpretations, past-present-future and researchers in an amalgam. Seeing and interpreting representations is, then, a local situated practice (Suchman, 1990).

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interpretations.

[20] For a more detailed approach to conversational characteristics and devices when producing visual evidence, see Amann & Knorr-Cetina (1990); Woolgar (1990).

[21] "The production of charts resulting from previous experimental runs appeals to the possibility that current states are compatible with a collective of established results. Repeatedly, then, attempts to render the present state sensible draw upon representations of the past. Documents are introduced as manifestations of the established past and juxtaposed with the present phenomenon so as to give it meaning" (Woolgar, 1990, p. 150).

This 'training' in seeing is part of the process of becoming an expert researcher, a process which students endure from the very moment they enter the laboratory, and in different shapes. (And as we know, it takes time, this is why students are co-opted before they become Ph.D. students, so that when they start "productive research" they are already able to see and perceive as a good scientist). Again, seeing is shaped through participation in experiments next to other colleagues which guide them into perception; and also through the guide of handbooks and protocols, which establish what steps one needs to take up in one experiment, as well as how results should look. Thus, as Law & Lynch (1990) claim, the capacity to see requires "an apprenticeship in a social organisation of 'reading' and 'writing' (p. 269).

This change in the capacity to see –from seeing nothing, to learning to recognise and then, to interpret- is precisely one of the phenomena accompanying and constituting belonging. The very process of constitution of somebody as a member of a community involves, among other changes, the constitution of this somebody's look and way of seeing. If a non-member cannot see and an old member enjoys acute vision, the process of belonging must then involve a disciplining of the sight. And thus, those moments in which one can see and feel like a member should are moments in which belonging is performed.

On other senses

Visualisation has been one of the most studied processes within laboratories, not only because it calls easily the attention of non-members, but also because of the centrality in the practices of scientists. Making phenomena visible is a process of construction of facts that moreover, renders them operable. Moreover, vision is key given the some of the possibilities it enables: for instance, through vision one can present absent things (Latour, 1990, p. 27)[22], thanks to the two-way connection through space and time that inscriptions make possible. But the importance of visualisation should

not confuse us into thinking that science deals only with vision; after all, science is an incarnated, embodied practice, and to make something visible does not only involve vision, but complex practices, as the process of purification of proteins and elaboration of gels testifies (the reader will find a description of these procedures in chapter 5).

Not much is said of other senses in the lab, one does not find a systematic disciplining of taste or smell. Neither is it normal to find recommendations in handbooks of how proteins should taste or smell, and these senses do not appear to be of great help. Nevertheless, this does not mean that they do not have a role in scientific practice. I could once witness how Ph.D. students were taught by a more experienced Ph.D. student how to carry out fermentation. In a fermentation, bacteria in a dissolution are kept under ideal conditions in a huge recipient and fed for several hours, to make their multiplication possible. These bacteria have been previously introduced a DNA plasmid carrying the genes of PCI, for instance, so that when bacteria reproduce and express their genes to create proteins, the protein of our interest is also synthesised.

This procedure is not regarded as easy, not because the steps required are technically complicated or difficult to perform, but rather, because it demands certain skills which are badly formalised or describable. The key point is that the culture in which bacteria are grown must be kept under stable conditions. For instance, the amount of nutrients must remain above certain limits –or else bacteria "hunger"- the problem being that there is no standard procedure to carry out fermentation, nor any pattern fixing the amount and frequency of the feeding. Worse still, when the culture gives signals of not growing as fast as it should, researchers do not directly know which of the several nutrients involved is missing. The only solution is to try adding one, and observe the reaction of the bacteria. If bacteria react after the addition, this nutrient was key –they were in need of it. If bacteria do not react, they should try adding another one. Tests go on until conditions are optimised.

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[22] See Latour's article for a proposal of how cognition, visualisation and inscription go hand in hand,

There are some factors that are important to carry out a successful fermentation. Researchers talk quite openly about 'intuition': some scientists have a special ability ('a sixth sense') to feel what bacteria may need. Another help is the smell: researchers with a lot of experience are able to recognise when bacteria are grown enough through the smell bacteria expel. Since the air of the container needs to be renovated and kept fresh, there is a tube that allows air to leave the recipient, and this air can be smelt. With experience and a good nose, then, researchers find help in the sense of smell, even though this is a skill difficult to transmit from Ph.D. student to Ph.D. student –some could differentiate smells, others could not. They let me smell twice; from the two different smells, they claimed, only the second was the proper one – a proper E.coli- smell. They all had a laugh at my expense: while I was smelling they mockingly wondered how I would manage to write down 'smells' in my notebook – particularly, fermentation smells. Since such description would be neither easy nor particularly nice, I hope the reader excuses my not even trying. This difficulty in apprehending and describing taste and smells in a precise way (so precise that the reader can then identify and distinguish them) might have something to do with their absence in handbooks and other training materials.

Emotions

While new Ph.D. students learn technical procedures, how to deal with new machines, and how to interpret results, they also learn the organisation of the laboratory. They need to know how to relate to each other, what the hierarchy is and what kind of relations it involves, what they can expect from whom, to whom they need to ask or account, and, to put it in Mertonian terms, the moral ethos linking members together. Ph.D. students learn all this while participating in every day practices –when being taught how to act, one is also taught how to be. New members are constituted as one of them, made a piece of the whole gear, so to speak: they learn

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resulting in a 'thinking with eyes and hands'.

what their position is and involves, whom they needs to address for certain things, what they can expect from their colleagues and what their colleagues expect from them.

Emotions –what to feel, what type of sentiments should be embedded in scientific practice- are also learnt during this kind of socialisation by the group. This includes feelings towards colleagues, such as the need to collaborate and help instead of engaging in wild competition, and feelings of belonging to a particular group, as it can be nicely observed in quarrels between groups. For instance, in the IBF there are sometimes tension between groups which share instruments and equipment; either one group feels the other group is profiting from their resources more than they can use the other's, or one group is not happy with the way the other takes care and cleans equipment. Whereas at the very beginning newcomers in one of these groups discover the existence of such unimportant skirmishes from an external perspective (such as mine, for instance), little by little they identify with the position of their own group: the beginner, who may have spent only some months in the IBF, will feel more entitled to property claims[23] on some equipment than the member of another group who has spent some years in the IBF, just because of the perceived categorisation – effect which is hardly surprising for social psychology, on the other hand.

Feelings and sentiments are also oriented towards work itself. Thus, they learn the amount of dedication that is expected from them, not only in terms of number of hours of work, but also in terms of passion felt for the work –which are related issues, since the number of hours one spends in the lab is taken as an index of the amount of passion and devotion one feels for one's work (and this not only in the IBF, for good or for bad...). One should not trivialise this aspect, for it has strong subjection effects within the laboratory; as a matter of fact, most of the members of the IBF spend more hours than a labour day would legally allow. Passion, love and devotion are usual images related to work. Half jokingly, half seriously, one of the seniors told me once

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[23] As Edwards & Strathern (2000, p. 151) put it "A claim on the place entails a claim on those things that belong to the place".

that a scientific career resembles monastic life: total dedication and absorption, locked in the cell, without (time for) personal relations.

Their work requires sacrifice and passion, and never only pragmatism. Shortly after we started our observations in the IBF, a visiting Ph.D. student from Morocco presented her thesis. This student had run out of money at the very end of her several-year stay, and all she wanted was to finish the research and go back to her land as soon as possible, which forced her to defend her thesis precipitately, even though it would have needed some more work. She knew it, but she accepted the risk, since the situation in Spain had become quite unsustainable for her. As expected, in her viva she was strongly criticised; but what surprised many other Ph.D. students was that she did not even try to defend herself –mentally, she was already far away, so to speak. The Ph.D. student who told me this story was floored: "If in my viva they told me what the evaluator told her, I would start crying in the middle of the room, but she did not seem to care. I think the problem was that she did not love her thesis".

Work at the bench with colleagues also teaches members what kind of emotion new discoveries and new experiments should elicit, and in what moments emotion should be shown or not. I once entered one laboratory just to find one of its members working nervously at the bench. He was always very solicitous and seemed to enjoy teaching me about techniques. That day, however, when I required permission to watch, as usual, he acceded on the condition that I remained silent, for he needed to be very concentrated. This was a very important experiment for him, he told me: results should corroborate whether he was going in the right direction or not, and a bad result would ruin all the work he had done so far. He was clearly fidgety: while doing the experiment, but especially, when reading and calculating the results. I have no doubt that his agitation was sincere: he had a lot at stake in this experiment. While sincere, his performance was reproducing the typical behaviour one would expect in 'crucial experiments'.

3. Constituting scientists... in biocomputing

The description offered so far of how scientists are produced is quite biased, in the sense that it narrates how a member of an experimental laboratory comes into being. But after spending some months in the IBF, we started having some suspicions that 'to be a good scientist' could be differently defined in a different context, such as a biocomputing laboratory. This point was brought home by members of the IBF themselves. Since the day of our first arrival at the IBF, we had continuously heard that computing people were "weird", "arrogant", "completely crazy", and that "they have rare reactions", "they follow their own rhythm and jus t do their thing", "nobody can understand them, they live in their own world". Moreover, they were said to have a very hard humour and be completely antisocial. And, on the top of everything, they had a fixation with Star Wars, bursting out shouting from time to time "May the Force be with you!" To make it worse, members of biocomputing would happily agree to all these impressions, trying to reinforce rather than weaken this wild characterisation. We will now present the way biocomputers think of themselves – how they are thought to develop, work and relate to colleagues. A short exposition will be enough to throw differences with experimental laboratories into relief.

Undergraduate students who decide to take up a scientific career usually enter experimental groups, partly because they are more numerous, partly because experimentation is more known[24]. Some, however, follow a different trajectory: they enter biocomputing laboratories. The reasons they give for this 'choice' vary: some students (who would not have thought of this option) were directly offered the possibility to join the group and accepted, others knew from the first moment this is what they wanted to do. In both cases they live this option as 'uncommon': that experimentation is the 'normal' option is shown by the fact that, whereas Ph.D. students working at the bench never needed to justify why they had not chosen biocomputing, biocomputers quite often mentioned being 'quite bad at experimental

work' as a justification for their choice, as if having to give an explanation for their unusual trajectory. However, people in the IBF do not hold the view that there are intrinsic qualities deciding whether one becomes a bioinformatics scientist –quite often is a matter of chance. Members of the biocomputing laboratory accept that some people working in experimental could have been good bioinformatics if they had shown interest:

> "C: He works in that experimental group because he's paid by this group. Perhaps if he were paid by our group he would now work with computers. ... Now I work with computers. But if you give me a pipette, I'll handle a pipette.
> A: If you take somebody from an experimental laboratory and have this person one year learning here with us, this person will learn exactly as we have done.
> C: This is exactly what I'm saying. It is not a matter of...
> A: It is a question of whether you are any good at it or not" (EG02-24).

This does not mean, however, than any person has the required skills to work in biocomputing. In the same way in which not everybody is good at experimenting, not everybody is good at biocomputing. If in the first type of lab you need to "have good hands", in the second abilities are equally important, but less concrete; since it cannot be known in advance whether the person is good at it or not, she has to start the training in biocomputing to discover if she can cope with it –to cope with the difficulty of the task plus the conditions of the environment. As non-biocomputing put it, "You need to have the proper brain to spend eight hours a day in that mushroom cave!" (EI05-20).

Given that one discovers one's own ability by giving it a try, no special requirement is needed to be accepted as a member (under trial) in biocomputing. Indeed, none of the members of this laboratory team knew about computers or computing language before they entered. Neither does it matter whether they have studied mathematics, chemistry, biochemistry or biology, for none of these disciplines prepares them for

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[24] Biocomputing is a very young discipline; the IBF was the first group to introduce these techniques in

what is to come. But, as they say, "the most fundamental thing is that you enjoy the job" (EG02-25). If one likes it, one will be willing to invest as much time and efforts as needed - then one can learn how to manage these complicated machines and their programs. One must invest a lot of effort because one needs to learn everything anew; it is not only that they have to learn practically what they have previously learnt only theoretically, as it is the case of experimental people. They have to learn a new way to conceptualise proteins, a new computing language, new programs, a new logic... everything. The problem is -or so they say- that these things cannot be taught –you don't actually learn by being told, you need to explore on your own (EG02-61) – reading handbooks, making mistakes, and practicing until finding your way through and learning the language and logic of computing:

> "Experimentalists say "somebody has taught me this technique". Here this does not exist: you teach yourself the technique. (...) People can't even recommend you the ideal book for you to learn" (EG02-25).

Work is an individual task in most of the cases –very rarely does one find teamwork or the bustling exchange typical of bench work. Of course, they may ask for help or pose some punctual questions to colleagues, and they do engage in some collaborations -but still, tasks are individually accomplished. Partly, they say, because work demands high doses of concentration -"the computer absorbs you a lot" (EG02-60)- and questions disturb. One cannot solve computational puzzles, conceive new instructions or detect mistakes in a programme while answering questions at the same time:

> "This is not like in experimental, where they are talking and going up and down the whole day. Here I have to think and be concentrated, and if you talk, you make me disconnect from my work and my RAM memory empties! It takes me ten minutes to decipher what I have on my screen, and if a newcomer asks me something, then she switches me off!" (EG02-60).

This is why they try to keep the number of questions and interruptions quite low, with the exception of questions from old members: "we allow questions among colleagues,

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Spain, and the senior of the group was the first Spanish Ph.D. student working in such topics.

because they are short, concrete questions, and you know, you scratch my back and I'll scratch yours" (EG02-60). But they offer active resistance against two sources of distraction: 1) first, questions of members of other laboratories who need to use the computers and do not know how to -for if they started to help them, they could spend most of their time on it, and, as they say, this is not their task, they "are not paid to do this" (EG02-35). Not surprisingly, their reluctance to help experimentalists –who are used to the cooperative work of their labs-, has made them unfriendly or 'asocial' to the eyes of the others. 2) Newcomers. In sheer contrast to the collective socialisation that the newcomer goes through in experimental laboratories, in biocomputing the newcomer is considered a disruption that makes them lose time and has to be kept at bay. Thus, it is not uncommon that the newcomer, after posing a question to those who are supposed to help her, receives half-offended as only answer a bare 'look it up in the handbook'. Old members are strict: one has to learn alone. Afterwards, when the person shares the language, the vision, and starts having a slight idea of what this is all about, they can start asking and talking. Then they will receive help, but not before.

The degree to which one learns absolutely alone may vary depending on the team. One computing Ph.D. student, who has been working abroad with another team, reported having been taught more directly and have had more support from the foreign team than from his group in Barcelona. But this is so, they say, because all members of this foreign group are working with the same technique: they can help each other because they all encounter similar problems. In the IBF, however, since each Ph.D. student works with a different technique, communication and mutual help are not easy. Only two people, the senior of the group and a Ph.D. student can discuss about shared problems, because, as they put it, "they share the water" (EG02-27) -which in a grove translation means that they share the technique and a similar way of conceptualising their object. Whether two people work with the same technique or with different ones conditions strongly whether they can talk and work together or not.

One more reason why they resist helping newcomers has to do with productivity. The radical process of learning they undergo means that the first one or two years they are

not able to contribute with any work: they simply learn. It is not until later that they start being able to carry out research, participate in a project and publish papers; this is why Ph.D. students are enrolled a couple of years before the end of the degree, so that when they receive their grant, they can start working in a productive way. If once they are productive they had to invest time to teach others, the levels of productivity of the whole laboratory would drastically decrease, for neither the old member nor the newcomer would be writing papers. Contrary to what happens in experimental, the taught person cannot produce and therefore, teaching does not yield results (two people are blocked by one problem, and no paper is published), whereas if the newcomer learns alone, the old member is still free to work and publish). Whilst experimental seniors justify their intense help in terms of collective productivity, such investments would not be profitable in this biocomputing laboratory: "isolate the period of training and the period of practice; do not mix the instruction of recruits and veterans" (Foucault, 1977, p. 157).

In short, 'becoming a competent scientist' in computing laboratory is much more a solitary task than it is in experimental, where every single action is embedded in a social scaffolding of help and supervision. These Ph.D. students have been inculcated that 'it is good to manage alone', and they even reject the possibility of receiving help –"here you won't find private lessons" (EG02-26,62), "we are self-didactic" (EG02-25). And they are proud to have made it alone (feeling which brings them to undervalue slightly the efforts of those who, belonging to other traditions, are directly taught). This way of conceiving the process of 'becoming a competent member' has created a dynamic in the laboratory not far away from a 'disciplinary regime'. They conceptualise this first phase as a selection, newcomers are under test, and have to show if they have the resources to make it: they have to prove whether they are any good as bioinformatics. If you endure the first year, then you will make it: you will become a respected, competent member of the computing team. If you fail –if you find it too difficult, too boring, too incomprehensible, and decide to leave- then this is the evidence that you are not made for it[25].

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[25] A person who does not pass this test is still welcomed to have a try in other experimental laboratories. Since biocomputing and experimental are two different worlds, incapacity to deal with one does not involve incapacity for the other.

From the beginner's point of view, the group is rather hostile: not only they don't help, but the more one asks for help, the more aggressive old members may react. Moreover, newcomers will soon discover that they are made the target for jokes, the object of ridicule. And they will continuously be reminded of the hierarchy in the group. Like army veterans checking the courage of 'recruits', old members will make sure they create the right 'hard atmosphere' for newcomers to realise that they are, after all, newcomers[26]:

> "A: yeah, our group is a bit hierarchical.
>
> B: very much hierarchical –but it is logical and natural! Newcomers depend completely.
>
> A: When you enter, you must realise you know nothing.
>
> B: Dependence, the word is dependence!
>
> A: A newcomer cannot enter the group and think he is a crack. You must enter with humbleness" (EG02-59).

Practices recreate a strong hierarchy, which they perceive to be necessary for the good functioning of the group. If the hierarchy is not perceived or respected, problems begin:

> C: some years ago it [the division old members-newcomers] was much more spectacular than now. We are very accessible for new members, absolutely accessible.
>
> A: well, yeah. Well... you mean, in comparison?
>
> B: yes! When I was a newcomer, I never got to talk to some of the old members!
>
> C: I think the problem with him [a newcomer who abandoned] was that he found us too accessible and thought he was at our level.
>
> B: we should have made it clearer from the beginning..." (EG02-61).

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[26] I had the privilege to witness the attempt of a newcomer to belong to the group. He failed –and it is difficult to disentangle whether he failed because of lack of skills or because of the group pressure.

To soften this picture, however, we should also mention that, once you are welcome in the group as "somebody who has made it", then, relations become much less hierarchical. Actually, among veterans, it is difficult to distinguish the senior from old Ph.D. students, not only because they all work in the same space, but also because the type of interactions do not mark status.

4. Tension between the collective and the individual

If we wanted to summarise the plot of their stories of how scientists learn, we could put it like this: after the preparation as undergraduate, a scientist comes into being the moment the person joins a laboratory group; although learning must continue throughout their lives, there is a ritual 'coming of age' in the viva of the thesis; experience increases, until the scientist feels mature enough to become –naturally, but not without effort- an expert head of a group, leading it until eventually retirement comes. This is a trajectory, then, that can be described with a developmental narrative of linear progress: from less to more, unfolding new possibilities through time; the person goes from stages of simplicity to stages with increasing complexity, from apprenticeship to expertise –more experience, more ability, more curriculum, more capacity for strategic thinking.

The anchorage at the university has transformed this linear, quantitative progression into a development through stages –as if scientists went through phases of their life span. Thus, the four figures presented in the previous chapter (Ph.D. students, postdocs, seniors and heads) are not only identified as hierarchical positions in the pyramid but they also describe a trajectory, the scientific career. This trajectory is conceived as *natural* –they refer to it as "the natural path of a scientist" (EI10-4,14; EI11-11). The parallelism with 'human development' is more than casual[27]: in both

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[27] There is a certain irony in the fact that the development of an individual works as a model of the development of a scientist: whilst people like Piaget have seen the scientist as the model of the developing child (i.e., the child as a lonely scientist exploring the world and learning through manipulation of the environment, discovering cause-effect relationships and the laws of the world), in

cases, development is seen as a linear process, which follows the irreversible arrow of time. If the adult person can never be a child again[28], the head of a group can never go back to the lack of knowledge and 'naiveté' of the student. Like the definition of a living being we used to learn at school, of scientists we can say that they are born, they grow, they reproduce and they die.

This developmental narrative also impregnates accounts on those changes suffered by groups. A group is conceived (i.e., begotten) by one mature scientist who manages to find space, material, resources and at least one Ph.D. student. From this moment onwards, if circumstances are in favour, the group experiences growth through time, welcoming new members, transforming from a small, simple, undifferentiated entity into a bigger, more complex one. Proper growth will be seen as a signal of a good and healthy development and, all going well, the initially two-member group may come to shelter up to twenty or more members. Once the group reaches its optimal size, 'growth' will not translate into further augmentation of membership, but into an increase of the number and quality of the papers and research. This relationship between group, time, growth and experience is reflected in their distinctions between 'young groups' - groups which begin- and 'old groups' - mature groups with 'tradition' and 'history'. The age or state of development of a group influences the number and type of collaborations it may establish, the amount of resources and equipment it possesses, and its strategies to attract and keep new members.

This developmental narrative of linear progress is often importantly disrupted, as we know, by lack of resources. For instance, what they call a scientist's 'natural trajectory' is interrupted by scarcity of budget and jobs at university, and the same happens with the development of the group: a bad anchorage of the latter in this institution will unavoidably provoke oscillations in the rhythm of its productivity and growth, impeding a linear progression and leading to an irregular cycle with 'ups and

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our account the scientist mirrors now the Western individual and its development. We will deal with a possible understanding of this mirroring later on in this chapter.

[28] The alleged infantilisation that our culture attributes to the last stages of one's life remains a comparison: old people become *like* children. Nobody really thinks that old people lose years or become younger.

downs'. Nevertheless, and despite disruptions, it is still interesting to notice that 'normal development' reveals parallelisms between individual, group and collective progress: the same linear time that propels science into the future also unfolds scientists' genetic process. The arrow of time is at work.

This developmental narrative is intimately linked to institutions and its disciplinary procedures. As Foucault showed in Discipline and Punish (1977, p. 167), discipline creates individuals[29], together with ideas of progress, linear time and genesis[30]. Indeed, scientists come into being as competent members with particular capacities (a way of acting, reacting, feeling, thinking...). The reader may retort that Ph.D. students were already individuals before undergoing training –there must be a previous locus experimenting 'vocation', having 'inner skills', and deciding to devote their life to science. Granted, but through 'scientific socialisation' the scientist emerges as a new *persona* which anchors in the (biological) individual. This is why somebody like Merton can perceive the scientist as displaying several roles[31] (Merton & Zuckerman, 1972).

One could insist that what appears is just a role, a professional role, if you want, one of the many that an individual can perform. But there is something disappointing in this claim, surely scientists think of it as more than simply a professional role! If from the perspective of the individual, 'profession' (being a scientist) is a role, from the

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[29] Discipline creates a particular kind of individuality: cellular, due to the game of spatial distribution achieved by tables; organic, due to the ciphering of activities prescribed by manoeuvres and movements; genetic due to the accumulation of a developmental time that the practice of exercises creates; and combinatory, constituted by the composition of forces that tactics dispose.

[30] Disciplining procedures –with the special contribution of the exercise- help construct a lineal time that unfolds towards a terminal point: in short, a developmental time. Microphysics of power to give birth to "the genesis of individuals", macrophysics of power (economical and administrative techniques of control) to enable "the progress of societies": "these two great 'discoveries' of the eighteenth century –the progress of societies and the geneses of individuals- were perhaps correlative with the new techniques of power, and more specifically, with a new way of administering time and making it useful, by segmentation, seriation, synthesis and totalisation (Foucault, 1977, p. 160). On the other hand, on trying to capture the mechanisms of the body, disciplinary technologies give birth to an organic body. "This new object is the natural body, the bearer of forces and the seat of duration; it is the body susceptible to specified operations, which have their order, their stages, their internal conditions, their constituent elements" (Foucault, 1977, p. 155). A body that unfolds according to internal dynamics goes through particular phases –a body that also develops.

perspective of the profession 'sociality' (being a member of society) becomes a role. We are more used to making the first perspective explicit: one is a baker, a psychologist, a taxi-driver, a scientist, but never *simply* that, since we conceive ourselves as more than a 'person with a profession'. However, the second perspective is possible -and now we know enough about vocation and the tension between career and the personal among members of the IBF so as to give full meaning to this change of perspective: being a mother or a husband or a friend can be seen as mere supplements[32] (even interferences) of professional identity.

In any case, given the rooting of the scientific persona in the individual on the one hand and the process of disciplining it suffers on the other, it should not surprise us that the individuality of the scientist impregnates science. Hence, they share a belief in the notion of the original, creative individual –the scientist who after years and years of work receives the Nobel prize is nothing but the exaggeration of such an idea: we all know that science (and scientists) stands on the shoulders of many who have been there before (who are said to be giants[33]), but we have no problem praising new ideas, originality and individuality: Ph.D. students talk of 'my papers', 'my thesis', 'my experiment', 'my data'; they compare themselves in terms of skill at the bench, knowledge, quality of ideas and productivity. There are plenty of moments of calculability where the individual curriculum is performed.

This prevalence of the scientist as individual helps understand the numerous scholar proposals to apprehend science with an economical analogy. Indeed, such quasi-economical models, in all their variations, attempt an approximation to the "mechanism of integration" of the scientific communities (Knorr-Cetina, 1982b, p.

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[31] These authors mention the followings: research, teaching, administration and vigilance. They are all in turn subdivided into subroles.

[32] Taking supplementation in Derrida's sense, we could suggest that 'being a scientist' is, on the one hand, a completed state on its own –as a vocation, this may fill your life; on the other hand, it is an incomplete state that needs to be supplemented by other identities or roles. The social persona would complete the professional persona. Note the inversion: we usually think of profession as "fulfilling" us.

[33] Interestingly, the possible recognition of the collective character of science when saying that one scientific contribution is sustained in the contribution of many before is somehow cancelled once these others are presented as extraordinary genius. The collective character of science is thus reduced to a simple accumulation effect of giant individual contributions.

103) –either in the version of science as a pre-market exchange, such as in Hagstrom's (1965) or Storer's (1966) proposals, or of science as a capitalist market. In this second, more radical tendency, we find Bourdieu with his symbolic capital (1975); Latour and Woolgar (1979) and their circle of credibility; and Latour insisting that a scientists is not *like* a capitalist, but, simply, a capitalist (Latour, 1994b). Even Knorr-Cetina, who has criticised such models, does not resist making her contribution with notions of value and selection (1981, 1982). Thus, interested scientists-entrepreneurs and scientists-capitalists populate pages of STS, making capital investments to obtain surplus, competing for scarce resources, appropriating work carried out by the 'lower hierarchies' of their group, striving to create their own group of 'heads' and 'hands' so that they can work for themselves and not for others...

These models have shed light upon some mechanisms in science, (especially the second version has progressively eroded an idealistic image of science as 'art pour l'art) and, as Knorr-Cetina reminds us, have the virtue of "demonstrating similarities between science and social life in other domains" (1982, p. 106). However, their application to science does not make them invulnerable to the critiques that economical models have received in general. From all of them (see Knorr-Cetina's (1982) paper for a short review), the one relevant here is that such models are either explicitly or implicitly[34] based on "a simplistic concept of man" (p. 106):

> "to describe a system in terms of accumulation and conversion of capital requires that we assume a corresponding individual behaviour, or specify some mechanisms to explain *why* the description of the system does not hold similar implications in regard to the units (the scientists) which constitute it. Quasi-economic models of science are continually interpreted in terms of *individual* interests precisely because a plausible mechanism of this kind has not been provided. In any case, such a mechanism is hard to imagine, since notions which refer to the accumulation, investment and exchange of

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[34] Latour and Woolgar's model tries to avoid the level of the individual by referring to the macro-level of science. However, as Knorr-Cetina claims (see note 15, p. 128), since they use concepts such as 'investment' and 'credibility', they fall prey of inconsistencies, such as asking 'What motivates scientists?'

symbolic capital or credibility have as their referent the respective actions of individuals, and *do not describe systems* in the first place" (Knorr-Cetina, 1982b, p. 107).

Whilst it would be unfair to accuse these analogies of individualism (after all, they aim at mechanisms of integration that make sense of collective activity), they do make the individual much more visible than the collective dimension informing scientific practices. This is partly due to microsociological approaches that, despite problematising 'big entities' as departure points of analysis, they do not always do the same with 'small entities' such as interested individuals. And partly, I think, because in their attempts to offer less idealised visions of community economic they reduce science to a coordination of individual enterprises –as if we were obliged to choose between a harmonious, gift-offering community embedded in morality or the wild capital war of the aggressive executive. What is more, even if one does not dare to suggest that these theories are sustained upon the *homo oeconomicus*, they do project such a being as effect.

And nevertheless, the account of how an individual scientist comes into being is highly collective. If the scientist is constituted as an individual, it is nevertheless *collectively* constituted by mediation of the group: the group has given birth to this persona, and they recognise it as collective through and through. The group is also constitutive of the scientist, a presence that it is all too visible to be erased: members of the group openly explain the extent to which they owe their knowledge and expertise to the group. Hence, their protests if one implies they help each other out of personal interest or profit: they help for free because they are paying back previous help (previous investments of the group in them, if one wants to put it in those terms). Besides, they are aware that, were they to try to research alone, they would fail boisterously. In contrast to the stereotypical image of the scientist alone, closed in a lab without the company of anything other than her tools, in the IBF we did not find – in spite of Kippling- "cats going alone".

Moreover, they are taught that 'property' is always group property, and that one's work will be legitimately appropriated by the group –by Science. From the very beginning of their membership in the laboratory, they are reminded that, actually, the experiment in which they work is not only their own experiment but also the experiment of the group. That their paper is not only theirs, but also the paper of the group, signed by those implicated in its construction. That their notes are not only theirs, but also the group's notes (and, therefore, they can be consulted by everybody who needs it; "this is not your notebook of personal secrets, you know" (EG03-30), they say mockingly); that their thesis and all the products and knowledge derived from it are not theirs, but possessions of the group, that will be conveniently administered by the head, etc. Consequently, when a member leaves the laboratory for good, he or she must leave behind notebooks, data and material. That one's authorship does not automatically involve individual property is an idea they grow with. Work is of a collective nature.

By now we have found an important ambivalence: constitution of scientists and shared values are perceived as being both, individual and collective. This combination of thoughts (individual and collective levels) allows them to conceive the scientist as both, an individual with existence previous to relations *and* as a collective achieveme nt, a kind of property of the group –the person owes it all to the group. One can already guess that this duality will open the door to tensions between the individual and the collective dimensions of their practices –noticeable in circulating stories about colleagues spying in each other's notebooks without permission, of heads inspecting secretly the same notebooks; of lab mates (but this not in the IBF) competing among themselves; of people copying ideas of others without acknowledging authorship. These stories recognise that authorship and property are highly discussed issues –and they must always find a balance between their awareness of being 'individuals at work' overlaps and the fact that they often work for the group, and not always for themselves:

> "When you publish papers, this makes it easier for the group afterwards, for
> instance, when it is time to apply for grants and all that stuff. Many of your

own publications will be useful, not for you, but for your group to go on" (EG05-33/4).

Well aware of the tension between interests of the group and those of individual members, heads give the impression to be investing efforts continuously to create harmony between different aspirations. As if they knew that the union is more precarious than the struc ture may let think, they make sure that belonging is not taken for granted, and work to reinforce the implication of each member with the group in different terms depending on the situation (by offering economical help, participation in scientific events, trips or projects, for instance). Thus, relations linking the group must be worked upon continuously by all members, in this difficult balance between perceiving themselves as both a collective and individuals, looking for ways in which the development of the group coincides with the development of individual careers, avoiding situations which may frustrate one of these progressions. In other words, they all seem to be involved in the increase of mutual potency between the group and members, between the head and the group, and between the head and the rest of members.

As analysts, we could eliminate the tension by proposing some clues of intelligibility. For instance, we could suggest that this is the tension felt when individuality is constricted by the collective, a reflection of the domination of the group upon the individual, whereby the person is made a piece of a higher gear; the person is subjugated so as to exploit its productive force. Thus, becoming a head could be read as a kind of emancipation, where one can finally work for oneself (Latour, 1994b[35]). This is the path that capitalist models seem to take, and with it, they make the collective difficult to apprehend as something else than 'a market'. Or, alternatively, we could enhance the collective dimension, and describe this tension as provoked by

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[35] "Pierre is facing a new decision, to work *with* somebody, or *for* somebody: if you work for somebody, only the leader develops. If Pierre gives in, he becomes somebody else's labour force and loses even the right to say 'I'. He becomes a part of a group, the shadow of a leader, the technician of a brain that is in another body. His name melts among those of many other and never leaves anonymity" (Latour, 1994b, p. 78, our translation).

ego-outbursts of ideological individualism risking disruption of the scientific ethos uniting gentlemen.

But the path I will follow here is slightly different - if only out of curiosity to see where it takes us, for the others are rather known. I will try to articulate an account to conceptualise their 'being together', but in such a way that it keeps this tension alive, without conceding the upper hand either to the individual or the collective dimensions. Put like this, it sounds as too ambitious an enterprise, but my attempt is actually quite modest. I do not try to offer a total account of what keeps the scientific community together –be it a normative ethos, gift exchange, competition in a market or investments in cycles of credibility miming wild capitalism. This is rather a partial re-description –a model, if you want- which attaches to previous ones not to substitute them, but to extend possibilities of interpretation and understanding.

This account both approaches and takes distance from native accounts. As it will become clear, members of the IBF do not explain themselves in the terms I will in short present. And still, this 'model' lets me make sense of their explanations, perceptions, and feelings, as well as the particular distribution of symbolic consequences that their accounts have, particularly visible in the complex, ambiguous and ambivalent figure of the head. This is not purely an intellectual exercise, but it responds to the need to make sense of a particular asymmetry in the womb of the group that, to an external observer's eyes is both surprising and suggesting. I was saying that members of a scientific group know that, in a sense, while they work for themselves they work even more for the group. They do not consider this exploitation, though, since they consider that they legitimately belong to the group – that is, they are a property of the group. However, to express this idea with more appositeness, their attachment is not only to the group, but to the head: after all, members' status as such is conferred on them because of their relation to heads and their research project. Attempts to understand the type of property relation (Hann,

1998[36]) animating the group and linking its members with the boss will lead to an unfolding of our model.

Either when we describe the trajectory of a scientist from a developmental point of view (according to which the person gains experience and 'ascends' from one phase to the other) or from an organisational one (with a scientist climbing up hierarchical positions within the group and the university), in both perspectives the person is going upwards, up the pyramid, so to speak. When the person is at the top of it, and the head faces one of her biggest tasks, the creation of a group, then perception changes. The constitution of the group happens downwards, 'down the pyramid'. Actually, the pyramid and the creation of the group are immanent – what is more, they appear as immanent to the trajectory and progress of the head. The more resources and experience she gathers, the more the group develops, as if the group was an unfolding of the head, a kind of genealogic extension. The link between the head and the group is not one of identity, but neither is it one of part-whole.

The head enjoys a privileged relationship with the totality of the group. If a Ph.D. student goes to another group, the group will still be alive, and after a while, will 'acquire' a new Ph.D. student... If a post-doc leaves the group, the group will still be the same. If the senior abandons it, the group will be seriously touched, but it will remain alive. If the head disappears... but, can the head disappear? If she retires, for instance, either one of the seniors will become the head – and automatically, the group will change 'property', it will belong to somebody else; or the group will split in so many parts as seniors, in which case the group will disappear as such. In both cases, a

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[36] "It therefore seems desirable to stretch the definition of property beyond the conventional anthropological formula, which proclaims simply that property relations *are* social relations. The word 'property' is best seen as directing attention to a vast field of cultural as well as social relations, to the symbolic as well as the material contexts within which things are recognized and personal as well as collective identities made. This usage may seem abstruse and at variance with both ordinary language and academic usages. It might seem too loose and open-ended, making the study of property relations coextensive with the entire field of social anthropology. However, the main advantage of approaching property relations in this way is that it carries minimal ethnocentric baggage" (Hann, 1998, p. 5). "There is no anachronism in studying property relations in other forms of society where the economic and legal systems are very different. If we adopt a broad analytic concept of property in terms of the distribution of social entitlements, then it can be investigated anywhere in time and space. This usage will necessarily differ from specific local understandings of what constitutes property" (ibid, p. 7).

change of head implies a change of group, and so intimate is this relationship that the latter emerges as a group exactly the day the head becomes a head, and disappears as a group the day she abandons this function. There is a sense in which one can say, in a very literal sense, that the head of the group *is* the group, as if her persona could encompass the whole of it.

The head can point at the group and say 'this is my group' in a sense that it is different from the way a Ph.D. student can point to the group and say 'this is my group'. The head means 'this is my *own* group' –'the group I *own*'. Both expressions show attachment and belonging, but the property relation involved in belonging is almost inverted: the Ph.D. student signals her involvement in the group, the group is where she belongs to; the head signals a different link: the group is not exactly where she belongs, but rather the group belongs to her! The group possesses the Ph.D. student, whereas the head possesses the group. This property relationship that bonds the head and the group in a special way may extend to other members of the group. Of course, a Ph.D. student may say –as they do- 'this is my head's. But the meaning changes when it is the head who says 'this is my Ph.D. student', 'this is my post-doc', 'this is my senior'. Members of the group belong to the head in a hierarchical sense: they are part of her group.

There is a recognition that if the group is the head's is because she has created it. It is the head who gives life to the group and its members, in a radical way. The head is responsible for an act of creation, a paternity/maternity which the rest of the members of the group implicitly recognise. To talk of paternity here seems ludicrous. Before the reader starts laughing (I can imagine members of the IBF bursting out at that point), may I remind ourselves that in Germany, the thesis supervisor is still called *Doktorvater*? (literally, 'doctoral father'). Some Ph.D. students justified the inclusion as author of a senior in a paper where he had not contributed, by saying that he was "their spiritual father" (EG02-42), by which they meant the one who had brought them up intellectually, so to speak. In one interview in which I implied that a particular person was a post-doc, the interlocutor told me off, saying that this person

was 'obvio usly' a senior, for "he is already a daddy, he is old enough" (EI13-30). I will not defend the claim that creating a group is like being a father/mother, and I do certainly not think that the structure of sentiments between supervisor and Ph.D. students is one of a paternalistic nature. The point is slightly different: if the head of a group and the head of a household resemble is not because a scientific group is like a family, but because both of them are sites of reproduction.

I would like to explore, then, how reproduction could be thought –how we can make sense of the way in which they conceptualise the production of scientists, so that the latter grants the reproduction of science. The way we conceptualise scientific reproduction must account for several things. First, for a feeling of continuity and transmission -the idea that science flows from generation to generation, each generation inheriting science and passing it onto the next one, with a movement that increases science both in quantity and in complexity. It must also make sense of the linear character of the scientific biography. Furthermore, this model of reproduction should shed light upon the type of property relation the head maintains with the rest of the group and what type of relation they imagine among themselves. That is, it should help us find a possible way to fathom their 'being together', the type of sociality uniting them as a group.

5. An anthropological exercise: flow of substance

Traditionally, anthropology has solved the question of reproduction with notions such as 'substance' and 'flow of substance'. Substance has been imagined as that 'something' which allows members of a community to conceptualise their links: united by something common, passed from one generation to the next, that connects, for instance parents and children, but also past, present and future. Those people sharing substance or participating in a common flow of substance are seen as related, and the traditional notion gathering this 'being and belonging together' has been that of 'kinship'. Now, in anthropology (as well as in Euro-American culture) substance

linking kin has often been conceptualised as biological, its transactions taking place in the procreation act. Thus, first blood and now genes are imagined as flowing downwards, bonding with ties which weaken through time (the amount of substance shared between a person and her mother is more than that between the same person and her grand-grandmother, for instance).

This way of understanding kinship [37], though, has been recently criticised, since it is biting into the Euro-American distinction between nature and culture. Anthropologists have learnt from other cultures that this is not the only way to perceive flow of substance, and that links need not only be biological. Thus –just to give a couple of examples- among the Malay, next to ties of procreation, one finds networks of relatedness and solidarity constituted through the sharing of food and living space, *both* of which constitute 'shared kinship' or 'shared substance of blood' (Carsten, 2000, p. 18). The Iñupiat in Alaska, for whom notions of labour play a crucial role in the definition of kinship, recognise biological ties without attributing to them any constitutive effect on kinship. The latter is conceived as purely relational,

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[37] The topic of socialisation and reproduction has been studied in anthropology in relation to the theme of 'kinship'. Whereas kinship was at the core of the discipline for a long time (actually all the classics have contributed to this topic), from the 1970s onwards the study of kinship lost importance. Schneider, in one of his most famous works, had shown how kinship was predicated on the division between nature and culture, -substance and code, biology and law- in order to show how culture had the upper hand. He showed how, in Euro-American imaginary, procreation is strongly related to sexual intercourse, biology and transmission of substance from generation to generation, just to claim that these biological acts have no significance until they are appropriated and signified by culture. Thus, for Schneider kinship was the social construction of nature. As some analysts have observed, while his ideas were revolutionary –in the sense that they questioned biologically reductionist assumptions, and opened doors for future culturalist studies- he nevertheless granted nature an ambivalent status. Nature was, on the one hand, a cultural construction, but on the other, it was still the material on which such construction was realised. Nature was still a pre-existence being appropriated and shaped by culture. Above all, Schneider failed to question the very division between biology and culture –accepting into the analysis, without awareness, an indigenous Euro-American conception.

In a later work, Schneider extended his previous critique to the very study of kinship itself. This time, Schneider called our attention to the fact that academic kinship studies have invariably applied our Euro-American conception of kinship to all works. However, other cultures have notions of kinship which are not predicated on the division between biology and culture. This is bad news for comparison studies: when we contrast different systems of kinship, we can never be sure one is comparing 'like with like'. The conclusion he drew was drastic: he advocated for an abandonment of the notion of kinship. As a matter of fact, many followed his advice. It was not until the 1990's that kinship studies recovered vigour, but now, under different premises. Indeed, the most interesting among new studies bring to the fore and question what had previously been an implicit assumption: the separateness of biology and culture/society. Many of the new contributions try to challenge this

and it lasts as long as exchange relationships last: when reciprocity actions cease, the relationship does as well. As Bodenhorn (2000, p. 136) informs, a claim such as "he used to be my cousin" makes sense in that universe. Lambert (2000) shows how in North India, caste relationships are accompanied and even disturbed by 'a minimal degree of relatedness' emerging out of sharing water, food, and milk, as well as breast milk and blood: "it would seem that who eats and drinks with whom –a form of sharing substance- not only reflects and expresses differences in purity between persons of different social categories, but also operates as an idiom for expressing degrees of relatedness between persons; and this, in turn, is because feeding serves to mark bonds of affection" (p. 84). Thus, on the one hand, we see how perceived circulation of substance, independently of its source, has been regarded as a way to do and undo boundaries of belonging:

> "I further assume that the subjectively experienced boundaries and connections that uphold all such communities are both reinforced and blurred by the circulation of key 'substances' and 'objects' between persons and groups" (Hutchinson, 2000, p.56; see also Lambert, 2000).

Studies show also how 'substance' has a flexible degree of convertibility. Often one principle may incarnate into several expressions, which may transform into each other to assure the non-interruption of the flow. By the Nuer, for instance, 'blood', the main substance of lfe, can transform into 'food' and 'cattle' –and the other way round[38] (Hutchinson, 2000). However, 'money' does not have blood, that is, it lacks procreativity power, and it cannot be equated to 'blood', 'people' or 'cattle'. Which means that there are cultural limits to substance transformation. In any case, "(t)he conversion and transformability of types of substance demonstrate the permeability of boundaries between objects, persons, and types of relations" (Carsten, 2000, p. 24). And in case these examples sound too exotic, we may also mention the extreme

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division –i.e., by showing how, in many cultures, kinship overcomes the separation between biology and society or culture.

[38] Since both 'people' and 'cattle' have 'procreative vitality' (both of them have 'blood'), these two principles may extend or substitute each other in sacrifices and exchanges (marriages, funerals, births, purification rituals), extending reproductive vitality. In this way, the principle of blood is anchored in

transformation power that a relational substance such as 'credibility' has in cycles of exchanged imagined by Latour & Woolgar (1979): subsidies turn into equipment which turns into data which turn into arguments which turn into papers which turn into readers which turn into acknowledgement which turns into rewards which turn into subsidies... and so on.

Presumably to some surprise from the reader given the essentialist connotation that talk of 'substance' and 'flow' carries, I will suggest that the analysis of flow of substances may now provide us with insights about the way collectives conceive their being together. I'd better hasten to clarify, though, before you pounce on me, that this is another way to look upon exchange, in particular, its entangling effects. However, we will argue, this is neither market exchange nor gift exchange per se, despite including elements of both. In chapter 5 we will deal again with all these issues.

Actually, authors who want to make clear the departure from biological ways of understanding kinship tend to opt for other nomenclatures, such as 'relatedness'[39] – which can be understood, as Lambert (2000, p. 7) for instance suggests, as "those social connections between persons that are collectively recognised and regarded as enduring, as extending beyond individual interpersonal relationships, and as carrying rights and responsibilities associated with being related".

What I will now propose, then, is an anthropological exercise: can we imagine scientists of a group linked by flow of substance? Then, we may ask ourselves what

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daily interactions, allowing the perpetuation of flow –and thus, the perpetuation of social life (Hutchinson, 2000, p. 59).

[39] The advantages of using such a term are several. First, it does not assume a theoretical concept, but it is an 'empty concept' which should be filled with the practices of different peoples. Thus, one should try to understand how other cultures relatedness, to try to "describe relatedness in terms of indigenous statements and practices –some of which may seem to fall quite outside what anthropologists have conventionally understood as kinship" (Carsten, 2000, p. 3). Second, it does not assume priority of biology in other indigenous notions of kinship. Third, it opens possibilities for comparison studies. These three advantages do not hide inconveniences of the term: "The obvious problem with relatedness is that either it is used in a restricted sense to convey relations in some way founded on genealogical connection, in which case it is open to similar problems as kinship, or it is used in a more general sense to encompass other kinds of social relations, in which case it becomes so broad that it is in danger of 'becoming analytically vacuous' (Holly 1996: 168)" (id, p. 5).

their substance could be, what kind of 'transactions' take place among them and what kind of transformations of substance make possible a continuous flow of science, a continuous embodiment of creativity and procreativity. I do not mean to imply one can imagine one single substance uniting the whole community. Our talk of 'substances' draws a field of transactions –partly reminiscent of gift exchange, partly of genealogic transmissions, a kind of exchange understood (as it is often the case in anthropology under the influence of Melanesian studies) as a mechanism of group and community formation (Carrier, 1998, p. 85; see Strathern, 1998 for a discussion).

6. Nurturing knowledge, knowledging nurture

6.1. Downward flow

Weird as the notion of flow may seem to us at first, the idea that scientists receive something which they in turn must pass on in order to pay back what they have received is present in the lab. Ph.D. students emphasise that when they enter the laboratory, they know nothing ("as if we were in kindergarten"). But they also insist on their feeling of gratitude for all they receive from older members: knowledge, tips, patience and time. They feel so much in debt as to feel the duty to reciprocate later on, as if accomplishing a transaction, an exchange, where instead of money, another good travels –what we could call 'knowledge'. With this name, we refer to the more cognitive aspect (theories, ideas, know-how, supervision, power of decision), but not only: for, as we have claimed, affectivity and relations are also taught (and all of these things received have constitutional effects on those who receive it: they acquire 'experience', they change 'ways of being'). Therefore, I keep the label 'knowledge', instead of a combined 'cognition-affectivity' out of conviction that knowledge already involves both.

All these things mentioned are perceived to be 'received' from the group. But above all, knowledge is perceived to come 'from above': either from a more experienced

Ph.D. student, from a post-doc, the senior or the head, all of which are figures higher in the hierarchy: since experience and knowledge is accumulative, those who have more time in science are seen as the source -the higher, the more experience. The figure of the head is, at the end of the day, the figure-source of knowledge. Some years ago, the head herself was constituted (trained, taught) by another group, another pyramid, another generation. Now she passes all this knowledge down to 'her offspring', to the members of her own group, through the topic and project of research, as well as through training. In many occasions, the head has trained the senior, who has trained the post-doc, who contributes to training Ph.D. students and consequently, the head becomes a connection between past and future; between tradition and established knowledge on the one hand and innovation and new discoveries on the other. The perception, then, is that 'knowledge' flows down the pyramid: from the head, to the senior, to the post-docs, to the Ph.D. students. And down, from one pyramid to the next, from one generation to the other.

However, this is not a simple transmission. If each person simply received and passed on the same, one would only find transmission of tradition. Important as this is, they all feel that the amount of knowledge transforms: each person receiving knowledge must contribute to the flow with new knowledge not included in the tradition inherited (innovations, new research, new discoveries). Novelty increases tradition *and* improves on it: the flow of 'knowledge' increases and is purified while being passed on (new hypothesis, models and theories join the whole, those which do not pass the test are abandoned). A quantitative and a qualitative transformation. Hence, the feeling of progress: each generation knows more than the previous one. Of course, this does not hold individually: it would be preposterous to claim that beginners know more than older members. Collectively, however, they feel that each new generation knows 'more' and 'better'.

The very act of passing on scientific flow is already regarded as productive, bringing about change –the more knowledge flows, the more it will transform itself (improve and increase). This can clearly be seen in 'teaching'; this task is never regarded as a

matter of simple reproduction because it has creative consequences. There is a constitutional change in the person receiving knowledge from others: she increases the horizon of ideas and know-how, the scientific persona becomes more competent. This is a first sense in which teaching is creative. But there is another one. To produce new members, or more competent members entails a multiplication of occasions to express creativity/productivity; indeed, as soon as Ph.D. students receive 'creative flow' from the group, they can start being productive –contributing with research results and papers to the productivity of the group. Therefore, teaching others is a double expression of creativity, highly regarded as a necessary task, a productive investment, and never a simple parasitical activity, a wasting of time.

The link between 'knowledge' and 'creativity' is very strong. A procedure to produce knowledge must involve the possibility of innovation: procedures bring new results and new publications; a new technique may allow them to carry out a new procedure; a slight modification of an old one may yield improvement, a new trick to be explained. Each of the practices in which a scientist engages is perceived as consisting of "practical steps" *plus* "thought", never pure reproduction, but production of novelty. This plus of thought is what justifies their differentiation between 'creative tasks' –those which require the knowledge and expertise of a scientist- and 'repetitive tasks' –which can be carried out simply with a bit of skill and therefore only require 'technicians' (such as, for instance, the production of litres of buffer, considered to be a bit of a chore). Laboratory technicians[40] should be in charge of them; when scientists carry them out, this is a waste of time and of money. From the perspective of an external observer, the difference between tasks which require the performance of a scientist, and those that 'only' require technicians is difficult to perceive, since both of them require know-how, practice and skill. But scientists avert the difference is there.

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[40] This type of technicians, slightly more specialised than maintenance technicians and less than TAQs, are rare in Spanish universities. This absence is often perceived as an impediment to reach European rhythms of research and production (European laboratories tend to have this figure), and its implementation has been for long one of the vindications of Spanish scientists.

Here we find, once more, the distinction between handwork or empirical knowledge and philosophical knowledge stemming out of reflection that Shapin (1991) has detected impregnating the science of Boyle's times. It is nevertheless interesting to observe that this distinction holds even though in the IBF the difference between these two types of knowledge is quite vague: the figure of the scientist and the laboratory technician collapse into one. Still, the differentiation thought and knowledge on the one hand and skill and practice on the other remains.

Creativity is both linked to the individual and independent from it. On the one hand, some scientists are supposed to be 'good' and have more 'genius'. This is, though, a projection back in time –when a scientist obtains a good result, she is said to be good. But they also hold that invention is independent from particular individuals. Thus, whereas they feel summoned by vocation, they simultaneously believe that if they were not there, somebody else would be in their place, without science suffering damage. They have the conviction that the world is there to be deciphered, and somebody will, it does not matter who; hence, the race to be the first to discover something.

The perception of contributing to the creative transmission of knowledge, while being completely replaceable, helps forming the feeling that a scientist is just one link of a chain: science was there before, and science will be there afterwards: a flow which does not start in oneself, but of which one is a simple transmitter. One receives an inheritance, and one passes it on, a bit engrossed if possible. Science flows downwards through scientists, from one generation to the next; down the pyramid, but nevertheless forward in time, towards the future, always in progress, science does not step back in time. This is one of the ways in which the feeling of progress is sustained.

One of the moments which is easily describable in terms of downward flow and accumulation is the discussion regarding authorship of papers. As we have seen, the work of the Ph.D. student can be 'appropriated' by those who are above her in the

hierarchy. But not by anyone, only by those who have some involvement in the paper, those who have contributed with knowledge, in any of its shapes, to the creation of the paper. In a way, in order to decide the 'paternity' of the paper a kind of short genealogy is done. Papers, in this sense, constitute a particular node. On the one hand, they are a point of departure, since they put new knowledge in circulation which will be passed on to others to accept, discuss or reject, flowing into the future. But at the same time, they are a point of arrival, a point where the productivity of the head mixes with the productivity of the Ph.D. students and the rest of the members of the group involved.

Even in those cases in which the head was too busy to supervise the work, she still signed the paper. This is a common practice in science (Shinn, 1995). We also saw situations in which some Ph.D. students secretly elaborated a paper, and said it to the head once the paper had already been accepted -and yet, the Ph.D. students added the name of the head to the list of authorship. Or, still, there were moments in which Ph.D. students spontaneously decided to include the name of somebody who had taught them before, although his person had not directly worked in the paper. Somehow, it looks as if, among members of a group, there was the awareness that, regardless of how much work for a particular paper the head has done, she has already amply contributed. This feeling finds a good explanation as soon as we recognise the 'transmission of flow' down the pyramid. The whole laboratory where members of a group are working, all the equipment and instruments, the theories they use, the field where they work, the money they consume –everything- is an expression, a translation, of the productive power of the head. Constant flow is feeding the group, and consequently, the head is continuously contributing to their work. The paper, one of the most important ways in which creativity is embodied, is regarded as the solidification of the contributions of many.

The way "creativity' or 'productivity' expresses itself in each person will depend on her position in the pyramid and on the structure of the group. The creative flow received by young Ph.D. students is invested first in their own ripening; later on, once

they have accumulated enough creative know-how, it will also be expressed first in the production of research and papers, and afterwards in the teaching of newcomers ('paying back'). The post-doc experiences a similar distribution of the received flow: maturing, research and teaching. Things change a little bit more at the level of seniors: the amount of flow dedicated to maturing or learning is lower (they already know a lot!), and the investment in teaching others is very high: being a reservoir of 'knowledge', seniors are a very important node for training and transmission of knowledge 'down the flow'. Contribution in terms of papers and results will depend on the number of tasks the senior is in charge of - if a senior must teach at university or deal with services, her contribution will be lower than if she can concentrate in research.

And what about heads? Like seniors, heads keep on learning (so that they are able to keep on sending knowledge down), but the amount of learning or ripening is proportionally lower than is the case with post-docs or Ph.D. students. Heads contribute to research too: maybe not by working at the bench, but by defining a topic and project of research, giving advices and theoretical help, and more rarely, by designing experiments. However, the head's main contribution in terms of creativity is to be found somewhere else:

> "The objective is –it was, and it will be- to constitute a potent group with people of different levels, so that we can work competitively in this field" (EI01-05).

If we forget for a while the grandiloquence of the sentence, oriented to persuade audiences, one cannot avoid being impressed by the way creativity is expressed in it by this head: the aim is not to produce research per se, or to work competitively, but to *create a group* which can achieve all these objectives; the constitution of a group is the head's raison d'être. But to understand this expression of creativity, we should first introduce the second component or principia which, together with 'knowledge', constitutes the flow travelling down the pyramid. Let us read the following quotation by one of the heads of the IBF:

> "The period 89-91 was one of the most productive for me, organising a lot of international seminars and meetings. And from the scientific point of view, it was also very productive" (EI01-05).

It was not after reading several times this claim that something caught my attention. First, the use of the word 'productivity' (which now should not surprise us anymore). Second, this person distinguishes two types of productivity, one which has to do with science, and another related to meetings and seminars, or, phrased more generally, to contacts with other scientists. The distinction between scientific and not scientific elements is common. Members of the laboratory know that more than 'knowledge' is needed to establish and lead a laboratory, and, especially, to maintain them as members of the laboratory: 'money' (translatable as 'resources', 'equipment', 'material'), and 'networks of contacts'. We could call this second aspect 'nurture' or 'feeding'; essential as this flow is perceived, it is distilled from the flow of 'knowledge', as the quotation makes explicit.

The origin of the nutritive flow is strongly identified with the head. Resources were in the lab before the Ph.D. student's arrival: the laboratory, the equipment, the space, the budget... nothing would be there if it was not for the head and her entrepreneurial activity. Even the economical remuneration of the members, although it is not directly produced by the head, is mediated by her: grants and university salaries are only possible if the head accepts this person as a member, and works to get funding (and offers her curriculum to Ph.D. students so that they can apply for a grant). The head is the origin and main source of nutrition of the group; she sets it in circulation down the pyramid. But this time the head does not receive and improves something she is in turn given by another group –nutritive flow does not come through her from 'above', from tradition, from previous groups[41]. Rather, the head must produce it on her own.

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[41] This is only the case when a head retires and the senior takes over the lab, already equipped. This is a rare situation, though. Usually the emergence of a new group entails the creation of a new laboratory.

In most occasions, scientists understand that 'knowledge' and 'nurture' can transform into each other. Money becomes equipment which enables new experiments which produce new results which are shaped into papers. A paper may lead to a collaboration which may increase funding. Funding allows conferences which bring about new collaborations which publish new results that facilitate more funding that translates into a new piece of equipment, and so on. Chains of convertibility can be further imagined, and it is precisely this power of transformation what Latour and Woolgar's cycle of credibility illustrates so well. This intermingling is perceived as necessary: flow must be heterogeneous so that it can be creative. If the head only passed on 'knowledge', after a while nobody could do science, for they would lack materials, equipments, money. If the head only passed 'nurture', members of the group would not learn; maybe she would still be doing research, but she would surely have no group. And members of the group know that unless they receive both kinds of flow, 'nurture' and 'knowledge', their belonging to the laboratory -and hence, their belonging to science- is endangered.

That this heterogeneity is essential can be clearly appreciated if we think of the moment of creation of a group. The constitution of one's group is a creative moment in a very radical sense: it is the highest way in which a mature scientist can be 'productive' or 'creative', the highest expression of the scientist's "natural trajectory". However, more than an accumulation of experience and knowledge is involved in the constitution of a group. Many seniors, after several years of work, are ready to create and pass down 'knowledge'; but, as we know, only those who manage to achieve a position within the university (or a company) will be able to produce enough resources to create the group. Thus, a first requisite is to find a good anchorage in the university pyramid[42].

Once the position is secured, the transition from 'senior' to 'head's will generally be a soft, gradual process. Whereas in theory nothing stops a scientist from abandoning a group with empty hands, obtaining a space and creating a group from scratch, this is

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[42] It is not enough for the position to be permanent, but it also must allow a scientific development. In other words, whereas a T.A.Q. could be permanent, people occupying such positions are not allowed to develop a scientific career. Lectureship and professorship are the only secure positions.

very rarely the case. Most of the processes we heard of followed a similar pattern: while still working as a senior for the head's group, the scientist applies for funding on her own name, and develops two projects in parallel: the project of the head and her own so that her trajectory starts gaining independence. For a time, then, the half-senior-half-head will remain in an ambivalent position, where it is not quite clear whether she is or not part of the head's group:

> "I'd say that we are still part of… of my group, of our group. I try not to say 'my group' anymore... at any moment he will adopt his own lines of research, because he is already a completely trained senior. And I have encouraged him to do it because I think it's a good thing. But I have also said to him that I am interested in not losing this relationship... if he is also interested" (EI01-12).

Thus, the obtaining of funding starts a transition phase which will conclude when the scientist's nurture is also able to produce a space. In some occasions, separations are more traumatic. If the new group decides to take up a new path that excludes possible collaborations with the old group, the excision may be seen as half a betrayal: not only a cut of flows but also a cut of relations, that is, a cut in the possibility to gather flows again. In short, for a new group to emerge, several conditions must coincide: enough experience, a secure position that allows applying for funding, a funded project and space of its own.

Once all conditions are in place, a cut will be performed, dividing both groups: the moving to another space will ritualise a cut (which had been planned and orchestrated before): a separation of both flows. From that moment onwards, the new head will be perceived as producing independently both types of flow –knowledge and nurture. Even if groups remain in tight collaboration, the perception gained by the cut will prevail: the joint project will be perceived not as a continuation of the intermingling of flow but as two different flows uniting for a particular project. Out of this cut, then, a new group may emerge, a new head (and with them, a new topic of research, a new orientation, a multiplication of perspectives: thus, a new dimension of complexity with science). Thus, this performed 'cut of flow' makes groups visible,

and allows claims of belonging to find boundaries: this person belongs to this group, this person does not (Hutchinson, 2000).

The following example may illustrate the importance of these perceived cuts. The senior of one group sent a paper for publication, and as usual, added not only her own name, but also her head's (let us call her B) –a normal procedure following what we have introduced as the normal agreements on authorship and publications, that would have been approved of in any other laboratory. By doing this, however, this senior was interrupting a practice that in her lab had become common: since years there was the unusual 'habit' of including in all publications the head's old head (A), because B felt to be in debt with her previous head A (due to old academic favours the nature of which we do not need to specify here). When B realised that the senior had excluded A's name, she asked her please to reconsider her decision: would she be as kind as to add A's name? But the senior's answer was clear: "my head (B) may be in debt with her old head (A), but I am not. A has not contributed with anything, therefore, I will not add A's name to my paper". This senior had done the research alone, not even her own head had contributed to it or supervised it, and nevertheless, she did not seem to have anything against her direct head (B) signing the paper –she was in debt with her head, one should always recognise the flow received from the head. However, her debt and obligation of reciprocation up the genealogy concerned only her direct head, and not further up (actually, there was no more 'up' for her). Her head's debts were not hers, and the senior had not received flow from A in any form. If B imagined her group still linked to A's, the senior saw instead a cut separating groups: "I owe her nothing". A did not deserve to have property claims upon the senior's paper.

6.2. Upward flow

Not everything goes down the hierarchy, though. In fact, some things must go upwards so that productivity can flow better downwards. For instance, knowledge. We know that heads provide the group with flow in the shape of 'scientific, creative

knowledge'. This means that they pass on everything they have previously learnt: line of research, interests, ideas and tricks. However, all this accumulated knowledge runs the risk of running out if it is not renewed. If heads have time to read and participate in research, they themselves are able to renovate their 'reservoir'. If, as it usually happens, they cannot invest time or efforts to update their knowledge, they still have another resource: to obtain knowledge from those below her. Knowledge produced at the bench by Ph.D. students or other members needs to 'feed back' the heads. Thus, they are expected to 'learn' every time they talk with seniors, post-docs or Ph.D. students about the problems encountered during research, interesting information in the literature, or doubts posed to them. Unless this learning process takes place, heads will remain completely disconnected from new advances, and unable to offer further help with knowledge and techniques. As a Ph.D. student once told me in a whisper: "you know, heads must learn. If they don't do it, this is a problem. They might lose the train".

One way to assure that knowledge flows upwards are regular meetings where recent bibliography, as well as conceptual problems are discussed. The main aim of these meetings is to make sure that members of a group are well informed of the topics worked upon by colleagues: some information may be useful for several subgroups and projects. But, importantly, these meetings make it possible for heads to keep up with literature and experimental work. Moreover, as if to confirm the hybridism of flow, in this meeting more than knowledge is discussed; an important part of time is dedicated to discuss matters of budget and 'nurture': how much they have spent in material (usually too much), how money will be allocated next, what new equipment can be bought or should be repaired, what conferences they should attend, etc.

But knowledge is not the only substance considered to flow upwards. The 'nurture' function of heads –obtaining funding, equipment, contacts, etc.- depends to a great extent on their curriculum, which means that the latter should increase as much as possible, so that advantages may accrue to the whole group. Since the head signs the papers published by all other members of the group, the more publications the group

produces, the more the head publishes. Thus, the publication of papers is a contribution to productivity that not only has downward effects, but also 'upwards' in the 'genealogy', so to speak –contributing to the curriculum of all those who are involved in it; since the creativity of so many people has been engaged in a paper, benefits need to circulate to all of them, but especially to the head, so that the she does not lose capacity to remain the point of origin of the flow.

To insist on our consumption metaphor, if the group feeds from the head, its members also need to feed the head back so that she can keep on feeding[43]. It does not matter whether heads participate or not in the work, nobody in the laboratory would ever dare not adding their name to papers produced. And not only because of power and hierarchy –which of course plays a role- but also as recognition of the downward flow. Recognition, to express it in another way, of the collective consumption of the head's productivity, a kind of payment for nurture[44] (Hutchinson, 2000; Stafford, 2000; Wagner, 1977, in Strathern, 1999).

Problems begin, however, when flow is interrupted. This may occur either because the head cannot put enough flow into circulation, or because other members fail in returning upward contributions. This is why a Ph.D. student who is not particularly productive will lose worth and interest for the head[45], whereas those who provide the group with papers will be cherished. Likely, if Ph.D. students (or post-docs, or even

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[43] "Once young people (both sons and daughters) are old enough to work and earn money, they usually hand over most of their income to their parents. But this is not yet 'support for parents'. A good proportion of this money is usually spent to cover future wedding expenses, including the preparation of the 'new room' (in the case of the groom) or the provision of a dowry for the bride cf. Chen 1985). The fact that children in this way effectively subsidise their own weddings may seem to diminish what I have been saying: that the wedding manifests a parental obligation which is part of the cycle of *yang*. But in fact this flowing back and forth of support (my assistance makes it easier for you to assist me) is at the very core of Chinese notions of parent-child reciprocity" (Stafford, 2000, p. 44).

[44] There are several ways to offer 'payment back'. Stafford shows how in China, the passing on of nurture (yang) from parents to children in childhood obliges children to nurture parents in their old age (Stafford, 2000, p. 41). Failure to do so may entail the termination of relations of descent. Likewise, Hutchinson (2000) claims that "a woman who has suckled and raised another's daughter may claim the 'cow of nurturing' (*yang romä*) upon the girl's marriage" (p. 62).

[45] If a not particularly productive Ph.D. does not spend too much money, or is seen as bringing other advantages into the group (as, for instance, teaching newcomers very well, or taking care of bureaucracy within the team), the presence of such 'unproductive person' will be further accepted - 'productivity' will be rewritten.

seniors) perceive that they do not receive flow from above, will start complaining about the 'unfairness' of the situation. This may be the case when the head does not make enough flow circulate (problems of quantity), or when this flow is seen as homogeneous (problems of quality): in the group of a head that neglects nurture (that experience, for instance, lack of resources, or inability to help economically members of the group in case of emergency), problems will emerge no matter how much knowledge and supervision she offers. Homogeneity may show the inverse composition: if a head takes care of materials and money, but is not able to provide with theoretical support and supervision, Ph.D. students will start showing discontent in several ways. Later on we will analyse an example of one of these situations.

7. Extensions

"Un home no pot anar caminant a la babalà, no són només els cecs els qui tenen necessitat d'un bastó que vagi temptejant un pam més endavant o d'un gos que ensumi els perills, fins i tot amb els dos ulls intactes un home té necessitat d'una llum que el precedeixi, les coses en què creu o a les quals aspira, els propis dubtes serveixen, a falta de res millor"[46]

(Saramago, 2001, p. 99).

"A world obsessed with ones and the multiplications and divisions of ones creates problems for the conceptualization of relationships. To be able to conceive of persons as more than atomistic individuals but less than subscribers to a holistic community of shared meanings would be of immediate interest for comparative analysis. Anthropologists already know all the pitfalls associated with representing societies and cultures as though they themselves were unique, bounded individuals. The question is how to think about the connections between them in a way that does not have to rest on that premise"

(Strathern, 1991, p. 52-3).

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[46] "a man cannot walk rootless, not only blinds need a stick to grope some inches ahead or a dog to smell dangers, even with two intact eyes does a man need a light preceding him, the things in which

As we have already discussed, scientists' being together is not that easy to conceptualise. We could portray the group as an organism –heads and hands working together to achieve the same goal, where the whole would be bigger than its parts – the collective would be emphasised over the individual. Or we could emphasise individual calculation and portray science as the juxtaposition of individual interests that articulate with each other only inasmuch as profit ensues from collaboration. Options seem to be constricted to imagine organic totalities or a combination of elements. This constriction might have to do, Strathern suggests, with Western mathematics, that is, with a particular Western way to conceive of numbers and units: we can imagine a group as a whole (a big 1) or juxtaposition of elements (1 + 1 + 1 +...), but partialities are for us more disturbing. This arithmetic is in turn sustained by our understanding of the individual as one unit:

> "The image of a person as an individual encourages us to regard number in a particular way –that we are dealing with ones (single entities), or else with a multiplicity of ones (innumerable entities). Two is already a plurality. This homely mathematics also compels us to see wholes made up of individual parts, center persons integrating a plurality of individuals as fragments of multiple centerings. Consequently, we seem caught between an atomistic view (a totality is constituted by the aggregation of independent elements) and a holistic one (where elements have no existence apart from a total structure or system) (Ingold, 1986, 43)" (Strathern, 1991, p. 25-6).

This reliance on 'unity' is revealed quite nicely by our common understanding of the person as performing roles (even when this person is a scientist!). Either if we think of the individual as a centre integrating different roles (singularity), or as fragmented into a multiplicity of roles (juxtaposition of elements), this notion remains unquestioned, for "integration and fragmentation coalesce as personified forms of number" (Strathern, 1991, p. 25-6) [47].

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one believes or to which one aspires, one's own doubts can do, if there is nothing better" (Saramago, 2001, p. 99).

[47] This has consequences for some post-modernist tropes such as collages, pastiches and bits and pieces: if they are fragments or bits, one can legitimately ask oneself of *what*. In this sense, what

Nevertheless, some images or perceptions that I could witness during fieldwork do not let themselves be apprehended completely by such common mathematics. Sitting in a university bar, in front of a coffee, a post-doc of the IBF told me that exchange of Ph.D. students between laboratories was very useful for the heads, because this allowed them to *set a foot* in other prestigious laboratories (see chapter 6). This person's analogy also suggested an extension or prolongation of the head in the Ph.D. students: the head is somehow present in the Ph.D. student, so that wherever the latter is, the former may also be present. Notice the displacement (but not substitution) this person was pointing at: when Ph.D. students visit another laboratory, it is the head who *also* enters the lab, challenging the assumed clear-cut difference between one and the other. I was immediately caught by the image he produced: an octopus with a strategic head and a thousand dancing feet, extremities attaching to and detaching from laboratories.

In this image, as this person sharply implied, entanglement is strong: we find an assemblage or collective being in which head and Ph.D. students, while distinct, confuse into a unique ensemble: neither one body, nor two. A difficult boundary: parts articulating without constituting a whole. This would be another way of interpreting their images of 'hands' and 'heads': not as parts of a whole, complete body integrated in an organic way, but as extremities or parts which assemblage: it is a re-membering, a re-collection of members -hinged parts, articulation. The question, then, is, how can we imagine this attachment? Is there a way to conceptualise their being together other than through integration into one collective or the simple juxtaposition of individualities? Here is when we can put our model to the test, since it might offer us a way to imagine articulations, in which the group appears as more than a set of individuals, but less than an integrated collective being.

We have shown how scientists can be imagined as related through flows of substance, by which we mean that they are in exchange: a flow of transactions that bond

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appears as a rejoicing seems more the lament for the lost unity. This is perhaps why Latour dubs

members, so that those who are in exchange belong together. They receive something from others –something other- and they pass it on, and it is precisely this circulation that constitutes them as members. Being a member means participating of this flow, sharing the flow of others, in particular, of the boss[48]. This is particularly visible, we have argued, in the constitution of Ph.D. students. As we have seen, the creative flow of science becomes embodied in different expressions: productivity may take the shape of papers, of research, of a new group, of collaborations, etc. One of these creative expressions is the constitution of Ph.D. students. A new member of the group is constituted by the knowledge and ideas, as well as by the time, money, efforts and patience of the rest of the group –as it is visible in the costs of teaching Ph.D. students. Ph.D. students, then, embody the flow of the whole group, which means, given the perceived source of flow, that they embody the head's flow. Since Ph.D. students are partly a concretion of 'knowledge' and creativity of the group, Ph.D. students incorporate the group in them partially -incorporate the head.

In this sense, the perception that the head awakens of 'owning' the whole group, a kind of identification –but not quite- with the whole group is a signal of this complicated relational entanglement. What gives her this special status is not that her persona encompasses the whole group completely, but something else: of the head it can be said that she takes part of (participates in) all the scientists of the group. She takes part in all exchanges that members of the group take among themselves and between other groups. This being so, we should not be surprised by the emergence of property and paternity idioms. When the head says 'this is my Ph.D. student', she is simultaneously recognising that there is something hers in them –there is something *of her, a bit of her* in them.

Thus, the property relation is not exactly one of possession, as when one acquires an object or pays a salary to workers. Property here refers to "extension of or gathering

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postmoderns as disillusioned moderns...

[48] This may explain why, when a senior leaves the group, a cut in the flow of substance needs to be performed: they must cut extensions, they must cut exchange transactions with parts that constitute them, with parts of which they are in turn part. Until they do not perform a cut, personas are too entangled.

into the self" (Strathern, 1999, p. 140): Ph.D. students are an extension of the head, an extension of her self into the group. The type of exchange and transactions imagined by scientists in the constitution of members of a group enables the perception that the head is partially present in the rest of the members of the group, while the rest of the members are also constitutive of the boss. And such a relation holds between all members. We can imagine such intimate connections between Ph.D. students and seniors, post-doc and Ph.D. students, etc.

Thus, Ph.D. students and their boss are not identical, as they are perceived as distinct individuals; but they are not quite a different persona, since they all embody her partially –they have been constituted by her flow. A post-doc and a Ph.D. student also share flow: they have also been constituted in mutual exchange. This creates a type of relationship between members of the group which can be properly apprehended neither with the scheme of self-other, nor by claiming that they are one and the same. A scientist is constituted by flow of others, and in that sense, even though the persona of the scientist anchors in the individual, it somehow extends beyond to include other individuals' contributions.

In order to think of a relation that allows perceiving entities in relation as attached without being reduced through integration nor simply juxtaposed without connection, we can find inspiration in the concept of extension. Whereas this notion has a long and interesting history within philosophy –from Kant, Descartes to Whitehead, just to name a few- we will use the notion as is reinterpreted by Strathern (1991), and put to work by Munro (1996). Extension is presented as a partial connection, a partial and provisional exchange with transformative effects (Munro, 1996, p. 266). To introduce the change in perspective that such connections entail, Strathern will suggest the relationship person-tool as a model through which to imagine them, precisely to disturb our normal thinking of the dichotomy self-other. Strathern's arguments apply to several figures: for instance, a blind person using a cane; a writer writing with a

pen (or laptop, if you will); an electrician repairing with a screw driver; "un hombre a una nariz pegado"[49]; or an anthropologist thinking through feminist theories.

Strathern is not the first author approaching relations between objects/artefacts and humans. Actually, she borrows and elaborates upon Haraway's (1991) notion of 'partial connection', which, as it is well known, is an interesting proposal within STS that challenges the very division between humans and non-humans and the asymmetrical way in which this dichotomy has been put to work (enabling, above all, an unequal distribution of agency, taking for granted unidirectionality: it is humans who act upon objects, through material and symbolic appropriation, for instance). Another questioning approach to the relation between humans and non-humans is undertaken, as is well known, by Actor-Network Theory and its actants -though with very different tools, and also the heterogeneous field of technology studies (Bijker & Law, 1992; Bijker, Hughes & Pinch, 1987; Winner, 1986) focuses, from different theoretical perspectives, on this relation. Heidegger's thoughts, not always acknowledged, were the fertile ground from which some of these proposals fed[50]. Since these contributions blur in some respects the differentiation between thing and person (or differently put, challenge the relevance of such ontological divisions and the self-imposed limitation of social sciences to deal with objects in their analysis), we will put them to use to understand not only how people may be extended in/by artefacts, but also by other people.

In tool-person relationships, Strathern says, we encounter surprising characteristics. A person who alone would see her abilities quite restricted is able to overcome those restrictions, and extend abilities beyond previous limits, thanks to the use of particular tools –where 'tools' is understood in a wide sense, not restricted to 'object'[51]. Thus, whereas the blind person would see her independent mobility

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[49] "a man attached to a nose". Taken from a popular verse by Quevedo (first verse of the poem "To a man with a big nose".

[50] Tool-person relationships have also been long worked upon, as the pioneering work of Vygotsky (1978; Wertsch, 1985) and contributions by the Theory of Action make evident.

[51] This is how Strathern, in a footnote, delimits her concept of tool: "I use the 'tool' here as a trope for 'culture' in Ingold's sense, that is, a vehicle for social life translating 'social purpose into practical

serio usly constrained without a stick, with it she gains a reasonable amount of autonomy. The pen or the writing machine allow the writer not only to carry out her work, but also to become a writer, to perform identity; and a similar thing happens with the electrician: can one think of an electrician without a screw-driver?[52] In the case of our anthropologist, she can expand her theory in different ways by using feminist theories, in a way that the latter make a difference in her position within the anthropological field. These two positions (anthropology, feminism) are not reducible to one (as it would correspond to an integrated self), but they are neither two independent positions (a reflex of a multiplicity of independent selves juxtaposed): the person remains an anthropologist, while at the same time it is also something else. Feminism extends anthropological possibilities; anthropology extends feminist possibilities.

In all these cases, then, our person can extend capacities through relating to a tool. Even though one is tempted to say 'through using a tool', 'usage' may not be the proper concept to clarify this relationship, for it suggests a unidirectionality alien to Strathern's conceptualisation: "I can make a conversation work for me as a tool works, as I can make myself work in sustaining a conversation" (Strathern, 1991, p. 39). Therefore, if we say that the person expands capacities through the tool, it is likewise true that the tool expands capacities through the person. Indeed, each of the extremes of the relationship may have an effect on the other: not only the person on the hammer, but also the hammer on the person (Ong, 1982; White, 1962; Vygotsky, 1978). If at first sight we experience certain resistance to bestow bidirectionality on this relatio nship, the example of language may help us capture the sense of this proposal. Whereas we can say that we use language, and that language is a very good tool to express and communicate, we all know, after the linguistic turn, that we speak language as much as language speaks us. If we use and create language, it is not less true that language create us. It is in this sense that Strathern can affirm: "What the

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effectiveness" (1986, 262). In reference to the discussion that follows, I note that a tool is neither body nor machine" (p. 126, note 31).

[52] Another example from STS literature that comes to mind is that given by Law (1994) about the relation between a head and all his 'props', such as telephones, offices, tables and secretaries.

extensions yield are different capacities. In this view, there is no subject-object relation between a person and a tool, only an expanded or realized capability" (p. 38).

To put it in other terms, Strathern re-conceptualises here the tool not as a simple intermediary –simply covering the gap between the blind and the street, the writer and the book, the mechanic and the car, the anthropologist and a paper- but as mediators (Latour, 1996a) that come into being in the very relationship. Whatever the person and the object were before relating to each other (can one say one is a writer, if one has not written anything? Can one say a pen is an instrument to write, if nobody has written with it?), they become something else on connecting. This constitutive moment affects both ways: a tool is a tool, only as long as it is found in a tool-relationship, that is, attached to something other, for which it works as a tool, appropriated by its use. As Deleuze and Guattari also put it, "The tool does not define work; just the opposite. The tool presupposes work" (Deleuze & Guattari, 1988, p. 397).

In this way, Strathern avoids the reification of tools, their consideration as simple inert, unanimated objects, which remain the same independently of the relationships to which they are party. They too, no less than people, are defined and created through relationships. But notice that she avoids also a definition of tools as simply symbolical, as if a 'tool' was completely defined by the other pole of the relation, by the act of giving it a meaning by humans. If a tool is less than a fixed, finished, invariable object, it is also more than a simple piece of matter awaiting human signification. It also determines effects within the relationship.

> "Second, the lack of proportion is not to be reimagined in terms of parts and total systems. At first sight, a 'tool' still suggests a possible encompassment by the maker and user who determines its use. Yet our theories of culture already tell us that we perceive uses **through** the tools we have at our disposal. Organism and machine are not connected in a part/totality relationship, if the one cannot completely define the other. (...) In turn,

> neither position offers an encompassing context or inclusive perspective. Rather, each exists as a localized, embodied vision" (p. 40, original emphasis).

The ambiguity of the person-tool relation is nicely expressed by the figure of the blind person with a cane. Is the cane 'self' or 'other' to the blind? Does the cane remain 'external' to the blind's self? Does it become one more part of her body, so to say? But to pose the questions in this way is already to assume and close alternatives: either it is a different thing, or it is the same. However, Strathern will suggest, a tool remains *at the same time* both part of the person and not part of the person –hence, a partial connection. My glasses are me and not me at the same time. For, in a way, a prosthetic relationship encompasses or shelters both perspectives: there is a sense in which related elements gain, if only partially, new properties and possibilities emerging out of the relationship; indeed, the extension or new realisation cannot be reduced to the nature of any of the parts. However, there is also another sense in which each of the elements still remains, if only partially, the same.

And if I present this version of Strathern's extension is because I want to make a twofold suggestion. First, her account allows us to consider an intimate attachment between the scientist and her props, instruments and equipment –with her *belongings.* It is not simply that it is impossible to imagine an experimentalist without a pipette, but, more radically, that she would not be an experimentalist without it –how else can she perform as such, without claiming this identity as a possession? They belong to each other: the pipette is of the scientist just as the scientist is of the pipette, in the sense that it is their being in connection that extends possibilities of being for both, throwing 'being an experimentalist' into existence. O put it in other terms, the self of the scientist *must* include the pipette. The enactment of the scientific persona requires the individual plus the pipette.

The example of the bioinformatics is still more impressive. We have seen that our description of their way of being is completely opposite not only to how Spanish scientists are usually characterised, but more concretely to how IBF members

perceive themselves. A comment made by one of the biocomputing members gave us a hint about how to interpret these differences in 'being' between laboratories. When inquired about ways to conceptualise the institution as a whole, the senior of the biocomputing laboratory insisted on differences between his own laboratory and the rest: biocomputers are more closed and 'asocial'. Besides, this description seemed to be consistent in all different biocomputing laboratories where he had been (Oslo, Cambridge, London, Madrid, Barcelona, Sao Paolo):

> "this is quite common in all these laboratories. People are very closed, they don't speak much and don't relate to other people, don't connect; in comparison to other laboratories, we connect a lot with the rest... You see, we don't have contact, we do not run around through the corridors with test tubes and proteins from one machine to the other, but remain the whole day here doing clic, clic, clic in front of the computer screen" (EI02-40).

When analysing the interviews later on, it seemed to me that he was giving us a hint about how to interpret their "weird way of being": he was clearly drawing a relation between setting and subjectivity.

It seems that the different type of assemblage in which bioinformatics find themselves, in comparison to experimental scientists, could affect subjectivity (Lévy, 1999; Rose, 1996)-explaining some of these 'personality differences'. We will see in chapter 5 how methodologies, practices and instruments differ between these two types of laboratories. Since "subjectification... is a product... of a heterogeneous assemblage of bodies, vocabularies, judgements, techniques, inscriptions, practices" (Rose, 1986, p. 182; see also Mialet, 1999), differences in the laboratory of belonging are likely to impinge upon members' ways of being. Of all the differences, one is particularly relevant: biocomputing scientists are in intimate relation with computers. These black boxes are more than mere instruments; in this lab, they become researchers producing data, an interaction partner for biocomputing members to which they must assemblage to produce knowledge. Likewise, the researcher becomes more an attachment to the machine, extending computer possibilities (a mutual becoming other that is hinted at in the way bioinformatics talk of themselves

as having RAM memory and switching off"). Bioinformatics and computers stand in a prosthetic relationship.

Thus, we can claim that the computer and the type of practices it enables have constitutive effects on people (Turkle, 1982), whose subjectivity is differently shaped depending on the assemblage they participate in. As Knorr-Cetina put it, "(n)ot only objects but also scientists are malleable with respect to a spectrum of behavioural possibilities. In the laboratory, scientists are 'methods' of going about inquiry; they are part of a field's research strategy and a technical device in the production of knowledge" (Knorr-Cetina, 1992, p. 119).

The computer allows a type of relation that Philip Fisher names 'participal act' (1978, p. 140, quoted in Brown & Lightfood, 1999). This proposal tries to understand objects not as inert things upon which humans exercise agency, but as partners of interaction. This requires not an anthropomorfisation of objects, but an exploration of new ways of understanding relations, so as to make place for such actants. Objects contribute to an interaction by defining possible actions and impeding others, by demanding of its user particular bodies, perceptions, positions and gestures, etc. A computer requires a particular type of approach so as to yield a productive interaction, and as much as the computer is put to use, so is the user, who progressively adapts to the demands of the assemblage to which is connected. This is a way to understand how sociality and materiality can be folded into one another (ibid, p. 3), and to understand how a different setting can end up producing different subjectivities.

But there is a second usage we can draw from Strathern's thoughts on extension and partial connections: I want to suggest that relationships between members of the group can be seen as prosthetic, that is, relations that allow articulation to extend selves into others without reducing them to unity —a partial identification or blurring of distinctions, a partial differentiation. An articulation which allows the perception of the group and of its members in different moments. An articulation which, through

extension, enlarges the person, who, for as long as she attaches to something other as a tool, expands beyond her body into others.

An illustration of this prosthetic relation can be seen in the extension of possibilities that members of the group enable for each other as far as research is concerned. When the moment comes that the head cannot carry out research directly at the bench anymore, other people (not flesh of her flesh, but definitely flow of her flow) is there to become her hands and turn a limitation into a further possibility: now the head can participate in research. More than delegation (as if the head 'ordered' research to a third party) or substitution (as if Ph.D. students took the head's place) is an extension that challenges the regime of presence and absence: through other members of the group, heads can *take part* in research –they play their part. At the same time, with their experience, advices and management, heads unfold continuously new possibilities of being for the group (we have insisted enough in this point). This is a similar subversion of the game of presence-absence to the one we could detect in the image of the thousand-feet octopus setting a foot where the Ph.D. student is: the boss is and is not there at the same time.

Therefore, the importance of this extension is that it allows us to understand how a self can magnify itself, acting in other situations, without for all that succumbing to an image of the self as an expanded air-ballon (or Serres' frog, 1995a). Through the use and appropriation of artefacts, the self can act at scale:

> "Outwardly, yes, the notion of a part depicts the mechanical, the adding of material to material. But there is more than a facile production view at work here. Inwardly, for the subject, the notion of part is dramatic and elicits the opportunity for another form of extension, that of performance. The addition of a part (object) extends the possibilities for cultural performance as a 'part' (subject). Object and subject, as effects, move hand in hand" (Munro, 1996, p. 260).

Thus, while the whole construction (person-tool) still remains located –indeed, using a tool does not grant global vision- it is as if one could reach, partially in terms of

space and time, beyond one's position. One perceives from the body one inhabits, and, nevertheless, reaches further: "it is an extension of it, an instrument made of different materials" (Munro, 1996, p. 39).

However, Munro warns, we should not understand extension-prosthesis as a simple mechanical union, "the adding of material to material". Or, differently put, we should not conceive this relation as a kind of manipulation or exploitation, a taking advantage of the other. Whereas this mechanical aspect may be there, there is more to extension than this, for, as Munro (1996, p. 262) suggests, Strathern plays with two different meanings of the term attachment: a) attachment as movement to and from something and b) attachment as feelings of belonging to something –not individual feelings, but a 'being moved', affectivity as a transformation force rather than an internal emotion. Thus, in prosthesis one can find 'affiliation and exchange'. For this is another of the effects of extension: not only does one part extend its possibilities of being and acting through the other, but it becomes the other. Extension, the move of attaching and detaching parts, of adding or exchanging parts, is a matter of ontological transformation. A performance with effects on being, a transformation.

Extension, nevertheless, does not always mean 'to become big'. Extension refers much more to being in particular relationships. One can be in extension and magnify, one can also diminish. Regardless of the effect, the relationship remains. As Munro reads Strathern, "we are always in extension. Indeed, extension is all we are ever 'in'" (p. 264). Spinoza clearly saw this when he distinguished between types of encounters. Encounters may compose harmonious or disharmonious relations –that is, relations which increase ability to act, or relations which diminish it. In the first case, considered within the order of the body, we find *euphoria* (an increase in power between bodies which agree), and *disphoria* (when bodies disagree[53]). Within the order of ideas, we find adequacy or inadequacy. When the individual places the idea of the cause of an event within the ideas of the mind, the person becomes active regarding the event, and increases its power to act; when the individual fails to grasp

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[53] For these ideas, see Brown & Stenner (2001).

the idea of the external cause (the idea is inadequately placed), it remains passive and diminishes its power to act. (Both orders converge, though: encounters giving rise to euphoria bring the individual to develop the means to exercise adequate ideas). Therefore, liminal encounters are neither good nor bad per se, but it will depend on the particular type of relations articulated, and the way they affect the conatus of those entities involved[54].

8. The group

These thoughts may help us understand a particular oscillation of pronouns when heads talk about the work of members of their group –singular and plural alternate in an interesting way. I once met the head of a group which had recently sent a Ph.D. student to another laboratory to carry out a particular research. I thought it polite of me to show interest and asked how this Ph.D. student was doing in London. The head answered (and I reconstruct the utterance after my field notes):

> "Oh, he's doing fine, thank you, we have already published a paper and are going to publish another one soon. We first wanted to publish on topic X, but they [heads of the host laboratory] suggested another topic to the Ph.D. student –and we have no problem with it either".

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[54] Likewise, when considering affects or feelings, Heidegger distinguishes between emotions and passions, each having different effects upon the *Dasein*. Emotions are the most basic type, resonating more with our *Dasein-being* than with our *Dasein-Being*. Emotions (*e-motio*) move us: we feel startled, overawed and overcome by emotion; hence they disperse us, they move our Dasein from one place to another, leaving us 'all over the place'. So much so, that they disperse[54] us in the being (*Seiend*): we 'fall' engrossed by the world, busy and taken in by the mundane, forgetful of ourselves and of the Being which informs us. In short, we are lost in emotion. The other side of the coin, though, is that this movement makes us aware of ourselves –on feeling moved, attuned by something other, we realise that "it is me" who is being moved. This awareness is a type of movement from an originary state of facticity towards our 'proper being' (which is the being that has taken the resolution to be in charge of this facticity). A movement which performs a condensation (*Geschlossenheit*): this is passion (love and hate), feelings which do not disperse us, but concentrate us, a cohesion or gathering that give consistence to our *Da*. A condensation *(Geschlossenheit)* that, unlike dispersion, allows us to leave the circularity of being and discover our centre -Da: we gather ourselves in our own being, giving consistency to the Dasein. Its effect, then, is not a movement of extravagation, but the strengthening of will (*Wollen*): the resolution (*Entschlossenheit*) to live, "being or existing beyond the limits allegedly settled by nature or tradition" (p. 111). Will to live: to let possibilities of being be.

How these collaborations work, and to what effects, will be discussed in chapter 6. Now suffice it to observe the curious alternation of 'he' and 'we', made with a naturality and unawareness that surprised me as much as the oscillation itself: there was no trace of awareness of 'appropriation' or 'alienation' in the air... For one cannot appropriate what one already owns.

We would be missing the point if we thought of Ph.D. students as workers being alienated from their work. If we accept Schwimmer's conceptualisation of alienation[55] as occurring "when the yield of such 'production' of the self is taken away by others who retain control over it and who return no equivalent gift" (Schwimmer, 1979, p. 296), we will have to admit that nothing of the sort takes place. Neither heads, nor post-docs, seniors nor Ph.D. students lose what Schwimmer calls 'identification' with their own work (only when somebody who participated in elaboration is excluded from authorship could we talk of alienation). Now, if heads use this plural in such sentences may well be the case that they *identify* (in the sense of Schwimmer, 1979, that is, as the contrary to alienation) with the work published; and if they identify (that is, if they feel that the work is also theirs, that there is something proper in this work), this is because they are also doing it... through their group.

The oscillation of pronouns may well signal an alternation of perception which has also been described as characteristic of prosthetic relations. This partial attachment allows a double perception, a switching from one figure to the other: now we see the two distinct parts, now we see the attached parts; we may now see the two entities as separated but linked (mechanical attachment where self and other are distinct, such as the blind and the cane); just to discover, in the next moment, a performance of unity, a-blind-with-a-cane; an anthropologist and a feminist theory, and an anthropologist whose position is defined through the use of feminist theory. Likewise, we can

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[55] The mixture of 'alienation' and 'gift' in the same definition may be surprising for some reader. Indeed, when examining Marx's concept of surplus value and Mauss' concept of yield (*hau*), Schwimmer concludes that they appear to be homomorphous, although they belong to different modes of production. According to this author, alienation is not only to be found in capitalist modes of production, and can be opposed to 'identification'.

perceive individual members working together, and the working-together of the group. Members juxtaposed in a group, and a group with members. A head and a PhD. student, and a head-and-a-Ph.D. student. Heads extending her members' possibilities, and members extending the head. Hence, we can sometimes perceive the individual work or contributio n ('I', 'he', 'you', 'she'), at other we see the group ('we', 'you', 'they').

Of interest here is the relationship in terms of potency that is imagined between group and individual. An emphasis in the individual does not involve turning the collective level invisible, and bringing the collective to the fore does not mean to construct the individual as endangering the integrity of the group. Now it is rather as if one dimension became the background where the other can project: the group gives visibility to the individual, whereas the individual makes the group visible. Nevertheless, whilst these two figures are there (parts and parts-in connection), one cannot see them simultaneously: "the seeing of the second figure involves a 'forgetting' of the parts of the first figure" (Munro, 1996, p. 261).

As Duque (in Martínez, 2002) puts is, every time I change my perspective, I can see previous aspects I had not yet seen, but with this movement, other seen aspects disappear 'behind our backs'. This movement of visibility/invisibility is what allows us to recognise particular figures in particular moments, while switching from one to the other. The effect is similar to these Gestalt figures: either one sees one figure, or the other. Every time the figure changes, the relation of inclusions and exclusions changes too, since the contour of the previous figure needs to disappear into meaningless background, so that the second figure may emerge: "the only movement is one of circulation: around and around from figure to figure –one figure picking up on what the other excludes" (ibid, p. 264).

This is a movement with Heideggerian resonances: this circulation allows a game of projection-retraction. Duque (2002), translating Heidegger, tries to clarify this movement with the example of the relationship between a hut and the surrounding

landscape. Hut and landscape, constituted by the same materials (wood, straw, stones), belong to each other in the sense that 'they are one for the other'. The hut is of the landscape, belongs to the landscape, since it 'makes' landscape: it retracts the landscape to a ground, giving it depth, direction... thanks to the hut, the landscape becomes landscape, a lively assemblage of relations. Looked at from the other side, the landscape makes the hut what it is –not only in the sense that it provides for materials out of which the hut is constituted, but also that it constitutes the background in which the hut makes sense: it shelters for it and produces it in the sense of bringing it to the fore. Thus, the hut and the landscape pertain to each other: the landscape belongs to the hut as the hut belongs to the landscape -the hut is of the landscape, and the landscape is of the hut. Or, to put it in Strathernian terms, the hut and the landscape extend each other, extend each other's possibilities of being, in a way that they remain intimately related, without for all that being reduced to identification.

As it can be noticed, unidirectionality is avoided. Whereas in a particular moment one of the figures is projected against the background of the second, which retracts into invisibility, the opposite perception is also possible: then the second figure comes to the fore, projected by the retraction of the first into a modest background. No perception is privileged, this is why Munro talks of 'circulation': there is no centre that gives one figure more weight. When individual contributions come to the fore, the group goes to the ground; when the group is emphasised, members recede. One can talk of 'forgetting' (Munro, 1996), in the sense that this movement performs a kind of displacement: one cannot have both perceptions simultaneously, one of them needs to be forgotten, ignored, made invisible. But it is not a complete substitution, for the other is integrated in perception. That is, one figure is necessary to sustain the perception of the other. Thus, individual members and their contributions can only come into existence because of the perception of the group as an assemblage. And the other way round, the image of the group can be seen as assemblage or articulation only because members and their performances are thinkable.

It is important to emphasise that we are not describing a simple integration of parts into an encompassing totality –for this would mean to think in terms of independent individuals –either juxtaposed or integrated into unity. If these images do not quite grasp the type of sociality we have tried to picture may have to do with the fact that members of a group cannot be imagined as independent persona. We have given several examples: the head being and not being a part of the Ph.D. student's, as a kind of present absence or absent presence; the head researching with and through the group; property not as appropriation or possession but as extension or belonging. They all suggest a different way to think about selves and others, about limits of persona and about numbers and relations than the one we are used to. It is as if the boss was more than one person (for instance, she can perform presence through herself but also through the Ph.D. student; what others do is also what she does) and less than one person (she alone could do nothing). And the same can be said of the rest of the members of her group (they all extend through her and could do nothing without her). And also, why not, as if they all were more than a person: they all include their belongings. Put in other words, we seem to find a kind of dislocation (or displacement) between individuals and persona.

This claim demands a first clarification –the distinction between individual and persona. The last is a relational entity, which does not need to coincide with an individual, even though for Western cultures it does. We perceive ourselves as individuals that enter in relations with others afterwards, a kind of already-there presence inhabiting the body upon which relations come, and through which it relates to other individuals. Whatever our selves are, their limit coincides with the limits of our body, i.e., our skin, and beyond it we already step into otherness. Thanks to anthropology, however, we know that this is not necessarily so for all collectives. And what I am now suggesting is that practices of the IBF leave some space for a conceptualisation of relations which seems not to fit with our "one-individual one-person" type of thought, as if, at least in some moments, a scientific persona did not map directly one-to-one with an individual. The limits of this persona do not coincide with the limits of individuals (neither of one nor of many). Each scientist extends into

others, while she is an extension of others. Scientists somehow know what we cannot put easily into words and scarcely allow ourselves to imagine. One individual alone could never be a scientist: without a group, a lab and equipment no scientist can be in place. A scientist is a relational, non-individual achievement.

When in transaction with the boss, a Ph.D. student extends the boss' possibilities, whereas the boss extends possibilities for her. Each contributes to the other's persona without being included in it. One can say that a member of the group is part and is not part of the boss at the same time. Such exchanges can be imagined with several people and artefacts at a time, constructing a type of intimate connection between each member and the group, that is seen as contributing to personhood. Extended through the group, the group extends in turn through its members. The group sets members to work, but members set the group to work for them too.

The argument is not that each individual is part of a single, huge person whose limits coincide with the whole group or the boss, as if they could include all the members. For this argument cancels partiality and reintroduces familiar mathematics: the persona would work as a globality of which individuals would be parts –whether we call it 'group' or 'head'. The group as a whole does not work as a persona, with 'collective will', so to speak. Neither is each member one, that calculates and takes decisions with independence on other individual wills. Partiality should be preserved: people and artefacts are parts that attach not so as to create a unity, but to enable a game of switching perspectives none of which subsumes others. There is no resolution into completion: just partial connections giving rise to partial scientific personae.

This lack of a one-to-one mapping makes it difficult to talk of reproduction. If reproduction is imagined as the repetition of the same, how can a scientific persona, constituted as a partial assemblage, be reproduced? How can the group, never exhausted by homogeneity, always such a heterogeneous and intimate entanglement, reproduce itself? If we lack unities, how can we think of a reconstitution of

partialities? Strathern, joining Haraway, will talk of regeneration. A process in which parts are attached and detached to renew an assemblage which is not a totality, and that it does not need to produce the same to continue to go on.

A group can accept new members that have not been constituted by the group –either temporary when a Ph.D. student visits a foreign laboratory for some months, or more lastingly, as when a post-doc may join a group for some years, or even stay. In this case, as soon as the new member enters exchange, the partiality of belonging is at work: extended by extending. Thus, if their being together seems to adopt the form of a genealogy, it is not an exclusive one (but neither inclusive! Spatiality is always a bad metaphor for partial connections):

> "These fractal graphics could describe the patterning of maps or genealogies, but they would be maps without centres and genealogies without generations. It is the repetition, the not-quite replication, to which the viewer is compelled to attend" (Strathern, 1991, p. xx).

Up to now we have presented an account that, differing from but also taking into account their self-descriptions, imagines relations: how they articulate and conceive their being together. Now we will see how this account allows us to make sense of a crucial aspect of the relation between science and other practices. An institutional look upon science tends to consider it as a distinct domain operating with its own regulations, that establishes relations of exteriority with other institutions. This is a notion that is commonly expressed by scientists, and that also permeates classic approaches within the sociology of science and of scientists. This view assumes boundaries between science and other domains such as 'society', 'economy', 'media' and 'politics', where the latter are mere context for the first. In the second part of this chapter, then, we will explore the way members of the IBF figure the relationship between science and nonscience, but we will do it in an indirect way.

We know that, given the contextual variability of their accounts, if directly asked, scientists will quite likely produce defensive claims directed to protect boundaries in

front of a particular audience –a nonscientific public which calls science to accountability. Therefore, instead of asking about this relation directly, we will trace its reflections upon the figure of the head, as imagined by members of the IBF in every-day accounts. Indeed, given her position as a source of flows, she is well located to reflect the type of connectio n uniting science and nonscience –and, as we will see, to carry the weight of the ambivalences and ambiguities with which such a connection is accompanied. We will suggest that scientists are able of keeping open ambivalent perceptions (merographic and partial connections) on the relation between science and politics, ambivalence which is in turn partially articulated.

Part II. Flowing science and cutting hybrids: the Perfect Tandem.

> "To speak of science in a political register, for example, would become 'science is only politics', an enterprise where power is at stake, protected by an illusory ideology, managing to impose its particular beliefs as universal truths. On the contrary, to protest that science transcends political divisions would be to implicitly identify the political register with the arbitrary, tumultuous, and irrational waves of human controversies that lick the feet of the scientific fortress, and in some cases, that take elements born in innocence and put them to perverse, harmful, or irresponsible uses. Each of these theses either asserts a reducibility or denies the possibility of a reduction in the name of a transcendence, which implies that the person who is speaking knows what he is talking about, in other words, that he is himself in the position of a judge. He knows, in this case, what 'science' and 'politics' are, and gives or refuses to one of these terms the power to explain the other. The principle of irreduction prescribes a retreat from this claim to know and to judge" (Stengers, 2000, 16-7).

1. Naive observers in the lab

It is scarcely news that scientists insist on the 'uniqueness' of science in comparison to other activities, whereby they are able to defend the existence of a boundary separating the scientific domain from others. This distinction, which sometimes also appears under the dichotomy content-context, has been accepted not only by many scientists, but also, as it is well known, by several theoretical approaches to science, such as epistemology as well as Manheim's and R.K. Merton's sociology. Precisely its rejection was Bloor's (1976) important contribution and it features now as an achievement in all the mythological accounts of the origin of the discipline (Ashmore, 1989; Domènech & Tirado, 1998; González de la Fe & Sánchez Navarro, 1988; Knorr-Cetina, 1995; Mulkay, 1979; Pickering, 1992; Woolgar, 1988a). Moreover, the lack of consensus among philosophers about the criteria that distinguishes science

from nonscience has brought scholars to doubt seriously the existence of such division (Böhme, 1979; Broad & Wade, 1982; Collins, 1982b; Knorr et al, 1980; Elkana, 1981; Lynch, 1982).

For instance, in her ethnography of a protein chemistry lab in Berkeley (California), Knorr-Cetina (1981) failed to detect clear boundaries dividing science from non-science. After exploring some of the images with which the scientific community has been understood, (such as the functional, precapitalist exchange model, a capitalist market economy, a (Marxist) labour interpretation...), and showing some of their limitations, she offers a different way to conceptualise the webs of social relations encompassing scientific practices. Her proposal is that the relevant contextual organisation of lab production cannot be described with an 'internalist' model of the scientific community, but it must be understood as *variable trans-scientific fields*, that is, networks of symbolic relationships which go beyond the boundaries of professional groups. These communities are hybrid as far as membership is concerned and therefore, to understand how a laboratory works, we cannot limit our attention to what happens within its walls.

A similar proposal, articulated in terms of networks[56], can be found in a tradition inaugurated with early proposals by Callon (1988) and Hughes (1987) and continued with later contributions by Actor Network Theory. To illustrate our point, we will take Latour's (1983) *Science in action*. In this book, he shows how in a laboratory, next to the typical scientist (the researcher with the white coat), there are other crucial figures, most notably heads of research teams, who do not match the stereotype. These heads resemble more what John Law (1991) has labelled the "scientist entrepreneur": a businessman mixing science, politics, negotiation of contracts and public relations. If one follows these figures around, the image of science emerging is quite different from the one we obtain observing work at the bench. Heads spend most of their time moving from setting to setting, often outside the lab, negotiating

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[56] Latour's networks are well known by now. To clarify differences between his notion and that of 'social networks', see Latour (1997).

with nonscientists, challenging fix scientific boundaries in that they spend more time "beyond boundaries" as "within science".

What should we do with such an exoteric figure? Should we decide that only those working at the bench are real scientists? If so, we have a problem, for the latter can do science only because heads engage in their apparently unscientific activities. Indeed, without their activity, very few members of the group could be doing what they understand as 'science' and 'research': "there is a direct relationship between the dimensions of the external recruitment of resources and the amount of work which can be done in the interior" (Latour, 1987, p. 148). The stereotypical image of the isolated scientist is not only stereotypical, but unreal, in the sense that an isolated scientist could never have the resources to research on its own. Thus, as Latour puts it, "technoscience has an interior because it has an exterior. There is a feedback circle" (ibid, p. 151). This division does not have explanatory power, rather, it is itself an effect one has to explain (Latour, 1990).

These observations match quite nicely what a 'naive visitor' would report: a person who had not been enculturated in perceiving the "science/context" division would hardly distinguish where 'science' ends and 'context' starts. Examples of this intermingle abound in our field notes, since money concerns permeate their everyday practices: decisions about what experimental procedures they should follow depend on their cost; topics of research are selected according to the budget, the kind of equipment they require, the teams they would have to compete with; during research they are quite often asked to control their experimental imagination if material is too expensive -or, actually, the other way round, to be very imaginative so as not to spend so much[57]; they have to invent strategies to prolong the life of certain equipments and avoid damages[58], and so on. Not surprisingly meetings mix bibliographic revisions with budget concerns with discussion of future projects and equipment acquisitions.

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[57] This ability to improvise and save contributes to Spanish scientists' value, said to be quite appreciated in foreign laboratories.
[58] This is why the head of some laboratories makes some regular visits to the benches to check that certain measures are not forgotten, such as keeping purification columns always in water. Any negligence may be too expensive...

These worries are totally integrated into their every day scientific practices –into their rationality and process of making decisions and selections (Knorr-Cetina, 1981)- and turn hypothetical differentiations between 'content' and 'context' rather problematic, to say the least. Scientists themselves emphasise this mixture and interdependence when they are asked to describe their work with some detail.

The intermingle of economical, political, social and scientific concerns is so intense, that it is somehow puzzling that IBF scientists, in other occasions, insist on portraying this mixture in terms of 'external influences' impinging on science. Indeed, the same people who in the lab take good care to let us know all the hindrances they must overcome and all the miseries to which economic scarcity condemns them can, in another context, emphasise boundaries between 'domains' or 'spheres': economy is economy, politics is politics, and science is science. In this bounded landscape science would not be intrinsically entangled with other institutions, but would simply establish 'relations of exteriority' with them, a kind of exchange (a give-and-take between 'money' and 'knowledge').

According to this spread view, then, science is ruled only by scientific principles –the rest is expelled to the margins of the context. Once this differentiation is settled, science can be reduced to ideas, and members of the IBF can claim that "science can be done in a bar, with a pencil and a piece of paper"[59]. This sentence brings together all those topics with which a particular epistemology has described the scientific task: decontextualised, immaterial, disincarnated, abstract, and even cheap! –it is a matter of ideas, cognitive achievements, not necessary of sophisticated equipment, and therefore, it does not matter where scientists are –in the lab, in a bar, under apple trees or having a bath- they can receive the Muses' visit and find their own 'Eureka'.

Previous works have made it sufficiently clear that scientists' claims are subject to contextual variability. When it is time to apply for resources, they will emphasise

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[59] It is not fortuitous maybe that this 'idealised' image of science is hold most strongly by PhD students. It seems that the higher in the hierarchy, the more the person needs to take care of others and their laboratories, and the less present this disincarnated image of science is.

their dependence on budgets, whereas when asked about methodology, for instance, they will insist on the independence of results from political influences or economical pressures. The notion of interpretative repertoires (Potter & Wetherell, 1987; Gilbert & Mulkay, 1994) has allowed us to make sense of this contextual variability. We also know that scientists engage in what Latour has called 'a moment of purification' (1988b, 1993): whereas they themselves have had to handle hybrid practices in their daily work, they nevertheless manage to perform disentanglement in certain moments: they can set a bound between 'scientific' factors and 'non-scientific' factors –social, political, economical... They 'see' this ordering limit as clearly as ethnographers, lost in a muddle, perceive its absence. What we want to analyse in this chapter is precisely one of these moments of purification: we will present members of the IBF engaged precisely in the defence and construction of such a boundary, that is, performing what has been called 'boundary work' (Gieryn, 1983).

The hybridising performance of the scientist-entrepreneur provides us with such a perfect occasion. As we will now explain, the figure of the head is perceived as having a foot in 'science' (flow of knowledge) and the other in 'politics' (used as an ambivalent label subsuming all they perceive as external to knowledge, a flow 'nurturing' the group). Given this double positioning, in the head these two domains come dangerously close, challenging narratives of purity. However, her heterogeneous activity does not go unnoticed, and triggers off a great deal of interpretative work to (re)create limits under threat. Precisely this work will be the object of this chapter; we will first present the analysis of their efforts to restore the boundary between 'science' and 'politics', as well as some subsequent symbolic consequences of this drawing of the line, particularly relevant for the figure of the head. Whilst this way of thinking limits is quite spatially inspired (one can be 'in' and one can be 'out'), we will contrast it with another perspective that is also held, though less conspicuously, by scientists, and that presents liminality as an ontological flickering between figure and ground. To end, we will elaborate upon the way in which these two different perspectives are connected.

The mention of the pair 'science-politics' may awaken particular expectancies. After all, the existence of an interface between science and politics, and the particular shape it can take, has kept many scholars busy, becoming a 'big issue' in different approaches to science and technology[60]. Nevertheless, if the readers expect a contribution to such debates, these pages will disappoint them. Here we will neither discuss normative issues of what the relation between science and politics should be, nor suggest possible articulations of political concerns once we accept that knowledge is intrinsically political. Our point will be more modest: we will discuss indigenous mechanisms to create and maintain the differentiation between 'content' and 'context'. We will also discuss different perceptions of how these domains can be reconnected once they are split.

2. Uniting flows: ambiguous hybrids

> "The steppenwolf had therefore two natures, one human, the other wolfish; this was his fate. It can also be that this fate is not so singular and rare. There have been many men that had inside quite a lot of dog, of fox, of fish or snake, without them having major difficulties in this life. In this type of people used to coexist the man and the fox, the man and the fish, the one next to the other, and none of the two would damage its mate; what is more, they would help each other, and in many men who had good careers and were envied it was more the fox or the monkey that had constituted them. Everybody knows this. In Harry, on the contrary, it was different; in him the men and the wolf did not run in parallel, and even less would they help each other, but they hated each other mortally and constantly, and each of them lived to torture the other. And when two are mortal enemies and are within the same blood and the same soul, then it all results in an impossible life. But anyway, each person has his own luck, and none is easy"
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> (Hermann Hesse, *Steppenwolf*).

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[60] See for instance, Braun, 1993; Guston, 1999, 2000, 2001; Jasanoff, 1990, 1996; Mitcham, 1990; Rip, 1994; Scott, 2000. Even STS Summer Schools are devoted to discuss how to rethink the relation science-politics once clear-cut boundaries are rejected (Wouters, Elzinga & Nelis, 2002).

The specificity of the head lies in the fact that the flow she distributes is heterogeneous in nature. Seen as the main source of creation and distribution of 'knowledge' and 'nurture', this figure is the point where the two fluxes unite and flow as one. Thus, she is one of the main locus where scientists imagine heterogeneity coming together –being tacked together, so to speak, without needing a movement of 'like to like': the head does not always move within science, but can relate to otherness, uniting science and money or science and bureaucracy, for instance. One could say that this figure is indifferent to difference, indifferent to heterogeneity (Hetherington & Lee, 2000). It is this 'structural-functional' ability to connect and keep together what is perceived to be part of different orders that approaches the head to the 'blank'[61]. As its name suggests, this is a figure whose value -like in blank dominos or jokers in games of cards- is not pre-determined, but changes depending on the situation, allowing the game to keep on going: blanks are underdetermined. Heads show similar properties since they allow a continuous flow of substance that crosses the perceived gap between science and context: "(t)hey are figures of the between space, communicators that pass between categories of difference as if they were not there" (ibid, p. 170-1).

If the head makes difference commensurable is because she appears as a kind of transformer, a translation centre where external resources (money, contacts) are obtained and converted in internal elements of science (equipment, material, new research). From that moment onwards, what was external becomes internal, that is, what the head sets in circulation becomes 'science'. The head may be in touch with politicians, bureaucrats and private companies to obtain money and projects (and she is seen as dirtying her hands to do so), but as soon as this money becomes 'budget',

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[61] The blank, or also called 'joker', is a notion developed by Serres, spread in the work of others (Hetherington & Lee, 2000; Schillmeier, 2001), to help us think about social (dis)order. I will try to show analogies between some of the characteristics of the blank, and the way we have presented the figure of the head here. Social order usually appears to our eyes as a compact, homogeneous ethos which acts upon us –hence, the difficulties that social thought has to conceive social change. However, this homogeneity is achieved only after certain elements –blanks- have tacked the heterogeneity of materials and people that actually constitutes our orders. It is thanks to their connective properties that

'pipettes', 'HPLC', or 'an invitation' for a colleague to come and give a talk, this nurture converts into 'scientific flow'. If the head has a foot in each domain, below her level one moves only in the realm of science.

As a blank, to the eyes of others, she finds herself in a very specific location: constituted by and trapped in the overlap of several orders, in the "uncertain space of connection" (ibid, p. 18), space which receives several names: the middle which is always a muddle (Cooper, 1995), the in-between space of ambiguity (neither/nor) and ambivalence (and/and), challenging the logic of the either/or (Stainton-Rogers & Stainton-Rogers, 1997). In that place, in short, where she is seen as belonging completely neither to 'science' nor to 'politics' (nor to 'economics' or 'society' for that matter). It is precisely this ability to stand apart from these categories, that is, of not being completely 'defined' or given an identity by one single category, that makes this figure underdetermined, and confers on it this connective property. In other words, the head is conceived as a hybrid, mixing knowledge and nurture.

3. Mixing hybrids, cutting orders

The figure of the head as a hybrid combining science and politics could be taken as a proof or demonstration of the heterogeneous character of science. Science would not work from like to like (from scientific idea to scientific idea) but would bring sameness and alterity together (for instance, scientific idea with money, discovery with patent, research with media, publications with contact, etc.). The hybrid would be pointing at the blurring of the boundaries between science and society, politics, and economics: only one seamless network. This interpretation is interesting, for it helps us challenge the idea that one can separate content and context in the way scientists claim.

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we perceive continuity rather than discontinuity in the constitution of society –blanks help us to override incompatibility.

However useful, though, this interpretation is not devoid of risks. For instance, to claim that one head can be seen as a hybrid mixing science and politics brings about a new problem, which, somehow, is common to all talk about 'hybrids and cyborgs': it may imply that 'science' and 'politics' are things which existed previously to being mixed, and that in a second moment came together –argument which would somehow essentialise these categories. This is exactly the criticism that Strathern (1996) directs to a very naive, and quite fruitless way of conceiving hybrids –as in the woman-snake, the robo-man or some other of the figures which perform in the ambulant circus of (a particular type of) cyborg literature. Interpreted like this, it is no wonder that hybrids emerge everywhere (Narayan, 1993; Papastergiadis, 1995), creating, in her felicitous expression, a feeling of surfeit.

In order to think about hybrids, while preserving the indefinition and heterogeneity of what is being mixed, Strathern will suggest, using (and criticising) some of Latour's (1993) ideas, a possible connection between the notions of "hybrid" and that of "network". For, if both concepts bring heterogeneity together, they do it in a different form: whereas a network is a way of thinking about relations crossing domains of a different order (that is, it connects heterogeneity by extension), the notion of the hybrid is a device concentrating heterogeneous relations into an entity. Thus –and this is the twist Strathern proposes- if a network hybridises orders, we could also say it is an expanded hybrid. A hybrid, in its turn, could be seen as a condensed network: "a network of disparate elements summated in an artefact" (Strathern, 1997, p. 525). Now, with this idea in mind, we are equipped to return to our data.

A head may be regarded as a particularly strong and complex nod in a network which connects many relationships and heterogeneous materials: people, ideas, equipment and machines of all kinds, university and government institutions, libraries, buildings, several laboratories spread out in several parts of the world, telephones and offices, industries, etc. The activity and relations of the head surely encompass more than what one can specify (one always needs to end sentences with "and so on" or "etc."), and they expand through different domains: politics, economy, industry, bureaucracy,

business, media communications, society (and so on), even though to put it like this is already biased: properly speaking, one could not say that the network gathers heterogeneity; this perception comes from an external perspective already assuming the existence and difference of domains (as when one takes for granted that money and contacts are external to knowledge). Rather, a network expands, ignoring already-made divisions, and creating a compatibility between things that under another spatial logic would appear as distinct.

This indifference towards what is united can be extended to science as a whole –the image is known: science as a seamless web. When looked from this network-perspective, mixtures of 'knowledge' and 'nurture' are not perceived as mixtures of two different things, they are simply connections among elements, relations. But the figure of the head calls for a different perception. As Strathern suggests, where we find networks, we may also find hybrids. Indeed, scientists usually imagine the head not as an extended network, but as a punctuated entity, as a person gathering and holding together a mixture of different domains ('science' and 'politics' or 'knowledge' and 'nurture'). The head is made to carry the weight of relations which before were softly distributed throughout the whole network. This condensation in one person has consequences: what throughout the pyramid is not distinguished becomes all of a sudden "heterogeneous" when accumulated in one person.

The position of the head contributes to the sudden emergence of heterogeneity. Crucially, as we know, the head is perceived to be the source of flows of the group. But whereas flow of knowledge is received and passed on from other pyramids, flow of nurture is perceived as coming from 'somewhere else' –members do not know where from or how; they know, though, that it comes from outside the lab: from talks with other heads, but mostly, with politicians, bureaucrats and journalists. The ambivalent head embodies this differentiation of loci: she is not confined to the bench or lab, but moves around, and therefore, she quite visibly changes contexts and audiences. She is seen, therefore, as the point of entrance of 'the outside', the point through which 'otherness' (money, contacts) enter science and join the flow of

knowledge. A necessary source of nurture, but also a source of impurity. Hybridisation: in her, scientific flow becomes heterogeneous.

The head, then, is a complex place. Not only where a difference is made visible (nurture vs. knowledge), but also the place where difference is created: what is united must be first perceived as different, otherwise we could not talk of hybridisation, and this is exactly what the figure of the head allows. Whereas on the one hand she is regarded as a place of mixture ('knowledge' and 'nurture' are hybridised together), it is also a place of cutting: a point where the difference between 'knowledge' and 'nurture' is performed. Exactly at the place where they are thought to intersect, they are conceptualised as substances of a different kind. Thus, the figure of the head is the locus where both, the difference and the union are imagined. The head, then, is the ambiguous place where 'knowledge' joins 'nurture', where 'science' meets 'politics', where 'content' gains 'context'.

'Hybrids' work as a mechanism for summating networks, for condensing extended relationships into manageable categories which allow to set limits to networks which, in principle, would have the capacity of extending for ever –and colonise the world (Lee & Brown, 1994). The act of summation equates to a cut or a stop. Whereas from an etic perspective one can assume that the network of science has the potential of extending endlessly (and scientists themselves are quite aware of this heterogeneity in many occasions), there are many other moments when the scientific community deals with this in-principle never-ending network in a completely different way: they are engaged in setting boundaries to this limitlessness: the network is cut to size.

As a mechanism for cutting, not only does the head allow the perception of two different substances (corresponding to the division between 'content' and 'context') which then have to mix, but she also helps perform a certain purification of those activities carried out in the pyramid. Once these two substances are mixed by the boss, flow below her is perceived as scientific. One could say that all members are in touch with both 'knowledge' and 'nurture': Ph.D. students fill in forms to apply for

material, post-docs negotiate the price of some experimental material with the merchant of a company that fabricates laboratory products, seniors check the budget of the group, and they all participate in networking in conferences[62]. However, only the boss is perceived as dealing with both substances, the rest deal with science, since the boss has already integrated 'nurture' into 'knowledge'. The tension between them, which *could have been felt* throughout the pyramid, is not lived as such, as if ambivalence was made to circulate and accumulated at the vortex of the pyramid. The head, then, is made to carry the ambiguity for all others– she is made to carry the burden of the differentiation.

This has symbolic consequences for this figure, though, as the following anecdote illustrates. While observing experimentation at the bench in one laboratory, I got to know rumours about a light skirmish between a Ph.D. student and his head. Not daring to ask directly what had happened (we were supposed to be interested in the real scientific stuff, and not in gossips!), it took me a while to find out what the cause of the quarrel had been -eventually the Ph.D. student involved told me spontaneously about it during a chat. As most of the 'problems' and tensions emerging within the group, it was provoked by an apparently very irrelevant situation: the Ph.D. student had finished his master, and had given it, once written, printed and bound, to the head. The head did one of the first things we all do: read the acknowledgments –just to find out, to his astonishment, that the Ph.D. student had generously thanked the senior and other colleagues of the group, but not himself –well, the Ph.D. student thanked the head for 'letting him be part of his group', but for nothing else. The head took offence, and complained. News of this incident spread not only in their group, but also to other groups and it became gossip at the coffee machine; however, after a while they forgot about it, and the matter did not go further.

The message sent by the Ph.D. student was quite clear: 'Why should I thank you? What have you contributed with, that you deserve acknowledgements?' With this, the

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[62] Whereas some heads do not exercise any pressure on their people to attend conferences, others heads think this is part of the training of being a scientist and insist that their PhD students participate in meetings. Some even have regular training meetings in English!

Ph.D. student was challenging the transformation of nurture into science: flow coming from the head is not seen as a heterogeneous mixture of nurture and knowledge, but as 'simple nurture'. The head's productivity is disconnected from science and is limited, at best, to nurture: the head may have something to do with the context (accepting him in the group) but not with the content (of the project). The answer of the head was equally clear, though: "What do you mean what I have contributed with? In whose laboratory do you think you are working? Whose senior do you think was supervising you? With whose proteins (both, as theoretical constructs and as physical material) do you think you are building up your thesis? With whose money and material do you think you are working? Do you really think these things have nothing to do with your project?" For the head there is no doubt that any work performed in his lab cannot but include him, since his nurturing activity transforms directly into science, blurring differences between context and content. A reproach on one side, a telling off on the other: a disagreement about the respective contributions.

Stories narrating how the head engages in activities not directly labelled as 'scientific' are not original of the IBF, but seem to be quite typical of laboratories. Thus, in Latour's *Science in Action* we can find the following example[63]: "While she [the head's collaborator] remains in the lab [researching], the head moves throughout the whole world. Is the head tired of laboratory work? Or is it too old to do a worthy research? (This is what is murmured in the lab at the coffee break). The same grumble is devoted to the constant politicking of [a head]" (Latour, 1992, p. 150, our translation). And in another laboratory, that of John Law (1994), we find a similar division between aspects which are scientific and other organisational aspects[64], embodied in the figure of the head, performing the tension between agency and structure. In these stories there are some common implications of how members of

[63] This example, Latour tells us, is not real, but re-constructed with real information.
[64] See chapter 3 & 4 for his difference between vocation and bureaucracy or entreprenship, as distinguishable modes of ordering. Law's argument is different than ours here: he is concerned with understanding how structure and agency are performed (in relation to two forms of heroic agency). But there are nevertheless similarities: first, his data show, like ours, a performed split between 'pure

the laboratories (not the analysts!) interpret them: first, it seems that the identity and prestige of the heads suffer under certain jokes, rumours and criticisms, as if evidence of their involvement in bureaucracy, organisation and politics put their scientific persona under scrutiny. Second, one can read sacrificial undertones: laboratories can develop research of quality thanks to the fact that heads engage in these other non-scientific activities. Our story seems to be quite common, then.

What we see, then, is that, since the boss spends an awful amount of time producing nature, the differentiation knowledge-nurture turns against her: she is perceived as spending most of her time in non-scientific activities. Such quarrels and other anomalies unsettle the conversion of flows into a unique heterogeneous flux and make the division knowledge-nurture visible, leaving the boss in a vulnerable position (closer to nurture than to knowledge). Now, in principle, there is no reason to think that a paper is more valuable or more necessary to science than a pipette, or PCR equipment, or a grant which will allow the group to pay the salary of some Ph.D. students. They all contribute to the flow, and without any of them, activity would stop. That only a part of these embodiments are considered scientific contributes to the personal drama of the heads, who, after working in order to keep their groups alive, are left with the impression that they are not in the 'business of science' any more –but simply, in the 'business'. This is the price the head pays for her performance of ambiguity (indifference to the heterogeneous nature of the flow) and ambivalence (compatibility between flow of knowledge and flow of nurture).

This ontological transformation of the head from 'scientist' to 'entrepreneur' or 'politician' is accompanied by a narrative of sacrifice (Girard, 1972), put to use especially by the heads, who try to counter implicit accusations of lack of scientific character by reminding members of their group of the necessity of feeding[65]. This

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science' and 'bureaucracy' within science; second, he also touches upon, though differently, the hybrid character of the head.

[65] Heads themselves sometimes oppose this prevailing vision (as when they try to increase visibility of their scientific persona in conferences and talks, by showing through comments and questions that they are up to the circumstances). Unfortunately, this strategy does not always bear fruits, for it makes 'repairing work' too visible, as humorous reactions to heads' interventions betray.

narrative displays the following argument: the head gives up research –gives herself in- so that the rest can keep on researching; the group consumes the head, or, at least, the reproductive force of the head. As heads insist, of course they would rather research at the bench, but somebody has to do the 'dirty job' of looking for funding. And to tell the truth, nobody discusses this: whilst people question the scientific character of their heads, they nevertheless insist on their 'irreplaceability'. If heads did not engage in these other tasks, the group would not be able to go on, and they often acknowledge that this care of the group is what is expected from a head (EG02-7). Comments such as "somebody must do these things, and that is why heads are here for" are common, and they all assume that this task of nurturing is "essential... it is necessary nowadays. If you want to get funding, you need to know the game" (EG05-22/3). Heads themselves take for granted the inevitability of their progressive abandonment of the bench to dedicate more time to 'nurture' –even those who try to postpone the moment:

> "I think it is important to try to lose touch with experimentation as slowly as possible... While assuming that one cannot do it all. One has to teach, manage the group... one gets tired eventually. I feel less fresh now than ten years ago" (EI16-23/4).

Everything happens as if sacrifice and ontological transformation occurred *after* crossing the line: from within science to outside -a punishing consequence for a dangerous proximity to a source of impurity. (The strongest the presence of nurture –or the more conspicuous the engagement of the head in activities to produce it- the more uncomfortable this coexistence would become, and the more intensive the pull towards a sacrifice/disposal). Nevertheless, the image of the head as a hybrid that progressively comes out of balance, risking exclusion due to contamination by non-science, reminds us too much of these stories so trenchantly analysed by Douglas (1966). She convincingly showed how ambiguous figures –disturbing clear-cut distinctions- are perceived as 'unclean' or 'contaminated' and subsequently excluded not so much because of their 'contaminating' power, but, more radically, because their acceptance would challenge the boundaries whereupon the community is sustained.

If we look at the data anew with Douglanian eyes, so to speak, maybe we can perform such an inversion: what if the perception of these activities is continuously (re)constructed as non-scientific precisely because the figure of the head is symbolically excluded? Thus, heads would not sacrifice themselves when they step out of science to look for feeding resources; rather, the perception of an outside is recreated through the very sacrifice. Differently put, the sacrifice is not a consequence of the border science-nonscience, but one of the ways in which such a border is performed: part of the head's belonging is sacrificed so that science can remain pure, simply a matter of 'knowledge'.

However, this symbolic sacrifice (partial exclusion from science) does not solve a couple of paradoxical perceptions. For instance, that if the head is a politician or entrepreneur, then they have to admit that their scientific group is led by a non-scientist! Moreover, it is difficult to claim that the figure responsible for the circulation of flow is not a scientist –after all, she has been perceived as the entrance of experience and tradition for a long time. How can they claim she is not a scientist, when, without her, nobody else could be doing science? And more importantly, how can one defend the difference between context and content, if the more the head engages in 'non-scientific' tasks, the better the group can engage in science? In other words, the partial, symbolic exclusion is not always enough to make invisible the perception of the head as a double source and point of transformation of flow; it is not enough to disguise the impossibility to distinguish clearly knowledge from nurture. All 'non-scientific' activities coincide in the figure of the head, throwing into relief a proximity which challenges the view that science is science and the rest is the rest. The classic perspective that science and politics should never meet is endangered: are jokes and rumours enough to stop the contamination power of the proximity of several sources in one person?

In the IBF a particular solution to this problem has emerged. Taking advantage of a particular situation we will now describe, a narrative of powerful effects has been elaborated, that allows them not only to overcome the challenging power of these questions, but also to improve on the distinction content/context. Such stories first gather the heterogeneity of non-scientific practices under one label; thus, all activities related to

'nurture' (such as establishing contacts and organising conferences, looking for funding, negotiating with bureaucrats and discussing with the media, etc.) are subsumed under the denomination 'politics'. This creates a dichotomised setting, 'science' on the one hand, 'politics' on the other. Once this landscape is created, a particular distribution of characters will ensue... As we will now see, the move is 'perfect' enough so as to enable a juxtaposition of 'science' and 'politics' while keeping them together, thus blocking the possible contamination of the first by the latter.

4. "The Perfect Tandem"

Two big groups of the IBF have created a special union –a kind of partial collaboration in order to achieve better results and resources in some fields. This does not mean that the two groups dissolve into one; this is a special union that functions only for some projects and topics of research. The groups still work in separate issues and with a different functioning. Nevertheless, in some topics they collaborate intimately, and when they do, it is difficult to distinguish when one group finishes, and the other starts: divisions blur. As one senior summed up, "when they merge, this is neither one head nor the other: the group is both of them" (EI10-06).

Members of the IBF, regardless of the group of belonging, consider this union as something quite positive, not only because collaborations are usually regarded as a good to be pursued, but also because, as they say, these two specific heads "combine very well" (EG02-19), they "complement each other very well" (EI09-11; EI07-21), because "what one lacks, the other has it" (EG05-22). The goodness of this coupling is so conspicuous, that members of the IBF have given it a name. This being together, this "neither one head nor the other, but both of them" has been labelled 'the Perfect Tandem'.

When describing it, people invariably refer to a special 'division of labour', a kind of specialisation of the two heads *only when they collaborate*. As the story goes, one of

the heads has assumed preferably tasks related to research, whereas the other devotes a lot of time to "public representation", to the "politics of the centre" or to "administrative tasks" –all of which is subsumed under the label 'politics', as opposed to 'science'[66]. This is a differentiation that appears often in the IBF, at several levels, including the heads:

> "What I like is doing science, not politics; if you want to have a good level of productivity and make interesting contributions to your field, you need to dedicate a lot of time to your work. If, simultaneously you need to have a public presence, a political life, with performances which demand a lot of time and 'corridors', then... it is very difficult to combine both things" (EI01-08).

According to this head, there are two different activities to be engaged in. One is science: "to keep a good level of productivity and make interesting things, and make contributions to your field"; the other is politics: "a public presence, a political life, a life of performances, which demands a lot of time and 'corridors'". They are two clearly distinct activities and they are, moreover, antagonistic, in the sense that the more time you dedicate to one, the less time you can invest in the other. In spite of the frequency with which such distinction appears in their argumentations (especially when they talk of the functions of the heads in general), members of the IBF never offer explicit definitions of this division; there is never an attempt to clarify what counts as 'scientific' and as 'politics'.

Labels are always kept under ambiguity: under 'politics' they sometimes mean 'contacts and networking'. At other times, they mean the search for resources in general, and those activities related to economical concerns. Still at other moments, 'politics' entails representation in front of society, media or public organisms. This ambiguity allows the dichotomy 'science'-'politics' to work to similar effects than

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[66] We should not fail to notice a tension in this account. Whereas people in principle refer to their joint projects, they also imply that this distribution of tasks may expand beyond strict collaboration –as if, in general, one head took care of politics and the other of science. However, this extension exposes the absurdity of the claim: a group with a head engaging only in 'science', or only in 'politics' would not survive. Yet, we do not try to evaluate the adequacy of the claim, but its symbolic effects on circulating.

the differentiation between 'scientific content' and 'context', provoking the effects we will expose next. If nobody needs clarification of what is meant by this indefinite pair is partly, I guess, due to the 'taken-for-grantedness' of these notions in our culture; I myself am a case in point: I 'understood' what they meant and took this distinction acritically. Here I paid a price for my lack of distance: I went native.

Personalising

Accounts dwelling on the Perfect Tandem perform several divisions: not only between 'science' and 'politics' ('science' and 'context'), but also one between the two heads, for accounts also offer an explanation for this labour of division: if they take charge of different aspects is because the two heads are (portrayed as being) 'completely different'. They are said to exemplify two different types of leadership (two extremes of a continuum) differing in the degree of control and autonomy they grant to members of their groups, in the style of competence vs. collaboration they promote regarding other groups, the amount of visits abroad their members take up, etc. But above all, they are supposed to differ in personality. Whereas one is closed in himself, focused in his work, and interested in increasing the competence level of his group, the other has an expansive character and searches actively for relationships and collaborations for his group. Therefore, so they say, their union is so beneficial because it allows each of the heads to concentrate in that which 'comes natural to them' and this is why, if we remember the beginning of this section, members of the IBF perceive that the two heads 'complement each other' and that "what one lacks, the other has'. And not only members of the IBF, but the heads themselves seem to believe so:

> "Maybe this is why we complement each other so well, because he is a person that enjoys devoting efforts to research or scientific production more than to the work of human relations. And I am not a very expansive person, but more than he in any case..." (EI01-07).

In this way, the accounts inscribe organizational divisions into personal tastes and personality. This is how the narrative of the Perfect Tandem divides 'science' and 'politics' and attributes each of these 'tasks' to one person: the expansive character gets politics; the less social one, science. This personalisation allows emphasising the radical difference in nature of activities: the two activities are as different from each other as the people performing them.

Inversing interpretations

It could be interesting, as modern symbolic interactionism would recommend, to see what this narrative is making invisible. For instance, and surprisingly, not only that the head perceived as doing 'simply science' needs to engage in exactly the same kind of nutrition practices as the rest, but especially, that he is well known for negotiations with the Vice-chancellorship and for achieving agreements with private companies on contracts and patents! Or, for instance, that the head allegedly doing 'politics' leads other projects beyond collaboration and that he does not engage in these 'political' activities out of a personal liking, but simply, for the same reason as any other head does: because they have no choice if they want their groups to bloom. Whereas the whole story is a creative rewriting and magnification of a *particular* sacrifice story –the big head giving himself in so that the institution progresses- it simultaneously diminishes the general sacrificial position of every single head: again, this head accumulates and carries ambiguity. This narrative diminishes the visibility of nutrition activities (to be found in every group, for all heads *must* engage in them), and magnifies its visibility accumulating them all in one figure: a 'political head's. The question remains, what does this peculiar distribution accomplish?

Let us follow again some of the accomplishments of the Perfect Tandem. Firstly, the narrative brings together two independent teams, in a particular type of union. For some projects they remain two single groups, for others they act as a single team. And when they do, the union seems to transform both groups in a way in which, as

our informant put it, "this is neither one nor the other anymore, but another thing". This move resembles the emergence of a third entity proper of hybridism and boundary crossing: two heads together create a 'neither-nor'. Thus, this hybrid union performs a first, expected ontological metamorphosis: before uniting themselves, both heads were scientists leading a smallish group; after uniting, the groups become 'a potent group' and heads turn into 'big scientists': both heads get more resources, publish more, they gather prestige. A tandem (we will later see why it is also perfect).

This is one of the directions in which ontologies change: from scientists to big(ger) scientists. However, this is not the only transformation that the narrative accomplishes, since, as we have seen, this union performs a new split very rapidly: a division of labour which is legitimised on personality differences. What is first regarded as a hybrid union, ("neither one nor the other") is split again ("the two heads are very different"). The reader may wonder why this narrative makes the heads unite just in order to split them afterwards in different functions. This is, however, a necessary step. If people did not perceive the heads as 'uniting in a tandem', they could not sustain the argument of the division of labour. A head engaging purely in 'science' would not be able to push her group further and neither would a head engaging purely in 'politics'. Without the union that allows a distribution of the two activities to two different people, one could not split 'science' and 'politics' without making the absurdity evident. Thus, this split does not render the previous union useless, on the contrary: *precisely* because the Tandem is working, you can tell one from the other- one head here, the other there; science here, politics there; content here, context there. The division of labour turns into labour of division (Hetherington & Munro, 1997).

The split that the narrative performs presents –through quite magic a trick- the two heads in a different light. One of the heads is the representation of the pure scientist: even though he is the head of a huge group, and has to sustain it economically, he nevertheless manages to keep his hands clean from anything that is not research. The other scientist cannot help but submerging slowly but surely into the sea of politics

and contacts. With this move the hybridising narrative of the Perfect Tandem makes ontologies change in a different, unexpected direction: one of the heads becomes more of a scientist, the other becomes more of a politician. Thus, whereas members of the IBF tell that these two people have united because their personal affinities complement each other, under our interpretation their identities as politician and scientist can emerge only because they are united. We have, then, a hybrid that instead of mixing its components in a blurred entity renders them distinct and ontologically different! This is an effect which has already been detected before (Latimer, 1998)[67]: a hybrid construction does not necessarily imply dissolution and a deletion of differences. The hybridising Perfect Tandem gives way to two clear-cut identities, subject to different symbolic effects.

As we have argued, the normal perception of the head of a group as a hybrid mixing knowledge and nurture, content and context, or science and politics endangers continuously these achieved divisions, since these two different sources coincide in the same figure, challenging the inherited notion that science and politics should remain distinct. In this context, the narrative of the Perfect Tandem brings the division further. Through the elaboration of a one-to-one mapping between personality and dedication, one head is identified with politics and the other with science; with this very same move, politics and science are kept away from one another. If a normal head must mix 'knowledge' and 'nurture', creating a unique heterogeneous flow, the Perfect Tandem allows the perception that these two flows separate clearly: one head produces and distributes 'knowledge'; the other, 'nurture'. In other words, the narrative of the Perfect Tandem completes a move that is only partially and ambiguously accomplished in each head, the differentiation of flows,

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[67] Latimer's paper shows how the hybrid works differently for the identities performed through hybridisation in the case of joint work by doctors and nurses in hospitals. In the case of the doctor-nurse unity, the author shows how the doctor embodies the technical, the nurse the social, and how this distinction is not weakened with the hybridisation, but, on the contrary, enhanced.

and it allows the purification of science, in the sense that 'politics' (and with it money, relations, context) is extracted from it[68]. Content and context are distilled.

This account also reminds members of the contamination power of 'politics' (Douglas, 1966), focused on those figures that, due to their ambiguous position, endanger those classifications on which the community is sustained. A narrative that makes sure that performances of ambiguity do not make the rest forget what should remain 'other'. A disposal of otherness that reinforces –patrols, cleans, reinstates- the boundary and purifies the identity of that which is perceived to remain 'within'. In this context, we can understand all these stories about "the perfect tandem" as a narrative, a cultural device, which, in its circulation, allows reparation (Garfinkel, 1967), the reestablishment of the ordering principle in which a scientific laboratory is sustained: that of the clear-cut distinction between science in its cognitive, naked-idea aspect, and that of the economical-political-bureaucratic constraints. No wonder this tandem is 'perfect': it allows the articulation of science and politics while simultaneously performing the difference between them. With every circulation of this story, science is purified.

5. Merographic & partial connections: mobility vs. motility

At the end of the first part of the chapter we have hinted at two different ways of conceiving the scientist. The traditional one would see an individual performing roles; the alternative offered here has presented the scientist as a persona in extension, and we have suggested that both figures coexist and are partially articulated. My proposal in this section will be twofold. First, I will suggest that the sacrificial narrative and the Perfect Tandem elaborate upon the first approach, whereas the image of the scientist as an extended persona makes possible another way of conceptualising the figure of the boss. Second, I will claim that, interestingly, each

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[68] The point is not that they label particular practices as either scientific or political. Rather, this divide allows them to play *as if* these divisions existed and ordered people. Through this cultural device, the very creation of the categories 'political' and 'scientific' is in play).

portray presents the boss as reflecting in herself a particular relation between science and politics. The first description is sustained upon what Strathern calls 'merographic relations', which we will proceed to describe briefly. The second is based upon partial connections. The rest of the chapter will present both characterisations, and will discuss the way each imagines the relation between science and politics.

The story of the sacrifice –of the head that, in her attempts to find nurture, ends up abandoning science- deploys a particular plot which can be imagined spatially. As a hybridising figure, she is located in a kind of overlap where several domains intersect. In the head, different fields meet and cross, and she needs to know how to manage in all of them: science (creator and leader of a group), politics (contacts and negotiations), economics (looking for funding) and society (public opinion, communication with media). The head, then, needs to set a foot in each, so to say, she must engage in all these activities which are perceived as 'nonscientific'. Or, to put it in a familiar vocabulary, the scientific persona needs to acquire and perform 'roles' that extend her activities from science to non-science. All these roles are acquired: once the scientist is in place, she will learn how to manage in neighbouring domains. This idea reproduces the sequence that our Western individual proposes: first the individual, second come relationship; the scientist core is there, and afterwards it enters into relation (be it politics, in networking, in searching for resources, in maintaining communication with the media, etc.). 'Supplements' making her identity more complex (-in a completion-deletion move [69]). This idea is even suggested by the progressive pattern of acquisition of roles through time, and the distribution of dedication to each area[70].

The effect is as if she moved out of science towards the domain of economics, society, politics or the media momentarily. However, these movements from science to nonscience should not alter the boss ontologically: the head must remain a scientist

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[69] I write completion-deletion, since we know that each adding of a part and of information it may also result in a losing of capacities or information: the incorporation of the roles of the politician-entrepreneur-bureaucrat has consequences for the scientific persona.

[70] Merton and Zuckerman (1972), for instance, finds that time devoted to research decreases along one's career, whereas dedication to administration and teaching increases.

at her core[71], or, if you want, the scientist must always come back to her centre. One can engage in politics in order to obtain good deals for the lab –but one should not become a politician. One can search for resources –but one should not become a simple entrepreneur. One can be in touch with the media to increase prestige of the lab –but one should not overdo it and become populist (hence, the dodgy position of a scientist that only does 'divulgation' instead of research). This movement to the 'exterior' looking for nurture has only one limit: the possibility to return. The head steps into politics, but she comes back to science; she may step into public relations with media, just to come back to the lab -a movement with return.

This hybridism of roles and identities puts her belonging at risk: will she still be perceived as a scientist? The danger involved in the ambivalence of their belonging is kept at bay as long as the group has a 'manageable' proportion that allows the head to keep on investing time into activities considered 'knowledge'. Then the head is still able to perform 'science' in front of her group; she can keep the difficult balance between roles that must remain centred around the scientist. However, this will become more and more difficult to achieve when the group increases dimensions, then it will demand and absorb most of the reproductive power of the head[72]. Then, the head may invest more efforts in searching for contacts, funding and resources than to supervision and keeping up with literature and research. Thus, even though the head's productivity does not decrease in the least (if anything, publications increase), it does change the balance between both principia –and it is this change that is symbolically dangerous. Since nurture is perceived as non-scientific, the more efforts the head dedicates to feeding, the less 'scientific' will her performance be regarded as; after all, she is left working all day long in allegedly nonscientific tasks!

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[71] This image is not alien to psychology. It can be found, for instance, implicit in Tajfel's theory of social identity, visible with his distinction between interpersonal behaviour and intergroup behaviour. As some commentators have observed, this distinction suggests that interpersonal behaviour may be closer to the personal being of the individual than intergroup behaviour: "This problem is compounded, as Schiffman & Wicklund (1992) note, by Tajfel's tendency to envisage multiple social identities hovering around the inter-group pole in strong contrast to more unified and solid pole of inter-personal behaviour. With this comes *sense of a 'core' self being extended toward a peripheral grouping of less well defined 'social' levels*" (Brown & Lunt, 1999, p. 6, my emphasis).

[72] The bigger the group, the more external tasks the head must engage in –let alone if the head occupies a management post (such as being the head of a department, or the director of the institution).

In other words, there will come a moment when the head will be perceived as taking too much distance from her scientific core –she has gone too far. The head's go-and-return incursions into other domains give way to a movement without return, where the persona does not step back into the scientific core. Thus, this narrative explains the sacrifice/disposal of the boss as a kind of decentring, producing the perception of an excess of a sort: the head becomes less and less of a scientist, and more and more of an entrepreneur-politician-public relations... In this account, then, the head appears as a mobile figure, moving in and out of science.

The figure of the head and her movements between domains, as well as the Perfect Tandem, with its division of labour, offer a native model to conceptualise the relation between these domains. For a start, as we have noticed, they are perceived as intersecting –and it is precisely in this intersection that we find the hybridising figure of the head. But domains can only intersect if they are perceived as distinct. Thus, the head offers a way to imagine a relation between heterogeneous fields. If she allows imagining how domains overlap is only because at the same time she is making the boundary between them visible. To understand the nature of this connection between domains we will, once more, turn to Strathern, particularly to what she has labelled 'merographic connections'. This notion is complex, and a more extended elaboration of it can be found in Strathern's *After Nature* (1992a; see also 1992b).

In spite of this disorienting denomination, the type of relations described should be rather familiar to us, since -unlike her partial connections (1991)- merographic connections account for a common Euro-American way to perceive relations between entities or domains. Whilst, as we will now see, they also involve partiality, they are also different from a part-whole relation. Two domains connected merographically, then, can be neither identical nor unrelated nor subsumed into each other. But let us unfold this notion more slowly. If we think of an arm, we will probably agree that we conceive of it as a part of the body, there is nothing in the arm that it is not encompassed by the body. But this is not the only way we have of thinking relations.

We often imagine domains to be different but related, in the sense that both domains have, simultaneously, something in common and something that differentiates them. Depending on the perspective adopted, we can see similarities or we can see differences. This ability to change perspective allows the perception that none of these domains subsumes the other completely (for if they did, then their relation would be one of part-whole). The connection is just partial, and hence, there is a certain overlap between these domains, but never coincidence –thus, none of them can be reduced to the other.

An example should simplify the explanation. A person is part of nature, but not only, for she is also a part of culture –and from this perception we deduce that nature and culture are overlapping in the person, but they do not coincide totally: there is more to nature than culture, there is more to culture than nature. Thus, we imagine nature and culture to be two analogous domains (independent fields with rules of their own) and different at the same time (one is connected to physical events, the other to human ones). Culture is part of nature (after all, it is a product of human beings) but not only, for culture extends beyond it (hence, the power of culture to improve on nature). Nature is part of culture, because culture has the power to signify nature; but not only, for there is more to nature than culture –the natural world. Thus, culture is both more and less than nature –and the other way round, nature is simultaneously more and less than culture, depending on our perspective.

Every time we establish connections between two entities, that is, every time we partially categorise something in something other, the effect is like projecting a new perspective on the entity categorised. For instance, when we insist that science is part of society, we are projecting a social look on science. If we emphasise that science can encompass society, then we are projecting a scientific look on society. To say that the individual is part of society allows a social look on the individual; to say that the individual explains society will produce an individualising look on society. Similarities between nature and culture allow for a naturalising look on culture and a culturalising apprehension of nature. Thus, each perspective has an effect of

contextualisation: to look at science socially is to set it against the background of society; to look at nature culturally is to contrast nature with culture. Automatically, the entity and the context emerge as distinct: science and society may have points of contact, but they are different domains.

If, as we implied at the beginning of the chapter, the notion of 'science' as a domain has resemblances with that of 'society', it may be a hint at the fact that connections between them are imagined as merographic. Science is part (and only a part) of society (it cannot escape social context and social influences), but there is more to science than society (for instance, nature), and therefore the identity of science cannot be reduced to the social. At the same time, society can also be imagined as part of science, as when we claim that Western society can only be understood if we account for the main role of science in its bosom (as when science is established as the difference between our civilisation and others –see Latour, 1993), without society being reduced to science, for there is more in society than science (for instance, conventions).

This is precisely the type of perception that the figure of the head both reflects and suggests. As a locus of overlap of different domains, she also allows to imagine how these domains relate: if science is seen as part of society, society is also seen as part of science (e.g., social influences in knowledge, biases of scientists). If science is part of economics (as national budgets show), calculation is also part of science (as heads and PhD students having to rationalise their resources well know). If science is seen as part of politics (as electoral programs reveal), politics is part of science (as they acknowledge on attributing importance to collaborations and alliances). What is more, 'politics', 'economics' and 'society' are also perceived in an analogical relation, in the way they all are thought to impinge on and influence 'science'. They all are simultaneously more than science (in the sense that they extend beyond it) and less than science (in that they are but factors influencing it), but above all, they are distinct from science.

Hence, by relating all these domains partially -each of them being part of another, but none of them encompassing another wholly- they can accept the influence of 'external domains' on science, without having to reduce the latter to any of them. Politics may use science as science uses politics; economics may take profit of science as well as science may make profit; society 'benefits' from science as much as science finds a 'niche' in society; media use science exactly in the same way in which scientists use media... Nevertheless, 'media', 'society', 'economics', 'politics' remain 'domains' external –albeit in merographic connection- to 'science'.

Merographic connectio ns help us understand why some of the proposals in STS are colliding against 'established sentiments' among scientists, as Stengers (2000) puts it. While these types of connections allow them to admit of points of contact between domains, it also gives them strength to fight reductions. To pretend that science is *only* social is to pretend this relation to be a part-whole one, as if society encompassed science completely, ignoring that there is more to science than society. Whereas scientists would accept that science can in some occasions be 'politics by other means', to pretend that there is nothing else in science than politics would arise protests. Of course one can analyse science from an economical perspective, but any equation between science and pure cycles of production would be perceived as reductionist. One can project social, cultural, political, economical perspectives on science: they will reveal the complexity of science's identities, but in no case does it reduce it to these domains: there is always more in science –nature! Rather, they remain 'context' against whose background one can think 'content'.

If we stopped here, we would have quite a nice account opposing etic and emic perspectives. Whilst we scholars could defend a network perspective that does not recognise boundaries between science and politics, scientists would perceive science and politics as two different domains with some overlaps. The figure of the boss would be placed precisely in this intersection, a hybrid being which, for scholars would be an occasion for the reconstruction of (inexistent) boundaries, and for natives would always be threatened by the danger of stepping out of science. A sacrifice of

disposal to protect symbolic limits for the first, a sacrifice on crossing dangerous limits to feed the group for the second.

However, there is something partial in both the sacrifices of the bosses and the exclusions performed through narratives. When we say that the boss sacrifices herself (in the sense that the bigger the group, the less time she has for science) we must nevertheless admit that this is a sacrifice only from a particular perspective. After all, the heads' salaries are better, their curricula and careers go on progressing, they have stable jobs, they obtain prestige through the job of the group and their representation activity, and their personal curriculum increases parallel to the productivity of the group. Whereas in some occasions the head may be regarded as more 'entrepreneur' than scientist, she may remain one of the most recognised figures within and outside the laboratory. Something similar happens with the disposal performed by the narrative of the Perfect Tandem. When they tell it to themselves, they may be reconstituting boundaries. However, the boss they are portraying as lost beyond limits remains one of the most renowned scientist of the IBF, and the same people who tell us that he is more of a politician can remind us in another context that nobody has as many and good publications, knowledge and experience as he has.

This other perspective reopens the question of the relation between science and politics. The previous account about mobility pictured science and politics in a tense relationship, almost antagonistic: the more time one dedicates to science, the less time one can invest in science. Now, the relationship suggested by these comments changes. Dedication to politics is not seen as detrimental to science, on the contrary, her position and curriculum is positively altered by this hybrid dedication. The more one dedicates to politics, the stronger the scientific persona becomes. If from a merographic perspective being a politician is a diminishment of the scientific persona, now, in this new perspective, being a politician is an extension of the scientific persona.

With this other account, then, the image offered by the head is different. She is not caught in the overlap of domains, travelling from one to the other. Actually, this image cannot be apprehended with a spatial metaphor of movement, but with one of translation as transformation (see chapter 6 for an extension of this argument). Under this light the head does not take up return-trips between science and politics, or science and media, always at risk of going too far and not finding the way back home –always at risk of losing her true identity as a scientist. Rather, the head experiences ontological transformations, a flickering movement, her belonging oscillating like that of Gestalt figures: now she is a scientist, now she is a politician, now a strategic entrepreneur or a daring public relations. If merographically being a scientist means to remain centred, now being a scientist is not occupying the core –there is no centre, just a switching of perspectives.

The visibility of these portraits changes depending on the perspective that juts out: at some moments, the head will be perceived as an entrepreneur, at others, she will be again the scientist in charge of the group. At some moments she appears as a scientist through and through, especially in front of colleagues; at others, particularly for Ph.D. students, she may be too much of a politician. She feels her identity switching from one to the other –being considered a scientist when it is time to give advice to the group or correct papers, or when she is giving a talk as an guest speaker at a meeting; being considered a bureaucrat when it is time to discuss with the administration of the university; being considered a politician when she has to convince the vice-chancellor about how good for the whole university it would be if the IBF had the chance to offer one more service; and so on. The identity of the boss cannot be reduced to that of 'scientist', neither to that of 'politician', nor to an 'entrepreneur' or a 'mass communicator'. The flickering between figures does not resolve into one, there is no perspective more true than another, though each of them leave imprint in this persona. If under a merographic perspective the head appears as a *mobile* figure (crossing boundaries in and out of science) we could now talk of her as a *motile* figure (Munro, 1999, 2003): suffering transformation without movement from one place to another – that is, we find once more movement as being affected.

This other way to conceptualise the figure of the head offers a new relation between science and politics, that is not merographic anymore, but suggests another articulation –which our reader might by now have guessed: science and politics can be imagined as partially connected in a prosthetic sense. We can say that this relation has something instrumental, in the sense that both science and politics can be put to work for each other: politics may extend scientific possibilities, as well as science may extend political possibilities. The head that becomes a politician or a public mediator or a bureaucrat extends scientific possibilities[73], while the head that becomes a scientist may extend political, economic and communication possibilities through science. A mutual extension: a becoming attuned that makes several practices come together to enable new possibilities of being, without any of these being reducible to the others. However, lack of integration does not entail a collage of unrelated pieces, and we know that the emergence of one perspective is sustained by the simultaneous existence, though displaced as background, of the rest. Only one perspective can be present, but the others are never totally absent, sustaining the others. When they see politics they know that science is in the background; and the other way round, when heads perform science is always against the background of politics.

To think the relation between science and politics or economics in terms of partial connection also involves accepting a mutual engagement - a more intimate connection between these activities, that goes beyond the 'mere gathering of resources'. When the boss is doing contacts, she is doing science in another way; engagement in politics is not only a search for resources, but it has effect upon scientific content (Knorr-Cetina, 1981; Stengers, 2000). To start a collaboration with a foreign group may mean to abandon one's topic and work in something new. To have a contract with a private company means to accept the imposition of a topic as well as a particular approach to it. To obtain funding from a particular institution may

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[73] It is also good to keep in mind that here too extension does not necessarily mean positive 'potenciation'. History offers us enough examples of how an intimate connection between science, politics, economics, bureaucratisation and propaganda can result in a diminishment of science.

involve an acceptance of the institution's priorities in research. To publish a paper in the media may be necessary to change orientation and interests, etc. These are the type of intimacy which an account in terms of independent domains tends to hide.

It seems then, that we can draw two different models or perceptions of the head of the boss, and through it, of the way science and other practices relate to each other. Therefore, as analysts, which should we privilege? Which one should we suggest scientists adopt? Is the head perceived as a figure that, through her feeding activity, enables science at the price of stepping out of it some when? Or is she the most scientific character of the group, challenging the very division between science and politics? This question was hunting me during analysis and writing –though driving me crazy would be more precise. I could not fix the IBF perspective; when I was about to decide that they took boundaries for granted, I had the feeling I was depriving them of a flexibility that scholars attribute themselves with (as in 'we see the lack of limits, but they are still stuck in an either/or logic'); when I was resolute to imply that scientists themselves can challenge these divisions, I felt as if I was projecting our theoretical concerns upon them (was it not obvious instead that they set clear limits to science?). Until I thought that if I was suffering under this hesitation, flickering from one conviction to the other, perhaps this was not casual. Perhaps this oscillation is to be preserved instead of dissolved by (our) choice.

What if, quite simply, scientists are able to switch perceptions? Now they see a "mobile head" that passes along in tension through domains merographically connected, now they see a "motile head" that hybridises and extends domains. When it is a matter of extending capacities, scientists will perceive their relations in terms of partial connections; when motility is so visible that would challenge symbolic boundaries, the Partial Tandem and its merographic associations will absorb ambivalence. They switch without any of these images enjoying any privilege regarding the other –none is truer than the other. Which is another way to say that this very switching of perspectives is partial, too. While they can put both perceptions to use, they cannot do it simultaneously –now they can see domains in tension, now they

can see extensions, now they see a mobile figure, now they see a motile one. Hence, to the ambivalence of this plurality of perceptions we have to add the ambiguity of the lack of resolution: neither one figure nor the other.

1. Connecting proteins

It has long been thought that agreement and consensus -reached through collective discussion- were necessary characteristics of the normal functioning of science. This idea is in accordance with a conception of science as an enterprise whose results enjoy universality, given that –it is assumed- nature speaks with only one voice: since the world is considered unique, singular and ruled by universal laws, different scientists, at different moments in time, in different places, will discover the same. Where differences in result emerge, they are surely due to mistakes and misconceptions, and further scientific progress will select those knowledge versions (theories, models, hypothesis) which are in better correspondence with observed facts, and dispose of inappropriate ones. The final version can only be one: the truth – rhetorical argumentation which Gilbert and Mulkay (1994) called "the truth will out" device. Put in other words, if there is only one reality to comprehend, any possible initial diversity of results must progressively converge towards homogeneity, and scientists, through discussion, will eventually agree upon the nature of this reality, reaching consensus on their positions. This view has been a predominant explanatory heuristic.

Nevertheless, this belief has been disturbed by researches and theoretical achievements coming from several sources. Not only from constructionists, deconstructionists and postmodern approaches, but also from contributions by the very natural sciences. Studies on Science and Technology have sufficiently emphasised the local, situated nature of both, knowledge and the very reality that science contributes to bring into being[1] (which, of course, amounts to a questioning of the division between knowledge and reality); contributions insist on the contextual, emergent and always-on-the-making character of the world, as well as on its multiplicity, challenging images of singularity and unity (Law, 2002; Mol, 2002b).

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[1] This claim lies at the heart of STS studies (Knorr-Cetina, 1981; Latour & Woolgar, 1979; Lynch, 1985a).

But then, these assertions raise important questions: if discoveries, ideas, theories, and scientific objects are contextual, it is not that easy to understand how scientists all over the world agree upon the validity of certain results. Consensus among scientists as well as convergence of results cannot be considered logical consequences deriving from the characteristics of nature, but a perplexing achievement. Locality on the one hand, multiplicity on the other. Then, how is consensus to be reached? How do scientists manage to work together?

One answer has been that of Gilbert and Mulkay (1994), who chose to focus on the question of agreement among scientists; using discourse analysis, they have shown how interpretative resources are used to construct the realm of collective phenomena. Their analysis shows how the production of consensus involves an impressive work of production of accounts on action and belief. Statements about agreement within the scientific community usually involve three elements:

> "First, in claiming consensus it is implied that the speaker has identified all the relevant members of the field, that is, all those competent scientists who must be considered as working within the area of investigation in question. Secondly, it is implied that the speaker can attribute scientific belief correctly to each individual scientist. Thirdly, it is implied that the cognitive content of the consensus can be specified accurately and shown to coincide with the views of all those who are said to belong to it" (ibid, p. 118).

Differently stated, consensus is an interpretative achievement. According to this proposal, in order to claim that there is agreement regarding a particular issue, scientists do not make claims about the topic in question, but rather, about the social field that will have to sustain this consensus: they limit who is entitled to participate in the discussion, they set themselves up as the right informants of this discussion, and they define what the object of agreement is. This third point is important: accounts do not refer to an external object; it does not actually matter whether scientists refer to the same, as long as they convey the impression that they *talk* about the same:

"The appearance of scientific agreement between these two researchers is maintained only at the terminological level. It is accomplished by their both using the same theoretical label, namely, 'chemiosmosis', to refer to scientific interpretations which differ considerably in substance" (ibid, p. 130).

Thus, whereas scientists may use the same notions to discuss, it is in no way clear that the meanings linked to such notions are shared. What is more, even the same scientist often oscillates between an idiosyncratic and a consensual version of a theory (swapping which enables the person to emphasise the originality of one's proposals, while claiming to be simply contributing to accepted knowledge). In this way, these authors remind us of the level of ambiguity impregnating not only discussions, but also theories, technical and knowledge arguments... This ambiguity allows differences to coexist next to agreement and unanimity. What Gilbert and Mulkay do not elaborate (simply, because it is not their aim to deal with such issues) is the fact that these accounts only rarely fly solo in scientific skies, so to say. Accounts are certainly crucial, so much so, that one cannot think of any scientific production that is not filtered through them –encapsulated in narrative (Knorr-Cetina, 1999).

But in laboratories, these accounts are mostly accompanied by other type of artefacts elaborated at the bench or on the computer (other artefacts that, albeit being linguistic themselves, cannot be reduced to words). Scientists usually demand from colleagues that their accounts do more than represent "versions of the state of collective belief" (Gilbert & Mulkay, 1994, p. 139); they demand the achievement of a type of link between accounts and a state of affairs –a link between the word and the world (Latour, 1999; Stengers, 2000). After all, accounts are associated, connected or linked to wider assemblages. Thus, the question regarding how scientists manage to work together seems to require more than 'consensus' as an answer.

By this we do not mean to imply that scientists refer to an external object 'out there', to which their knowledge corresponds, as in the classic interpretation of

representation. Simply, that scientists must exhibit a link between their interpretations and those interpreted objects, link which must be elaborated in the same process of construction through which 'objects right here' emerge. Once we accept that representation is a contextual achievement (Lynch, 1994[2]), the task can be expanded to observe how this local accomplishment is extended to create possibilities of collaborative working.

This argument does seek to belittle neither interpretation nor the crucial importance of symbolism. We know that artefacts per se do not signify; facts are not in themselves talkative, and interpretations are required so that they make sense. But this argument cuts both ways: interpretations need artefacts so that the first have something to make sense of. This is why 'working together' involves a domestication or disciplining of accounts, but also of materials. Not only accounts must be coordinated to create consensus, but also those artefacts themselves, so that they enable and disable particular interpretations. Scientists, as we will show, weave words and interpretations, but also numbers and images, amino acids and graphics, electrophoresis and radiographies, interpretations and artefacts sustaining them. The same flexibility that Gilbert and Mulkay (1994) attribute to accounts can also be granted to heterogeneous chains of associations (that never go beyond, but are neither reducible to, language). Hence, STS contention on the importance of the engineering character of reality (Law, 1986a, 1986b, 1993), inseparable of power.

These concerns are at the base of the present chapter. In the next pages we will narrow our focus to lab workshop and lab negotiations so as to explore scientific collaborative work: the type of work which, if it does not necessarily produce consensus, it does allow the emergence of a common universe upon which scientists imagine that their collective enterprise is sustained. In order to analyse what type of

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[2] In a challenging paper, Lynch (1994) argues that (social constructionists) attempts to show that scientists do not represent reality are still prey of a representational logic, since scholars try to give a better picture of how science works, namely, that 'in fact', scientists really do not represent. Instead, he proposes to focus efforts in looking what we are *doing* when we talk of representation. Even though proposals differ, in this respect he shares Callon's (1997) advice to avoid the dichotomy realism-relativism altogether.

relations are involved in collaborative work, while paying particular attention to scientific engineering, we will show some "short cuts" of research (in the cinematographic sense) by several groups on (what they consider) the "same" object.

In particular, we will follow the vicissitudes of a classical object of study in the IBF: a potato protein they call PCI (potato carboxypeptidase inhibitor). The advantage that this protein offers to us is that it is studied by several laboratories: 1) a laboratory working experimentally with protein engineering; 2) a laboratory working theoretically with computer simulations; and 3) a laboratory working experimentally with crystallography. The first two laboratories, of which we can offer observations, belong to the IBF. The third does not, but IBF members collaborate with it, and they facilitate us some information (enough to understand conjoint work, but not enough to hide the asymmetry in quantity and quality of our description when compared with the other two).

Each of these laboratories uses different methods to approach the PCI. However, since their perspective and results vary, the door is open for a perception of multiplicity; this calls for negotiation, so that eventually a unique PCI can emerge out of three very different settings. Precisely this negotiation or weaving work to coordinate results will be of our interest. To make our point, it will be necessary to go into some detail to understand the methods they use and the way each method portrays its object of study. Whereas some explanations of specialised knowledge are unavoidable if one wants to understand where my claims come from (and have the chance to disagree), I have tried to simplify biological and technical procedures as much as possible (even though not always successfully), for I take it that readers are not so interested in biological information as in some of the insights their practices may offer us.

In order to illustrate the plait of practices that weave the PCI, we will need to account for variability, variability intrinsic to the different type of laboratories. However, and in spite of this, we do not want to provide a complete, global picture of the

characteristics of these three different settings. In other words, we do not aim at a characterisation of different types of labs (Knorr-Cetina, 1992), neither do we approach different labs to see how they are constitutive of, and constituted by, different epistemic cultures (Knorr-Cetina, 1999). Our aim, more modest, may require comparison, but only in as much as this helps us understand the constitution of the object we are trying to follow.

Knorr-Cetina defines epistemic cultures as "amalgams of arrangements and mechanisms –bonded through affinity, necessity, and historical coincidence- which, in a given field, make up *how we know what we know*" (1999, p. 1, original emphasis). With her attempt to define cultures in terms of knowledge production – while approaching knowledge production culturally- she works with the idea that knowledge can be a principle of variability: social groups and institutions can be distinguished according to the type of "texture of knowledge" their practices produce (ibid, p. 2). Thus, different collectives produce different "strategies and policies of knowing", different "expert systems" which weave technical, social and symbolic dimensions. If Knorr-Cetina's proposal were to approach science from a cultural perspective, hers would be no innovation. The novelty lies in the claim that, rather than constituting one unified culture, science is constituted by several, distinct cultures.

Science, she complains, is far from being this homogeneous community that epistemology and many analysts of science have presented, with a single unified method and ethos, a single organisation, a single community. This vision hinders us from appreciating the fragmentation and diversity of cultures which constitute 'science' –or, better, 'sciences'. Thus, whereas most analysts still discuss the difference between social and natural sciences, the huge variability even within natural and social sciences alike escapes them[3]. Knorr-Cetina makes her point by

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[3] Divisions have been usually admitted as 'disciplines' or 'scientific specialties', but she rejects these notions, since they do not reflect the complexity of such division; epistemic cultures differ in their 'epistemic machinery': "different architectures of empirical approaches, specific constructions of the referent, particular ontologies of instruments, and different social machines" (id, p. 3).

illustrating how one can track these differences between two such epistemic cultures, High Energy Physics and Molecular Biology. To do this, she allows both collectives to play against each other; the description of each of them mirrors and opens to analysis the description of the other one:

> "Using a comparative optics as a framework for seeing, one may look at one science through the lens of the other. This 'visibilizes' the invisible; each pattern detailed in one science serves as a sensor for identifying and mapping (equivalent, analogue, conflicting) patterns in the other. A comparative optics brings out not the essential features of each field but differences between the fields. These, I believe, are far more tractable than the essential features; in fact, one might argue that they are the only tractable elements available to us" (Knorr-Cetina, 1999, p. 4).

Whilst our pages go along Knorr-Cetina's proposal to take epistemic variability and multiplicity into account, I am a bit more cautious with the 'cultural move'. Not because I want to question the radical sense in which symbolism is constitutive of our reality -which, after the turn to language, discourse and hermeneutics in social sciences in general, and in social psychology in particular, I don't think it could be done[4]. My main concern is rather the adoption of such a term exactly at the time when it is under critical scrutiny in its mother discipline. Anthropologists are divided, but not few voices have been complaining about the side effects that this notion has provoked[5]. To name just one, this notion tends to enclose variability between cultures, while assuming homogeneity within –and here nothing is changed by the fact that one proclaims, not a big border, but its multiplication in several smaller ones: problems intrinsic to the 'drawing of the line remain', just differently distributed.

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[4] Without forgetting, however, that a focus on the symbolical has tended to emphasise human agency and ignore the multiple ways -not only through meaning- in which objects have constitutional impact on our world, i.e., participate in this constitution

[5] This is not the place to open such a heated debate, that leads in some cases to an abandonment of this notion, to others to a redefinition. One can find these arguments, for instance, in Bhabha, 1994; Cohen, 1995; Clifford, 1988; Feliu, 2001; Ingold, 2000; Stolcke, 1995).

For instance, if we defined engineering, crystallography and biocomputing as three different epistemic cultures, how could we make sense of differences within each of the laboratories? Just to give an example, biocomputing practices are so heterogeneous, that several different conceptualisations of their object of study coexist:

> "C: The difference lies not only in experimental vs. biocomputing, I think. There is also a huge difference between you [A], who are a biologist and we two [B and C], biochemists –you think at another level. And the same happens with our senior: he sometimes says "how many charged points do you have?" and I think, "no, no, you don't have charged points in the computer, you have a protein!" Among us there is also a gap" (EG02-23).

They work with 'charged points', with a 'structure of bonds' or with a 'biological entity'. Then, what should we make of these differences? Should we think of these subdivisions as different epistemic cultures? As sub-cultures? Where and how does one draw the line differentiating one culture from another? Do we risk taking 'natural divisions' in 'laboratories' and 'specialities' for granted? These concerns are enough to make us feel uncomfortable with this concept.

Another precaution needs brief mention. If dangers of homogenisation are unavoidable in any type of research –as we have seen when discussing problems in the representation of the other- the opposite risk is also present –to exaggerate distance. The next pages will flesh out differences among settings and, to this aim, we will maximise distinction. We will argue, for instance, that each assemblage produces, and is produced by, different practices upon the protein. We will distinguish between three types of actions that could be designated as 'construction', 'representation' and 'simulation', and will argue that each of these actions upon the protein predominates in each of the labs respectively. This contrast is useful rhetorically, in order to underscore particularities of each articulation; however, up to a certain extent, all these creative manipulations can be found in each lab -one could always argue that every scientific object is constructed, since it is not simply discovered; that it is a representation, in that it claims to talk on behalf of some event;

and a simulation, since we reject the idea that scientific achievements stand in a relation of correspondence with an external referent. Therefore, our claims have to be understood as useful, if temporary, images, obtained out of a particular game of magnification and diminishment. Needless to say, a different focus would have produced a different landscape.

We also join Knorr-Cetina in the rejection of a lab as a mere setting or context –a scenery- where scientific practices take place. One does not need to share Knorr-Cetina's (1992, p. 134) definition of a laboratory as the place where science performs "a particular reconfiguration of the natural order in relation to the social order" to accept that a lab is more than "a locale in which methodologies are put into practice" (ibid). Indeed, a laboratory is not simply a physical place, but a setting continuously organised by what has been called 'topical contextures' (Lynch, 1991) –by different complexes of equipment and practices, that are, according to Lynch, simultaneously symbolic, material and social. If a lab is a physical space, it is so constituted by actions that dwell in it (ibid, p. 53[6]). Thus, as ethnomethodology has shown, context and content are constructing each other: different laboratories contribute to the emergence of different objects; different objects require the construction of different laboratories. In short, laboratories, researchers and objects of study are effects of, and take part in, complicated entanglements or assemblages[7] of partial connections interweaving and constituting each other. Their description and articulation is what we will now proceed to.

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[6] "Although walls, entrances, hallways, and the like have the advantage that they are readily described in diagrams and they do not disappear when the persons inhabiting them go away, this does not necessarily give them a more fundamental or 'objective' role in activities than those less obvious contextures of space, technique, and language that disappear when relevant activities come to a close. Although the latter are essentially bound to activities, this does not make them 'subjective', nor does it imply that they are mere effects of a skeletal architectural machinery. The topical contextures ... are not readily tagged to a material base; they generate material for analysis at the same time that they inhabit a material ground. They elucidate physical space while endowing the 'physical' with distinctive configurations" (Lynch, 1991, p. 73-4).

[7] Discussions about the appropriateness of terminology did not take us long –we even considered terms such as 'epistemic orderings', as an attempt to hybridise 'epistemic cultures' and 'modes of ordering', agencement, assemblage, etc. In any case it should be clear we are emphasising intertwinement, mutual constitution and a radical distribution of agency among all elements involved.

2. Three methods to approach proteins

A. Protein Engineering: an experimental approach

a.1. Introducing the lab

The appearance of this laboratory comes close to the prototypical idea we all may have: quite big a room with several benches, complicated machines and equipment, shelves with bottles full with reagents, buffers, gallipots and alembics, a couple of very old screens with graphics and numbers, test tubes, pipettes. And noise: people standing up or running up and down crossing the doors of the laboratory to enter the next one and come back, people asking, showing and borrowing things to and from each other, machines at work... A lab booming with activity.

After learning that most of the members of this laboratory worked with the potato protein PCI, we would have expected to see some of these tubers around. Still, I could see none: scientists have developed techniques that allow them to obtain these proteins without having to manipulate potatoes! Instead of taking care of a field of potatoes all year long so as to harvest, peel and smash them to obtain proteins, they can, for instance, buy this protein from a private company –why investing so many efforts in this long procedure if somebody else can do it for you? However, this timesaving strategy is quite expensive; therefore, the laboratory has developed another possibility to obtain protein: they have artificially synthesised the gene responsible for the production of PCI[8]. If they have the gene, they can produce as much protein as they need in a couple of days, every time they need it (without having to depend on the season). Notice, then, the degree of decontextualisation involved (Knorr-Cetina, 1992[9]).

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[8] For a detailed description of the procedure, see Molina, Avilés & Querol (1992).

[9] "There are at least three features of natural objects which a laboratory science does not need to accommodate: First, it does not need to put up with the object *as it is*; it can substitute all of its less literal or partial versions, as illustrated above. Second, it does not need to accommodate the natural object *where* it is, anchored in a natural environment; laboratory sciences bring objects *home* and manipulate them on their own terms in the laboratory. Third, a laboratory science does not need to accommodate an event *when it happens*; it does not need to put up with natural cycles of occurrence

They have conceived an ingenious device to keep the gene: they insert the latter into bacteria (*Escherichia coli*). To do so, they first freeze amounts of bacteria together with amounts of gene-plasmid. On freezing, the plasmid attaches to the membrane of the bacteria (the effect is very similar to that experienced when we touch an ice cube: the ice cube sticks to our skin). Afterwards, they increase the temperature brusquely (some to 42ºC, others to 37ºC)[10]. This is what they call a thermic shock, which makes the membrane of the bacteria suddenly open, allowing the entrance of 'our' plasmid. Since this is a sharp procedure damaging the bacteria, they need to be left to rest in a regenerating liquid for some hours. Since they usually start in the afternoon, in order to leave the bacteria the whole night in this liquid, they call this procedure 'overnight'.

Now we have the gene inside the bacteria, but we want to obtain PCI, which means that we need to bring the bacteria to express the genes –that is, to synthesise proteins. Thus, bacteria need to grow and reproduce. This part of the procedure is called 'fermentation': bacteria are introduced in a culture medium (in a huge glass container, with tubes to allow the injection of liquid and nutrients) and fed. Together with its own bacteria genes, bacteria express the gene researchers have added, that is, our PCI. To distinguish this artificially synthesised PCI (synthesised by bacteria) from 'natural' PCI, also called 'wild' or 'native' (synthesised by the potato plant), they call the former recombinant PCI (rePCI). Please, notice that the difference between a recombinant protein and a natural protein does not lie in the fact that one is elaborated in the lab, and the other is picked up directly from nature without mediations (an impossible situation, on the other hand). Both of them are laboratory constructions, and demand exactly the same type of steps to be produced. The difference is that the

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but can try to make them happen frequently enough for continuous study" (Knorr-Cetina, 1992, p. 117).

[10] One Ph.D. student told me that, whereas some heat the bacteria up to 42° C, he prefers to do it only to 37°, since it has been shown that the second temperature increases the productivity of the cell in the long run. When heated to 42° the bacteria are damaged, and they need more time to recover; this is why to 37° the production is after all better. The Ph.D. student was very keen on emphasising that he chose the second procedure not for the bacteria's sake, but for productivity's sake: "I exploit and

recombinant protein is expressed from an artificially produced gene, whereas the natural protein is expressed (through the same procedures) from a gene which has not been modified.

Now we have a medium containing our protein, but also other bacteria proteins, lipids and other impurities [11]. From this messy liquid, and after adding a substance that helps the protein fold into its 'native' shape, we need now to 'fish' our protein PCI, and only our protein. The question now is how? If proteins were visible and apprehensible, like pasta-letters in a letter-soup, our researchers could just pick them up with a spoon. Since proteins are not visible to the human eye, some other mechanisms have to be invented. Several procedures have been created, all of them called 'purification of proteins', aiming at the separation of our proteins from the rest of impurities of the culture medium. With this explanation of this method we will narrow the focus of our camera, so to say: instead of illustrating on broad brushstrokes their practices, I will offer a more detailed perspective of one technique, hoping to gain some insight in the way scientists come to conceptualise their object of study.
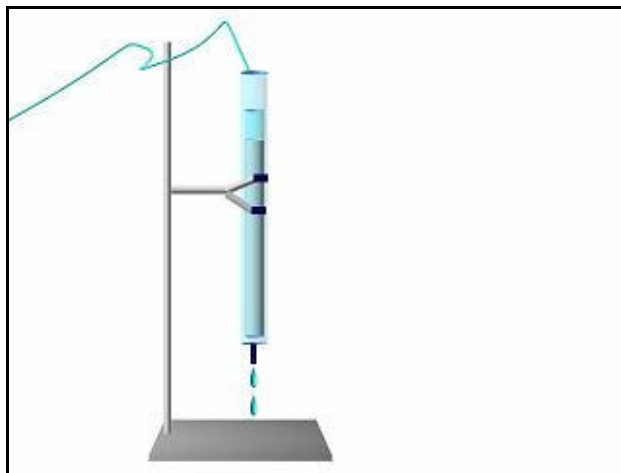
a.2. Introducing techniques: two examples

There are several methods to purify proteins, based on different principles. Some of them separate proteins from each other according to their size using mechanical principles (a kind of filtering). Others use chemical or electromagnetic principles to separate the protein they need from other proteins and impurities. To avoid overwhelming the reader with unnecessary information, here we will simply offer two very different examples.

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misuse bacteria like everybody else; bacteria are our slaves, we do not try to treat them right. I just do it like this to get a better productivity".

[11] PCI is such a small protein that crosses the membrane of the bacteria and floats in the culture medium. In other cases the target protein remains within the bacteria me mbrane, and scientists need to add a chemical substance to make it explode and thus recover their protein.

Example 1: Purification by ion exchange

We have stopped the narration when we had in our hands a culture medium with several particles in suspension: our protein PCI, some other bacteria proteins, rests of the membranes of bacteria, lipids, etc., and now we want to separate our protein from all these mixture. To do so, scientists use what they call a 'column'. It looks like a big syringe, whose size may vary- in whose interior there is a filter. This filter can be imagined as a kind of sponge impregnated by a resin, allowing fluids to pass through. One can connect a thin, long tube at the beginning and another at the end of the column, to make the solution that we need to purify cross the column.

This purification will take place in several steps, which are based on a combination of creation and destruction of interactions. They create conditions so that the protein interacts with the column (it gets attached to it), and then they change the conditions, (either altering the hydrophobic-hydrophilic balance or the ion concentration) so that this interaction is disturbed and the attachment is broken. There are several variations, we offer a summary of one of the possible procedures.

1) They first carry out a step called hydrophobic exchange: they let the solution (with the protein) run through the column. Proteins, salts and other molecules
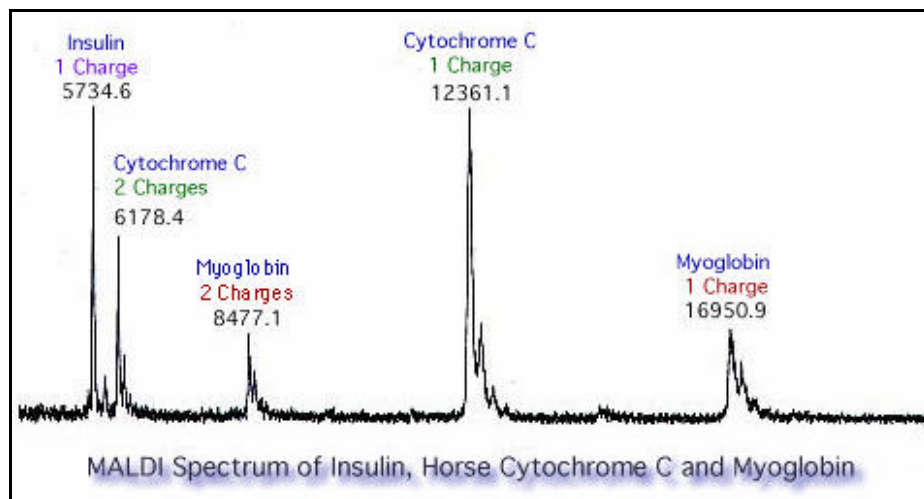
attach to the resin. PCI interaction with the resin is strong. They sequentially let both water and a buffer run through the column, so that salts and non-specific interactions (which are weaker) are washed away. The expelled buffer is supposed to contain only impurities, uninteresting substances for them. Nevertheless, scientists often collect it in a test tube, to make sure that the protein is not carried away by it inadvertently ("just in case", they told me…). Then they let the buffer run again but with a higher concentration, so that this time even our PCI leaves the column, and then it is collected.

2) Then the PCI is diluted in another buffer (let us call it A), so that it becomes negatively charged. The solution runs through the column again. The protein will get caught in the resin, since the latter is positively charged. Again, not only the protein attaches to the resin, but also other substances which have a weaker link. In order to recover the protein of interest alone, they carry out one more step, an elusion.

3) For the elusion, they let another buffer (B) run through the column that has a higher concentration of salts. This means that in the solution there are negative ions which would also attach to the resin of the column. But remember that the column is already 'occupied' by our protein and other molecules. This means that our protein and the new negatively charged substance compete with each other for the attachment. They change buffers gradually: first the solution will have 100% of the neutral buffer they had been using (A) and 0% of B, one with high concentration of salts; then 90% of A and 10% of B; then 80% of A and 20% of B…. 40% of A and 60% of B, and so on. At the beginning, at low concentrations, only weak links will surrender, and they will be expelled out of the column. While the buffer increases its concentration, more substances will detach from the column, until little by little, all the substances previously linked to the resin will be eluded (our protein included). Scientists collect each of the substances expelled in test tubes, which will be later on analysed.

4) In one of these test tubes scientists obtain the protein. To separate it definitely from other molecules, they let the substance run through a gel, in order to

filtrate our protein. The solution runs through a column with a kind of filter with holes. Big molecules do not fit through the holes, and take a shorter path through the column. Small molecules go through a much more intricate path, and take longer. Thus, molecules go out of the column selected by size through time.

Some machines, like the HPLC, carry out such purifications, with the added plus that the machine allows to measure exactly when a particular substance leaves the column, with what dissolution, and in what amount. They already know that with a particular dissolution at time t, the PCI will be eluded, so that they can collect it. If they did not know, or if they want to be sure, they can analyse the collected substance. They obtain a graphic such as the following:

This allows the instrument to build a graphic such as the following:

MALDI Spectrum of Insulin, Horse Cytochrome C and Myoglobin

One of the picks will correspond to the protein they have purified.

This type of graphic is very recurrent when working on proteins: chromatography, HPLC[12], MALDI[13], all these instruments produce a similar inscription. Now, this

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[12] HPLC or high performance liquid chromatography.

inscription is produced by the movement and performance of the protein, and it is considered by scientists as a trace or visual information of the protein –but not as the protein itself[14].
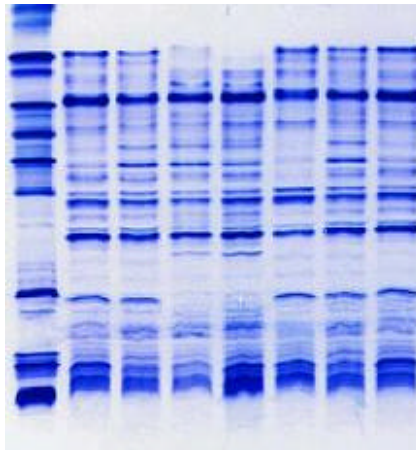

Example 2: Protein gel electrophoresis

Imagine you prepare a rectangular piece of thick, transparent jelly: an acrilamide /bisacrilamide gel[15], with a net structure. In one extreme of the gel you introduce sample of proteins, (proteins which have been charged electrically) and then you apply an electric field to the gel. Electricity creates two poles, a positive and a negative: positively charged molecules (cations) move towards the negative pole (cathode), negatively charged molecules (anions) towards the positive pole (anode). In this way, proteins separate from each other. However, since they have to move through the gel to reach the poles, bigger molecules cannot travel so fast, so when the electricity stops circulating, proteins will have moved a different distance: bigger

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[13] MALDI-TOF (matrix-assisted laser desorption ionization time-of-flight) mass spectrometry technique.

[14] When they talk among themselves, they may point at the graphic and refer to it as 'the protein' and not 'the graphic of the protein'. These are the type of short cuts that language allows. But if asked, they restore the difference immediately and it would not be fair to claim they think the graphic is the protein.

[15] Its construction is not easy, it requires a good coordination of steps and timing. We will not describe this procedure here, but suffice it to say that the capability to fabricate good gels differentiates experienced Ph.D. students from beginners.

molecules will have moved less, whereas smaller molecules cross the structure of the gel faster, and will go farther. This means that electricity and gel together provide a means to discriminate proteins according to size[16]: two proteins with similar size will be placed next to each other.

When scientists have to interpret a gel, though, problems start. Since proteins are not visible, and they carry no names, it is not easy to know which protein is which. They can guess this, though, with a formula that, depending on the distance done by the protein, will allow them to calculate the molecular weight of the protein (which equals to knowing its identity). Now, if all the gels of the world had the same properties, and reacted in the same way, one researcher would know that the distance made by protein A in gel 1 is the same as the distance made by protein A in gel 2, or protein A in gel 3, or in gel 4, or in gel 5, etc. Knowing the distance, we would immediately recognise the protein. Unfortunately, this is not the case of the electrophoresis gel: standardised measures are impossible to obtain, for the distance made by a protein within the gel depends dramatically on the consistency and properties of the particular gel.

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[16] Actually, separation is carried out on both criteria: size, and charge-to-mass ratio. Since we only want the gel to discriminate size, researchers denaturalise the protein before putting it into the gel, so that the influence of the charge-to-mass ration can be cancelled. Afterwards, with the help of formulas which link the mobility of the protein to the molecular weight, they can identify the protein: log (Mr) =

This variability would seem to challenge the possibility of measurement and comparison between gels. Thanks to an easy but practical convention, a big part of the world, pace Great Britain and its Empire, has reached an agreement to standardise measurements. If we can say that this book measures 25 cm here and in France, for instance, it is just because we have agreed upon using an instrument of measure whose characteristics can be more or less made stable (in the case of metre, variability is usually dismissible), and then comparison is possible (let us not forget that to measure is to compare). If the distance done by the protein is not expressible as standard measure, and it changes from gel to gel, though, how can one interpret distances in such variable conditions? How can we compare? The solution invented in the case of the gel is ingenious: they introduce the "metre" within the gel, so to speak, so that it is also affected by the same conditions! Together with the sample of proteins to discriminate, they also introduce what they call 'markers', which is a protein whose molecular weight is already known. This marker will be their reference: the position of the proteins will always be calculated having into consideration the position of the marker.

Such a simple solution is nevertheless vital: without markers they cannot interpret the result of the gel: they see the proteins separated into different bands or strips, but since they lack the dimension of reference, they cannot calculate the distance they have moved. If they forget to add the marker (as it happens to new Ph.D. students from time to time) they can throw the gel away, it does not matter how clearly the protein bands can be seen and distinguished. I once attended the viva of a project where the Ph.D. student was criticised for not having added the marker in the presentation of the results. The reader of the project could see bands, but had to accept the interpretation of the Ph.D. student because they could not contrast results and marker, fact which did not impress the jury. This is one more illustration of the contextuality of measurement. Once scientists detect which protein is which, they can cut the piece of gel where their protein is, and dissolve it in order to recover and manipulate the protein.

C* Mobility + D   (where c is a constant that depends on gel properties; mobility is the distance the

a.3. The constitution of the protein

Invisible proteins

We have shown their techniques with some detail in order to illustrate some characteristics of their methods and objects of study worth of some discussion. Judging from how they work with them, proteins appear to be elusive entities: invisible and inapprehensible, difficult to grasp and locate. Indeed, since proteins are too small to be detected by the human eye, we could somehow say that we are blind to proteins. We can only perceive their presence if we have lots of lots of proteins in a solution, because the liquid appears turbid, but still, we never perceive 'a protein' in the same way we seem to perceive an object.

Granted, after so many perception studies, we know that even if we feel we perceive objects in an unmediated way, a lot of mediation (energetic, neuronal, interpretational, cultural) is involved in perception. Nevertheless, in the case of proteins, the lack of immediacy is of a more drastic nature: researchers can only testify their presence through indirect mechanisms, detecting *traces* (Bastide, 1990; Knorr-Cetina, 1999). They can see that before the purification of their substance, the liquid they have in their test tube is rather yellowish; afterwards, when impurities are removed, the liquid is white. They can measure through light beams the amount of purified substance that falls into their test tubes. They can calculate the time when the substance leaves the column and its molecular weight. They can see the blue-died stripes in a gel which allegedly correspond to different sizes of different proteins. They can see the picks and graphics representing the protein. But they can never see directly a protein: it does not matter how long one works in experimental laboratories, researchers could have no notion of the possible shape of proteins, had they not seen

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protein has moved).

graphics and drawings in biology handbooks (without implying, though, that these drawings are an immediate description of the protein).

Allow me an anecdote to illustrate the elusive character of the protein. As we have seen, in some of the steps of the purification, but not in others, they pick up the cleaning buffer leaving the column to make sure that the protein remains in the column, and it is not inadvertently thrown away. They check this in some steps, but not in others. For instance, when they first introduce their protein, with the assumption that it will attach to the resin, they do not usually check if they lose protein. When I asked them why they did not check it at every step, they burst out laughing, and replied that 'one needs to have a bit of faith'. If I mention this anecdote, it is not to point (once more) to the important role of believes within science[17]. Rather, it is the resonance with a biblical story, that of St. Thomas, which I find interesting.

According to the New Testament (John, 20, 24-29), Thomas, absent when the disciples saw the resurrected Christ for the first time, did not believe them immediately when they narrated to him their first encounter, scepticism which he expressed with the famous words: "Except I shall see in his hands the print of the nails, and put my finger into the print of the nails, and thrust my hand into his side, I will not believe". Eyes and hands, seeing and touching are pictured here as the most common and easy ways in which we give reality to things. We know whether something exists if, like Thomas, we can see it or touch it. The end of the episode is well known: Thomas is friendly admonished for being so 'faithless'. This story, then, helps define faith as believing, or even better, as knowing or as having certainty *without* seeing: "blessed *are* they that have not seen, and *yet* have believed". This faith or certainty is of the kind found among scientists: they know the protein is there –it must be there- even though they cannot see it or touch it directly[18].

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[17] This was an important contribution of the Strong Program. The practice of science, like most other human activities, involves trust and believe. But once the point is made, one needs to move forward.
[18] To say that faith is an element of science is neither a disqualification nor a new piece of information. This is frequently admitted even by scientists such as Max Planck: "... because science also demands a believer spirit. Anybody who has seriously taken part in a scientific work knows that at the entrance of

This lack of vision is dramatically illustrated when an experiment goes wrong: when they fail, they assume that they have made a mistake, but they usually do not know where or when the mistake took place: was the quantity of added markers enough for them to be visible in the gel? Did they put too much or too little silver dye? Did the gel solidify too early or too late? Was the proportion of added water too little or too much? Is this unexpected peak in the graphic the result of a new, surprising substance, or is it simply that the column is dirty? Did one read the gel in the right direction, or up side down, mistaking one's protein for another? All these (witnessed) situations illustrate the kind of problems experimental researchers face due to the relative blindness they experience regarding their object of study. With experience, Ph.D. students learn to deduce the most likely cause for failure in each case, and, even, to avoid it. As we have already said, the skill and the easy to detect and avoid errors is precisely what distinguish new from experienced Ph.D. students. But the skill of more experienced researchers to avoid errors does not mean that they enjoy a more privileged perspective on the protein.

Scientists will complain: they are not blind. They may not see directly the protein – how could they, if the protein is too small for the human eye? However, they will insist, this does not mean that they cannot see what they are doing. The picture of a blind, faithful scientist remains inadequate; certainty is not limited to seeing and touching on the one hand, and trust and belief on the other, for they have developed devices and mechanisms to cross this gap: certain visualisation techniques which allow them to keep track of all the steps and transformations that proteins suffer[19]. All

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the temple of science, one can read written on the door: *You need to have faith*. This is something scientists cannot make without" (Planck, in Wilber p. 211, our translation, original emphasis).

[19] First, experience: they have repeated those procedures so many times, during several generations of Ph.D. students, and they already know at what time is the PCI expected to leave the column. They already know how much the protein is supposed to weigh. Second, a colorant (coomassie blue): they can add a blue dye to the solution, which attaches to the protein, which is then visualised as a blue band in a gel (they can also use a silver dye, in which case bands take a dark colour). Third, fluorescent dye: they can add a fluorescent compound which reacts with proteins, allowing to trace the protein through the fluorescence. Forth, autoradiography: they can detect the protein through its radioactivity. Fifth: immunoblots: they can localize the protein when an antibody against it unites to it. Sixth: they can observe the reaction of the protein, and see if the behaviour is the expected one. Seventh: they can sequence the protein, and see if the chain of amino acids obtained corresponds with the known

these techniques are mediated ways of constituting the possibility of visualising, when 'seeing' is not possible: they have to know whether the steps they engage in are producing the wished results, whether the protein is responding to manipulations in the desired way. Through mediated visualisation, which nevertheless cannot show 'the protein', they are able to monitor their actions [20].

Untouchable proteins

If proteins cannot be seen, they cannot be caught either. You can take a test tube in your hands where allegedly thousands of molecules of proteins are dissolved –and in this sense, if you want, you can say you have 'proteins' in your hands. Still, you can never catch a protein as you would catch an object. Now, if researchers cannot see proteins, and they cannot touch proteins, how can they manipulate proteins? How can one move proteins from one place to the other, and perform changes upon them? In the same way in which researchers have found indirect ways to 'see', they have also found indirect ways to touch or act upon. Which translates as more mediation: manipulations have to rely on intermediary substances acting between researchers and proteins. We have seen how they use substances which attract or repel proteins: chemically or electrically charged substances which catch them in an assemblage or expel them out of a relationship; neutral liquids which transport them from one place to another; gels and electricity which oblige them to separate by size; using electricity and gels to order them so that they can constitute interpretable patterns; antibodies catching the protein in order to reveal their identity, etc.

In this mediation, then, we find the same move: attachment and detachment. Another example will illustrate this. When preparing the DNA plasmid which will 'express'

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sequence. Eight: they can calculate the molecular weight of the protein through MALDI-TOP. Nine: observe its activity in the spectrophotometer. Tenth: observe the function of the protein, and deduce what kind of protein they are dealing with from its behaviour.

[20] If some reader wants to deepen insight in the topic of visualisation, the following works can be strongly recommended: Aman & Knorr-Cetina (1990); Bastide (1990); Knorr-Cetina (1981, 1999);

their protein, researchers sometimes modify the gene so that the protein is enlarged; instead of having its normal length, the protein possesses now an extra tail of histidines. They do this because histidines are very easily attached to the resin of the purification columns. Thus, when the protein with the histidine tail goes through the column, it gets caught by a chemical substance through its tail. Once the protein is purified, researchers cut the tail off with another chemical substance (they "displace the histidine tail"), so that the protein recovers its normal length. Here, then, researchers mix, hybridise and attach; once the attachment has accomplished its function, the mixture is detached: the resin frees the protein, the tail is cut off. In this sense, their knowledge practices proceed through a continuous movement of attachment and detachment. Of mixing and separation, of hybridisation and purification. It is also worth observing that every mediating step has ontological consequences on the protein (change of shape, of chemical interactions, etc). This point –that mediation implies transformation- will be expanded in the next chapter.

Attachment and detachment are often united to another dynamic: it is frequently the case that researchers take up an action which acts 'per excess', and needs to be partially corrected. Let me give a couple of examples. First, when we let our solution run through a column, not only does our protein get stuck, but also some other impurities which create a nonspecific link; they need to be removed by letting another buffer run through the column, which binds our protein stronger, and eludes impurities. A second example can be found again in the process of purification. When our protein is caught in the resin of the column, we may free it by letting run buffer with pH 10; this buffer changes the shape of the substance in the resin holding our protein back, and such a change allows our protein to be able to escape the chemical interaction and to elude. The problem, though, is that this pH also alters the structure of our protein and endangers its survival in the long run. Therefore, the action of the buffer needs to be corrected, by removing it through dialysis. A third example may come from an electrophoresis gel of proteins. We remember that, in order to be seen, proteins need to be died with a blue tincture. The tincture attaches to too many

Lynch (1985a, 1985b, 1988); Lynch & Woolgar (1990). For a detailed analysis of drawings, pictures

substances in the gel, and not only to our proteins, which means that it is quite useless to us: to look into a completely blue gel is as little informative as to look into a completely transparent one. What we need is that the colorant attaches only to those substances we want to identify. To get this, we need to submerge our gel, died in excess, in a decolourant solution to remove part of the colour. Again, an action which goes too far needs to be partially corrected.

Elusive proteins

To summarise, in these practices, the protein appears as an entity without apprehensible borders, suspended in liquid or trapped in gels, whose movements are difficult to follow and control. No wonder, then, that they talk of 'hunting proteins' and 'fishing genes'. This vocabulary, unlike 'plucking' or 'harvesting', reflects the elusiveness of their object of study, and makes reference to all these strategies that are necessary to catch the protein in networks of power-knowledge. A consequence of this constitution is that they can never take identities for granted; on the contrary, identities need to be checked –and not only to convince others in papers, but also to be sure one is dealing with the object one thinks is dealing. The next quotation, extracted from a paper, signals the uncertainty surrounding identities:

> "The chemical purity of the final rePCI preparation *was verified* by subjecting samples to HPLC analysis through a reverse-phase column. The product was homogenous. A sample of PCI from potato was also submitted to the same chromatographic conditions and natural PCI-IIa was found to show the same retention time as rePCI. *A further proof of the identity* of the rePCI was obtained by FAB mass spectrometry as reported by Calvete et al. (1991)" (Molina, Avilés & Querol, 1992, p.135, our emphasis).

As we see in this paragraph, the object in question is put to the test to observe its behaviour: results produced by PCI-IIa are compared to results by PCI; if

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and other kind of representations, see Bastide (1990); Myers (1990).

performance of both proteins is similar, then they are assumed to share identity. Another example is offered by this episode narrated by one researcher: once she misread a protein gel (she was interpreting it upside down), and had consequently picked up the wrong protein to work with. She detected her mistake, however, on putting the protein to the test: it was not performing as expected in a couple of tests, revealing another identity.

It may be surprising to discover that scientific practices are so enmeshed in uncertainty, invisibility, and vagueness as in certainty, visualisation, measurement, and precision. However, it is important to resist the temptation to think that the first terms are impediments for knowledge, situations to be overcome, whereas the other constitute the ideal of the scientific practice. As Knorr-Cetina (1981) put it, when it comes to scientific production, indeterminacy is constructive rather than destructive, opening possibilities to change. Scientists have learnt to deal with these characteristics, to accept them as part of life and its expression; they have learnt to produce knowledge based on conditions quite different from those ideally described in epistemology books. Ambiguity and uncertainty are not problems to overcome and eliminate, but characteristics one needs to accept and play with, if one wants to understand the mechanisms of life.

Indeed, scientists can never enter in unmediated touch with their object of study - mediations is all they have. Or, to put it better, mediation is all they are in –entangled in mediation. To know they do not cut but multiply relations and links with their objects of study. This evidence questions one of the inherited views according to which 'knowing' is 'discriminating': being able to establish differences between concepts, to distinguish between different realities, eliminating the superfluous and keeping the distinctive essence of a thing, keeping the distance between subject and object. Separating and purifying would be key actions for knowledge.

However, as soon as we direct our attention to the practices carried out in a laboratory, a more complex picture emerges. If the previous image equates the

scientist to a kind of cleaner, somebody separating the interesting from the uninteresting, taking the impure layers away from the pure object, in the laboratory we have discovered a scientist that mixes and hybridises whatever is necessary in order to achieve the desired result (Latour, 1991). The process is never one of eliminating the superfluous until the object is visible in its purity, as the word discover would imply: in order to detect a protein, they never observe from a respectful distance, to contemplate the object emerging before them. Instead, they interfere with, disturb, manipulate, hybridise, cut, past, dye, discolour, dismember and re-member their object of study. It is not only that they interact with the protein, in order to obtain knowledge from it, for this would convey the impression that the protein is 'already there'. There is more to it than this and, as we have seen, the researcher's role is more radical: if the object gains existence at all, is only as a result of this complicated engagement: there is a continuity between the object and the scientist's actions. Object and subject intermingle (Latour, 1999-even though distance may still be achieved as an effect; see Stengers, 2000).

Constructing proteins

When researchers of the engineering laboratory talk about rePCI, they usually admit to all the processes of construction involved in its 'fabrication'. They never claim to have 'discovered' the protein or to have 'found' it in nature. From the very beginning they talk about it as their own construction. Whereas scientists often show awareness of their active involvement in the elaboration of their object in the lab when they work with 'native' proteins (also called 'natural' or 'wild'), it is even more so when they deal with a recombinant protein, obtained out of an artificially created gene: this protein is conceptualised as a kind of engineering prototype, a replica or double of the 'natural protein'.

Interestingly, (and exactly as it happens when they work with wild proteins), their awareness of the amount of construction involved in the delivering of this

recombinant protein to the world does not diminish an inch the character of reality that the protein is attributed. In a very nice illustration of the double meaning of the word 'fact' (made and not made up simultaneously; Latour, 1999), they see no contradiction in claiming that the protein is made and is an entity with existence in its own (precisely because it is made!). This double perspective is also to be found in laboratories where scientists work with non-recombinant proteins: as if they enjoyed the capacity to look at their objects in the same way one looks at Gestalt figures, they now see the construction, they now see the entity with life of its own[21].

This 'replica' or 'double' is capable of interacting actively with the scientists and the setting they create, so that scientists can observe and register its performance. Thus, the protein will have to cross gels and columns, it will be dyed and cut and manipulated so that scientists can be sure of its properties and characteristics. These manipulations will give rise to inscriptions, elaborated then not only by scientists, but also by the performance of the object. By forcing the protein to go through a purification column, and by being crossed by light beams, scientists manage to make proteins leave traces in the shape of graphics; by dying and electrifying the protein, they manage to enrol it to constitute a gel. Translation after translation, the protein is made to collaborate to produce evidence testifying of its existence -and of the power of the scientist to make it testify.

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[21] An anecdote is here appropriate. We were once observing how three scientists were learning to manipulate a complicated machine, a MALDI or a sophisticated mass spectrometer. There is a part of the procedure in which scientists bombard with electrons a part of a sample, so as to detect whether there is protein in the sample, and what its identity is. Scientists know if they are bombarding a zone with protein because peaks appear on a screen. The characteristics of the peak depend to a great extend on the number of bombardments they direct, and on the way they mould the peak. It is common to hear scientists saying "bombard it once more, until we get a nicer peak". Once, on hearing this, we expressed out loud our surprise for their explicit acknowledgment of their active role in the constitution of visually aesthetic results; the two more experienced scientists smiled and nodded. Suddenly, the third scientist, a younger Ph.D. student, completely confused, burst out "but what do you mean we make the peak? Is there a peak there or not?!" The rest of us -scientists and psychologists- had to reassure her of the existence of the protein in the sample... The younger Ph.D. student (as myself at that time) was somehow caught in a false dichotomy: either the peak is the result of an external reality or is invented by scientists. The more experience scientists have, the more they seem to agree in the simultaneity of these alternatives.

Some inscriptions will be seen as ontologically disconnected from the protein: if you ask a scientist, she will tell you that the peaks drawn on paper by a machine responding at the amount of protein crossed by a beam are not the protein itself, but traces left by the protein. Other inscriptions are regarded as more intertwined with the being of the protein: the inscription on a gel is constituted by the protein itself, so that the blue bit of gel they identify as the protein *is* the protein: they cut into the gel and take the bit of gel and dissolve it in a test tube to recover their protein. In none of these cases is a resemblance between the inscription and the protein assumed; if inscriptions are representations of the protein, they are so in that they are translations, transformations, traces left by the protein, and not images reflecting the protein.

We witnessed plenty of ways in which an experimental protein may object to manipulations –and surely there are even more: fermentation with bacteria may fail or spill, protein may resist purification, gels often go wrong, sometimes they do not solidify, sometimes they solidify too fast, tincture may burn the gel so that proteins are not visible anymore, the protein may break, it may hide, it may resist sequenciation, etc. Scientists take these incidents with patience and resignation: this is, after all, the way in which an object may give proof of its agency: objects object! The unavoidable and uncontrollable nature of these objections is food for jokes and stories through which scientists (especially Ph.D. students among themselves, or seniors to Ph.D. students) comfort and calm down each other when problems occur: these objections happen so often, that practices are from the beginning oriented to avoid frustration and promote acceptance: they learn that what they sometimes jokingly call "the imponderables" are an intrinsic part of their work.

B. Crystallography: a structural approach
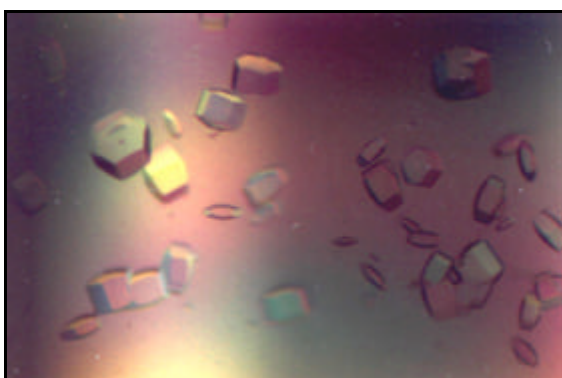
b.1. Introducing the lab

The IBF is a reasonably big and well-equipped institution in Spain, with a variety of methodological and theoretical approaches populating it. However, there is one type of laboratory that is not part of the institute: crystallography. This absence does not mean that members of the IBF cannot work with crystallographers -this is why collaborations are there for: some groups of the IBF work together with a group of crystallography of another laboratory in Barcelona, another with a German group. But it was definitely an impediment for us to observe crystallographers at work and their techniques, so we can only tell what we were told about it. However, this minimum of information will suffice to make our point: they way crystallographers work with PCI has other effects on the constitution of the object of study than those found in the previous laboratory. To illustrate our argument, we must first introduce some basic ideas regarding the structure of proteins.
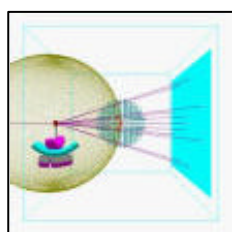
b.2. Introducing the technique

Proteins are constituted by a chain of amino acids. These interact with each other, that is, they may establish links, chemical bonds, with its attractions and repulsions. These chemical interactions mean that the chain does not remain flat, but it folds onto itself, reaching a specific shape. This conformation is the three-dimensional structure of the protein, and it is different for each protein, conferring on it different properties. The structure of the protein is not an irrelevant feature, it conditions, for instance, with what kind of other molecules a protein will interact and how; for instance, how enzymes react with their substrate, how proteins and DNA interact, how two different proteins interact with each other, etc.
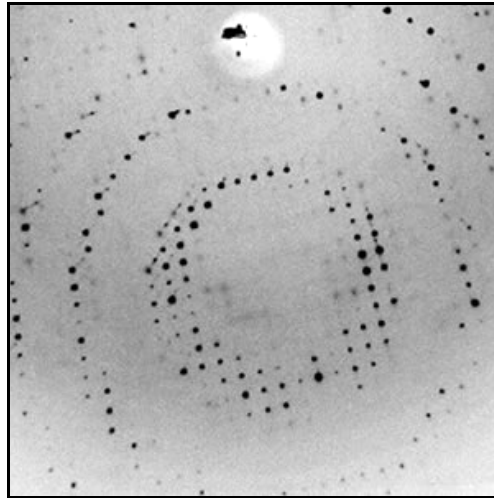
In most experimental laboratories (including laboratories of enzymology, protein engineering, basic and applied molecular biology) structures cannot be observed. Scientists may deduce information about the structure by observing how the protein folds and unfolds, how it reacts in particular situations, or by observing the sequence of amino acids. But they cannot see or determine the structure of a protein. There is

only one way to find out the structure of a protein experimentally –or, as they call it, 'to solve a three-dimensional structure': through crystallography. That is, a scientist specialised in crystallography needs to stabilise the protein in the shape of a crystal, procedure with a high degree of difficulty, since it is hard to produce those conditions under which a crystal solidifies regularly. Moreover, posterior manipulations of the crystal may endanger the crystal if it is not strong enough. The construction of a crystal counts as a great achievement.

Then, once the crystal is constructed, it must be analysed through X-rays or NMR (nuclear magnetic resonance), to obtain 'a picture' of the structure. The procedure is the following. The crystal is bombarded by X-rays; electrons collide against the protein and are refracted. A screen behind the crystal receives the impact of the electrons and registers the inclination and deviation of the electrons respect to their initial position. These data allow calculation of the structure of the object against which they have collided. This is the reason why one needs to bombard a crystallised protein: a crystal has quite a regular structure; if they bombarded a protein in vitro, the dispersion of the electrons would be impossible to interpret. Actually, they do not bombard one crystal, but several, and calculate the sum of the points. The more dispersed and far away from the centre, the easier it is to calculate the structure.

b.3. The constitution of the protein

Since we have not observed crystallographic practices, there is not much we can say with certainty about the way a protein is constructed in this type of laboratories. One can guess, however, that the construction of a crystal turns the living protein into a fixed, immobile entity. As we will now see, the protein is solidified, captured in a moment in time, so to say. It is made an object that does not need to perform actively, but passively: it must react well. Furthermore, its stability is tied to fragility: this construction may break.

In the case of crystallography, the protein itself remains as invisible and untouchable as the engineering protein. The body of the protein, its contours, are never perceived directly. However, this type the inscription provides not exactly traces of the protein, but a way of seeing into the protein –a way of drawing its skeleton, so to speak. The crystallised protein opens itself to visual exploration –not as a dissected body, rather as a scanned body.

Representing proteins

The protein in the shape of a crystal is also an achievement. But this time scientists do not attempt to create a replica, which behaves and reacts like a native protein. As if they wanted to make a statue, they try to fix the protein in a stable, static gesture. Whilst an experimental protein needs to be able to interact and pass tests, the opposite is expected from a crystallised protein: it should remain still and stable, at least long enough to allow scientists to 'take a picture' of it (X-rays or NMR). If the protein changes its conformation, no information can be obtained of its structure, this is why scientists need to freeze it in a moment in time. Only after achieving this can scientists obtain the type of inscriptions they are interested in.

We have already seen that obtaining a crystal is difficult: the protein, then, has several ways of objecting: it may resist crystallisation; it may crystallise in an unstable way and fall apart on being bombarded; it may produce an illegible inscription. This is why also in this case scientists need the collaboration of the protein in order to get a successful inscription. However, the type of engagement expected from the protein is different than in experimental inscriptions. The protein does not produce an inscription leaving traces while performing in a test. Rather, the setting asks of the protein to remain fixed and simply oppose material resistance to the electrons. If this procedure reminds of taking a picture is not only because the inscription captures a moment in time; but also because the inscription, unlike the previous ones we have seen in the engineering laboratory, stands in a relation of resemblance to the object. When observing a gel or the peaks of a chromatography, nobody would claim that there is a relation between the shape of the inscription and the shape of the object; in a successful X-ray representation of a protein, on the contrary, there is a more direct relation between the pattern drawn by electrons and the structure of the protein. Needless to say, unlike in a picture, we cannot perceive the similarity visually: on the one hand we cannot see directly the structure of the protein and on the other we need to calculate to deduce the shape left by the electrons. But the principle remains: the inscription aims at representing the protein not only in

the sense of taking the place of the protein, but also in the classical sense attributed to the notion of representation: the inscription is supposed to be an image of the protein, a mapping. This characteristic is what makes some people say that crystallography comes close to being 'the eyes': they are the only ones capable of visualising structure, of seeing into the actual protein.

C. Biocomputing laboratory: a simulation approach

c.1. Introducing the lab

The IBF has something which not many other laboratories have: a laboratory of bioinformatics where they can model and predict protein structures. When you go from an experimental laboratory to another experimental laboratory (say, from an enzymology laboratory to a biomolecular laboratory), you scarcely notice the change -which is not to say that there are no differences, but that differences are subtler. You still see benches, the same instrumental and equipment, the same material, the same distributions. When you enter a biocomputing laboratory, though, you definitely notice it. A quiet room, with fixed temperature due to the potent computers that fill the room. Tables and screens is all you can see, and silent people sitting in front of them, staring at drawings and numbers, clicking frantically the mouse or tapping without pause. Each person sits in front of one computer, without circulating within the lab, without exchanging places with others. We will later see how this space and the practices that constitute it are highly individualising, determining what communications are useful and what should be interrupted (Foucault, 1976, p. 147). From time to time, a short question, a fast comment; then, silence again. When they work, of course, for when they are not very concentrated or make a pause, then they become very loud, joking without pardon –believe me, you do not want to be the aim of their jokes.

This change in atmosphere is so remarkable, that this room is called 'the dungeons' or 'the cave'. This name responds to the first impression on entering the lab: one has the feeling of entering a closed space. Surrounded by four walls with no window to the exterior, the visitor steps into a room in the penumbra, scarcely illuminated by the artificial light of the computer screens. The disciplining of movements and activities that one finds at the bench is intensified in the biocomputing laboratory, especially in terms of space: working space is divided in zones, in this case defined by the space around a computer, and each individual is placed in one of them. The group is decomposed in elements: there is a specific concern to coordinate the position of people as pieces of the production apparatus: bodies, machines and activities must be appropriately distributed. The principle is clear: each person must occupy the right place (Foucault, 1976). Other members of the IBF refer to this laboratory almost as a place of confinement, where one can only sit and work, and biocomputing members themselves, who claim to be working "in a different world", also share this image.

If this is a closed space, however, it is also, simultaneously, one of the most open of the IBF. In a way, one can say that this laboratory is open to the world, since it is continuously connected to the net: they obtain information necessary for their work from Internet and public data bases and make their own results and programs available to others through these public spaces; if they work with the help of others, these others are not those working elbow to elbow on the bench, but those with whom they are connected through the uninterrupted connection to the e-mail system –when they have a question they may ask the colleague sitting next to them in the lab, but they may as well ask somebody sitting in London, Madrid or Valencia. Thus, what do we make of this simultaneous opening and closure? This is more a tension than a contradiction: if their "here and now" takes place in quite enclosed a space, we see how technology opens a split of virtualisation, of deterritorialisation, of exit from the here and now that makes new relations possible. A dislocation of presence: "synchronicity replaces unity of place, interconnection replaces unity of time" (Lévy, 1999, p. 22, our translation). This duplicity will also be found in the type of work they elaborate, oscillating between a strong actuality with effects in the here and now,

and an impressive virtuality impregnating simulation. As in the two previous cases, to understand these thoughts, we first need to learn more about the way they work.

But before this, a clarification is needed. Members of the IBF admitted of radical differences in methods and concepts between experimental and biocomputing orientations. So did biocomputing members, who recognised 'a great divide' between themselves and the rest of colleagues in "the way we approach a problem and the way we look at things" (EG02-23). Nevertheless, as the latter also warned us, we should avoid portraying homogeneity within biocomputing approaches. Differences among several biocomputing techniques, and therefore in the way they conceive their objects of study, are considerable, as we have illustrated in the introduction of this chapter[22]. This differences notwithstanding, in this section we will emphasise communalities among biocomputing approaches. It should be clear that we are relying on a strategic simplification that enables comparison with experimental practices.

c.2. Introducing the techniques: working in the dungeons
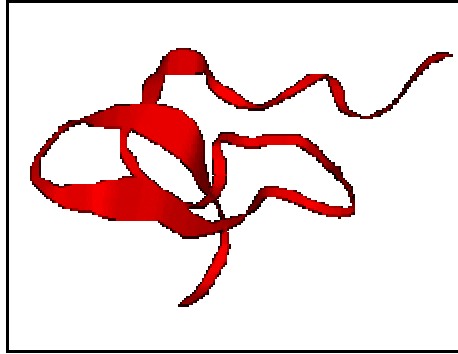
The first day we visited the IBF, one of my team colleagues, impatient, asked the boss accompanying us in our tour how they managed to visualise their work. Smiling and proud, the boss answered: "you will see this in a minute". He took us downstairs to the biocomputing laboratory, and said: "they are our eyes". Indeed, on the screens of the computers wonderfully coloured proteins seductively shone out at us. We were not confronted to traces, to inscriptions whose form had nothing in common with the shape of a protein; we seemed to be observing proteins, appearing clear-cut and well formed, in front of our eyes. If a graphic with peaks demands translation for the
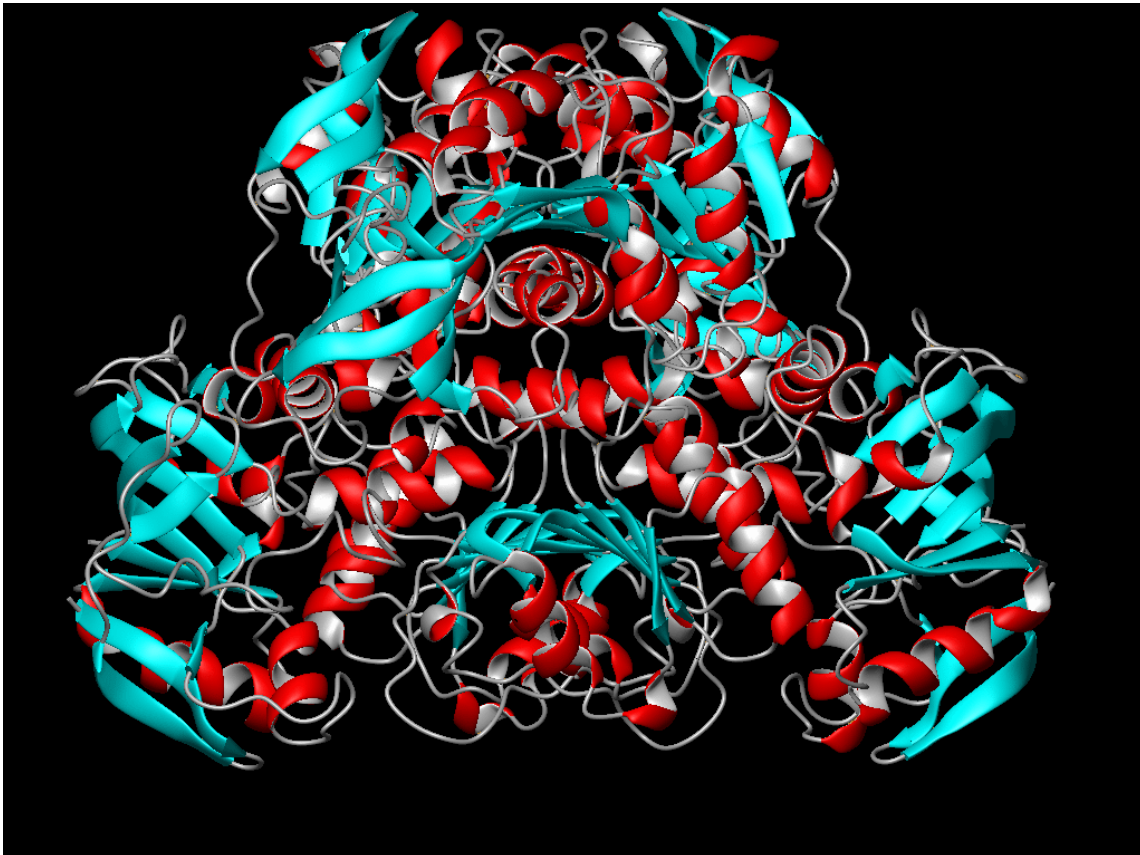
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[22] This was the quotation we have offered:
   "C: The difference lies not only in experimental vs. biocomputing, I think. There is also a huge difference between you [A], who are a biologist and we two [B and C], biochemists – you think at another level. And the same happens with our senior: he sometimes says "how many charged points do you have?", and I think "no, no, you don't have charged points in the computer, you have a protein!". Among us there is also a gap" (EG02-23).
A gap, big enough, at least, to impede a complete understanding of each other and fluid collaboration, and to make it difficult for them to help each other to the extent found in experimental laboratories.

interpreter, these images seemed to offer a direct, intuitive comprehension of the shape of the protein. For instance, this is the image of the PCI, a relatively simple protein.

But proteins can be so sophisticated as that:

Of course, these images seem intuitive *only if* (and this *if* is important) one is acquainted with the images of proteins that handbooks and biology books offer. How false this first impression of immediacy is will soon be clear. These computer images are elaborated by complex programs which produce a three-dimensional representation of the protein on the screen of a computer (and on slides which can be presented in conferences and used in publications such as papers and thesis, including thesis by annoying social psychologists). To create these images, these programmes require information about the protein, especially its sequence (the two dimensional chain of amino acids), extracted with experimental methods (such as X ray crystallography, NMR, or other spectroscopy analyses). The advantage of such programs is not only that they enable visualisation of proteins in a way which has no resemblance whatsoever to the peaks and strips which experimentalists read, but also that they allow the calculus of certain interaction energies and other electrostatic parameters as well as many other operations. In other words, they make feasible certain operations upon the protein which would not be thinkable upon the engineered protein.

For instance, these programs can become good tools to attempt a classification of proteins, not according to sequence, but to structure. That is, they may develop programs which compare the conformation of the protein with others, and categorise together those with similar three-dimensional structure, while separating those with different structure. Another crucial advantage of such programs is that they can predict the likely structure of a protein whose three-dimensional structure has not been solved through crystallography yet. If they have the sequence of amino acids of a protein but not its structure, but they know that this protein is very similar to a second one whose structure is known, they can model the former protein using the structure or shape of the latter, producing a predicted three-dimensional image (which some when may be corroborated or refuted by further empirical data if they manage to obtain a crystal of the protein). This is what they call the 'comparative modelling' of proteins.
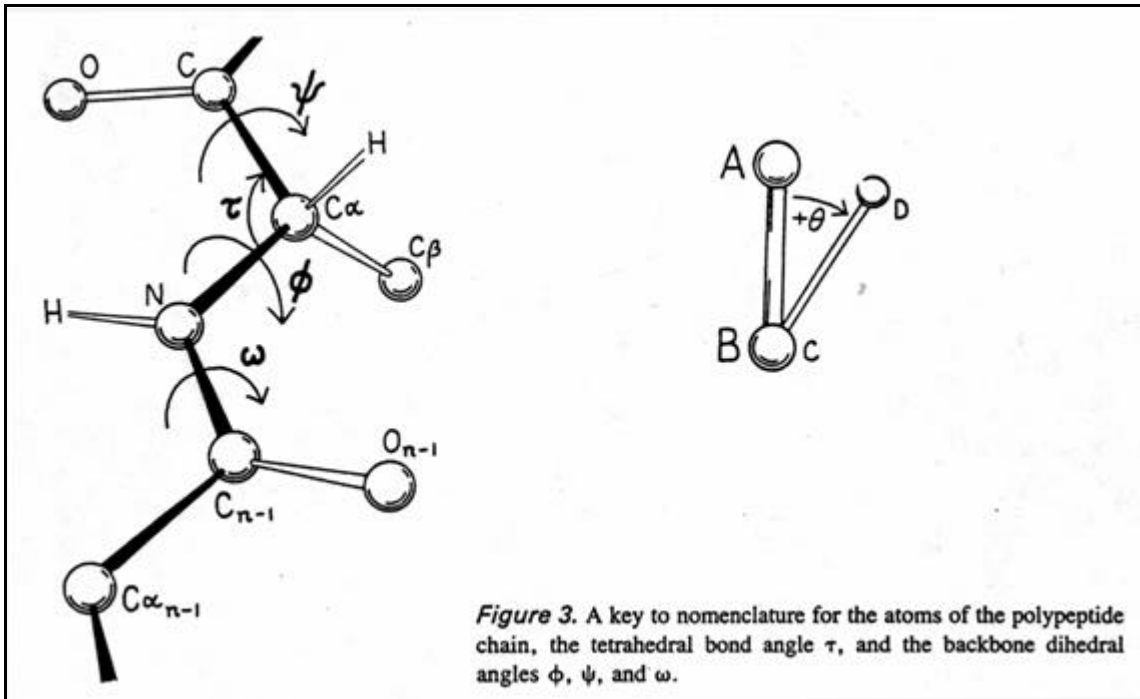
One can also play with such models trying to guess how proteins would react under such and such conditions. In other words, they can do simulations, studying their object's behaviour through experiments in the computer instead of doing it on the bench. For instance, they can calculate the best description of the interaction between proteins or DNA, or between two different proteins (field of study called docking). Or they can analyse the folding and unfolding of proteins (field of study called molecular dynamics) –for instance, one can make a protein and its mutants lose their natural conformation, that is, unfold them into a flat chain of amino acids, and then look what energies, time and conditions are required for proteins to fold again like in their native form (in case they reach it).

These programs are complicated. In order to understand them, one needs to be familiar with computing language and informatics. Actually, the work in this laboratory remains incomprehensible for those that are alien to this world, who in conversations easily recognise that "biocomputing work is very esoteric; in spite of seminars and presentations, we never quite understand what they do" (EI09-21). Members of biocomputing claim to understand the experimental world better than the other way round. Partly because the degree in biology prepares them to confront an experimental world, and it does not always include knowledge on bioinformatics[23]; in this sense, biocomputing is still a marginal perspective compared to the classical knowledge in molecular biology, for instance. But partly, too, because of the opacity of the informatics procedures, full with mathematics. As biocomputing scientists frequently joke when talking of others (and of themselves at the beginning of their training), "as soon as they see an integral sign, they all get scared. You have to enter the world to understand it" (EI02-28). The reader should not get scared, though: we will attempt to gain a glimpse of this world through a detour, trying to understand the logic underlying some of these programs while avoiding their calculus.
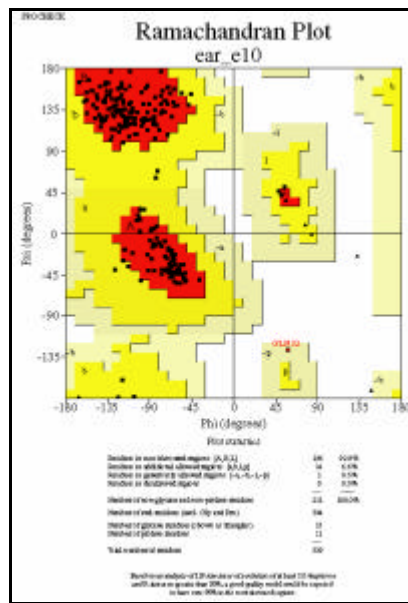
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[23] When we carry out fieldwork, bioinformatics and computing techniques were not taught at university. Nowadays some universities have included this field in their degree programmes.

We know that proteins are chains of amino acids. There are 20 different amino acids, which differ from each other in the type of residue they have. The residue, then, is the informative part of an amino acid. Imagine we wanted to introduce in a computer data about the three-dimensional structure of a protein. A solution could be to specify the position in space of each of its residues for the three axes x, y, and z. Then each amino acid of a protein would be represented by three numbers. The problem being that every time the protein changed place, we would need to actualise the coordinates. That is, even if the three-dimensional structure remained invariable, every displacement in space would entail a recalculation of the coordinates of each of the amino acids of the protein. This problem, which would make calculus with structures a very laborious and troublesome task, persists as long as we define the structure of the protein as a function of external coordinates (x, y, z).

To overcome this difficulty, scientists in biocomputing have designed a new frame of reference: instead of providing information about three-dimensional structure through external spatial coordinates, they do it by indicating the degrees of the rotation angles that residues define respect the peptide bond. Information about the angles within the structure is completely internal to the shape of the protein, and is not dependent on the position in space. As we can see in this drawing, each residue is involved in one angle phi (F) (Ca-n) and one angle psi (f) (Ca-c). This two angles define now the residue, that is represented by two values.

Figure 3. A key to nomenclature for the atoms of the polypeptide chain, the tetrahedral bond angle τ, and the backbone dihedral angles ϕ, ψ, and ω.

The two values attached to each residue are what they call internal coordinates: by specifying the degree of these two angles for each residue, one can describe the structure of the protein independently of its position in space, and since now each point can be described with two numbers instead of three, calculus are further simplified. Moreover, each residue can be represented in a two-dimension grade, whose axes represent the two different angles. This representation is known as Ramachandran plot:

Each residue falls into an area (for instance, those residues which are part of regular helix structures are defined by angles of 45 degrees. Other areas are for beta sheets and for irregular loops). The whole area is divided into sections and each section is symbolised by a letter. In this way, if a residue has the letter A, for instance, one already knows it is part of an alpha-helix. And, consequently, one already knows how one has to draw it in space. That is, one does not try to find out the exact degree of the angle, but the area in which it falls, and deduces from it the 'ideal degree' of the residue –one exchanges exactitude for simplicity in order to accomplish representation. Repeating this operation with each residue, one can reconstruct a partial conformation of the three-dimensional structure of a protein. Of course, all these operations are carried out by a program. This method, then, a) achieves a certain *decontextualisation* (from external to internal coordinates), b) simplifies calculus (each point is not defined by three values but only by one) and c) allows the translation of three-dimensional structures into a sequence of letters.

A consequence of this procedure is that not only graphics are possible, but also comparison of different structures of proteins. If we simply had the graphic, we could deduce only visually whether two proteins look similar or not. If we also have a sequence of letters which informs us about the three-dimensional sequence, we can

compare them to find out about structural similarities. Following this logic, one can construct programs whose function is the comparison and classification of proteins according to their structure (e.g., SCOP, CATH). One of the members of the team, for instance, is involved in the construction of a program which compares and categorises loops, a very irregular structure of proteins which always offers a certain resistance to classification and prediction. In this sense, the graphic nature of the protein enables the protein to inhabit an interface between a two- and a three-dimensional space. If, as Latour said, the two-dimensional character of inscriptions allow them to *merge with geometry,* partly thanks to perspective (1990, p. 46), the same applies to the computing protein, but even more radically: a one-dimensional information (sequence) is able to be connected to a three-dimensional space, blurring the distinction between nature seen as fiction, and fiction seen as nature (ibid, p. 29).

c.3. The constitution of the protein

Fixing proteins: seeing and touching

We are describing a setting quite different from an experimental laboratory, a different assemblage scientist-object. 'A protein' appears as a different type of entity here when compared to an experimental protein. The first characteristic which stands out when observing biocomputing practices is their highly visual nature; every change is represented in the screen as a three-dimensional figure, a protein with fixed boundaries, a body, with a shape that can be observed from different external perspectives. Moreover, many operations reproduce a visual exploration; some of their programs comparing proteins require, in spite of all calculus and program sophistication, a phase in which similarity needs to be detected visually, that is, through pure observation through the "naked eye".

The impression of dealing with a visible entity with clear boundaries is helped by the fact that many of the operations that these programs perform can be conceptualised as

manual manipulations of the proteins; it is almost as if these programs were the hands of the scientists. They explained to me that when the computer compares the structure of two proteins, for instance, it is *as if* the computer put one on the top of the other, subjected them together and calculated the degree of overlap of the two. Whereas this is clearly not the way program instructions are formulated, this is the way in which the program operations may be translated and understood –at least, it seems to be the way in which they make sense of them, and not only when explaining them to me, but also when talking among them. When they compare proteins, they talk of 'fixing' the amino acids, and they described the functioning of the programme saying that "it is as if you subjected the protein with two drawing pins in these two points"[24].

There is another clear example to illustrate how the program acts as a manual operation on the protein. When they calculate a molecular dynamic, for instance, they are interested in seeing if a particular protein may fold or unfold. To do this, they introduce instructions in the computer that simulate forces pressing the protein until bringing two parts in contact, or separating them. Thus, instructions 'force' folding and unfolding, in the attempt to simulate the 'natural' attraction and rejection forces inherent to the protein. They do not say 'these two radicals attract themselves with such and such a force', but rather, 'to get this bond I need to apply such and such a force'. Thus, again, it is as if instructions worked as hands manipulating the protein, pressing to join, pulling to separate. Likewise, the program which calculates the most likely form of interaction and docking between molecules plays with several possibilities in the same way as a child could try and fit two Lego pieces together, or two pieces of a puzzle –that is, moving the two pieces around, trying which conformations fit best, or, in our case, which conformations obtains the best values.

When one brings together the strong visual component of the results of these programs and the kind of manipulative explorations they allow, we could claim that these biocomputing programs give birth to a particular hand-eye relationship. Of

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[24] In other occasions they subject or fix the protein through other elements, such as bonds. For instance, the program developed by another member team compares the structure of the protein, but "fixing the disulfide bridges", that is, fixing the proteins to be compared at the point of these bonds.

course, relationships are highly mathematic and formalised, but their effect is to create a substitute for hand manipulation: instead of moving the protein around physically, scientists can do it conceptually through mathematical movements. As if they obtained mathematical hands to move the protein around accompanied by their explorative look[25].

We have seen that the type of operations one can do with proteins are different than in the experimental laboratory. They do not 'catch' or 'fish' proteins, they do not need to capture them through attachments and detachments, or try to guess their presence by uniting to dying solutions or fluorescent reactions. When observing the way members of the biocomputing team worked and talked of their programs and proteins, it became quite clear that proteins were not anymore those elusive, indefinite beings we have encountered in the laboratory of Protein Engineering. One can take it, turn it around, watch it from every single perspective, fix it, anchor it, subject it, overlap it with other proteins, fold it and unfold it directly. Here the limits of the protein are quite clear: they are things that can be drawn and visually compared. And in one sense, they can even be touched. For them, the protein is there, clear in front of their eyes, more solid and apprehensible than the invisible, untouchable experimental protein.

Simulating proteins

The protein elaborated in the bioinformatics laboratory offers an interesting challenge. Like in the two previous cases, the protein itself is an achievement: they need to create programs in order to make the protein appear and act. Moreover, the protein can be put to the test exactly as in an experimental situation: one can make proteins fold and unfold, one can make it interact with other proteins or molecules. Or one can simply observe it and classify it. Like other objects, the computing protein can *object* : a protein may resist folding or classification, a program may fail.

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[25] In this sense, we could say that the topical contexture of this lab has points in common with what

However, unlike the two previous cases, the object-protein is not enrolled in the production of an inscription: rather, it is itself already an inscription!! Indeed, the protein is not perceived as an object partially created by scientists, but as an entity resulting almost entirely out of scientific manipulation. This protein is constituted by data coming from the other two: by the sequence of amino acids of the experimental protein and by structural and energetic information of the crystallised protein. That is, the nature of this object is perceived to be the same as the nature of inscriptions: data, signs, information. This inscription is believed to be, not the result of a previously existing object, but the result of introducing structure and sequence data in the computer, or, differently said, it is perceived as the result of the performance of the scientists, and not of the performance of an object. In this case, the distance between the protein-object and the protein-inscription is cancelled. If all inscriptions aim at being representations of something, this is a representation without reference to an object, a representation whose finality is not to point beyond. This protein is a "virtual simulacra" (Duque, 2000) or, in other words, a simulation.

Their belief in the simulated nature of the protein does not prevent them to treat it as an important source of exploration; the simulated protein is a real entity unfolding its being in front of their eyes, and their practices reveal how real and present it is for them. Nevertheless, when directly asked about the status of the protein, they describe the relation between the latter and the empirical proteins in terms of subordination:

> "A: we are very clear about the fact that what we do is an approximation of something which we don't even know if it works; actually, what interest does it have, whether it works?, ha, ha. No, really, what they [experimentalists] do is what it is valid. Our work is about predicting something very fast, so that they can test whether it works. It is not necessary that our guess is right, you know. I mean, what is important is that you set bounds to the possibilities. For to set bounds -to set bounds to a mutation, for instance- may mean saving three months of work.

Lynch (1991) called digitality.

B & C (simultaneously): And money!" (EG02-33).

The unbearable virtuality of being

One afternoon, the boss of the Protein Engineering and biocomputing laboratories summoned all the members in order to tell them off: the group had spent more money than expected; if they kept on spending at that rhythm, the budget would not be enough to cover the needs of the year, so the boss had a list of 'strongly recommended' saving measures for the future. Some Ph.D. students, having a fast coffee around the machine while waiting for the meeting to start, told me jokingly about the rebuke awaiting them:

> - We are going to be told off now –said a member of experimental.
>
> - But this does not affect us, we don't spend –added with a provocative smile one of the members of the biocomputing team.
>
> - Of course not, only those who work spend –answered back another experimental Ph.D. student, arising suppressed giggle.

It was not the first time that experimentalists and bioinformatics had such a funny grudge. These occasional jokes were accompanied by short anecdotes implying that there was something 'light', and 'easy' in biocomputing, a lack of constraint or impediment to invent explanations. Such comments were slightly reminiscent of the classic division between blue-collars and white-collars, so nicely portrayed by David Lodge (1988) in *Nice Work*: workers using hands against workers using the mind, as if the disappearance of the bench from the biocomputing laboratory was an irrefutable proof of their lack of steadiness. Or even better, as if the biocomputing protein was a question of ideas, and not of materiality:

> "B: The good think in biocomputing is that 'idea you have, idea you can realise'. It is not like in experimental, where you find impediments or you lack the money to do it. Here not, all you need is the head, and that's it" (EI02-28-9).

This description, which is reproduced by either members and non-members of the biocomputing team, is frequently contrasted with experimental practices. In spite of the important degree of decontextualisation inherent in experimental laboratories, our informants insisted that, in such labs, more things need to be brought together and controlled in order to produce results:

> "A: Bioinformatics work a lot, definitely, but it is a clean type of work, I mean, if the first idea is good, you can foresee whether the result will be successful or not. On the bench this is not the case: the way is never straight. You go forwards, then you go backwards because you have to repeat something that went awry... or because numbers never tally, because the image of an electrophoresis is not clear... Experimental work is not particularly kind..." (EI11-17).

This probably explains why members of the IBF think of biocomputing techniques as 'theory', even though they 'run experiments' -deal with data, test hypothesis and produce results. This ambiguity as to whether biocomputing is theoretical or empirical is also reported in the bibliography. Thus, Turnbull (1995) needs to make sense of a similar discussion between experimentalists and computing scientists in turbulence research: "While it is true that they are not running a 'real' flow field and are only solving equations, it is nevertheless possible to get empirical results that are beyond the capacity of the experimentalists (...) If it is possible to obtain empirical results which are not simply inherent in the equations or the coding for graphical display, then it is feasible to claim that 'experiments' can be done with a computer" (p. 24). This type of work is called both 'theory' and 'experiments' because it does not correspond with the way they usually conceive experiments: they are experiments without empirical data. Thus, with all these jokes and comments, scientists were expressing something like the scandal of 'the unbearable lightness of being' –if Kundera allows me to borrow his title.

However, we should not accept so easily the claim that the materiality has disappeared in biocomputing. It rather looks as if it has embodied in a completely different way: no solutions, buffers, instruments and gels, but data, program instructions and computers. All these claims can be seen as pointing to the change of assemblage: the disposition of materials is different. Actually, the biocomputing protein is still strongly rooted in materiality: the constitution of a biocomputing laboratory requires a huge budget, and the investment in impressive equipment is a necessary condition to be able to produce any results at all. The protein consumes high potency computers, sophisticated programs, hours of work and thought of scientists. Nevertheless, if not dematerialisation, one can say that the biocomputing protein supposes a certain desubstantiation, due to a process of *gramaticalisation* (Lévy, 1999): the biocomputing protein is elaborated from data of both experimental proteins, and is translated from substance into code –a reduction to language and information[26] that enables decontextualisation from the here and now.

This desubstantialisation can be appreciated in the lack of deterioration of the protein after consumption. Every time scientists take up an experimental manipulation of the protein, they lose matter, as it is very clear in purifications: the more pure they need the protein to be, the more matter they lose in the process[27]. This wearing-out effect is not to be found in biocomputing: it does not matter how often they make an operation with the protein, the latter does not lose matter –how could it, if it has already been translated into codified information? They do not have to tinker with substances to re-actualise the protein again and again. If anything, the more they consume the protein, the more stable it becomes through an increase of knowledge. This desubstantiation, then, may have something to do with these implications of 'lightness' we encountered before. Perceived as being of the order of information (on the side of words, so to

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[26] This is, to Latour's view, a general characteristic in science: "they [scientists] always move on the direction of the greater merging of figures, numbers, and letters, which is greatly facilitated by their homogeneous treatment as binary units in and by computers" (Latour, 1990, p. 41).

[27] And this loss is important enough for them to introduce 'saving practices', such as spinning test tubes to concentrate the drops spread throughout the walls of the tube, and then recovering the liquid with a pipette. Or such as changing the type of column depending of the aim of the purification –one type if they want to maximise the amount of purified substance, at the price of a low purification level, or another if high levels of purification are preferred, in spite of loss of substance.

speak), they cannot directly retrace it to an empirical referent. They cannot show what Stengers calls 'a conquered link between words and things' (2000, p. 137). They deal with a virtual simulacra (Duque, 2000) or, as they put it, an "entelechy" (EI05-22). The biocomputing protein appears as a *virtualisation* of experimental proteins.

The virtual character of the biocomputing protein introduces certain differences when compared to experimental proteins. The latter is strongly tied to the here and now of a particular context. For a start, it exists only under certain conditions (certain temperatures and pH, in certain solutions and in the company of some substances and not others), and only as long as these conditions remain, and therefore, it can only be perceived *in situ*. Furthermore, one can say that, in a way, the protein unfolds and demands its own time-space: if scientists want to bring the protein to existence, they need to create an adequate context. This claim must be made more precise, though. It will be said that, up to a certain point, scientific practices achieve a manipulation of time and space. Indeed. We have seen, for instance, how scientists avoid cultivating a field of potatoes a year long and reproduce proteins in much faster cycles. But if this is so, it is because the experimental protein already achieves a certain virtualisation respect to less manipulated situations [28]. This is why scientists need laboratories: places of creation and invention, of simulation and imagination that create new entities. This amount of virtualisation notwithstanding, the experimental protein is still able to impose its own speed: scientists cannot drastically alter the speed with which a protein folds and unfolds in the test tube, just to give an example.

All these considerations change slightly when we observe the biocomputing protein, since this protein comes into existence only after the space-time coordinates of the experimental protein have been strongly modified. Of course, the protein at the screen is still a protein in the present, tied to the circumstances of the here and now; but the tight is more loose: it can be observed in many computers at once; it can be recorded into floppy disk and transported and reproduced in another computer; it is socialised

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[28] Less manipulated, not natural or not manipulated. It is not quite clear on what grounds could one affirm that agriculture is 'natural', as in 'without artifice'.

in Internet, where it can be consulted by everybody[29]... The biocomputing protein has not a stable place of reference, but it takes place with every actualisation in 'here and now' when somebody uses it and manipulates it. Thus, the protein stops being a quasi private event, only reachable for those witnesses physically in the lab, in order to become an almost public event, open to all those who can enter the circuit where the protein circulates.

This virtualisation allows to pose questions about the protein which would be otherwise unimaginable. If the experimental protein is not to be found at the beginning of experimental practices, but at the end, so to say, as a result of them, the biocomputing protein performs an interesting reversal: it assumes the actual experimental protein (it considers it as an answer that has entered the world) and problematises it (it tries to head towards the possible questions for which the actual protein is an answer). With their models, biocomputing scientists pose questions about how it moves, how it interacts, how it can be classified... the virtual protein interrogates the experimental protein: it interrogates its space-time, it interrogates its possibilities by playing with it, by bringing the protein to experience its limits, by turning it inside-out.

In a way, the biocomputing protein is a tool to think about proteins. As any tool, it performs a substitution, a 'taking the place of': instead of manipulating the empirical protein, one manipulates its translation into information. Not only is the empirical protein substituted in this process of virtualisation, but also the interaction between the scientists and the empirical protein on the bench. The assemblage eye-hand-protein is virtualised giving way to a new assemblage scientist-computer-protein. In it, the protein enjoys more flexibility of manipulation, given its more loose connection to the here and now. Even when it comes to time. We have already seen that biocomputing allows a dissociation between the 'human' time needed to simulate and obtain the results of a folding of a protein (which can be of up to a year), and the

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[29] One can reply that the experimental protein can also be transported. Indeed, it can be transported but only after having been transformed into an inscription or another type of substance –for instance, a lyophilised protein. That is, only after having been virtualised in another way.

sequence of 'protein life' this simulation represents (biocomputing programs only manage to simulate nanoseconds of 'protein time'). Whilst one could think that the biocomputing protein entails a deceleration of the natural protein (an engineering protein unfolds faster than a biocomputing one), biocomputing work usually involves acceleration in comparison to experimental work at the bench; actually, many people consider that computing works are faster tha n experiments:

> "These machines go so fast, that if you have three or four people with a suitable brain for such things, all you need is a couple of good ideas to obtain some good results in a reasonably short time" (EI05-20).

The advantage of biocomputing, as a person put it in a somewhat overstated way, is that it allows "two-minute experiments instead of two-month experiments" (EI11-19). However, this increase of speed only happens in some type of biocomputing works. Others, such as molecular dynamic, may require exactly the same amount of time as an experiment at the bench, they can even last a whole year. Nevertheless, regardless of whether it is fast or not, computing allows parallel work, that is, one can work in more than a project at a time. All of which means that the biocomputing team is rather prolific when it comes to publications. (Which explains, some say, why bosses are so interested in this team). The effect, all together, is an increase of the speed of production.

Another example of the manipulation of parameters that bioinformatics allows is offered by a particular orientation of molecular dynamic practiced by one of the members of the biocomputing team. When trying to reproduce the folding and unfolding of proteins, biocomputing scientists face a problem: programs can only reproduce nanoseconds, whereas the actual performance of the protein takes longer (microseconds). This means that they could only reproduce a very short part of the whole process. However, some scientists attempted a short cut: if one could manage to make the protein move faster, then it would undergo more transformations per time unit. That is, if they could manage that the simulated protein moved faster, in the same amount of time the protein would have reproduced a longer piece of the process

of folding, and scientists could observe more than before. The question was, how can one make the protein move faster? In order to introduce more energy into the system, what scientists do is increasing the temperature of the simulated environment: if most simulations reproduce conditions at 'room temperature', these scientists are trying their experiments at... 600ºC!![30]

This is a way to introduce energy into the system and accelerate the motion of the biocomputing protein, but it is also the best way to scorch an experimental protein, which shows signs of commodity at the moderate temperature of 37ºC! (For this protein, oscillations above 42ºC or below 4ºC are already thermic shocks). Only in a simulated environment can a simulated protein survive at 600ºC. These artificial conditions have the aim to reproduce possible end states of transformations of the protein, and not the process through which the protein reaches them, so that they discover reasonably fast what kind of constructions are feasible or not. These strong manipulations should allow a fast production of results so that experimentalists themselves can go faster in selecting courses of action.

In all truth, we have to add that such modifications do not go unquestioned by some scientists, not even by those working in computer simulations. Thus, for many, including some members of the biocomputing team, the tactic of increasing the temperature is problematic. First, detractors say, nobody can assure that the end result of a simulation taking place at 600ºC will be the same as –simply faster than- the end result of a the same simulation at 37ºC. Second, even accepting that the end result is the same, nobody can assure that the process or trajectory followed by the protein will be the same in both cases.

The dispute between these two ways of conceiving molecular dynamic simulations had reached the laboratory, dividing it in two: on the one hand, a Ph.D. student using

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[30] Molecular dynamics is based in notions of energy: the total energy of the system remains constant, and it is subdivided in potential energy and kinetic energy. A key formula in this field is: $r = r(0) + v * t + 0,5 * a * t^2$. Speed (v) is determined through temperature: manipulations of temperature allow an increase of the kinetic energy, that is, of movement.

the more aggressive technique (high temperature), working in collaboration with a very prestigious European scientist from outside the IBF; on the other, the rest of the biocomputing team, some of which had been working with the softer version of the technique (low temperature). Whereas the first team had been obtaining very good (and publishable) results at that time, the second fraction remained sceptical; they were not particularly impressed by the results and considered them to be "artefactual", since the method was "anti-natural". This second group made its disagreement quite noticeable: every time conversations in the laboratory touched the topic of molecular dynamics, somebody would drop a joke about this technique. My attempts to discuss the issue with the Ph.D. student working with the aggressive technique were indefectibly followed by comments such as "Don't talk to him, Psycho, he is on the dark side of the Force!" (which, once translated from Star Wars language into English, means something like 'he works beyond the scientific realm'.

It is interesting that scepticism within the biocomputing team takes a form not dissimilar from scepticism between experimental and biocomputing laboratories: insinuations of artefactuality. This is not a surprise, since, to the eyes of long accepted methods, biocomputing techniques challenge some basics –such as the need to link one's construction to an empirical referent- and are therefore perceived as having much more scope for invention. As Stengers puts it, members of the computer team are "researchers whose engagement no longer refers to a truth that would always silence fictions, but to the possibility, whatever the phenomenon, of constructing a mathematical fiction that reproduces it" (2000, p. 136). They put mathematics to a fictional use; they join a multiplicity of factors made to perform on stage, and do not perform the type of purification or abstraction which Stengers describes as typical of experimentation; neither do they need to concern with experimental control; laws are not abstract deductions, but are constraints imposed on the performance. In short, a different assemblage seems to emerge where old limitations are ignored, and new possibilities invented. The novelty lies in the new ways of creating explanations that in a more indirect way can be put to the test; in the creation of possible, coherent narratives that will have to prove their capacity to render the world meaningful. In

short, biocomputing techniques participate in the emergence of a different assemblage, enabling new relations between descriptio n, explanation, and fiction; in other words, between truth, reality and practice (Stengers, 2000).

3. One protein, two proteins, three proteins: the hard work of coordinating the object

We have presented three different ways of studying and analysing proteins taking place in three different laboratories. Three different practical engagements giving shape to three different PCIs –different consistency, different information, different characteristics. Indeed, if objects can be identified through their performance in space and time (in short, by its envelope, to use a Whitehead's term actualised by Latour, 1999), we are facing here three objects which behave differently: they come into being through different methods and procedures, they react differently to the stimulation of scientists, they constitute scientists' practices differently, they originate different types of inscriptions, they provide different information and they display a different relation between the object and its inscriptions... The kind of operations they allow are not the same, and different conclusions can be extracted from each of them.

For instance, each of these proteins propose a different way of understanding the similarity between proteins. I remember assisting to the viva of a Ph.D. student of the biocomputing team. He was presenting a program that classifies similar proteins according to structural criteria. One of the professors of his tribunal asked him whether his classification took the functions of proteins into account; the implicit complain of this professor was that one could not analyse similarity ignoring function, for, as he said, two proteins are similar not because they have the same structure but because they do the same. The professor considered problematic what the Ph.D. regarded as advantageous: his program was able to classify proteins according to structure *independently of sequence and function*. Some time later I was in the audience of a talk by one of the leading scientists in computing nowadays,

invited from London (and collaborating regularly with the IBF through the work of one of the Ph.D. students of the computing team). One of the claims he made in the conference was the following: if we only consider similarity in terms of sequence of proteins, we will be missing another kind of homology, structural homology; there are proteins which do not have a similar sequence but have nonetheless a similar structure.

These two cases already show three different ways of conceiving protein homology: proteins can be similar when they have a similar function, when they have a similar sequence, when they have a similar structure. Function, sequence, structure, these are important notions in relation to proteins, and we already know that not all proteins hold information regarding the three of them. An experimental protein informs of the type of function the protein develops, the type of performance and reactions the protein shows in certain situations, as well as the sequence of amino acids of the protein. Experimental scientists have no access to direct data on structure, even though they may deduce structural characteristics of their object from its reactions in certain tests. Whereas the crystallised protein is a very good informant of structure, it says nothing directly about function and performance. One can deduce the function out of the structure, but this remains a deduction, and it is not information directly observable in the inscription. The biocomputing protein provides different information about structure and about structure in movement: it permits to visualise and make inferences about structure, dynamic and interaction of molecules. It may allow inferences about the possible reaction of the protein under certain circumstances. Whereas some suppositions may be possible up to a certain point, it does not allow direct deduction of sequence of unknown proteins and it cannot affirm the biological function of a protein whose function is not known.

At the moment we consider having different proteins, -different assemblages or entanglements object-subject-context- a conceptual problem may emerge, since scientists insist on calling them all 'PCI'. Do we have three proteins, or simply one? Which one is the 'real speaker' for the PCI?' What are the implications of claiming

the existence of three different proteins? Are they simply three ways of representing an external reality, the real, natural PCI? Some would claim this is what scientists could answer –however, I would swear 'my scientists' never said such a thing. Are they three different, independent constructions, proving the existence of multiple PCIs and challenging the existence of a unique, real protein? This they did not say, definitely not. "So"- you surely want to ask- "what on earth did your scientists say?". Scientists did not share our puzzlement, they did not seem to be particularly troubled by this type of thoughts, and I know that my insinuation that they deal with three different PCIs would make them smile (poor Psycho, after so long among us she did not understand much, did she?). Nevertheless, instead of laughing at the lack of reflexivity of scientists, or at their naivety for thinking that reality is singular, it could be a good idea to observe how scientist themselves deal with this question.

Let us start with a somewhat easy example. Scientists recognise the existence of two different proteins when comparing natural PCI to recombinant PCI. But if they admit to this difference, their aim is still to achieve a replica that behaves exactly like PCI, so that conclusions about recombinant PCI are useful to understand natural PCI. In other words, unity is their objective - a project, a desire, and never a taken for granted situation. As we have seen, they continuously need to test that they have been successful, and that the artefact (in the good sense, as in 'gadget') they have created it is not an artefact (in the bad sense, as in a spurious artificial creation of the setting); they need to put their protein to the test to be sure it behaves properly. For instance, read the next two paragraphs:

> "A procedure to purify rePCI to homogeneity from *E. Coli* culture medium has been developed. Recombinant PCI-IIa had the predicted Mr, was biologically active and showed the same Ki as the natural PCI-II. It reacted strongly with serum raised against PCI from potato, and showed the same chromatographic behaviour in reverse phase as the natural PCI-IIa. These results indicate that rePCI folds properly and that its three disulfide bridges are correctly formed" (Molina, Avilés & Querol, 1992, p. 135.

"The chemical purity of the final rePCI preparation *was verified* by subjecting samples to HPLC analysis through a reverse-phase column. The product was homogenous. A sample of PCI from potato was also submitted to the same chromatographic conditions and natural PCI-IIa was found to show the same retention time as rePCI. *A further proof of the identity* of the rePCI was obtained by FAB mass spectrometry as reported by Calvete et al. (1991). According to this method, rePCI had a Mr of 4295, which coincides with that expected from the predicted as sequence of rePCI. This demonstrates that rePCI had neither unexpected chemical modifications nor additional residues and therefore shows that the OmpA signal peptide was correctly cleaved during the secretion of the recombinant protein" (ibid, p. 135, our emphasis).

We do not need to be acquainted with technical vocabulary or the particular test to understand the information of this paragraph: we are informed that the two proteins, the 'natural' (PCI-II) and the 'recombinant' (rePCI-II), have the same Mr value, the same Ki, and the same chromatographic behaviour. Therefore, they deduce, the protein they have created has the same properties as the wild one, and behaves exactly as the natural one does. The performance of the protein is taken as an index of its identity. (Notice, on passing, that performance is not only used to be sure of the identity of the protein, but also of its structure: the protein "folds properly and that its three disulfide bridges are correctly formed". The structure, though, they cannot see – the simply deduce this information: if the artificial protein had folded badly (or, differently than the natural protein), it would not be able to perform equally in the tests). The conclusion, they deduce, is that they must have created a good replica: if it behaves the same, it must be an equivalent protein[31]. The better the results, the more the difference between the natural PCI and the recombinant PCI can be ignored.

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[31] Another way to try and be sure of identity is to submit the object to two different tests expecting convergence:

> "The amount of purified rePCI was calculated by an inhibitory activity assay and also from the A280 of the final solution, using the PCI extinction coefficient E 0.1% = 3 (Hass and Ryan, 1981). *Both methods gave the same result, indicating the high purity of the product*" (Molina, Avilés & Querol, 1992, p. 135, our emphasis).

Here, the scientists' strategy is informative: they do not treat the possible difference between these two problems as an unbridgeable problem, but as a gap which demands work if it is to become narrower and narrower. Separation is not treated as a proof of ontological difference, but as a project of coordination: the more we know, the better we can produce recombinant proteins, the smaller the difference between these two different productions, until reaching the dream of epistemologists: two different methods would be able to show the same results: convergence! Now, I did not find radical differences between the way scientists try to unify rePCI and PCI and the way in which scientists try to establish a common version between engineering, crystallography and simulation proteins. In all cases a strategic work of co-ordination is displayed. I will offer some more detailed exposition of such a strategy.

4. Coordination

To start with, we have to understand that if coordination is possible at all is because these three proteins are not completely different, separated or independent. Actually, the three methods we have presented already assure the existence of some contact points between the three proteins, for, in a way, there is a continuity between them, as if they were a transformation of each other: the crystallography protein is a solidification of the experimental protein, and the biocomputing protein is recreated with the data obtained from the other two. In these transformations, there is always a change, a crossing of a gap (Latour (1999)), while there is also something which remains constant. Thus, the computing protein is connected to the experimental protein through, at least, the same amino acid sequence, whereas, as far as sequence is concerned, the X-ray protein remains unconnected with the other two. But if we take the angles of the peptide bonds into consideration, then the X-ray protein appears all of a sudden linked to the computing PCI through structure; but not to the experimental PCI, since its angles cannot be directly measured. Experimental PCI informs the researcher about the function of the protein; computing PCI does not inform directly of the function, but it definitely allows functional deductions from the

structure and its way of folding and unfolding and interacting; crystallography protein does not inform about function very well. Thus, function[32] links the experimental and the biocomputing PCI, but leaves the crystallised PCI unconnected.

None of the proteins shares completely the information of another, for, if they did, they would be redundant, there would be no gain of new information. If they did not share anything, new information could not be added, and the three proteins would remain for ever three different proteins. Being partially connected, each protein contributes to the construction of the PCI, while enriching the whole with new information:

> "Our work (theoretical or biocomputing) rests on already-known structures, and then we research the way the protein functions, what type of activity it has (...). This is why you need the crystal, it is always a mutual help between theoreticians and crystallographers" (EI02-10).

Since information from one source should either corroborate or complement but in no case contradict information of the other two sources, a lot of coordination is needed to create the achievement of a single, objective protein. Each change in one of the bits will have effects on the others, each new and corroborated information in one of the proteins needs to be incorporated in the constitution of the other two. In this sense, the three proteins shape each other. This being the case, a good, fluid flux of information between crystallography, protein engineering and biocomputing is crucial. The better one laboratory is connected to others, the more it can profit from new information to modify and perfect its protein. This is why the existence of multidisciplinarity and hybridism of approaches in the IBF leaves its groups in a privileged position: the co-existence of experimental and biocomputing laboratories

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[32] There are two different ways in which 'function' is mentioned. When they say 'function', they usually mean the 'task' that the protein has in its organism –for instance, to digest other molecules, to attach to the substrate, to inhibit another protein, etc. But the also talk of 'function' in the sense of 'how the protein works' or 'its functioning' –how it interacts with the molecule, how it folds and unfolds, etc. This distinction is clear in the following quotation –notice, though, the interpretative efforts that this distinction requires: "you try to find out what type of activity the protein has, what function the protein has; well, rather, how it functions, how it works... because you already know the function" (EI02-10).

(plus intimate collaboration with crystallography laboratories) under the same institution is of great advantage in the coordination of results. And this is an advantage which has not gone unnoticed by other scientists –among them, potential and actual collaborators. As members of the biocomputing team put it:

> "A: One of the advantages we have in this house is that we are mixed. And other scientists from other institutions realise it immediately. For instance, that boss in Strasburg decided to collaborate with us because we also had experiments.
>
> B: ...We are lucky, because we touch both topics, other groups don't" (EG02-35).

When coordination succeeds, there are no problems of incompatibility between the character of the proteins; thus, virtual and actual information flow without restrictions,

> "[biocomputing] is an entelechy, but an "entelechy" in inverted commas. You can never visualise a molecule in a test tube, and you have to believe what you see on the screen. But then, what you see has a clear translation into reality, I mean, the results you obtain from the screen can be applied and they have a real correspondence with the molecule drawn on the screen. Therefore this molecule on the screen is a good model of what you can have in the tube... this is a promising tool, and very useful" (EI05-22).

Coordination works at its best when it happens during the process of elaboration of the proteins. For instance, on doing an experiment trying to find out how a protein behaves when a foreign sequence is inserted, an experimentalist may ask for help to the biocomputing team. Then, a member of the latter could model and visualise the structure of the protein, and provide information about where the fragment should at best be inserted, in order to optimise the possibilities of success. Or, for instance, computer people may provide experimentalists with information about what experimental courses of action are theoretically absurd, and therefore, not worth pursuing. More entangled ways of collaboration emerge when two people of the two

teams work in the same issue, with different procedures. For instance, if an experimental researcher wants to explore how several mutants or scrambled forms of a protein will fold and unfold under certain circumstances, she may profit a lot from the predictions about "the same" mutants or scrambled forms that the biocomputing team may produce: the computer program may already inform about which of these mutants are worth constructing, and which not, for they would never be feasible[33]. That is, such an information would allow to decide which possible course of action is likely to be successful, while excluding theoretically absurd choices, saving consequently time and money (and frustration, one may add). These cases, especially the last one, pose more interpretational difficulties for members of the two groups.

5. Limits of coordination

In spite of good will, and in spite of similarities between proteins, there are limits to the possibilities of coordination between experimental and computing approaches. One of such limits is very often 'time'. Indeed, the three conformations of proteins do not necessarily exist in the same time-space scale. Time does not play such an important role when one deals with crystallised proteins. Or when one wants to classify proteins. Classifications freeze proteins in time; you forget that proteins are subject to changes and dynamics, and construct an ideal image of it, so that you can compare the protein and classify it. If you are doing docking, dynamic and time play a role, for you have to take into account how two different molecules fit and interact with each other during the time they are in contact. This does not mean, however, that the research needs to reproduce the same time-scale (that is, if molecules are in contact x milliseconds, the calculus of the computer may last several seconds).

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[33] To clarify, the researcher may start with a linear protein (a protein which has lost its conformation and it looks flat), and see whether and under what conditions the protein folds back into its natural three-dimensional structure. Results might indicate, for instance, what kind of strength one must apply to make the protein fold, what environment is better (with or without water, with or without electrically charged particles), at what temperature, etc.

In one case, though, that of molecular dynamics, time proves to be a crucial issue. This is the technique that resembles more the way experimental people work: they create a theoretical model, i.e., what we have been calling a biocomputing protein, and set it under certain conditions in order to try to reproduce through computer particular states that the experimental protein suffers at the bench. Actually, this is the first technique that the bosses were interested in developing, for it promised to be a very useful tool for predictions providing direct information for experimental researchers. The problem is, however, that this computing technique can only simulate dynamics for the duration of nanoseconds, whereas experimental procedures may need hours. That is, the technique can only simulate nanoseconds of the protein time (it simulates the transformation suffered by the protein during x nanoseconds). In spite of this, this technique may take weeks or months to yield results –calculus of a molecular dynamic may last one year, and whole thesis are dedicated to one such work.

This difference of scale between the times of the experimental and the biocomputing protein endangers commensurability between these two different approaches, since the technique cannot reproduce a complete simulation of a whole bench experiment: a one-to-one correspondence between experimental and biocomputing is still impossible. The dream of some heads of group –to have computing researchers calculating what kind of result a particular experiment will have, or what kind of solution is the right one- is still far away. When it comes to predictions, one has to limit oneself to connecting descriptions. Connections between the two proteins remain still lose:

> "There are projects which are still impossible. You can obtain some theoretical result on the computer, and you can communicate it to an experimentalist, so that they can try and test it experimentally. Or the other way round, maybe an experimentalist observes an interesting protein behaviour, and we can try to explain it. But you can never try to look for exact numbers, the exact result, a concrete prediction" (EI02-34).

The moral is, then, that coordination between proteins has limits: the two proteins cannot be simply connected at any point one wishes. For instance, numerical predictions are difficult to obtain; hypothesis about possible global conformations are a bit easier. To make two proteins out of two different methods coincide in the same behaviour is difficult; explorations with one method of the results yield by another is easier. Coordination is a learning process.

6. Moments of coordination

6.1. Weaving sources: a paper

Either when researching or when publishing the information, the partial pieces of information provided by the three PCIs need to be coordinated and mounted into a coherent, if partial, PCI. Obviously enough, they cannot say things like 'our experimental PCI says this, but our modelled PCI says that" (even though, as we will in short see, there are moments in which they just have to admit that the data are slightly contradictory or does not make sense directly). Rather, they have to mount the bits and pieces while showing that each single piece informs (in the double meaning of the word, 'tells us something about' and 'gives shape to') the commonly constructed notion of the protein. I would like to present a couple of examples of the way in which they coordinate and interlink the information obtained in these three different settings, that is, an example of what we will call 'weaving', a co-ordination or plait of information, through which the constitutive characteristics of the three proteins are braided or intertwined together. To do this, I will take my examples from a paper in which they compare experimental and computational results of several proteins. The reader will notice that by doing this we abandon temporarily our main character, the PCI. The reason for this choice is that its three-dimensional structure is well known. I have selected a paper about other proteins whose three-dimensional structure is unknown, to illustrate how old data, new discoveries, deductions and

experimental data are brought together. Let us introduce, then, the main argument of the paper.

Among other molecules, pancreatic secretions contain considerable amounts of three proteins: PCPA1, PCPA2 and PCPB[34]. These proteins, in their mature state, will work as enzymes, concretely, they digest other substances. However, these enzymes should be active when secreted in the digestive track, but inactive when they are stored in the pancreas, otherwise they could digest the organ itself! This is why in the pancreas they are 'deactivated', that is, they adopt a different structure, and in this shape they are called 'proenzymes'[35]. In what follows, then, the words 'protein', 'enzymes' or 'prosegment' will be equally used to refer to the three proteins. These proteins are to be found in humans, pigs, cows and rats. So we will have, for instance PCPA1 human, PCPA2 rat, PCPB porcine, PCPA1 bovine, etc... The paper will be particularly interested in completing the information known about the human system, that is, about PCPA1 human, PCPA2 human and PCPB human, since information is as yet incomplete. The sequences of each protein are known, there are experimental data about them, but the three-dimensional structure of none of them was solved through crystallography. What the authors present is a biocomputing prediction of the three-dimensional structure of PCPA1, PCPA2 and PCPB. Their computing predictions, therefore, can help interpret confusing experimental results and indicate new possible routes of research.

The paper starts with a challenge: IBF scientists have detected that the sequence of amino acids of PCPB, published by Yamamoto and collaborators (1992) may not be correct: if one tries to construct such a chain experimentally, the protein does not fold properly. The reason, researchers claim, is that the sequence has a mistake in one of

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[34] The P stands for 'pro', the 'CP' for carboxypeptidase (precisely the type of proteins our PCI inhibits), and 'A1', 'A2' and 'B' for three different sorts of enzymes.
[35] They have a different shape, which does not allow the substrate to reach the enzyme, and therefore prevents the reaction to take place. If I express it in the way they told us, the enzyme wears 'a hut' which makes it inactive. This hut is a prolongation of the sequence of amino acids: the chain is longer, and a piece of it folds onto itself, creating this typical conformation (the hut), called "prosegment moiety", or "activation segment". To summarise, our protein is now divided in two zones: the mature enzyme on the one hand, and the prosegment on the other.

the amino acids. The IBF team suggests a new chain with a correction in the wrong amino acid[36], sequence that is reinforced by the fact that the new chain folds naturally. Notice that this argument establishes links between sequence, behaviour of the protein and structure. Such connections are secured along the paper:

> "Among other differences, the human A1 and A2 forms differ from the human B form in their putative substrate-binding sites at residues 243 and 255. These residues, located at the centre of the binding pocket of the catalytic domain, are crucial for positioning aromatic or aliphatic substrate amino acids for cleavage by CPA or basic substrate amino acids for cleavage for CPB" (p. 151-2).

But our reader could remain sceptical: why should she prefer this new sequence than the already published? To strengthen their position, authors provide the reader with more data; using the new sequence, they predict Mr values that resemble those obtained experimentally with the protein in vitro. Thus, predicted sequence data are linked with experimental data: this link should convince us that the predicted protein is in "close agreement" with the experimental protein. And even though nobody would claim that both proteins are one and the same, the clear implication is that both the prediction and the experimental result inform (of) the same phenomena:

> "Based on the deduced amino acid sequence of the isolated cDNA, it is predicted that human procarboxypeptidase B is translated as a protein with an *M*r of 45870. The mature enzyme is predicted to have an *M*r of 34825 after the removal of a pro-segment of 95 amino acids, which is in close agreement with the *M*r of the enzyme determined by sodium dodecyl sulfate-poyacrylamide gel electrophoresis (Pascual et al., 1989)".

Whereas the coincidence in numbers remains convincing, one should not forget all the translations which were needed to establish such a coincidence: cloning,

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[36] They present this evidence with one of these translations so typical of experimental science: they clone a bit of DNA (the gene which will later on express the PCPB), and sequence it. Once they have the sequence of nucleotides, they translate it into the sequence of amino acids. Once the chain of amino acids of the PCPB is explored, evidence is presented that the already published sequence has mistakes: the new sequence has an additional amino acid (a Cys at position 138), and another amino acid must be exchanged (at position 101, Asn should replace Asp).

sequencing, predicting, calculating, purifying, etc. Here we find an effect similar to that reported by Lee (1994) in another context: the high presence of mediations contribute to a higher effect of 'immediacy'.

Now that the reader is convinced that the team has the right sequence, we are given the results of the modelling through biocomputing predictions. As said above, nobody has yet solved the three-dimensional structures of PCPs in humans, this is why it makes sense to make a prediction of these structures, to see if these predictions advance some intuition about the relationship structure-function. Now, if they do not have the crystallographic empirical data about the structure of these proteins, how can they hypothesise its structure? No problem: since human PCPs are very similar to its bovine and porcine counterparts, researchers can take the sequence of human PCPs, and model them onto the crystal structure of bovine and porcine PCPs. To model here means the following: they take the 'skeleton' or three-dimensional conformation of a similar protein, and fill it with the chain of amino acids of the protein whose structure they want to predict -or if you want to put it differently, the chain of the second is shaped according to the angles of the first.

In the case of PCPA1 and PCPB, this strategy is not problematic, since porcine/bovine PCPA1 and PCPB crystal structures have been previously solved, and therefore, the 'skeleton' is available. They can even calculate the degree of similarity: human PCPA1 and porcine/bovine PCPA1 have an 82.1% identity. Human PCPB and porcine PCPB share an 83.7% identity (p. 152). With such a percentage, they can be quite sure of the appropriateness of the model. Now, the case of PCPA2 is less straightforward, because the crystals for porcine and bovine PCPA2 have not been solved, and therefore they must use the crystal of bovine and porcine PCPA1. Moreover, due to other factors which do not need concern us here[37], the percentage of identity dropped to 64%-62.9%. Whereas this identity percentage is still sufficient for modelling, they nevertheless tried to improve the structure prediction through computer programs. That is, in the case of PCPA2, they had two predictions: the first

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[37] the alignment has some penalizations (two gaps had to be included)

modelled the structure by filling the skeleton of another protein; the second was elaborated through computer through calculus (which would be too complicated to explain). The first prediction was then improved with the new information provided by the second. If results yield imperfections, they can claim that this is just a prediction, and any further empirical evidence (whether from experimental results or from crystallography) which contradicts provisional predictions will put the model into question. But meanwhile, as long as no contradiction emerges, the prediction done is not questioned, it is used to obtain and unify information. It helps to coordinate the different sources.

As we have just seen, the structure is intimately connected with the sequence, since it is modelled from it. Changes in the sequence should be reflected as changes in the structure. Indeed, the differences found between PCPA1/PCPA2 and PCPB in the sequence of amino acids are also to be detected when one compares the *modelling* of the PCPA1/PCPA2 and that of PCPB:

> "The second *sequential/structural* difference found is a four-residue insertion in PCPBs (VTQI)[38]. These four residues are structured forming a 3_{10} helix not present in the A1 and A2 proenzymes, which accounts for the enhanced structural rigidity in this region of the PCPB that leads to the null intrinsic activity of the zymogene (Coll et al, 1991)" (p. 153, emphasis and footnote are ours).

The paragraph first reminds us of the intimate connection of sequence and structure by noting that extra residues are altering both, sequence and structure: in particular, that difference in structure takes the shape of an helix found in PCPB and not in PCPA1 or PCPA2 (actually, the paper states that this helix is found in all PCPA2 proteins, regardless of their origin, and in none of the PCPA1 or PCPB). Now, once one settles the link sequence-structure, (that is, once one establishes that these four residues take precisely the shape of an helix), then one is entitled to deduce that this helix –and hence the four residues- may be involved (may "account for") in a particular characteristic and behaviour ("the enhanced structural rigidity leading to

the null intrinsic activity of the zymogene"). The link residue-function is established through the translation 'residue-structure' and then 'structure-function'.

Nevertheless, such connecting attempts are not always directly successful. Next quotation is an example of a frustrated connection function-structure:

> "The already mentioned longer a-helix in the connecting segment of PCPA2 apparently implies that the activation of PCPA2 should be slower than that of PCPB (Avilés et al., 1993). Since this is not the case (Pascual et al., 1989; Reverter et al., unpublished results) further data are necessary to gain a deeper insight into this problem" (p. 153).

In other words, this attempt to establish a connection between structure and function through deduction is not supported by the connection to experimental results; on the contrary, experimental results explicitly obstruct this connection: activation should be slower, but it actually isn't. Here a deduction would be hasty; appearances would deceive us (it only "apparently implies"), one needs to wait for more research before one risks conclusions –in other words, one needs to wait until one more piece of our puzzle is produced that allows us to connect satisfactorily the fact that PCPA2 activates faster than expected with some other sequence or structure information. More connecting-work is needed here.

Not only experimental and biocomputing data need be united. We have seen how crystallography data is also co-ordinated through the inclusion of crystallography data in the modelling of the proteins. But crystallography contributes to the constitution of the PCPA2 in another way: data extracted from the crystallography, and calculated through computer programs allow them to gain insight into the distribution of energies in the interactions between residues:

> "the electrostatic interactions become positive (repulsive) for PCPA2. Thus, it is possible to envisage that the activation domain of PCPA2 is maintained in place by the connecting segment in the proenzyme but, after the first tryptic cleavage, the structured helix of the connecting segment is no longer able to

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[38] VTQI are the four amino acids to be seen in PCPB and missing in PCPA1/PCPA2. They will tell us

compensate the unfavorable interaction between the other two moieties, the whole pro-segment being subsequently released" (p. 153).

These interaction energies allow deduction of structure, and structure allows deduction of function: if electrostatic interaction between prosegment and enzyme are repulsive, it is thinkable that, as soon as the stability diminishes, the two pieces fall apart. Notice that interaction energies are not obtainable experimentally. However, since this data are immediately connectable to the structure, and through it, to function, it does not appear as disconnected of empirical results either: one can now try to find out experimentally whether the two fragments fall apart after a 'tryptic cleavage'. Again, with meticulous work, new connections are laboured into being.

After these deductions (notice the expression "it is possible to envisage"), they risk a more general deduction (that is, they generalise through deduction):

"Interestingly enough, the interaction energies between connecting segment and carboxypeptidase are more favorable for PCPA2 and PCPB, becoming less favorable for PCPA1. This is not directly related with the size of the connecting segment helix since PCPB has the shorter one. Thus, the interaction energies between connecting segment and carboxypeptidase *must* be somehow related with the nature and packing of residues at the interface" (p 153, emphasis added).

And with this last deduction they implicitly suggest an answer for a paradox they settled at the beginning of the paper: even though PCPA1 and PCPA2 have more homology than PCPB (they are more similar), PCPA2 and PCPB fold more similar than PCPA1. The paper, without being able to provide any conclusive argument, manages to suggest that this must have something to do with ("must be somehow related with") the interaction energies.

To give one last example of how information from different sources are coordinated, we can analyse the next paragraph:

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next that these four amino acids take the structure of a 310 helix present only in PCPB.

"The representation of the surfaces of the three modelled proenzymes, and also of the reference structures, allows visualization of a hole located over the enzyme active site in PCPA1 and PCPA2. This hole coincides with the space occupied by the 3_{10} helix present in the activation domain of PCPBs, a piece of secondary structure not present in the other two proenzymes. This cavity could be partially responsible for the intrinsic (residual) activity shown by PCPA1 and PCPA2 since small molecules, such as substrates, could penetrate it. Preliminary X-ray diffraction analysis of human PCPA2 permits a clear visualization of such a hole on the surface of the proenzyme (García-Sáez et al, unpublished results)" (p. 153-4).

In this paragraph we see how they mix information of different sources: PCPA1 and PCPA2 have a hole in their structure. PCPB has no hole, but an helix instead, in exactly the same place. This *predicted* structural difference (a hole in the place where in other proteins a 3_{10} helix can be seen) is then *deductively* linked to the function (the hole may allow substrate to penetrate the protein, partially explaining the intrinsic (residual) activity), and reinforced with preliminary results (preliminary X-ray diffraction showing a hole on the surface of the proenzyme). Deductions are reinforced when they mention that through a second method they also find a possible hole: if both, prediction and X-ray point to similar results, such results gain credibility (with the same mechanism that when two sources with credibility say the same thing, we tend to believe them).

6.2. On the backstage

We have analysed a moment of weaving as it is presented in papers. But for us it is also interesting what happens on the 'backstage' –in the lab and in corridors. There, coordination work is more arduous, polemical and open than admitted in a paper, and it usually implies a diversity of actors and arguments in negotiation. I learnt about one of those moments of polemical co-ordination through an anecdote about the past

circulating in the IBF. It seems that some years ago a member of the biocomputing team made (and published) a computer prediction about the behaviour of two proteins –a protein and one of its mutations, a scrambled form. He predicted that a protein and its scrambled form would behave differently under certain conditions, and gave an interpretation to justify this results. Some months later, one member of the experimental group decided to test experimentally this prediction. Embarrassingly, the prediction was not confirmed: the protein and its scrambled were behaving in the same way. Even though no mistake was found in the predicted results, the general feeling was that, given the discrepancy, experimental results were more credible. They simply assumed the prediction had not been corroborated by empirical results – not a drama, something which may happen. However, some time later, a third member of the experimental team repeated the same experiment, discovering a mistake in the first experimental elaboration of the scrambled proteins. Once the error was corrected, the experimental results corroborated the predictions obtained through modelling.

This story shows the difficulties involved in determining where the 'correct' results lie; as soon as we have two different results, which one should we believe? Thus, the anecdote illustrates a particular distribution of credibility –first, when only two results are available, experimental results take the upper hand. But divergence is also likely to create a tension that needs to be solved with the help of a third work: then, convergence of two results settles the balance (in this case, a second experimental result makes the first result appear as 'wrong').

I witnessed a second situation where discrepancies were detected, in the elaboration of an alignment. This is an exercise which compares the sequence of amino acids of different proteins, trying to detect similarities and differences among all the sequences, in order to calculate, for instance, the percentage of identity of two proteins. A member of an experimental laboratory elaborated one alignment per hand and obtained some particular result. Some months afterwards a member of the computing laboratory elaborated independently the same alignment but per computer

program. When the first experimental researcher got to know that a biocomputing researcher had worked upon the same alignment, she compared the two alignments just to find out, to her surprise and despair, that they were different (it may also be interesting to point that whereas the experimentalist was interested in knowing the computer results, the computer scientist had not shown curiosity for the experimental results). If they had been members of two different competing teams, maybe the contradiction would have been welcomed as a controversial possibility for publishing a new paper. But these two members belonged to the same team and they could not publish two different results. What to do then?

The first attempt to soften the contradiction was done by the biocomputing scientist. He warned that his prediction was calculated using probability and statistics, and it did not necessarily need to be biologically significant; in other words, the fact that his proposal was energetically feasible did not imply it was the one to be found empirically. Therefore, a contradiction should not lead to a dismissal of one of the two models, maybe both had its 'raison d'être'. This argument was interesting, for it involved a first attempt to question the need of convergence in a unique result: the two models would be correct in its own way (although this argument does not challenge the idea that only one model or result could be corroborated empirically). This explanation could have been a good solution to the conundrum, if something had not come in the way: some already-published experimental data confirmed the computer alignment, contradicting the first and other previously accepted results. Once more, experimental data made the balance favourable for computing results against other experimental data. Biocomputing results obtained credibility after being corroborated by empirical data. Once more, convergence of two different results against a third proved to be the defining criteria[39].

Until now, we have presented examples where experimental data played a definitive role settling disagreement. However, experimental data do not always have priority. In our two previous examples, the discussion was about data on performance and

sequence respectively. We know that both proteins, the experimental one and the biocomputing one, are able to provide information about them –the experimental protein directly, by forcing the protein to perform and cutting it into pieces, the biocomputing protein through simulation of behaviour. What would happen, however, if disagreements involved an information which cannot be obtained experimentally –namely, structure?

The folding and unfolding of proteins provides us with another opportunity to see how differences are negotiated: in this case, the same object (scrambled forms of the PCI) are studied by two different people with two completely different methods. One researcher tries to fold and unfold these mutants of PCI experimentally (at the bench), the other through molecular dynamics (a computer technique). At the time I was there the experiment was still being carried out, and results were not ready; the biocomputing results, on the contrary, were about to be published, and they had proved very interesting. Foreseeing possible tensions, I asked the computer person what would happen if the future experimental results contradicted his results. Surprising answer: "nothing!" -he claimed: neither experimentation nor computing have any direct access to structural data; those who really 'see' structures are X-ray scientists. Which means that engineering results on structure could not endanger the validity of biocomputing results. However, if X-ray crystallography solved a three-dimensional structure, he said, any result which differed from it would be automatically disqualified. Notice that such an argument barely questions the primacy of the empirical level. This researcher does not claim that theoretical, predicted data are as much valuable as empirical data. This argument simply displaces the locus of the decision of how credibility should be distributed –from experimental to crystallography. But still, empirical data have priority.

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[39] It would have been interesting to see if two coincident computing results were enough to challenge a dissenting experimental result. Unfortunately, we did not witness this situation.

7. Discussion: objects as complicating projects

The analyses we have presented in this chapter show what is scarcely news –and to which decades of work testify: that scientific practices do not simply discover a reality that exists independently of these practices, but contribute to the constitution of that which they analyse. In all these cases, a protein appears not as a phenomenon with a prefigured shape waiting simply to be discovered, but an effect of all these procedures, the product resulting after a hard process of tinkering and manufacture (Knorr-Cetina, 1977, 1981, 1999)[40]. However, the metaphor of the construction through which this process has often been conceptualised may be misleading, because it might refer us to an image of bricks and building: scientists manipulate meaningless matter (building blocks) which they then signify according to previous agreements, as if scientists constructed reality by putting bits and pieces together following some kind of pattern: the mental design of the mind for some, or cultural meanings for others. Notice, moreover, that in this conception, it is the form given to matter (and not materiality itself) which is supposed to depend on culture –people impose a design on matter, so to speak. Matter becomes the surface or screen where culture and meanings are projected[41].

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[40] For a debate both interesting and exhausting between realist and constructionist positions, see Collins, 1982a; Gieryn, 1982; Knorr-Cetina 1982, 1992, 1993; Sismondo, 1993a,b. This evidence has made people say that scientists never touch 'nature' or 'reality', but only human products (Knorr-Cetina, 1981). The evidence that such a notion of nature or reality is lacking in the lab has turned tables in favour of the symbolic: if there is no 'nature per se', then everything is socially constructed. Notice, though, that the exclusion of nature or reality from the domain of science is dependent on a very purified notion of these entities –what epistemology has usually presented as an 'external reality untouched by humans'. That philosophers have maintained this version does not necessarily mean that all scientists hold it (added to which one should distinguish between what scientists say to non-scientists in front of a recorded machine, or what they say to themselves while they work on the bench, for instance). It sometimes seems that analysts are the only ones insisting on 'natural worlds' and 'correspondence with reality'!! During the months I was in the IBF, I very rarely heard words such as 'reality', 'nature' or 'truth' while scientists were working. They only appeared in my notes or interviews when they tried to answer our questions about the relation between experimental and biocomputing techniques (with the exception of their use of 'natural protein' as opposed to 'recombinant protein). Another example is found in Mulkay & Gilbert (1986), where the authors say that a replication "in no way implies that the empirical regularity correctly represents some feature of the natural world" (p. 33). Scientists quoted in the paper, though, are concerned with whether 'data say what he says they say' (Nesbitt, Ga, p. 31), whether 'interpretation is wrong or right' (Cookson, Gd, p. 31) or whether 'a not untrue interpretation consistent with facts' is a reasonable deduction from their facts (Barton, Ge, p. 32). The translation into 'correspondences with reality' seems to be the authors'!
[41] For a critique of the ways in which objects have been (mis)understood in social sciences, see Latour, 1994

The main problem of the claim that scientists project symbolism onto matter is that it assumes the division between meaning and things, as if they were distinct and extricable -together with the asymmetry between objects and subjects. This is illustrated in expressions such as 'material culture', 'cultural transformation of nature' or social construction of reality (where culture and materiality, culture and nature, and society and reality are thought of as different entities)[42]. As Ingold puts it, "(u)nderstood as a realm of discourse, meaning and value inhabiting the collective consciousness, culture is conceived to hover over the material world but not to permeate it. In this view, in short, culture and materials do not mix; rather, culture wraps itself around the universe of material things, shaping and transforming their outwards surfaces without ever penetrating their interiority" (2000, p. 53). From the moment one assumes these divisions and places creativity on one side, any attempt to talk of materiality is, *ipso facto*, a turn to realism, and any attempt to distribute creativity is a fall into animism or anthropomorphologisations[43].

Whereas most scholars would agree that the manufacture of results is a process through which the world becomes meaningful to us, it may be possible to think of it differently, in a way which does not presuppose implicit divisions between nature and culture and that enables us to make sense of the complicated entanglements one finds in labs. In this sense, Ingold (2000) provides us with an image that can describe these type of assemblages: weaving. This is a movement that does not involve the production of a shape on the surface of something (as when culture is supposed to inscribe itself on materials), but a skilled movement that constructs the surface itself, *without* interior or exterior.

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[42] As Strathern (1992) has shown, the social, the symbolic, the cultural become perspectives illuminating the world –a kind of net one can project onto the world, capturing almost anything. Hence, the proliferations of 'social construction of', as well as attempts to detect how cultural assumptions and values condition or shape the production of knowledge.

[43] On this point, one may want to review debates such as Collins & Yearly (1992); Callon & Latour (1992); Pickering (1994).

That is, weaving is a radically generative movement, where it makes no sense to separate between a previous meaningless surface (object being inscribed) and the subject inscribing (ideas, culture, mind), but demands an understanding of the involvement and mutual constitution of people, objects and environment. Understood like this, this notion may help us change this dichotomic setting into a situation constituted by a field of forces emerging out of the "sensuous engagement of practitioner and material" (ibid, p.57). This 'morphogenetic field' emerges out of this complicated involvement, out of the relation among all entities set up by 'skilled movement':

> "Where the organism engages its environment in the process of ontogenetic development, the artefact engages its maker in a pattern of skilled activity. These are truly creative engagements, in the sense that they actually *give rise* to the real-world artefactual and organic forms that we encounter, rather than serving –as the standard view would claim- to transcribe pre-existent form onto raw material" (ibid, p. 61).

If one takes this proposal seriously, then, emerging forces transform the whole assemblage, that is, all elements participating in/of it, including the person, who does not remained unaltered by the action: "Through this autopoietic process, the temporal rhythms of life are gradually built into the structural properties of things" (p. 61). Thus, this proposal avoids focusing creativity on the side of humans, as if it was an action coming from the person and moulding matter, but a force that "cuts across the emergent interface between them" (p. 57). In that conception, the artefact resulting (in our case, the scientific object) can be seen as "the crystallisation of activity within a relational field, its regularities of form embodying the regularities of movement that gave rise to it" (p. 62):

> "the forms of objects are not imposed from above but grow from the mutual involvement of people and materials in an environment. The surface of nature is thus an illusion: we work from within the world, not upon it. There are surfaces of course, but these divide states of matter, not matter from mind. And they

emerge within the form-generating process, rather than pre-existing as a condition for it" (ibid, p. 68).

Thus, construction of objects is only possible because we are always already engaged in weaving, or better, engaged by a weaving that constitutes us, as much as what we imagine as 'our constructions'. If we can mould the world into meaningful forms is only because we are in turn moulded and already engaged in this world:

> "Opposing the modernist convention that dwelling is an activity that goes on within, and is structured by, and environment that is already built, Heidegger argued that we cannot engage in any kind of building activity unless we already dwell within our surroundings. 'Only if we are capable of dwelling', he declared, 'only then can we build' (1971: 160, original emphasis). Now dwelling is to building, in Heidegger's terms, as weaving is to making in mine. Where making (like building) comes to an end with the completion of a work in its final form, weaving (like dwelling) continues for as long as life goes on- punctuated but not terminated by the appearance of the pieces that it successively brings into being. Dwelling in the world, in short, is tantamount to the ongoing, temporal interweaving of our lives with one another and with the manifold constituents of our environment (see Ingold 1995)" (ibid, p. 68-9).

Regarding this question, we can also resource to an illuminating work by Latour (1999). According to his proposal, more directly centred on scientific knowledge than Ingold's, meaningful results are not achieved by projecting meaning onto matter[44], but by scientists entering and contributing to the constitution of a hybrid mixture allowing for continuous transformations, where each of the elements taking part of the assemblage is subjected to change. On the one hand, scientists manipulate matter shaping it into form, until reaching meaningful constructions, "making it cross the gap that separated it from form" (Latour, 1999, p. 57). (This is why they do not imagine themselves imposing meaning on data, but extracting meaning along the

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[44] See Latour (1996) for a critique of this conception.

process of constitution of the fact itself). For scientists, each of the steps in this manipulation is not an arbitrary (collective) decision, but it is felt as an ontological derivation from the previous step:

> "In none of the stages is it ever a question of copying the preceding stage. Rather, it is a matter of aligning each stage with the ones that precede and follow it, so that, beginning with the last stage, one will be able to *return* to the first" (p. 64).

In each of these transformations, there is something constant, and something that changes: something remains the same, while a part is substituted by something other, so that eventually, we have moved a long way: the end result has no resemblance to the initial step. We find, then, a chain of substitutions. However, as long as each substitution is (perceived as) legitimate, the end product will be considered a faithful representation, that is, a delegation, of the first stage. This is how the last stage is seen still to correspond to the first, not because they resemble, but because they are linked by transformations. Thus, in a metonymic movement, reality and nature enter the lab:

> "The succession of stages must be traceable, allowing for travel in both directions. If the chain is interrupted at any point, it ceases to transport truth – ceases, that is, to produce, to construct, to trace, and to conduct it. *The word 'reference' designates the quality of the chain in its entirety, and no longer adequatio rei et intellectus.* Truth value *circulates* here like electricity through a wire, so long as this circuit is not interrupted" (Latour, 1999, p. 69, original emphasis).

Each transformation, then, supposes a crossing of the gap, a discontinuity, the main one being the step to text, to inscription: a transubstantiation. As Latour (1999, p. 42) puts it, data are never data, but sublata, that is, achievements generated out of practices of transubstantiation –ontological transformations, in which each new state stands for (that is, substitutes, replaces, re-presents in the political sense) the previous, in a chain of translations that mediate between the world and the word. Like in Ingold, knowledge does not derive from contemplation but from these movements

of involvement with the world (Latour, 1999, p. 39; Ingold, 2000, p. 64)[45]. In both cases, then, knowing is attaching oneself to an assemblage, an arrangement, the act of bringing elements in connection, to create connections that constitute the object of knowledge and allow its appropriation.

Thus, the difficulty lies in the articulation, in the assembl*ing*. Once the assemblage is in place, the object emerges, surrendering to affection: it yields itself to new possibilities of being -to the potency of the researcher. And the other way round, the researcher extends possibilities of being by attachment to the object. The subject of knowledge is a connecter that can only emerge as such at the price of connecting herself in turn to this chain. This is why identities, human as well as non-human, are a relational accomplishment, constituted through chains of both human and non-human relations. Knowing as an act of distancing (distancing so as to see better, discriminate better, achieving objectivity or a separation between object and subject) may be impossible, if we understand it as a remaining disconnected from the arrangement through which the object of study is constituted. Rather, when distance or separation appear, they are an effect, a particular positioning regarding the object that is as much subject to the assemblage as the strongest of the involvements –just a different way of being connected.

It is precisely this weaving, this articulation of connections, this collective working for the creation of new objects, what we have tried to illustrate with the data. Firstly, the weaving taking place in each laboratory in their attempts to give birth to a protein with characteristics solid enough so as to resist the tests to which they and other colleagues will submit it (Latour, 1987). Secondly, the on-going process of

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[45] Not only should this notion prevent us from thinking that scientists simply 'uncover' entities which pre-exist in exactly the form in which they constitute them, but also from considering that scientific inventions come out of nowhere (*ex nihilo*). For this idea assumes that agency is all on the side of the scientists, as if they simply combined bits and pieces of matter, like Lego pieces, to present new constructions in a zero-sum act, where bits combine without anything new entering the world. Any scientists (as well as any Lego-constructer, I suspect) would reassure us that not any conformation is possible. Or, if we want to express this idea in other words, scientific practices imply the continuous experience that agency is not completely in the hands of scientists, but distributed among all entities participating. In that way, accepting that none of the agents has the control, we open the way for the understanding of the scientific discovery as an event (Latour, 1999; Stengers, 2000).

coordinating different results and sources of information to contribute to the constitution of a 'single object', the PCI. And, of course, the intermingling of both processes, such that modifications of one protein or changes in its interpretation have constitutive effects in the other proteins. But the observation of this weaving poses some questions to us. If each assemblage gives birth to subjects and objects in a process of becoming or emergence, then, we seem to be confronted with three different assemblages: three different labs, sheltering, and being constituted by, different practices, different equipment, different ways of becoming researcher, and, relevant to our point in this chapter, different ways of becoming protein.

Therefore, the question may arise: do we deal here with one single protein expressed in three different forms, or are these three different proteins? Differently stated, does each laboratory put into practice a different way of approaching a protein that pre-exists their practices, as a realist position would maintain? Or do these settings, with their different practices, construct locally three different contextual proteins? This question kept on bothering me for a long part of the fieldwork, in my attempts to make sense of this multiplicity. However, I was puzzled by the conspicuous absence of this question among members of the IBF: it was simply a non-issue; scientists did not feel the need to defend a constitutive equivalence between them. As if multiplicity did not translate into multiples, as if multiplicity of data did not overflow the PCI, as in, in short, multiplicity did not pose a numerical problem. This intriguing absence changed slightly the focus of our attention. What was the difference between our horizons that made the question possible for us, and non-existent for them?

On meditating about the question, we could argue that it only makes sense in a context in which one is aware of two epistemological positions –realism and constructionism- and requires the acceptance of one at the expense of the other. Either the protein is one, approached by several methods which should converge, given the unity of the real entity they approach, or we face three locally situated constructed proteins. This explains in part the defensive position that scientists automatically take when discussing *against* social constructionist positions: on

showing that 'things could be otherwise', construccionism(s) implied the fictive character of scientific achievements, recreating an 'either/or' scenery: "the more constructed an object is, the less real it becomes"[46]. To reveal an object as socially constructed equates to destruction (Latour, 2000). Realism and constructionism are constructed as antagonistic perceptions (while simultaneously they beckon to each other, invigorate each other and reinforce each other). Nevertheless, are they so antagonistic? Or, to state it more softly, even if accepting this antagonistic nature, does this mean that these perceptions cannot coexist in the same person or community? What if the absence of conflicting perception between the singularity and the multiplicity of the protein signals the absence of a conflicting perception between discovered and constructed proteins?[47]

In order to resist the temptation to take sides in what could be read as one more relapse of a debate "realism vs. constructionism", we may find it more useful to focus upon the movements that scientists themselves make, for, in a way, their practices cut across this dichotomy. Instead of inhabiting a landscape where it is necessary to defend one position and eradicate the other, their practices suggest that both perceptions have a place in a lab. Indeed, whereas they still assume (they *must* assume) that the protein they deal with pre-exists their attempts at configuring it, and that the three proteins are not disconnected, they are also quite aware of the constructive power of their practices[48], of the radical impact that their practices have

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[46] This dichotomic, almost bellicose, scenery has also had as effect a reification of scientists' position: analysts present scientists actively constructing reality while still defending that they simply discover. As if to claim that 'things could be otherwise' one needed figures supposedly defending that things are how they are. With this movement, social constructionism shares something characteristic of projects denouncing ideological exposure.

[47] Indeed, retrospectively, when I look at my field notes, I seem to be the only obsessed to detect and testify of the presence of constructions, sometimes marvelling at the openness with which they talk of their clearly interventionist ethos, with the impossible mission of deciding whether they are realists or constructionists. This dichotomy seems to have been a self-created problem, rather than a direct concern of scientists. Attempts on my part to grope my way towards their position in these matters encountered incomprehension –they did not seem to understand me, not because the question is too difficult, but because the question assumes separations they do not make.

[48] Mulkay & Gilbert's (1986) scientists differentiate systematically between data and interpretation, which yields a "divorce between replicability and meaning" (p. 32). This differentiation has usually been used against scientists, as when they are portrayed claiming that 'facts are facts' but interpretations can be wrong. However, scientists quite often accept that –given their awareness that experimenting means constructing- the key element is interpretation, to the point that a proper

in how the object comes into the world –and that, therefore, these three proteins are constitutionally different, that they act, collaborate and object in different ways, depending on the practices of each laboratory.

Indeed, these two perceptions did not seem to exclude each other[49], as if they could articulate both ideas: as if by creating and inventing links that hold trials, they had discovered which of the many possibilities the world is willing to support or tolerate; a way of acknowledging the duplicity already hinted at in the word "facts": "that which is made and that which is not made up" (Latour, 1999, p. 127). As this author puts it, a scientific object can be considered simultaneously an invention, a construction, a discovery and a convention. An invention, since it is the result of the imaginative practices of a community without which the object would never come to life; a construction, that is, the product of these practices; a discovery, allowing the emergence of a form that we feel existed -or, as Lévy (1999) suggests, *insisted*- previously to our intervention; a convention –in that it is fruit of a series of methods, points of view and attributions of meaning that only make sense as agreements of a community.

By this I do not mean to imply that scientists claim to be constructionists. Neither that we offer a more reliable portray of what scientists think. Both are futile endeavours, given the interpretative variability that scientists' accounts are prone to display. Even after performing (what we read as) a constructionist move, chances are high that they have resource to a realist repertoire if asked directly about it. Rather, the point is another: simply, that their practices –including what they say to themselves while they work- make space for interpretations which do not necessarily fit a mapping in terms of an exclusionary division between constructionism and realism. This

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interpretation may even show that facts are actually artefacts. When asked, researchers in the IBF admitted unproblematically that results in themselves "say nothing. You always have to add an interpretation, the problem being that many interpretations are possible". (Pepo, diari).

[49] There may also be differences according to the discipline. In engineering protein, for instance, constructivist accounts will abound in comparison to other laboratories where they do not aim at the creation of 'doubles' or 'prototypes'. A physicist working on cosmology, for instance, may still describe his or her enterprise in terms of adjustment between theories and observations (Pujolàs, personal conversation).

dichotomy, that has had us so busy for years, does not have the same relevance in the IBF. Granted, they are concerned with the dichotomy constructed-real in relation to artefacts, that is, when construction is interpreted in a negative sense, as 'false' or 'fictive'. However, when 'constructionism' is taken in the positive sense (something is constructed, and precisely because of this, it enters the world), demarcation does not seem to take place[50]. At some moments they treat the protein as something pre-existing, sometimes as something they construct, and they do not need to close this ambivalence by deciding either one perception or the other.

This tolerance towards an alternation or co-existence between these perceptions helps us understand why scientists are not particularly bothered, as we were, by questions about the numerical nature of the protein -this, to put it mildly, may be a false problem. As we have seen, in many occasions scientists act as if the PCI was one protein –for instance, when two contradicting results are not accepted, and a third needs to decide the balance in favour of one of them. Moreover, if asked directly, scientists would not answer that there are several PCIs, even less that they have 'invented' them. (When answering, they usually have in mind the protein as an end-result or as the provoking agency).

From this, one could deduce that scientists *are* realists –in the sense that take for granted the pre-existence of the protein which is simply expressed in different ways depending on the methodology with which one approaches it. But with this move, we make invisible other moments in which the PCI's multiplicity is organisationally visible, especially *during* research (in the middle-muddle, one could say): in some occasions one can observe IBF-scientists perfectly aware of the fact that each protein

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[50] The overwhelming presence of construction in everyday scientific practices may be one of the reasons why scientists, like ethnomethodologists, are quite aware of the thin line separating facts and artefacts, and the amount of work that the settling of the boundary requires (see Lynch, 1985a, ch. 4). This is so especially with what Lynch calls 'negative artefacts'. Whereas positive artefacts are accountable as intrusions or distortions in an observed field, negative artefacts are absences of effects where one was expected. Attempts to account for and eliminate negative artefacts put in evidence the active "work of finding", the "management of circumstances so as to *bring out* an intended result" (ibid, p. 119). "There was no attempt to 'clean' materials from all 'contamination' of artefactuality in general, as such an enterprise would have been absurd and would have returned the inquiry to the detached contemplation of a living and macroscopic animal" (ibid., p. 121).

informs about certain aspects and not others, and that there are gaps between them. But this does not seem to worry them, as if this multiplicity/variability was not a challenge to their belief in the coherence of reality. Actually, they do not seem to have to solve this problem a priori, as if they had to position themselves regarding the nature of the protein before entering assemblages.

How could one defend the claim, a priori, that the three PCI are one (or three, for that matter)? Doing so would mean to place oneself in the position of the external observer that enjoys a privileged perspective, distanced enough to be able to enjoy global vision, decide and integrate all the different partial perspectives into one: the parts sum up[51]. (Like Laplace's demon, if we are in the right position, we would always see the same thing –the 'real' one. However, scientists lack the globalising perspective from which they could already know how their object of study is –not because they cannot reach the position from which to enjoy such perspective, but because this position cannot be reached by anyone, one can never adopt an 'external perspective'. Each perspective is simply a result of a particular engagement between person and object that construct relations as distal instead of proximal (Law & Cooper, 1995), but it is no less of a relation for that, and, therefore, the distanced observer is not less caught in an assemblage than the tactile explorer. Which means that an acknowledgement of difference of positions does not justify privilege.

Scientists gather different perspectives, of which none is more global: they simply enter particular assemblages with the protein; each assemblage scientist-instrumentation-protein will give birth, not simply to different aspects (parts of an underlying whole), but to different ways to constitute researchers and proteins. Nevertheless, nothing prevents scientists from trying to coordinate assemblages -to melt or integrate three perspectives, so to speak- whereby a new chain of connections between researchers, proteins, equipment and practices can take place. This is not integration into a totality, simply an open, partial coordination, a project, an on-going process that never comes to an end. However, if this external, privileged position is

unreachable for scientists, it also is for social scientists. As analysts of scientists'
practices, we should not hasten to close the decision that scientific practices leave
open, lest we want to claim for ourselves what we deny others –the position of the
judge who knows more than those we study. We may avoid this false problem if we
follow the connection of assemblages.

To understand how scientists deal with it, we may find inspiration in an old debate in
SKK, the replication debate[52]. Mulkay & Gilbert (1986) -in their response to Collins'
(1975, 1981a) challenge of until then accepted views on replication- reported
scientists as mistrustful of such a procedure: it was not considered a satisfactory
epistemological strategy. As scientists put it, an artefact can also be replicated, and
therefore, obtaining of the same result several times does not add validity to this
result. (Notice on passing that such an argument is quite a thread for the
epistemological argument that validity of knowledge is established by
correspondence with reality: the fact that an experiment 'works' is not taken as
evidence of correspondence; see Knorr-Cetina, 1981, 1982). Instead, scientists seem
to prefer another strategy, designated by these authors as methodological variation:
scientists consider that a result has been replicated if, after *changing* some condition
of a previous experiment, results are confirmed. That is, security in one result
increases not by repeating the same 'it works', but by achieving a coordination of
several 'it works' that manage to assemble together, widening the picture[53].

This strategy, also detected by Lynch (1985a, p. 120), confronts them quite often with
the situation that different methods produce different results when approaching the
'same' object. This circumstance, which may provide "conditions for doubting the

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[51] This integration requires work, though. As John Law (1993) reminds us, when the whole is bigger
than the parts, there has been some ordering.

[52] For an analysis of how this debate contributes to the point of this paper, see Collins, 1975, 1981a,
1981b; Collins & Pinch, 1982; Mulkay & Gilbert, 1982; Pickering, 1981; Pinch, 1981; Travis, 1981.

[53] With this description I am in danger of taking on board scientists' claims as transparent descriptions.
One should not forget that scepticism towards replication is also linked to the fact that scientists know
how difficult it is to obtain the same result (even though they claim it is possible 'in theory'). The
notion of replication allows them a couple of sleight of hands. When dealing with accepted results,
failed replication is explained away as "I couldn't get the technique to work". If results are not
consolidated, failed replication will arise suspicion of artefactuality.

independence of those uniquely available 'structures'" (ibid), may in some occasions lead to accusations of artificiality. But more often, it is simply a signal that more work of assemblage is needed. In short, for scientists, variability is not a thread but a challenge: through variation in experiments, and coordination of results, their objects of study are seen as gaining ontological status. Scientists do not search for clear-cut repetitions of facts in order to increase certainty of their interpretations –they have long ago learnt that facts do not give certainty, or that facts and interpretations are not in a simple correspondence. Scientists do not search for the repetition of the same, but actively create diversity that has to be collectively coordinated with previous results – a collective weaving. Thus, heterogeneity of results spurs their efforts on to engineer and articulate diversity. This is why scientific objects, much as they can be considered 'achievements', can also be characterised as 'projects' (Knorr-Cetina, 1997) or 'aspirations' (Franklin, Lury & Stacey, 2000) -projects with the aspiration to become achievements. That is, effects of a partial, open-ended process out of which unknown, uncompleted, open, ever-changing entities emerge (Rheinberger, cited in Knorr-Cetina, 1997).

This strategy of coordinating heterogeneity does not differ much from skills required to deal with the diversity of results regarding the PCI. When they face the variability in results they do not live this as unsettling, as a questioning of their belief on the unity of reality, but simply, as a demand for them to do what they know how to: coordinate and manage this variability in the construction of their object of study. Thus, even though they have moments of perception of both (unity and multiplicity), they never face the problem of having to decide between one or three proteins. PCI is neither one nor three, but a becoming, a project of constitution of a protein: they face a multiplicity which they know can be articulated together to act as one. Whereas they cannot affirm they deal with three different proteins (since they know they all must be *the* PCI), they cannot claim to have in their hands a single protein yet, at least not until they have done their job. What the object requires in order to appear as

precisely that, an object, is the never-ending work of coordination of a collective weaving[54].

One would be tempted to say that the stronger the connections are, the more singular PCI will appear, and the other way round. There is definitely something wise in this claim: the more and stronger links two entities have, the more fixed and non-dissociable they are, the more they act as one. However, we should not forget that the three proteins are not attached *after the fact*, as if they brought together three already-finished proteins, rather, this coordination is already part of the process of constitution. It is not so much that results about the three proteins need to coincide afterwards; rather, data from one of the proteins is already incorporated in the constitution of the others, which contributes to their compatibility.

This is not (only) a physical union (as if proteins were tacked together) but also a more relational one -proteins entangle and intermingle: information obtained through biocomputing can substitute information obtained through engineering, which means that one part or 'limb' of one protein can be inserted in another protein, as if the part of one protein could replace or fill in the empty place of the part of another protein. There is a re-membering, a kind of 'reshuffling' or mixing of substances, where each of the proteins constitutes an extension (Strathern, 1991; Munro, 1996) of the other two –affecting and being affected by the others, a transformation, a mutual, complicated becoming-other.

What we find, then, is a process of information –of giving form. This is tantamount to say that the emergence of the protein is indebted to coordination between assemblages. Links must be established not only between the three objects, but between practices, groups, ideas. Different laboratory groups exchange information, collaborate, and attempt to attune results. While trying to define the object, these three different laboratories (plus all other laboratories accepting this protein, either as data on which to build, or as data to challenge) are partially connected by this

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[54] As if the object they manipulate is at the same time present and complete (the object as good as we

weaving, through the project of fabricating the PCI. If the PCI dissolves into three unconnected proteins and some of the results are considered artefacts, these three teams will remain disconnected, uncoordinated. If the PCI gains the stability of a single object, it will also solidify relations between the labs. There is, then, a mutual stabilisation between objects and groups. The protein performs, then, as a quasi-object, quasi-subject (Serres, 1982) creating collectives, an effect that we will encounter again and discuss in the next chapter.

This interweaving or intermingling also suggests that bringing the PCI to existence does not always require a reduction of multiplicity to homogeneity. Scientists can often tolerate a perception of diversity that does not call upon simplification to the same –we could say that they discover the "discordant accord as the condition of possibility for the harmonious accords" (Smith, 1996, p. 34). Thus, for instance, when results refer to different aspects or dimensions of the protein, they are not expected to coincide or converge –e.g., when some results refer to structure and others to sequence; in these cases scientists show a high degree of "indifference toward difference": divergent results are simply juxtaposed, without the need of reduction to singularity. At other moments, when results refer to similar aspects, or perspectives which complement each other, scientists try to weave them together, and then information provided by one protein is integrated in another. As long as connection is possible, multiplicity is assembled. Limits of coordination between assemblages, then, constitute the limits of the integration of the protein.

We have also seen some of these limits. When two methods attempt the same exercise (e.g., when two groups produce an alignment of proteins), then they are supposed to yield the same result. In this case, divergences may trigger off engineering in order to achieve reduction[55]. When interweaving is not possible anymore, then divergences are dealt with through exclusion: one of the results must

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can know it), *and* the unfolding absence of the object we know it will become (Knorr-Cetina, 1997).

[55] If divergence is not produced by the same group (or groups in collaboration), but are to be found between competing groups, divergences may be welcomed as possible attacks to other groups, as possibilities for publication.

be wrong. This exclusion can be performed in several ways, of which we have presented two: either through a reckoning of the distribution of forces (two results against one) or through a general challenge to the validity of a method (i.e., molecular dynamics as a method transgressing scientific boundaries). However, this small moments of crises are never articulated in terms of plurality –danger of admission of a plurality of proteins- but in terms of artefactuality. If connections between assemblages cannot be established, and more than one version appear, one will be preserved as more likely and factual, the rest will risk disposal under accusation of being an artefact. Thus, connectivity appears as key to grant existence.

If connectivity is achieved, then the object emerges. The PCI is one protein not because it speaks with one voice, saying the same in every context irregardless of how, when and by whom it is interrogated. However, they can still claim that the PCI is one because articulation is achieved –articulation which only in some cases involves a reduction to the same. If the three PCI (engineered, crystallographied, simulated) act as one, it is because scientists achieve connections that allow circulation from one to the other: one can connect the 'hut' of one protein with the letters of an alignment, the speed of the folding with the chain of amino acids. To put it with a comparison, if each of the PCI is elaborated in a different language, emergence of 'the PCI' as an unchallenged entity does not require translation into a single language, but a construction of bridges between languages. The PCI emerges thanks to these connections, while it transforms into a contextual translator, a kind of round-about connecting assemblages.

This conclusion is sympathetic with those of several authors, who have shown that collaborative work does not always require homogenisation (of claims, of results, a coincidence of projects, etc.) but rather, learning to work together in spite of disagreements and differences (Star & Griesemer, 1989; Fujimura[56], 1992; Turnbull,

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[56] "Post-Mertonian sociology of science has focused on controversies in science and has taught us that consensus is a rarity rather than the norm. Instead, scientific work is heterogeneous in both method and substance.... How do these different words with different methodological and substantive concerns succeed in cooperating to produce new knowledge?" (Fujimura, 1992, p. 168).

1995). The PCI can be understood as a boundary object that allows the connection of heterogeneity. Each object is then just a bridge, an aspiration, half successful, half failed, at the crossing of the gap, pointing at new directions of improvement, weaving in search for the event, for the 'here, we can' that they know possible (Stengers, 2000).

Only when multiplicity is coordinated (either as juxtaposition, a complicated interweaving or after reduction) can the PCI act as one. At the end, there is one protein –an achievement. What we find, then, is a kind of inversion already pointed at by ethnomethodology (Garfinkel, Lynch & Livingston, 1981): if we usually assume that the existence of the object grants convergence of different methodologies, what we have seen is how the work to weave results gives birth to the object. Which means that, whilst the latter is presented as a consequence of the previous existence of such an object, practices show how this relation is almost reversed: the existence of a single, unified, coherent object emerges as a certainty only after scientists have achieved coordination.

These assertions raise one more question. If, as we have claimed, the three contextual proteins do not integrate into a global, abstract one, how we could imagine this union? If we insist on the existence of three different proteins, result of three assemblages, we force scientists to appear as realists: what else can they do but to insist upon the existence of a unique PCI? If we accept their claims about dealing with just one protein at face value, we lose insight on the moments in which these proteins do not match and on the way these three emergent constructions come into connection. Is there a way to think of the parts as being both distinct and still united by an intrinsic relation? Is there a way to imagine how these parts may come together without producing a totalising object (that can only be dreamt of, but never produced)?

Here we may find Strathern's reconceptualisation of Haraway's "partial connection" useful: parts which add to, but not add up; parts which affect each other and have

constitutive effects upon each other, engaged in a continuous process of becoming other. And since this union never resolves into a totality -a whole object- partiality never abandons this construction. The PCI is not an abstraction extracted or deduced from the three local proteins, which would be reduced to being a mere 'expression' of a unified, pre-existing object. Rather, it is immanent to them. The three particular proteins are not subsumed into a generic, abstract one, but they co-exist and co-belong to each other, in a relation of mutual constitution/information, a process of actualisation and differentiation (Duque, 2002; Smith, 1996). The partial PCI and the three partial PCI *are and are not* the same. That is, attachment between the parts is simultaneously a physical coming together (parts united while we can still perceive each one as a distinct entity), and a more 'chemical' or relational coming together (where parts in relation transform into something else).

This notion also helps us conceptualise the flexibility of perception in the labs. At one moment scientists can perceive three partial proteins that act and produce information on its own, just to perceive, in the very next moment, the also partial three-as-one PCI protein. Now they see the parts, now they see the parts in connection. This is, again, the movement between figures we have encountered in the previous chapter. In this way, partial connections allow more space for manoeuvre: both the parts (engineering, crystallography and biocomputing PCI) and the partially-connected PCI have the capacity to extend potency, to affect. Each single protein is able to perform things which the other two cannot: a total, complete resolution or integration into one single protein would be a constrain, since they would lose degrees of freedom, so to speak... If they reduced the three to one, they would lose information, if they kept three, they would lose the object. Coordination does not require a reduction to the same, but a suspension that makes the protein neither one nor three. The dichotomic alternative between singularity and plurality gives way instead to multiplicity.

To summarise, social scientists are not the only ones who know that a multiplicity of perspectives and practices may open multiplicity in the object. Multiplicity is inherent to objects, and scientists have always had to engage in its coordination. Thus, IBF

scientists do not let themselves discourage in front of it, as if multiplicity could overflow the object, multiplying it –one, two, three proteins. They never face 'many independent objects' that they have to unite artificially; nor one single, united, coherent object with different expressions. They never feel they have to choose between one PCI or three PCI, for they treat this protein as a multiplicity in need of coordination (neither one, nor multiple).

This is a coordination, though, that does not create a total, finished object, but remains partial. Scientists experience the partiality of a single perspective –or, more radically, that they can never abandon partial perspectives which add to but not add up. Nevertheless, they do not abandon the assumption that the object can be made to act as one: each approach being a project of PCI-in-process and, therefore, informing it -and not informing *of* it. They face a multiple, partial emergent object in need of coordination –a multiverse (William James, quoted in Latour, 1999). In other words, the multiplicity of their objects is not an epistemological problem for scientists, but an ontological and an engineering one.

Part I. Mediating movement, moving mediation

> "I'm not recommending that we make the margin a new center (e.g. we are all travelers) but rather that specific dynamics of dwelling/traveling be comparatively analyzed"
>
> (Clifford, 1997, p. 101).

1. Power, mobility and marginality in the practice of science

Nowadays, within the realm of STS, it is not necessary to insist in the local and situational character of technoscientific knowldege. The acceptance of this claim has posed new question marks to the field, since the contextual character of science seems to be at odds with the universality of scientific knowledge that many defend with conviction. As scientists proudly insist, (and one cannot help but being astonished that they resource to the same examples), the gravity law works here and in any other part of our planet, and even beyond it! The whole universe is dancing at its dictates –when scientists say 'universal', they mean it. It does not matter with what method one explores a piece of reality; if correct, results should converge into unique data and a unique explanation. And what is true in this laboratory over here must be true in the lab over there. The tension between these two perceptions –contextuality and universality- has openned the doors for interesting proposals.

Some authors have pointed to movement as a key notion to understand the relation between contextual products and universal effects: if science works everywhere, this may be due to its ability to transport itself everywhere. Thus, attention has been focused on the variety of social strategies and technical devices which produce order, enabling local knowledge and local practices to be moved from one research site to another (Turnbull, 1995; Latour, 1987). To account for this mobility, several notions have been created, such as immutable mobiles (Latour, 1987, 1990; Clarke & Fujimura, 1992; Law, 1994), boundary objects (Star & Griesemer, 1989), mutable mobiles (Law & Mol, 2001) -and even *spectral* textual object with mediate partial audiences (Carter & Michael, 2003). These concepts are known enough, so we do not

need to dwell on them[1]. What is of interest, though, is that all these proposals have tried to explain through movement how different localities are connected generating a global dimension. In this way movement has become crucial to understand technoscience and its elaboration. With the two-way movement (Latour, 1990) of these objects, scientists manage to surpass/overflow the walls of the laboratory, spreading science and 'raising the world' (Latour, 1983; for criticisms see Scott, 1991), for movement is presented as intrinsically linked to power: mobility also implies mobilisation (Law, 1986a, b; Latour, 1990). Scientists appear as actors who negotiate and try to have their versions of nature and society accepted (Law, 1986, Latour, 1983; Callon, Shapin, 1984, Pinch & Bijker, 1985). These authorised, authoritarian, versions are constructed through the mobilisation of the world (Latour, 1990). This is why laboratories are privileged places "to do politics by other means (Latour, 1988)".

One of the consequences of this way of conceiving circulation has been the constitution of a landscape of centres and peripheries: centres of calcule where resources and material from the periphery are attracted, worked upon, calculated, and then exported if and when convenient. Thus, centres and peripheries are potential effects of the ordering action of heterogeneous sociotechnologies acting from a central position on the periphery (Law, 1994, p. 104). This point of view has been useful to analyse power. However, since movement has been linked to the constitution of big players and big centres, attention has subtly slipped from movement itself to those points from and to which artefacts or scientists travel. Labs, seen as stable receivers and senders of artefacts - have become a crucial setting, a centre regulating flow of artefacts. In a way, the emphasis in elements moving from

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[1] Immutable mobiles would be "objects which have the properties of being *mobile* but also *immutable, presentable, readable* and *combinable* with one another" (Latour, 1990, p. 26, original emphasis). "Boundary objects are objects which are both plastic enough to adapt to local needs and the constrains of the several parties employing them, yet robust enough to maintain a common identity across sites" (Star & Griesemer, 1989, p. 393). Mutable mobiles would attempt to explain "variation without boundaries and transformation without discontinuity" (Mol & Law, 1994, p. 658). See also Law & Moll (2001) for an attempt to think mutable immobiles. Carter & Michael (2003) present texts that have a fractional and long-term impact; that allow a reversal of intermediation (mediating agents can enrol the source without making it change identity); that do not quite manage to coordinate different

lab to lab has left untouched the notion of the laboratory as a fixed point, as a centre, as an institution: a space circumscribed by four walls. People and mobile property may move, but the house rests behind, unaltered by movement. This image suffers from the same basic problem that Clifford (1997) identifies in anthropology: it only accounts for the logic of dwelling, it only explains the way of being of those who inhabit a space, who dwell in a space. Inscriptions may circulate back and forth, but the centres and the peripheries remain unaltered.

This conception entails an almost geographical understanding of power. Indeed, the differentiation between centre and periphery is one that resonates culturally with our way of conceiving both, space and relations. In concrete, the dichotomy between centre and periphery is supposed to reflect unequal distributions of power in society. Somehow, it is assumed that powerful people inhabit the centre, and from it, rule over the periphery; or that they take over all resources, leaving the periphery too poor even to survive. Power brings (sucks?) everything to the centre, where they are processed, put to use, consumed. Things happen in the centre:

> "the Centre can be defined as the place creating the communication which rules the rest of the Space. Thus, important and transcendent facts are those that take place in the Centre, and therefore, whoever wants to be part of facts, be an event, or participate of the world and of reality, has to head to the Centre, do things in the Centre; and if by any chance another unusual, inescapable, fundamental fact occurs in another part of the Space, this other part will itself up as Centre, leaving the anterior point aside as surrounding" (Fernández Christlieb, 1994, p. 319).

In other words, the differentiation centre vs. periphery has often been equated to that between powerful vs. marginal: power is spatially distributed, accumulated in some places (centre), lacking in others (periphery). This understanding informs what Law (1991) has challenged as a 'heroic theory of agency': if an actant is a powerful hero, is because it has managed to constitute itself as a centre. But also attempts to

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social worlds and that only reach a proportion of the target audiences. They claim such texts cannot be

compensate this 'managerial bias'. For instance, Star (1991, 1992) has outlined the importance of paying attention to marginality, to devalued work, to that which goes underrepresented in encounters with technology, to those who do the invisible work (Shapin, 1989), to those who are delegated to, the disciplined, whose work is often erased: secretaries, wives, laboratory technicians and janitors... Likewise, Mort and Michael (1998) insist that to follow the actor ends up turning this actor into a hero; like Star, these authors demand more attention towards the marginal, as they do with their notion of 'ghost intermediary', those who are not part of the network anymore, but still have influence on those connected.

Whereas we agree on the importance of including multiple membership and multiple marginality in our analysis, we are quite suspicious of an easy equation between marginality and periphery. Equations between 'power' and 'centre' were borrowings from geography in order to denounce unequal distributions but have somehow started to become too literal, power becoming distance, as if one really had to walk miles to go from the periphery to the centre. Moreover, whereas many authors hasten to clarify that centres and peripheries are not givens, but effects, there is sometimes too fast an identification between periphery and marginality, as if the analyst could already assume, simply deducing it from somebody's position, whether this somebody is marginal or powerful. (Hence, the summit of the politics of location, the attempts of talking on behalf of marginal others, and the seriousity with which one is asked: "And you, where are you standing? From what position are you saying this?"). Which leaves us wondering, sometimes, who and with what criteria decides who or what is marginal...

The tension between centre and periphery in the context of movement also reveals a Foucaultian notion of power that understands it as the result of practices occuring in 'centres of reclusion' (Foucault, 1977). According to this logic, power may be exercised once bodies are gathered in a same space and time, so as to be disciplined with local practices of inscription (Tirado & Domènech, 2001). Indeed, this logic

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fully apprehended with notions such as immutable mobiles or boundary objects.

resonates in the type of explanation that ANT provides of the production of knowledge in centres of calcule: effect of gathering, combining and inscribing different heterogeneous materials in an immobile point of ellaboration. The difference between these two logics is that, whereas the Foucauldian logic was basically antinomadic, ANT goes one step farther, and explains action at a distance. However, in both of them the centre is thought as an immobile point attracting resources from the periphery, sending effects/products to the periphery:

> "So, an ordering centre is (probably) constituted by gathering, simplifying, representing, making calculations about, and acting upon the flow of immutable mobiles coming in from and departing for the periphery" (Law, 1994, 104).

This might explain why both institutions and laboratories have become the focus of these theoretical approaches. In that sense, it is interesting to remember the friendly critique that Deleuze expresses regarding the Foucauldian model of exercise of power. On the one hand, this exercise rests on close, immobile centres of kidnapping (Deleuze, 1987). On the other, it betrays a 'logic of solids', to use Bergson's felicitous expression: knowledge is produced in one place, through inscription on a stone or body under surveillaince.

2. Centre-periphery and the Spanish science

If the dichotomy centre-periphery has been sometimes too easily equated to central-marginal in some theoretical works, this identification can also be perceived among scientists too. An argumentation based in such dichotomies can be found in a special issue of the prestigious scientific journal *Nature*, dedicated to "Science in Spain". This special issue portrayed the situation of Spanish science in the nineties in quite an ambivalent light. After praising (relative) efforts made in the last years by government, universities and European funding sources to get Spanish science out of

the state of backwardness in which several circumstances[2] had plunged it, the journal concluded that the latter is not at the level of other European countries yet, still "catching up" in terms of building a strong scientific base. Spanish science is still of a marginal, peripheral nature. This diagnostic did not offend Spanish scientists, on the contrary[3]. Members of the IBF talked about this special issue as a vindication, a way to attract attention towards "their precarious situation", and they would stress that Spanish science was marginal, in the attempt to denunciate the perceived scarcity of resources that the Spanish government dedicates to science. Indeed, it is difficult to talk to Spanish researchers without this theme popping up at some point or another of the conversation, almost becoming part of the self-presentation and identity of Spanish scientists.

The seemingly marginal and peripheral nature of Spanish science been accepted not only by scientists, but also by scholars of science studying the Spanish situation (Santesmases & Muñoz, 1997, p. 188; see also Sanz & Santesmases, 1996). According to these authors, this 'peripheral role' does not originate in our present, but can already be traced to other historical periods. They show how, during the period 1950-1970, when Spanish scientists tried to create and consolidate molecular biology and biochemistry as disciplines on their own, they thought they needed to look for guidance abroad, in the models and contacts from those countries where the disciplines already existed: "Thus, the diffusion of biochemistry and molecular biology from the foreign centres to Spain was accompanied by a process of imitation

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[2] Among historical reasons figure a supposedly "absence of tradition in science in Spain" (p. 1), to which one must add the stagnation suffered in the times of Franco dictatorship. As present difficulties the issue lists the following: the bureaucratic structure in which scientific research is embedded –in both, universities or centres constituting the CSIC, the governmental scientific institute); laws and slow dynamics which impede a fast adaptation to actual demands; precarious situation of postdocs, who, not being able to find a stable job at university and facing the scarcity of jobs in R & D in private sectors, many of them, are forced to go abroad if they want to continue with their research career- which means that the State loses its investment in their training and that groups lack future and stability.

[3] Even though it could have. After all, the argumentation of Nature sketches the periphery in quite an archetypical way: when described from the centre, the periphery tends to be portrayed as empty, a space that can still be colonised and populated (Lightfood & Martinez, 1995). When *Science* claims that in Spain there is no solid scientific tradition, is this not assuming a scientific desert, that now needs to be slowly populated? Is this not making invisible existing scientific fields and works?

of the norms and procedures of foreign societies" (p. 209)[4]. In short, for these authors, looking for one's reflection in the mirror of the European scientific elite reaffirmed and recreated the division between a centre and a periphery -a productive nuclei which disseminates ideas, and a receptive periphery which simply imitates those ideas: "The strong international ties betokened a kind of dependence that typically characterizes the relationship between peripheral countries and the core of scientific development" (p. 211).

However, what Santesmases & Muñoz (1997) never question is the validity and stability of a division such as centre-periphery. Does this dimension have such an explanatory character? Can we really understand the present situation of a Spanish laboratory using this dichotomy? Is a Spanish laboratory doomed, given the socio-economic situation, to remain in the periphery, contended with seeing the 'real players only from the bench'? Is any attempt to connect with foreign laboratories a perverse move that reinforces a peripheral situation, furthering subordination and inequality? Do they make something else than 'mere imitation' possible?

3. International centres and national peripheries?

We should not fail to notice that, in this argumentation –accepted and (re)produced by scientists and scholars- the dichotomy centre-periphery gains its explanatory power in a landscape populated by states[5]. Notice, for instance, that for Santesmasses and Muñoz the subordination does not lie so much in asking for help, as in asking for help *abroad*. With a movement that nobody seems to feel the need to justify, being in

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[4] A clear sign of this mimetic relationship, and of the acceptation of a hierarchy between international and national science is to be found, according to the authors' point of view, in the decision of *not* creating a Spanish scientific journal, in order to promote the publication in foreign journals in English. This decision, according to Santesmasses and Muñoz, "revealed the SEB's [Spanish Society of Biochemistry] consciousness that it constituted a peripheral scientific community with respect to the mainstream" (1997, p. 200), with the result that "Spanish physiology and chemistry journals became less and less relevant for the community as its members preferred to publish their results in foreign journals, and mainly in English" (ibid, p. 200).

[5] Another reference working with this division in clear connection with states would be Christopher K. Vanderpool (1974).

the centre or in the periphery is a matter of national belonging. There are countries which enjoy a central position, others remain in the margins, and one can tell whether a particular country progresses by its displacement from periphery towards central positions. Regardless of how one values exchange between countries, national borders are still preserved [6].

Not surprisingly, this national landscape informing the dichotomy centre-periphery is also impregnated with a developmental notion. The connection between centre-periphery, First and Third world, or developed countries vs. developing countries is not gratuitous:

> "The image of unilineal time informing theories of development, i.e. time running from lesser to greater development, is logico-structurally consistent with the binary space of centers and peripheries" (Kearney, 1995, p. 550, references eliminated).

This clear-cut division assumes that there is a strong and clean centre opposing a dirty, powerless periphery. Whereas this division emerged out of an attempt at calling attention towards (and criticising) a particular distribution of power, it has had damaging effects, as denounced by people belonging to the alleged periphery that have tried to challenge this geographical-spatial distribution (the tradition of post-colonial studies bears witness to this). One of the first critiques it has received is that this dichotomy makes invisible the presence of 'peripheries' in the very centre. Thus, one should not lose sight of the numerous 'third worlds' one could find in the core of

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[6] National belonging is also used as an indicator of 'centrality' or 'marginality'. The development and organisation of science within a country is strongly directed by the scientific policy of that particular state (Solingen, 1996) –notion under which we understand 'collective measures taken by a government in order to foster, on the one hand the development of scientific and technological research, and, on the other, so as to use the results ensuing from this research for general political aims" (Salomon, 1977, p. 45-6, cited in Elzinga & Jamison, 1996, p. 92). It is usually believed that the more powerful the state is, the more money it can invest in science; the less resources the state has, the smaller the budget for science. This relation is however not so blunt. As analysed by Solingen (1993/6), how 'rich' the nation-state is only one of the factors playing a role in the constitution of a budget for science. There are several more: the kind of general policy the State leads, plus the attitude of the state towards science in particular; the degree of implication within international commerce, within international conflicts; the degree of animosity towards other countries, etc. Nevertheless, it still remains true that the potency of the country determines drastically the amount of PNB directed to science.

the First world, the number of 'souths' to be seen in the North, the number of peripheries invading the centre (Kearney, 1995).

Secondly, this image presents the margins as powerless. Whereas the centrality and accumulation of resources in the hands of a minority cannot be hidden, and to pretend so, by claiming that 'power is distributed everywhere', does nothing to solve the situation, several authors have nevertheless reminded us of the power of the margins (Deleuze & Guattari, 1988; Bhabha, 1994; Braidotti, 1994, Spivak, 1990; Kaplan, 1996, just to name a few). These authors, especially those coming from postcolonial studies, complain that the centre-periphery dichotomy presents the periphery as an 'other' which only 'receives' and never produces knowledge, "whereby the transmission of most cultural innovations proceed from the dominant center to the passive periphery" (Lightfood & Martinez, 1995, p. 487). The periphery is thus erased as an active subject, 'perpetuating a kind of colonial discourse in the name of progressive politics" (Kaplan, 1996, p. 88)[7]. Thirdly, this binary opposition obscures all the transactions taking place between both 'areas':

> "Yet definitions of locations as 'centers' and 'peripheries' only further mystify the divides between places and people. Centers are not impermeable, stable entities of purely defined characteristics that come simply to be contaminated or threatened by 'others' from elsewhere". Rather, they "are dynamic, shifting, complex locations that *exchange* goods, ideas, and culture with many other locations. In the context of transnational economies these exchanges cannot be characterized as utopian or neutral transactions. Yet the social model of powerful, unified centers and vulnerable, crisis-ridden peripheries obscures the vital and complicated nature of diverse local economies and cultures which produce and exchange materials, practices, and value on a global scale" (ibid, p. 102).

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[7] Not only are centre and periphery perceived to be separated and of a different kind. They are also imagined to be populated by different inhabitants. Thus, whereas the centre is full with Westerns (usually portrayed as male, white, middle-upper class, sane, etc) the periphery is populated by tribes & nomads outside the state and civilisation, or by Gypsies and other liminal figures outside the metropolis: "These romanticized figures are always positioned in colonial discourse as closer to nature, purer or simpler, and near to vanishing" (Kaplan, 1996, p. 90).

Notice, moreover, that emphasis on centre/periphery divisions is also implicitly accepting a competitive context, in which centre and periphery are opposed to each other. If the periphery ignores the directions imposed by the centre, it cannot survive in the highly competitive environment of scientific research. If the periphery tries to imitate the centre, the second nevertheless wins, for this strategy does nothing else than extending the submission of the periphery (Von Gizycki, 1973; Vessuri, 1996, both of them quoted in Sanz & Santesmases, 1996). This difficult balance forces more marginal laboratories to engage in calculations –strategy known as "differentiation" (Gilbert, 1977; Mulkay & Edge, 1973; Bourdieu, 1975, 1979): what topic is interesting enough so as to enrol support, but not too interesting so as not to attract too many competitors? What topic does not require resources beyond one's capacities, but not too little that any lab can engage in it? Is it more profitable to choose a topic that enables to use the skills and equipment so far accumulated even if many work on it, or is it worth starting a new topic with less competitors? For instance, some laboratories of the IBF avoid working on high competitive research (such as AIDS), for they know they could not veer with the high speed-research of a well-endowed centre. Nevertheless, they cannot choose a complete marginal topic, lest they condemn themselves to the low speed research of an uninteresting periphery. When the story is told in these terms, centre and periphery, national and international science are antagonistically related. Unless...

... Unless one tries other strategies to disturb that division. For instance, finding the balance by working with important topics (such as cancer research[8]), but in sub-fields accessible to their possibilities. To understand how they do it, we must turn to our data. In particular, we will focus on the type of relations this institution establishes with other foreign laboratories. We will show how, by using certain mobility strategies, this scientific group enters in connection with the global network of international relationship, overcoming in this way the dychotomy centre-periphery.

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[8] See Blanco-Aparicio et al. (1998).

4. Follow scientists around... if you can!

We entered the IBF equipped with Latour's dictum "follow scientists around" (1987). And if Latour says 'around', is because he realised that science can go on thanks to the movements of some scientists crossing contexts. Therefore, the task of following scientists wherever they go may be exhausting: the head travels a lot, has constant meetings with different kind of people (academic, but also solicitors, entrepreneurs or politicians), gives conferences and, basically, obtains the necessary resources for the rest of the group to research. Latour's argument, then, is constructing a division among scientists: neckties against lab coats, or, put differently, scientist-entrepreneurs moving around and scientists anchored at the bench. Following the latter, then, should not be a problem; if you stay long enough in the lab, you can observe all their movements. But... can you?

Few days after our being accepted in the laboratory, when we were still shyly approaching the unknown jungle of the bench, we met a postdoc bustling about, rushing from one bench to the other, visibly under time pressure. We would not have dared to disturb her, had it not been she herself who addressed to us, smiling as if wanting to apologise for her frentic activity. She introduced herself, and explained that she was in a hurry because in three days she had to go to a German laboratory, in Heidelberg, for a three-month stay. She wanted to take up an experiment there, and needed to take with her purified, lyophilised protein. "I always try to planify it with enough time, but I always run short of it" –she excused herself. After some minutes chatting, we learnt that these trips abroad were part of a global strategy of hers: she does all her teaching and research in the IBF in one semester, so that she can travel to another laboratory abroad during the second one, as a special type of postdoc. However, it would be more appropriate to say that this is a global strategy of hers *and her head*: not only did the head of the group have nothing against these absences, but he supported them enthusiastically. As a matter of fact, when some years ago the

postdoc applied for entrance in the group, the head set as a condition for her acceptance the postdoc's willingness to travel abroad.

This last piece of information called our attention and we decided to pay heed to these movements to other laboratories. But, honestly, one did not need to be particularly observant to take note of this mobility. Ph.D. students kept on going abroad and coming back subsequently, so much so, that the number of people working in the laboratory in the IBF at a time could oscillate between 3 and 10 depending on the moment in which one did the recount. Within six months several Ph.D. students left the IBF group for Edimburgh, London, Cambridge, Strasburg, Heidelberg and Munich. And all of them, as we will next explain, were organised with the collaboration of the head. Even those Ph.D. students coming from abroad to do the thesis in the IBF were encouraged by the head to spend some time in a foreign laboratory or in another laboratory in Spain. It became clear, then, that movement was, and still is, a common and necessary characteristic of the normal functioning of this group. The question is, then, why (Urry, 2002).

Not every laboratory shows such a high level of collaborations. The heads of some groups in the IBF are known for their reluctance to accept them. But whereas this is not the only group relying on this collaborative strategy, it does make quite conspicuous a use of it. Its importance cannot be properly guessed on observing the movements of a single person: the frequency of trips abroad may oscillate from several times a year, to once or twice in four years. Nevertheless, the visibility of movement comes to the fore on considering the group as a whole, since then we realise that there is always one or another Ph.D. student staying in some laboratory abroad. Collectively regarded, movement is considerable, particularly visible when several stays abroad overlapped. For us, who had known the laboratory seething with hectic Ph.D. students running up and down, it was quite surprising -even a bit sad- to see the bcal laboratory almost empty for some months. Of course, the calm lasted very little, and after some weeks the laboratory recovered its usual appearance.

There are several ways of establishing collaborations so that a Ph.D. student of the IBF can travel to another lab to carry out research. The majority, with few exceptions, require previous agreements between the heads of those groups in relation (either through direct contacts if they already know each other, either through contacts in conferences). They discuss and arrange when and how long the Ph.D. student will remain there, how the stay will be paid and what the topic of collaboration will be. Negotiations may often include agreements on the distribution of the expected fruits of the collaboration –for instance, the order of signatures in a future paper. After arrangements are made and when it is convenient for all sides (the host laboratory, the IBF and the Ph.D. student), the latter goes to the foreign lab for a period of one up to three months (some remain only two weeks, and only in exceptional cases do Ph.D. students stay much longer, but these are rare exceptions). The stay, then, is relatively short; first because the length of the collaboration depends on the amount of money available for the IBF to defray the visit; but second because Ph.D. students themselves tend to prefer short stays, in order not to disrupt too much their private lives at home.

When either the work or the money is finished, the Ph.D. student will come back to the IBF again. Some collaborations may be completed with one visit, others may need a sequence of visits, depending on several factors: the amount of work; the interest of the research or of the collaboration per se (that is, how interesting it is to keep the relationship with this laboratory); the budget the IBF group has at that time to invest in collaborations; and the availability of the Ph.D. student to travel and spend some time abroad. Likewise, the yield of the stay varies from case to case. Some collaborations produce a common paper; others a common piece of research to be further elaborated; some joint works do not bare fruits at all, and any further contact with this lab is ruled out; some strengthen the relationship between laboratories, and leave the door open for future collaborations (with the same Ph.D. student or with other ones). In some cases, relationship are considered profitable enough to establish quite a settled and frequent 'route of exchange', as it is the case of

collaborations established between two groups of the IBF, one laboratory in Girona, and a research team of a hospital in Houston.

5. Translating exchanges

When asked about the reason for their journeys abroad, Ph.D. students and heads list some of the benefits that these stays in host laboratories bring, and that are perceived as giving sense to their trips. I would like to describe them with some detail.

Learning new techniques

A Ph.D. student frequently travels to another laboratory to learn a technique in order to bring it back home and develop it in the laboratory of origin, where it was not until that moment practiced. The first question that may come to mind is: why should somebody go so far away –and at that cost- just to learn a technique? In principle, techniques are supposed to be well explained in handbooks and in the 'Method' section of published papers. If one looks up references in a handbook, one can find a list of the techniques needed. Still, neither papers nor handbooks seem to provide enough information for a person to replicate an unknown technique.

J. illustrates to us this impossibility. Unlike the rest of the members of his group, J. does not research proteins, but genes, which means that he cannot be helped by his groupmates, not even postdocs and seniors, since they all work with quite different techniques. This involuntary isolation has been responsible for some delay in J's delivering of results and for a long learning process based on error and trial, with the consequence that J. had to read tens of papers and handbooks, try new techniques on his own, make thousands of mistakes, learn of these very mistakes and even invent new procedures with the resources at hand when the available standard techniques failed –let alone the impossibility to publish as much as desireable. No wonder that this lonely learning process has hindered the progress of J's research and career.

Stories like this one are not unfrequent, a similar one is S's case. Just like J, S had to start a new and innovative project, with procedures and techniques hitherto unheard of in his laboratory. For months, and in several occasions, he tried to develop an experimental technique essential for his thesis. Nevertheless, success resisted him: one trial after the other failed. In order to help him out of this fix, his thesis supervisor suggested an empirical approach. First, the Ph.D. student had to pick up all the protocoles that could give the solution to the problem. Second, since they had no way to know a priori which of them would be effective, she asked him to test them all. None of them worked. S followed the instructions to the letter, revised them repeatedly just to reach the same conclusion every time: he could not detect any mistake, but all procedures failed.

But S was not willing to undergo the same cross as J: after getting permission from his supervisor, S searched for a laboratory with tradition in this type of techniques; they established contact with the group and S went there to spend three months to learn them. Another postdoc supported this strategy: "it is better to go to some group which dominates the techniques you need, even if they don't work in issues similar to yours, than to go to a group where they work the same issues but with different techniques. In this way, members of the group can tell you what problems they encountered, and how they solved them, and you save a lot of time". Indeed, S's research progressed faster once he came back with these new skills.

Experimenting with other's resources: samples, programs, instruments and machines

Some Ph.D. students travel to another laboratory so as to obtain some resource they do not have available in theirs. Such a resource may be a sample of a special protein, a particular protein sequence, a molecule treated under special conditions. For instance, G travelled to Munich, to a laboratory of similar equipment and resources,

in order to create a 'chimerical protein' with the collaboration of the host group. P. stayed some weeks in Heidelberg, this time in a laboratory of bigger proportions and equipment, in order to attempt a secret, risky and little conventional experiment (which we will not reveal here). But Ph.D. students may also travel because of a computer program which allows them to perform complicated calculations and tackle questions that otherwise would remain out of reach. M, for instance, went to Edinburgh in order to work with a team with which he regularly collaborates. Not only did this visit make the collaboration possible, but also the use of a computing program not patented yet, still under improvement and revision, which he could use only because of his intimate collaboration and temporal belonging.

Moreover, the acquisition of new equipment may be so expensive that many laboratories cannot afford to buy them. The only possibility they have to use such machines is by establishing networks of collaboration which avoid the duplication of resources. We have already mentioned the case of S, who periodically goes to Heidelberg with her protein, with the aim of processing it with a piece of equipment the IBF does not posses. As a matter of fact, the connection between both laboratories is so strong, that the research itself takes place in, and belongs to, both laboratories.

Writing a scientific paper in collaboration

One of the best measures of the success of a collaboration –though not the only one- is the writing of a joint publication after the common work. Even if the visit is only planned as a joint experimentation, it is often the case that the collaboration is rounded off with a paper –or at least, the plan of a paper. As the head of a group visiting a German laboratory told us, his visit had been so productive, that apart from the bench work, he had even had time to write a paper with the head of the other lab. In other cases, the writing of a paper may well be the explicit aim of the visit. Before his trip to Cambridge, B had to conclude the last details of his program; make sure he had all he needed to understand and explain his procedure to others; revise once more

the results of his more recent empirical work; and take all this information to the host laboratory. Once there –where parallel actions should have occurred- they double-checked results, discussed them and wrote the paper. Once the paper was written, it was corrected and signed by the heads of both groups –home and host; finally, it was submitted for publication.

This type of 'writing-up collaborations' helps solve possible difficulties with the English language. We should not forget that journals with a decent impact index require papers in English, and only non-native speakers fully appreciate the impediment this is! To write a paper in a foreign lab usually increases the likelihood to find somebody with a good level of English. This is the case when English people assume the writing, or help the Ph.D. student with corrections and doubts. This help should not be underestimated, given the normative character of English in scientific disciplines, the difficulty to express accurate ideas in this language for non-natives, and the linguistic rigurosity that scientific journals demand in their processes of review.

6. Mediation, translation

In these situations, the simple circulation of immutable mobiles (e.g., inscriptions such as the protocol of a technique, handbooks, a programme, data, papers) does not guarantee that the same result is achieved in each laboratory –universality is in danger, what works in one place could not work in another. There is something which is not captured by the immutability of these mobiles, though necessary to be able to 'practice science'. On asking more about learning techniques, for instance, people started mentioning some procedural details that could make a difference that could, actually, be responsible for the success or the failure of the procedure.

In order to give us a measure of the importance of those small details and variations, people unfailingly helped themselves of a comparison: to work at the bench is like

cooking. They say that protocols are a 'recipe', but similitude goes further: just following the recipe does not grant that the dish will taste good. "This is like making paella" –they insisted- "we all have the recipe, but when you try to cook it, it does not taste as it should. The best thing is to observe somebody cooking paella, and be told what to do. Besides, each cooker has his or her personal tricks". These are what they call, extending the cooking comparison, "Maggi tricks". Indeed, it is otherwise difficult to perceive the subtlety of the procedure. Sometimes, the difference between a successful or failed technique may be the speed with which one adds a liquid to a sample, or the type of hand movements with which one mixes solutions.

Now, the problem is that these details or tricks appear very rarely in formal inscriptions. Only those basic, obligatory steps appear in a protocol and method section of papers –not to mention the increasing tendency to simplify or omit details in the latter. These less formalisable tips are not included in immutable mobiles, but require a less standard procedure to be transmitted (Jordan & Lynch, 1998). This is why this type of learning requires a person-to-person teaching –actually, a teaching closer to an apprenticeship, craftsmanship than to a formal teaching. The acquisition and development of a technique not known in a laboratory often requires that a person, usually a Ph.D. student, moves to another laboratory where the skill has already been socialised and generalised, learns the steps and tricks, and comes back to his home laboratory, where he will have to teach it to others so that the new acquisition does not die with him.

Therefore, the success of a new technique in a new laboratory depends on a transformation: a) adapting people to the particularity of the process and b) adapting the process to the particularities of the new laboratory. This is no simple transmission, but translation. If a Ph.D. student did behave like an immutable mobile, the whole enterprise would be doomed to failure. Thanks to the mutable, flexible, changeable behaviour of the apprentice practical knowledge can travel, move from one context to another. And the same type of transformation is required to deal with a

new machine, a new programme, a new piece of equipment, etc. Without this change, there would be no learning process.

In other words, the scientist-traveller is not an intermediary –transporting proteins, genes or data from one place to another; it is rather a mediator, a sort of mobile device of translation. When our Ph.D. students travel to another laboratory and come back, they make possible the emergence of entities which would not become true were it not for their travelling. In other words, their movement is ontologically productive: new papers, new works, new techniques, new relations, new biological constructions come into being. Thus, something more than a simple transportation of things from one point to another is taking place. The very moment of transportation is one of transformation –which actually, makes it difficult to talk of transportation at all (Latour, 1996a, p. 238). Hence, the observation that 'traduction' is 'trahision' (Law, 1997; Paz, 1971) –and that therefore, translation remains intrinsically linked to power (Asad, 1986). One never goes from the same to the same, each act of translation involves similarity and difference, it always involves novelty. There is always interference –a small deviation, an inclination, a clinamen (Tirado, 1997), that turns the relation from a dyad to a triad: "a mediate, a middle, an intermediary" (Serres, 1982, p.63).

In contrast to the idea that science speaks a universal language within which comprehension takes place, where understanding is taken for granted, Ph.D. students are confronted with quite a different situation. They often bring two groups in contact, which do not work in exactly the same topics, or do not share exactly the same theoretical approach, or do not use the same methods. The task of the Ph.D. students is to cover the gap, to make each position clear to the other, looking for spaces where communication and agreement are possible. Mobile, diverse, always changing, these 'go-betweens', as de Certeau (1997) names them, do not speak a universal language, but must engage in local mediations, local operations (Deleuze &

Guattari, 1988), solving problems and incompatibilities while working[9]. Translation, then, goes hand in hand with mediation (Clifford, 1986)[10].

Now, mediation is an event, that is, a process without subjects and objects, without a point of origin, a source of action; without causes and effects, actors and acted-upons, inputs and outputs (Latour, 1996a). Which means that Ph.D. students translate as much as they are translated, they transform as much as they are transformed. They are mediating as much as mediated -mediation constitutes them as what they are. This claim can be illustrated with the example of P. He never managed to obtain a grant from a Spanish institution for the Ph.D. thesis and has periodically received money from European projects. For him the possibility of being both a scientist and a member of the IBF is linked to periodical stays in foreign laboratories and participation in international training seminars. Paradoxically, being abroad is the only way for him to be a Spanish scientist. Another example is that of C, another Ph.D. student of the IBF who exhausted her source of funding before finishing her thesis. In an attempt to obtain enough money for her to bring it to completion, his head managed to find a grant to go to a French laboratory. Thus, being abroad becomes for many the only possible way of belonging to science.

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[9] With their actions creating commensurability between different contexts, Ph.D. students can be seen as 'shifters', notion that de Certeau draws from Jackobson's theory of linguistics. Shifters are those "who can identify information that can be memorized in its general form, who retain it, and then retransmit it in a particularized translation that is set into a specific situation according to the requirements of the interlocutor, the circumstances, and the context of the transmission.... *translators* who decode and recode fragments of knowledge, link them, transform them by generalisation, convey them from one case to another through analogy or extrapolation, treat every conjuncture of events by comparison with a preceding experience, and, in accord with their own style, shape a juridical logic of the general and the particular, of norms, and of qualities of action and time" (de Certeau, 1997, p. 117, original emphasis).

[10] Not quite, of course. This is the problem with comparisons: as soon as one wants to highlight similarities, is automatically reminded of differences. According to Berg's comments upon Latour's work (1996, p. 254), the notion of 'mediation' betrays less strategic Maquiavelism than 'translation', and welcomes Latour's move from the latter to the first as an acknowledgement of the unpredictability, diffraction and directionless that every merging of two actants entails –a redefinition, therefore, of the notions of 'action' and 'actor'. However, Latour objects that even 'mediation' is a notion prone to too many misuses as to be taken in aproblematically, and recommends a certain distance from it (now that we all approach it, partly thanks to his work!!): "Mediation depends too much on the difference between 'intermediaries' and true 'mediations' so that the sentence 'technology mediates human actions' can be understood with completely opposite meanings. I would now abstain from using this word too much" (Latour, 1996a, p. 269).

7. Hybrid mediation

All these examples show situations where movement is required to create something, a mediating movement. If until recently, mediation was thought to be carried only by humans, an innovation of science studies was the widening of this role to nonhumans: prosthesis, objects, technologies became new candidates. Before the insistence on the mediating role of objects, our data appears at first sight to call our attention towards the old and more known human mediation, also to be appreciated in Shaffer & Shapin's (1985) classic, when they narrate Boyle's case and his trips through Europe, and in Latour's dictum in *Science in Action* (1987) to follow scientists around. Likewise, Abir-Amin (1997) illustrates how the main discoveries within molecular biology could have not taken place had it not been for the exchange of researchers between prestigious centres of the discipline. However, instead of falling again into a discussion which would have the effect of reifying the distinction between humans and non-humans, we prefer to emphasise the hybrid character of mediation.

This does not mean that there are no differences between the humans and the non-humans and their respective mediations –argument which forgets that similitudes and differences might be constructed almost in every comparison depending on the perspective. Rather, it is more a matter of acknowledging that both instances are never found in a pure and differentiated state, but they are constitutive of each other (Latour, 1994), being more interesting to analyse those processes of mutual constitution, those moments in which mediation reveals itself as a mixture between the human and the non-human. Once we look closer, we detect a kind of continuity between different mediations. As Edwards and Strathern (2000) put it regarding Western culture, "(i)f persons belong to one another through what belongs to them, then these mediators may be other persons, possessions, individual characteristics –in short, things material or immaterial, human or non-human" (p. 153).

This notwithstanding, the effects of sending people or sending objects need not be the same. In one of my corridor-encounters with the head of the team, he greeted me with circumspection and told me that they were in a fix. A small accident could rarefy collaboration, long ago established, with a laboratory in Houston. This laboratory had recently sent the IBF some sample of protein, so that the IBF could sequence it. Unfortunately, the protein arrived broken, and could not be analysed. The head diligently contacted the US-group to let them know that the trip had damaged the protein (the immutable mobile had been irreversibly mutated, so to say), and asked them to send more samples. But the second sending could not solve the problem, since the protein arrived broken again! The IBF-head, slightly embarrassed, hesitated about what to do next. Although he knew they had done nothing wrong, he was afraid that the partner group would think they had accidentally broken the protein, or that they were trying to gather more amount of protein to do an experiment on their own. Interestingly enough, as soon as the object was broken, the relationship was endangered, under risk of being interrupted too, supporting Ingold's (1986) suggestion that sociality is embedded in mobile artefacts.

This is where we re-encounter again the importance of the flexibility or mutability of the 'mediating factor'. If one sends a protein to another laboratory, there is already a restriction on the amount of different things this other laboratory can do with a protein. For example: proteins are fragile and can break; the host group may change its mind and want to do a different experiment than the one agreed for; or the laboratory may be so busy, that it simply puts the protein aside, ignores it, and goes on working on some other project. The (certain degree of) stability of some goods does not leave much margin for manoeuvre. Conversely, a Ph.D. is a much more flexible, adaptable good, with open potentialities still to negotiate (which is not void of risks either: as the head of the IBF once implied, you can not always convince a Ph.D. student to behave as you would like her to do). This is why the more flexibility the particular collaboration needs, the more likely it is that exchanges involve people. If the collaboration is solid and successful for a long time, it may be enough with

sending artefacts, but if you are still trying to build and reinforce the collaboration, more than immutable mobiles might be needed[11].

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[11] What is more, you may need to exchange objects or people depending on the aim of the particular phase of the collaboration. For instance, in a collaboration reported in the literature, an exchange of results during a joint research was combined with personal meetings when it came the time to discuss how to write the paper. See, Law & Williams (1980) and Law & Williams (1982).

Part II. Exchanging goods, connecting groups

> "That is why the relation of exchange is dangerous, why the gift is always a forfeit, and why the relation can attain catastrophic levels. It always takes place on a mine field. The exchanged things travel in a channel that is already parasited. The balance of exchange is always weighed and measured, calculated, taking into account a relation without exchange, an abusive relation. The term abusive is a term of usage. Abuse doesn't prevent use. The abuse value, complete, irrevocable consummation, precedes use- and exchange-value"
>
> (Serres, 1982, p. 80).

1. Profitable collaborations, collaborative profits

The movement of Ph.D. students has some characteristics which are worth keeping in mind. First, Ph.D. students do not take the initiative of leaving the IBF. Rather, they are sent. This does not mean they do not want to go. On the contrary, most of them are quite happy with the project of spending some months abroad, and it is not rare to meet Ph.D. students who have themselves asked the head to be sent somewhere. In principle, it is in their advantage (for their c.v. but not only), to have been abroad. But the fact that they accept collaborating with the strategy of the head –more or less actively depending on the case- should not let us forget that we are in front of a collective strategy; this is not individual displacement, but a particular type of movement, very well orchestrated by the head.

Collaborations are, in principle, regarded as very positive not only for the groups collaborating, but also for science in general. Indeed, the very word 'collaboration' has positive connotations, directly related with the norms supposed to inspire the ethos of science (Merton, 1942), and it was indeed mentioned to us in our first meeting as an underlying motivation for them to accede to our studying them ("collaboration has always been a value for us"). As they often say, collaborations belong to science –sharing information, learning from each other, helping each other.

Consequently, to spend some time in another laboratory is mostly regarded not only as a personal chance, but also as an extension of scientific activities, an expression of cooperative spirit and connectivity. As one Ph.D. student put it when asked about stays abroad, "I like science, and this is part of science -to make contacts, to get to know interesting people. I think it is worth it" (EG02-19).

Much as they defend the collective and cooperative nature of collaborations, they never claim that they are 'disinterested', 'just for the sake of knowledge', but acknowledge that collaborations entail advantages for all of them. We have just seen some of the benefits they produce. Every trip leaves our IBF group more rich in knowledge, resources or c.v., more prepared to compete, to apply for new grants, etc. And this is even more so when the host laboratory is more prestigious than the group sending the student (i.e., with a better c.v., more renown, etc.). In short, these exchanges are strategically oriented.

The sending laboratory is not the only one profiting from collaborations; benefits accrue to both sides. The host laboratory also obtains advantages from the visits of foreign Ph.D. students, and therefore, their visit is usually welcomed. For a start, visiting Ph.D. students bring with them innovative ideas, or new materials, new projects to be worked upon, all of which means new possibilities of expansion for the host laboratory. The latter may show in memories that they receive visits from laboratories abroad –which is always a sign of distinction. Furthermore, they are offered sending back a Ph.D. student to the IBF if they want to (although host labs do not tend to do it, due to several reasons[12]). Other transactions are possible. Thus, if one or several heads of the IBF organise some conference, they may invite some collaborating heads as guest speakers; or they may invite them to spend some time researching in the lab or as jury in the viva of Ph.D. theses, etc. -with the added attraction, they say, of a country as Spain.

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[12] First, not all laboratories have the tradition to send Ph.D. students abroad, not every laboratory has the same level of mobility; second, Ph.D. students try to go to countries and laboratories more prestigious. Thus, if an English, French or German Ph.D. student wanted to go abroad, chances would be high that they chose a lab in the USA. Nevertheless, the possibility of sending a Ph.D. student in exchange for accepting one is there.

One of the most important profits that the host laboratory obtains lies in the production capacity of the visiting Ph.D. student himself. Since the latter will spend some time working in the host laboratory with its methods, techniques, programmes, material, space, etc, the host laboratory is entitled to re-appropriate part of this investment, and it does so through the co-signature of all the papers related to the work done during the visit -signed by the group of belonging of the student, plus the group who has temporarily adopted her, which translates as an increase of production for the host laboratory. This increase is not dismissible, since a visiting Ph.D. student is 'particularly motivated': he knows he has only some weeks to perform a task in a place with more resources than its origin laboratory, and feels he is working against time. This pressure makes him work harder than 'autochthonous Ph.D. students' –and this opinion was shared by both, Ph.D. students and heads alike. Three months of work by 'visiting students' may amount to a paper, whereas this is often not the case with regular members of a lab.

In this sense, visiting students are not a burden, as they could be considered due to their consumption of resources and time of another laboratory (or, to put it in terms of 'flow', their consumption and incorporation of somebody else's 'knowledge' and 'nurturance'). Rather, they are considered an attractive, if temporary, acquisition. Moreover, we should not forget that all the gain produced by the Ph.D. student is almost 'net gain' for them, since they did not need to invest money and time in the training of the Ph.D. student –this has been a cost on the side of the laboratory of origin.

In any case, their investment (the fact that during her stay the Ph.D. student will incorporate scientific flow from them) will allow the host laboratory to claim as its own the Ph.D. student's work. Likewise, the laboratory sending the Ph.D. student can also claim property on her work, since, at the end of the day, the Ph.D. student is hers, an embodiment of the group's flow. Put differently, one and the same task is profited by two groups, through the momentary double belonging of the Ph.D. Hence,

the joint publication. Precisely because both sides may have gains and losses, it is so important that at the beginning of the collaboration the heads negotiate the conditions of the transaction, so to speak: what will each side obtain? Described like this, it almost seems we are witnessing barter: what do I give, what do I get? But this is not individual barter, but collectively organised transaction: laboratories entering into exchange relations.

A certain amount of trust must be put into circulation –must be 'deposited' in the relation– in order to perform exchange in the first place. The host laboratory can never be sure that the Ph.D. student will respect the deal and, especially, that it will work as intensively as they expect. Thus, they offer hospitality while accepting a certain risk. Of course, they expect some benefit for their hospitality, we know this, but this is hospitality after all, the giving of a 'carte blanche' –as one head put it, "everything was possible because at the beginning they put their trust in us –and for this we must be thankful: they let us do" (EI13-19).

The IBF group, in its turn, also needs to set a certain amount of trust upon the 'invitation' of the host laboratory: what if the laboratory does not accept the new Ph.D. student as a full-right member of the laboratory? What if the Ph.D. student is not allowed in certain restricted areas or is barred from certain information or researches? What if the Ph.D. cannot use all the resources and materials of the laboratory? What if the Ph.D. student is considered more as an intruder than a guest? These risks need to be accepted. As German language says, this risk needs to be taken *in Kauf* –one needs to 'buy the risk', to take the risk into account, that is, enter it in one's account. Without the acceptance of this risk, exchange cannot take place, for acquisition and risk go hand in hand. In other words, even though each side may expect a benefit, some compensation, this benefit is not like a fixed tax that the other side pays automatically.

Moreover, benefits may be postponed in time. Thus, the gratitude for accepting one's Ph.D. student may be shown by inviting the head of the host laboratory to a

conference a year after; or by accepting some months later a Ph.D. student travelling in the opposite direction... –a kind of compensation will ensue, like a gift that must be returned, even though the way and the occasion is not defined. This interval of time entails uncertainty and risk; the later the return is given, the less sure it is. But simultaneously this delay allows several laboratories to settle, through these exchanges, a field of reciprocity, forcing the emergence of trust. If transactions were immediate, the relationship would not prolong in time and involvement would be reduced; thus this temporal gap leaves the relationship open. Thus, trust is not so much a feeling that precedes exchange, but both, a condition of possibility and an effect of it (Schillmeier, 2000, 2001).

At this point of the narration, the reader may already have noticed that the vocabulary with which we are describing this exchange is performing a slight of hand: in the same way in which a Ph.D. student can be considered an incorporation of group substance, 'knowledge' and 'nurturance', it can also be regarded as an 'investment' of time and money; if we can say that co-publication is a recognition of the double contribution in the constitution of the student, we can also say that it is a co-appropriation of the labour force of the Ph.D. student, sold (rented?) temporary by one lab and borrowed by the other. Communication and agreements between the heads may be called 'negotiations' trying to reach an 'informal contract' that allows the distribution of 'benefits, profits and gains'. Thus, collaborations are also describable in economic terms.

We have already seen in chapter 3 that other authors have shown that science is 'productively' apprehensible by an economic analogy. Thus, Latour and Woolgar (1979), indebted to Foucault's political economy of truth (Domènech & Tirado, 1998), talked of 'the commerce of science', to refer to the complex process through which scientists produce credible data and activate credibility cycles. Scientists set information (statements) in circulation, which constitute the capital that allows new investments to obtain more gains. Latour (1994) has even drawn a more explicit link between science and capitalism:

Since Marx one defines capital as that which circulates with no other aim than renewing and expanding the very circulation. In science everything seems to happen as if some scientists invested capital with the aim of increasing it. This capital of credibility is not restricted to the symbolic recognition which the scientists have for each other, (…) but encompasses the whole circulation – including data, truths, concepts and papers" (1994, p. 78, our translation).

In the context of this capitalist interpretation, one wonders if the circulation of Ph.D. students might not be read as a way for the group to keep capital in circulation. For, once we admit the importance of the benefits groups obtain through them, the strategy of sending them abroad does resemble the definition of a capitalist act, as Weber put it, as one based upon the expectation of profit by exchanges carried out peacefully and with its own laws (Weber, 1967).

Under this light, exchange of Ph.D. students comes close to the circulation of commodities, "understood as objects, persons, or elements of persons which are placed in a context in which they have exchange value and can be alienated. The alienation of a thing is its dissociation from producers, former users, or prior context" (Thomas, 1991, quoted by Callon, 1998, p. 19). To put it differently, Ph.D. students could be regarded as capital, a kind of money, "commodity money"[13] (Burns, 1998), translatable into money even, through which the head may strike a deal with other heads[14]. Furthermore, it is tempting to insist that their circulation produces 'surplus value': they invest a certain amount of time and money in the Ph.D. student; after the exchange, they obtain back the Ph.D. student plus a gain –be it a paper, more

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[13] "Anything with intrinsic value which is generally acceptable for payment or exchange. Objects such as shells, cattle, salt, furs, coins of gold or silver, etc have functioned as money (see later). In a certain sense, exchange with such valuables is close to 'barter' and the concept and utilization of the valuable as money emerged through repeated interactions and organic processes" (Burns, 1998, p. 1).

[14] "In this process of obtaining things from a stranger, a contract is made, and a debt arises. The debt may be liquidated immediately by the payment of money, or it may remain in existence for a day, a year, or a century. In any case the debt, looked at from the opposite point of view, is a credit; that is, a credit is an uncompleted contract –the creditor has done his part, but the other party has not yet performed his. Now anything which is generally accepted as means of completing contracts, and liquidating debts, is called money" (Lehfeldt, 1926, p. 7).

knowledge, a collaboration. The increase in c.v. that this gain will produce will allow for new acquisitions and investments:

> "The circulation commodity-money-commodity, that leads to an equality of value (W-G-W), has been replaced, Marx said, by a circulation money-commodity-money, that is non-productive except when it is unequal (G-W-G). This is the definition of the surplus-value" (Latour, 1994, p. 82, our translation).

Nevertheless, suggesting as this argument could be, the argument equating Ph.D. students to commodities fails because Ph.D. students never really change hands. We do not find laboratories that send a Ph.D. student to another laboratory in order to receive another Ph.D. student from the other laboratory in exchange: rather, the same Ph.D. student comes back. He produces surplus value only if he comes back, that is, only if he never stops being property of the IBF. What impedes us from considering Ph.D. students to be commodities is the lack of one element of Thomas' definition of commodity: for something to be a commodity, it needs to suffer a certain amount of alienation, a "dissociation from producers, former users, or prior context".

This dissociation or decontextualisation is precisely what does not take place in all this movement and travelling of students. Dissociation, decontextualisation, or disentanglement, as Callon (1998) calls it, means that a particular 'object' (be it a thing or a person) must gain some independence from the context by cutting off the relations between the object and the rest of entities surrounding it. This is why the object may travel from hand to hand performing exchange. Without this detachment, the object is no commodity, and there is no market transaction taking place. If the object passing hands were not cut from relations, the receiver would become entangled in relations on receiving the object in question "If the thing remains entangled, the one who receives it is never quit and cannot escape from the web of relations. The framing is over. The debt cannot be settled" (ibid, p. 19).

This conclusion is quite suggesting: Ph.D. students never turn into a kind of coin of exchange among laboratories, for they are still too imbued with use-value, so to speak. They are never disentangled, in the sense that they are never disconnected from the group that has constituted them and sent them. But this characteristic, Callon challenges, may be quite common to money, actually; disentanglement and decontextualisation –traditionally attributed to money- look very interesting on paper, but they are rather impracticable in real life. One never achieves total disentanglement, not even with the allegedly most decontextualisable device, money:

> "Money, whatever its degree of abstraction and dematerialization, by the mere fact that it circulates and that its circulation is calculated by agencies engaged in transactions, leaves traces: those of its successive attachments, the points through which it passed, the agents in whose hands it landed at a given moment, only to move on again" (ibid, p. 34).

Money is useful precisely because it is able to leave a trace. Unless we can follow the trajectory of money –that is, unless we can know who has paid, how much, to whom, for what, etc.- we could not settle transactions; money as a currency must point to its users/exchangers, must define a space of circulation. Those who need to disguise 'dirty money' know how difficult 'laundry efforts' are. And if this is the case with money, even more so with other types of goods. One can never achieve complete disentanglement. What is more, according to Callon, the more efforts one invests to disconnect an entity from its relations –operation which he will call, following Bourdieu, framing- the more connections one creates. For instance, in order to hide money from taxes office, those who accumulate dirty money need to invest it again somewhere else, creating thus new links, etc. In the effort to cut money off, one gets more entangled.

The case of our Ph.D. students is simply a more blatant case of entanglement. And if this entanglement prevents us from talking of commodity circulation in the classical sense, one can nevertheless insist on the economic gain obtained from this quasi-exchange. For we should not mistake economy with the presence of money.

Anthropologists after Mauss know that exchange is not only linked to money, and that it is also possible in situations of high entanglement; indeed, Mauss presented a mechanism explaining how a social system could be based on exchange, combine individual interests, and still be different from what we usually understand as market exchange (Douglas, 1990):

> "We shall describe the phenomena of exchange and contract in those societies that are not, as has been claimed, devoid of economic markets –since the market is a human phenomenon that, in our view, is not foreign to any known society- but whose system of exchange is different from ours. In these societies we shall see the market as it existed before the institution of traders and before their main invention –money proper. We shall see how it functioned both before the discovery of forms of contract and sale that may be said to be modern (Semitic, Hellenic, Hellenistic, and Roman), and also before money, minted and inscribed. We shall see the morality and the organisation that operate in such transactions" (Mauss, 2000, p. 4).

2. Reciprocating

I will now turn to Mauss to understand some more characteristics of this reciprocity and exchange. My use of Mauss may be polemic: by resorting onto insights in gift economies, I do not mean to imply that 'Ph.D. students' are 'gifts' given to and fro. This would be as partial a reading as the claim that they are commodities in a market exchange. The aim is rather analysing exchange in conditions of strong entanglement. Thoughts about gift economy may help us understand some of the implication and binding that takes place through the circulation of Ph.D. students.

In order to find Mauss' ideas useful, we first need to get rid of our Western cultural idea that gifts are free and disinterested. In gift economies, giving a gift is not this type of voluntary act, even though it may be lived as such. Rather, these acts are actually constrained and self-interested: "Almost always such services have taken the

form of the gift, the present generously given even when, in the gesture accompanying the transaction, there is only a polite fiction, formalism, and social deceit, and when really there is obligation and economic self-interest" (ibid, p. 3). But in spite of being self-interested, these gifts cannot be understood from an individual point of view, as if the underlying motivation to give was spontaneously emerging from every single individual. This type of exchange needs to be understood in the framework of the group, for "it is not individuals but collectives that impose obligations of exchange and contract upon each other. The contracting parties are legal entities: clans, tribes, and families who confront and oppose one another either in groups who meet face to face in one spot, or through their chiefs, or in both these ways at once" (ibid, p. 5). If exchange is collectively organised, collective are also the benefits obtained.

What Mauss will observe, however, is that these moments of exchange are not simply about giving and receiving things. Rather, the circulation of the thing from hand to hand creates a kind of link between donor and giver. The very fact of receiving something from somebody else forces the receiver to give something back, this re-muneration[15] creating a cycle of reciprocity that weaves collectives together. But if this was the observation, the question was still to answer: why does the passing of a thing create this obligation to reciprocate?:

> "What rule of legality and self-interest, in societies of a backward or archaic type, compels the gift that has been received to be obligatory reciprocated? What power resides in the object given that causes its recipient to pay it back?" (ibid, p. 3).

Drawing on fieldworks in North America, Samoa and New Zeeland, Mauss will indicate that the reason for this obligation lies in the particular connection between the thing possessed and the possessor, a connection that is not simply of property (as Westerns usually understand it), but of a spiritual nature. Thus, for instance, Maoris see things as possessing *hau*, a spirit that animates them, but that at the same time

links them to the possessor. As Mauss puts it, it is not only that things posses a soul, but that they are of the soul (ibid., p. 12). This means that an exchange of things is also an exchange of souls, and that "a tie occurring through things, is one between souls" (ibid., p. 12). And this link has consequences. Mauss' argument is well known: when somebody gives me a gift, s/he is not simply passing a thing to me, but also his or her soul, a part of his or her spiritual essence. If I mean to retain the thing, I am putting myself in serious danger spiritually speaking, since I am preventing the *hau* to return to its birthplace –to the clan, to the owner. To cancel this danger, I must pass a gift on, either to the receiver, or to a third person who will also feel the obligation to reciprocate:

> "The *taonga* [exchanged objects] or its *hau* –which itself moreover possesses a kind of individuality- is attached to this chain of users until these give back from their own property, their *taonga*, their goods, or from their labour or trading, by way of feasts, festivals and presents, the equivalent or something of even greater value. (...) This is the key idea that in Samoa and New Zealand seems to dominate the obligatory circulation of wealth, tribute, and gifts" (ibid, p. 12).

If this story sounds strange to our Western conception is not because we cannot understand the impulse to reciprocity. Our gift exchange may call us to reciprocity as much as the one Mauss described. As Douglas (1990) puts it, "(i)f we persist in thinking that gifts ought to be free and pure, we will always fail to recognize our own grand cycles of exchanges, which categories get to be included and which get to be excluded from our hospitality. More profound insights into the nature of solidarity and trust can be expected from applying the theory of the gift to ourselves" (p. xv).

However, our notion of the gift also involves differences regarding the way it is conceived above. Basically, for us, a gift is "an alienation which mobilises bonds of attachment" (Edwards & Strathern, 2000, p. 159). Our gift creates a link, an attachment between the donor and the giver, and the giver may be forced to give

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[15] Remuneration, (*remuneror*, from the Latin *munus* = favour, present, among other meanings) meant

something back, even if a 'thank you' (Serres, 1982). However, the act of giving is also an act of separation, of severing ties: once the gift is given, it is given forever, and the ties between the gift and the donor should not be retraced[16]. In contrast to this understanding, as Edwards and Strathern warn, the way the notion of the gift is used in anthropology points towards the "inalienability of bond-creating substances" (2000, p. 165, note 22). The given thing never stops being part of the giver, and with its double belonging links the donor and the receiver in a special tie.

This contrast hints at where Mauss' ideas may be of utility to us. If we have seen that Ph.D. students cannot be considered alienable goods –goods that definitely change hands- we may find it interesting to analyse the kind of bonds that these inalienable entities help produce in their circulation from lab to lab and return. Granted, we are dealing with a different type of sociality and morality from the one found in Western laboratories. Nobody would claim that Ph.D. students possess *hau*, a spiritual power passing from person to person when Ph.D. students move from lab to lab. Nevertheless, I suggest that, since entanglement in the lab of origin continues in spite of their travelling, some effects of this movement are not so far away from Melanesian ones.

Maybe a Ph.D. student does not participate of the soul of the head, but of her productive or creative capacity: we know that a Ph.D. student incorporates the head, the group: its knowledge (ideas, skill, affectivity) and its nurturance (money, support, time, effort, etc). And once we accept that students can be regarded as extensions of the group, we can understand why, on travelling to another laboratory, it is not only the individual who is moving, but a bundle of relationships: on accepting the Ph.D.

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to give back a present, to do a favour back, to give a reward for something.

[16] This is why, they suggest, the "transfer of human capacities" in the context of reproductive technologies is usually understood culturally as a gift: for instance, the surrogate mother gives a present to the biological parents –the carrying for some months of the embryo. "As in the case of the surrogate mother who nurtured the frozen embryo, reproductive medicine may create ties of substance (carrying the child entailed the surrogate's 'nurture' of it) that do not get translated into social kinship. The surrogate mother, so called, transferred her claims to the commissioning mother through the sacrificial Euro-American gesture of alienation, the gift" (p.159). Once the baby is delivered and given to the commissioning parents (given for ever), the chain of links between the baby and the surrogate

student, the host lab also opens its doors to the other group –especially to the head. In that sense, the double belonging of a Ph.D. student involves the union of two groups, if temporal, and also an exchange of flow, a mixing of flow[17]. For as long as the visit lasts, the Ph.D. student, entangled and entangling, acquires properties not too distant from those of the gift as read by some anthropologists:

> "What makes the gift more than an inert object is its capacity to convey or prolong the person of the giver beyond the spatial-temporal bounds of his own immediate self. As the material embodiment of an intersubjective process within which persons are constituted as purposive social agents, gifts are seen to be empowered with a creative principle. But the 'spirit' of the gift, its vital force or subjective load, exists only in the current of time; outside that current the gift object reverts to its original condition as static thing" (Ingold, 1986, p. 138).

3. Networks of collaboration

If Mauss has helped us to understand the possibility of an economy based on entangled exchange, now we can turn to Lévi-Strauss to understand why exchange should never be reduced to economy. As it is well known, Lévi-Strauss interrogated himself about a wide observed phenomenon: in many different civilisations around the world we could find exchange of women between groups, women who are circulated between clans, lineages or families as valuable goods. These phenomena had not gone unnoticed by other anthropologists, on the contrary, several interpretations have been already offered. Previous authors, such as Frazer, had interpreted the exchange of women under an economical perspective: women

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mother is blocked, deactivated –that is, the gift uniting the two mothers also separate them. Moreover, connotations of 'adultery' are also blocked through gift parlance.

[17] According to Turner, In Samoa a couple may transfer or hand over their child to the mother's sister and her husband (the mother's brother in law) after birth, so that they rear it. This sacrifice of biological bonds is paid back by the relatives to the parents through presents for as long as the child lives. Whereas the child is seen as part of the substance of the aunt (which is the same as the mother's), the presents received are seen to stem from the uncle's side: "This sacrifice [of the natural bonds]

exchange would have originated as a way for poor groups to obtain women. If a man does not have enough possession to convince a family to give him a woman, he can nevertheless exchange his sister for somebody else's sister. Thus, for Frazer, the exchange of women followed economical principles, it was a matter of 'simple barter'.

Lévi-Strauss found some problems with this economic interpretation, for this explanation could not account for the richness of the empirical data. There were groups which exchange goods with a reciprocal value, that is, the amount of valuable possession after the exchange remained the same as before. If exchange brings no gain, it is difficult to interpret this action in economical terms. But even more difficult is this interpretation when two groups exchange exactly the same[18] -for instance, the same type of pottery, the same number of women, or even the very same women in successive exchanges (as when women that the group A had given to the group B on a previous occasion are given back to the group A[19] in a future exchange). As Lévi-Strauss concluded, from an economical viewpoint such exchange is not only null, but also absurd. This made it clear that the important event in such situations is not the exchanged thing and its value, but exchange itself.

Lévi-Strauss' lucidity consisted in realising that women exchange was related to another phenomenon, to rules and incest prohibitions regulating marriage, and from this he deduced that the circulation of women could be understood as part of a single system of kinship exchange (Paul, 1998: 32): marriage rules forced groups to obtain their women from other groups. According to his interpretation, women would be a means to bind and tie the other, through linking -connecting associating, joining, and

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facilitates an easy system of exchange of property internal and external to the two kinship sides" (cited in Mauss, 2000, p. 9).

[18] As we have seen when revising Marx' conception of commodities, in order for exchange to take place, we need to exchange goods with different qualities, that is, that embody qualitatively different use-labour. The absurdity of considering these exchanges from an economical was also observed by Mauss: "It can happen that the identical things one has acquired and then given away come back to one in the course of the same day" (Mauss, 2000, p. 30).

[19] These women are given back not as 'the same women of the group A who return to group A (situation which could be interpreted as a rejection, and therefore, an insult), but as women of the

uniting with the other. This exchange brought groups in relation with each other, and often in dependency: none of the groups was self-sufficient; they needed each other for completion. In this way a type of collaboration emerged –and with it, symbolism and the step from Nature to Culture[20].

This exchange system was for Lévi-Strauss formalisable, similarly as one could formalise linguistic relations. Thus, he approached women exchange as a type of communication, a kind of language that allowed a group or community to establish communication among its members and members of neighbouring groups. Women exchange would be part of an exchange system with signification: women as signs can take values depending on the particular transaction. (Lévi-Strauss took this thought far enough as to perceive a similarity between 'women' and 'words' –we will come back soon to this point). The important point, then, was that this communication or reciprocity establishes a type of union between exchanging groups, and this is a consequence or gain that goes well beyond economy:

> "But what turns transfer marriage into a holy mystery in every social thought is the very fact that, in order to cross with each other, [groups] have to unite, at least for a moment. During this time every marriage abuts on incest. Even more, it is incest, at least social incest, if it is true that incest generally consists in subsisting through and for oneself, instead of doing it through and for others" (Lévi-Strauss, 1981, p. 654, our translation).

Whereas economy creates bonds, not all bonds are reducible to economy. This is why Lévi-Strauss will conclude that exchange is not a modality of economy, but economy a modality of exchange. This is precisely the idea that can be helpful for us to conceptualise the type of bond or union between scientific groups that Ph.D. students' movements achieve. The two groups, thanks to the connection lived through the

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group B –that is, women who, since they were once exchanged, now already bear 'the mark of otherness' (Lévi-Strauss).

[20] Collaboration between groups is established through connections of reciprocity: I renounce to this woman of mine if you renounce to yours. In other words, the postponement of the satisfaction of one's sexual desire, in order to make place for exchange, allows the emergence of sociality. With the

student, establish important links, so much so, that in regard to a particular project, the two groups function as one: one project, one Ph.D. student, one paper (but still, interestingly, two heads –neither two groups nor one). During the time of the exchange, the two groups partially melt, not unlike what happens in the narrations of Lévi-Strauss. Exchanges of Ph.D. –as well as exchanges of other goods- have the effect of making collaboration possible. Such exchanges are never only about wealth and property, acts of economic transaction, they are always also an "act of politeness", constituting an event, a moment which gives birth to a "much more general and enduring contract" (Mauss, 2000, p. 5).

To summarise, then: in the case of our Ph.D. students, we have a situation in which entities in circulation are highly entangled before and after moving: in their laboratory of origin, a Ph.D. student is attached to the group; loosened enough to allow displacement to a host laboratory, this attachment is nevertheless not lost. On the contrary, new attachments are added: the Ph.D. constructs links with the new context, by embodying knowledge and nurture from another group. In the figure of a shared Ph.D. student, the creative forces of two groups mix, and this high entanglement does not allow us to talk simply of market exchange or economical transaction; there are too many relations involved, exchange is too personal and framing is impossible. The frame is never defined in terms of impersonal calculations (the work of a Ph.D. student as exchange for a paper), but it rather tries the opposite: to give birth to new entanglements, to new relations. Ph.D. students do not change hands; rather they bring two parties to shake hands! This is not to gainsay that Ph.D. students' visits abroad produce gain, even economic gain, profit. However, it seems that more than transactions are involved here, and if we conceive this situation as a pure economical phenomenon, we are limiting possible interpretations.

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emergence of this split -this duality in the nature of women, perceived now as both stimulus and

4. Exchange: being on other hands

If we accept Callon's arguments on entanglement, there is no intrinsic characteristic differentiating gift exchange and money exchange [21]; the difference lies in the amount of interweaving of relations. Indeed, to enter reciprocity has advantages, but it also leaves us in the hands of others. To receive a gift *and* accept it is to accept a relationship with the person giving the present –which often implies to open yourself to the other, to make yourself vulnerable. This explains, for instance, the feeling of incommodity we feel when we receive a present from somebody we do not particularly like. Whereas rejecting a present would be too disdainful, we feel that accepting the present puts us precisely where we do not want to be, at the doors of a reciprocity we do not desire. Donor and receiver are not completely independent, not indifferent to each other, but implicated.

The relation is slightly different when we discuss an economical transaction. Needless to say, barter or buying are not activities carried out in a vacuum of power, where seller and buyer are in egalitarian conditions, one simply offering what the other wants to acquire. Unfortunately, we all have had plenty of chances to know that this is not the case, and often a market exchange happens under pressure, as when somebody is obliged to buy at a much higher price than the one affordable, or when somebody is forced to sell at a derisory price. And yet, the kind of inequality is of a different type from the one established by exchange of presents. We feel an inequality in power, autonomy, resources, but the link established by the transaction is not of a personal nature [22]. Words like generosity or magnanimity, meanness or stinginess are,

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symbol, value and sign- the step from Nature to Culture is performed.

[21] Contrary to Callon, Serres takes this difference from granted (1982). Actually, this is a heated debate within anthropology. See, for instance, contributions by Carrier, 1998; Douglas, 1990; Douglas & Isherwood (1996); Gregory, 1997; Strathern, 1998; Thomas, 1991, cited in Callon, 1998, among others).

[22] If we are talking, of course, of a pure economical transaction, where other modes are not mixed; if we refer to a transaction between friends, for instance, then personal relations are involved, but not because of the transaction itself. In principle, in a market transaction, personal factors do not enter the

in principle, attached to gift exchange, not to business. Reciprocity, then, has other rules than market exchange, in that personal relationships are part of the transaction itself. As Mauss (2000) says, "(t)o refuse to give, to fail to invite, just as to refuse to accept, is tantamount to declaring war; it is to reject the bond of alliance and commonality" (p. 13). And at the same time, to accept entails the responsibility of the relation; the moment one accepts something from the other, one knows one will need to give the other something from oneself; in other words, the receiver becomes indebted.

Debts occur in both types of relations, of course, but the hurry with which one wants to settle this debt, Callon and Bourdieu remind us, speaks of the nature of the exchange. If we do not tolerate the tension of the debt, and pay it back very soon –for instance, giving a present to somebody who has just helped us, instead of waiting for a future occasion to help, *quid pro quo-* our hurry will mark our transaction as an economical one, similar to a business exchange: one favour as immediate exchange for another, just as in a shop we give money in situ for a product (notice that only in shops where the owner knows the client is it possible to buy on credit) -we wanted to get rid of the weight of the relation as fast as possible, which, of course, shows clearly our discomfort in front of the perspective of the personal relation. Callon (1998, p. 15) quotes Bourdieu quoting La Rochefoucauld: "Being in too much of a hurry to pay a debt is a form of ingratitude". On the contrary, if we accept being indebted, we accept the relation that entangles us with the other, we maintain the possibility of reciprocity open[23].

This openness to the other that underlies reciprocity is the reason why we would miss the point if we interpreted exchange between laboratories through the figure of Ph.D.

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calculation, and this is partly why we usually do not like to make business with relatives –for instance, as when one is forced to sell something cheaper to one's aunt than to a foreigner, or when one is horrified when a relative cheats on another relative, not to mention the situation of bad-taste when relatives quarrel because of money. We Westerns imagine money and family to be a bad mixture (unless the whole business belongs to the family, but this is another case; and even there, the mafia connotations of this expression should make us hesitate).

[23] Just try and answer this question: to whom is more likely that we give a present as an act of gratefulness for a favour, to an acquaintance or to our own mother?

students as simply gain-oriented. It also involves an acceptance of the need to find completion through others. It is a radical acceptance of collaboration: a choice for collaboration instead of competition. But this choice involves a difficult balance, for donor and receiver very rarely find themselves in a setting of equality. Indeed, there is a difference in the social prestige one harvests if one gives or if one receives:

> "To give is to show one's superiority, to be more, to be higher in rank, *magister*. To accept without giving in return, or without giving more back, is to become client and servant, to become small, to fall lower (*minister*)" (Mauss, 2000, p. 74).

To express it in Callon's terms, the entanglement of the thing exchanged deters groups from being quits: giver and receiver are not on equal grounds; one of them, indebted, still owes, which means that some kind of equating practices will have to ensue. Hence, this inequality triggers off acts of reciprocation. In the collaborations we have been describing, the roles of the giver and the receiver alternate. The IBF group gives as much as receives, but the inequality of the situation is to be noticed in those cases in which, in order to maximise gain, the IBF head chooses other groups who are considered superior in terms of curriculum and prestige. The advantage is clear: when publishing a paper with this type of groups, the resulting publication has more chances to reach a journal of more impact than if the publication is signed by less prestigious groups.

But this strategy has also disadvantages –the more one obtains, the more one needs to give back: one has to bargain and offer a lot to a prestigious group so that they are tempted to collaborate with a more modest one. For instance, in these cases, the IBF group tends to adapt to the research project of the host laboratory approaching as much as necessary the topic and interest of the host laboratory –as well as those of the IBF, of course. Furthermore, when a publication ensues, members of the IBF signing it are usually willing to renounce to the best positions in the signature of the paper. The order of the authors is discussed between the two heads, not infrequently before the collaboration starts. In order to make the collaboration more attractive for the

other group, the IBF head may accept not being in the last position, for instance. This sacrifice sometimes hits not only the head, but also some senior. This implies, as one may guess, that heads need to display a great deal of skill for negotiations and bargaining.

The difficulty to maintain an egalitarian exchange is what puts some groups off. As some complained, in collaborations with 'powerful groups' they tend to obtain the worst part: "when we distribute the tasks, they always try to give us the most boring bit" (EG05-20). Some heads prefer to work alone, "without anybody's help". These are two different strategies the heads may decide to engage in. There are heads that show their cards in order to collaborate with others, there are heads that prefer to hide information and fight for the publication alone, rejecting entering collaboration[24]. Some Ph.D. students expressed the difference saying that the former accept "to learn from others", whereas the latter consider that "groups working in similar issues aren't friends, they are competence" (EG05-20). This is why the latter are called 'self-sufficient', in a double sense: self-sufficient as in arrogant or 'uncommunicative', since they think they can do without others, and self-sufficient as in self-providing, as the group can produce enough 'nurture' on its own, without needing anybody else.

It is nevertheless informative that, for some, collaborations are tantamount to "receiving help". On the one hand, it is an expression of individualism to think that if one does something alone, one obtains it with its own merits, whereas collaborations diminish the value of one's achievements. On the other, it implicitly gives support to the thesis that they perceive something of a 'gift' in networks of collaboration. This duality illuminates, then, the difficult balance that collaborations involve. If both teams are seen as contributing equally, collaboration is still in place (but one comes closer to a commercial transaction: one thing for another as fast as possible). If, on

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[24] To opt for collaborations means partially to renounce to the strict group identification and boundary maintenance that a more competitive, colonising strategy strengthens (Lightfood & Martinez, 1995, p. 478). But this is strategy nonetheless: we do not find warriors protecting symbolic territory which have to prove to be better than rivals, but ambassadors who, through collaboration, win space for its group. If this is conquest, it is one of a very untypical kind: to let themselves be conquered: "war without

the contrary, one is seen as receiving more than one gives, then one is perceived in a position of inferiority, depending on too many favours (that is, one's position would depend on the generosity of others accepting joining their names). If moving to the market extreme of the relation is dangerous –then one can be seen as selling oneself- to be too close to the gift relation entails its risks as well. For a collaboration to be in place, the relation needs to move ambiguously between these two extremes.

5. On ambivalence and ambiguity: signs, jokers and quasi-objects/quasi-subjects

Groups face a situation in which competition may force them to confront each other and veer. As an alternative, exchange establishes some bonds that make collaboration more likely. Exchange makes possible that for a while two or more groups have and share the same artefact – in this case, the Ph.D. student or any of the inscriptions exchanged. That which is shared is placed in-between groups, inter-esse, an interest or catalysing factor mixing borders, creating union. Thus, to have the same interest means to share a bond, to be caught in an assemblage that distributes identities and roles for all the elements involved.

This distribution is linked to power, in the sense that 'interests' are, as Callon & Law (1982) say, a temporal stable situation resulting from processes of enrolment. Put differently, interests are attempts at ordering the world, a mapping or ordering of society. Not surprisingly, interests and translation are related notions, since translation consists precisely of the process of moulding interests –that is identities and roles for one and others[25]. Configured and reconfigured by both groups in an

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battle lines, with neither confrontation nor retreat, without battles even: pure strategy" (Deleuze & Guattari, 1988, p. 353).

[25] 'Interests' should not be understood here as the privileged knowledge of a group; nor as the profit or gain a group expects to obtain. Neither as a theoretical concept that the analysts projects to the data and actors in order to make sense of the scene (Barnes, 1981). For a discussion on the notion of 'social interests' within STS, see Barnes (1981), Callon & Law (1982), MacKenzie (1981) & Woolgar (1981). The focusing on interest as inter-esse or being in between has for me the beneficial effect of giving us a bit of a distance with the economically loaded connotation informing notions of interest. (Not by chance do the following expressions abound in Callon and Law (1982, our translation): "mercantilism"

attempt to stabilise a particular relation of force, it is through these successions of translations that the map of relations takes shape.

If women were the mediator factor between Lévi-Strauss' groups, Ph.D. students mediate between laboratories. Now, if Ph.D. students become mediators, beings in between, where does the connective power of the mediator come from? Several authors point to a certain duality in its nature. Duality which can be read either in terms of ambivalence and of ambiguity. Let us start with the first. I have mentioned before that Lévi-Strauss perceived a certain similarity between women and words: both entities were, simultaneously, a name and a sign (or as he himself also put it, a subject and an object; with both, use and exchange value). Thus, the mediator is also both sign and value. One part of this figure is 'full': it is a thing, with use-value, with meaning, with life. The other, though, is 'empty': a symbol, a thing in the place of another, without meaning or value of its own, that may adopt, in a mimetic effect, the value required to make communication flow, to make relation possible, to grant exchange.

This duality is what allows this figure to play a mediating role, fitting in any situation as if it was one more piece of the whole assemblage, while also playing a configuring role in the whole game. As a 'full position', a person with theoretical knowledge, with know-how, that travels, learns, works and comes back. As an 'empty position', the same Ph.D. student is a sign, a token that circulates creating links, adapting to the situation to make collaborations possible. In this sense, the question we posed before about whether they could be regarded as commodities was not so out of place as it could seem at first sight.

The claim that Ph.D. students have use-value may more or less be accepted. However, is it not stretching the point to say that they are a 'sign' for communication? Is this not 'emptying' the Ph.D. too much? An anecdote is here appropriate. One day, I met the head of the group in the corridor. He came to me with

(p. 54), "enrolment as initial attribution of a value to the world and subsequent attempts to transform

a smile, and asked how everything was going. I interpreted his friendly approach as an invitation to talk, and asked about the Ph.D. students who had recently gone abroad. He confirmed that all of them were making progress with their research: "we have already written a paper" -he said, with that typical use of the first person plural that we have already discussed. He went on explaining that one of the Ph.D. students, P, went to London in order to do a particular task, but that once he arrived there, a change of project was suggested to him. They (P and the head) accepted. The head did not look particularly worried about the change. I could not help having the impression that to keep the collaboration alive, while satisfying the host laboratories' wishes, was more important than the particular topic or content worked upon. If needed, then, the Ph. D. student will be reconfigured to keep the relation.

In a way, Ph.D. students are so attractive for the host laboratory because they are empty-pieces, whose movements and relations are still to be defined. As Deleuze & Guattari say of the Go pieces, they do not have "intrinsic properties, only situational ones... pure strategy" (1988, p. 353). This joker-property, the property to anchor and adapt to wherever one goes, is crucial to understand the role of these mediating figures. We have already encountered a figure like this: the blank. In chapter 3, the head took the value of a blank in particular circumstances. Here this value is taken by Ph.D. students. This is not so much incongruence as an exemplification of the changing nature of such a position. A blank has a double value: now it is a particular figure occupying a full position, now it is a circulating structural void. Or, in Rotman's (1987) words, a sign and a meta-sing[26]. At one moment, the ambivalence is thrown onto the head, who becomes the mediating point; in another context, the head becomes a fix, full position, ambivalence is gone, and the latter is now thrown onto another figure with the responsibility of weaving the collective further. The blank, the joker can inhabit any of the angles of the relation:

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this value" (p.56), "to win customers" (p. 57), or equations between interesting = profitable.

[26] The consequences of this double value and ambivalence of these figures is beautifully explained through a completely different example by Rotman (1987) in his analysis of the cipher zero: "It is this double aspect of zero, as a sign inside the number system and as a meta-sign, a sign-about-signs outside it, that has allowed zero to serve as the site of an ambiguity between an empty character

"And this power comes simply from the fact that he is the relation and not fixed in the essence, that he is not fixed in a station but is in the functioning of the relations in his being part of the warp and woof, that he is relational and thus that he is multiple and collective" (Serres, 1982, p. 64).

To think that Ph.D. students (or women) can be used as pieces of exchange, or pieces in a game, is this not to treat them as 'things'? We know that Lévi-Strauss defended his proposal from the charge of 'objectifying' women by reminding us that women are never simply object, but also subject. The ambivalence, dual nature of his 'mediating factor' prevented his argument from falling into reductionism. Serres will allow us to give another –different while similar- answer, based not only on ambivalence, but also on ambiguity: the mediating factor, as such, is neither exactly an object, nor exactly a subject. It is nevertheless something, this is out of question, since, as Serres puts it, they are in this world (1997, 346). However, when one wants to classify this something through its characteristics, one is slightly confused, for some of them resemble an object, but not quite, and others resemble those of a subject, but not quite. Serres' proposal is to call these ambiguous entities quasi-object, quasi-subject (1982) [27].

(whose covert mysterious quality survives in the connection between 'cyphers' and secret codes), and a character for emptiness, a symbol that signifies nothing" (Rotman, 1987, p. 13).

[27] We can try to illustrate this notion with a far too long foot note. Imagine your are a teacher looking down from the window of your classroom, observing the schoolyard, seething with children playing, running in their after-lunch pause. Looked from above –and even looked from within, if you still have recollections- the multitude of screaming sweating children is an indefinite, ever changing mass of impossible borders, where no clear formations are to be seen. Little by little, from this multitude, some more regular formations will appear: here, a group of children will distribute themselves around a ball (even though it would be more accurate to say that they will be distributed around by the ball); there, a group of girls gather around a skipping rope, giving rise to a new formation, quite different in structure than the previous one. A bit farther away, another group is playing hide and seek. Here the formation resembles more the one resulting from children playing football than the one around the rope, but there is no ball organising the distribution: a person seems to be substituting the object, but not always the same person –only if you are touched, you become marked, and then you must find someone else to pass on (and therefore get rid of) this contagious quality you have temporarily acquired. In a corner far away from all this mess, a group of more grown-up girls are having a chat, united this time by words: the secrets and week-end stories they tell each other. And further down, as if imitating them, younger children sit in a ring, playing *ring-a-ring-a-roses* and risk having to pay a forfeit if caught in the act of passing a stone or a rumour -early apprenticeship of what life will later on repeatedly illustrate: that rumours circulate creating unions, divisions and social bond.

All of a sudden, one event will alter this kind of 'autopoietic formation': a new object has been introduced into the equation. A boy has brought a chromo-album, awaking the curiosity of many. The album will pass from hand to hand, without plan but not meaninglessly, from friend to friend

With this not particularly intuitive nomenclature he tries to call our attention to a moment of indecision, to their undecided, indecisive nature: still under constitution, these entities do not carry the weight of an identity and cannot be labelled as either subject or object yet. They are a mixture of 'either/or' and 'neither/nor'. They are in-between: between a relation and a monad (Tirado, 1997), that is, of them one cannot tell if they are a being or a relation. Of interest for us now here will be especially their movement and circulation, because it is precisely in the effects that quasi-objects, quasi-subjects have when they pass from hand to hand (from station to station, as Serres says) that we may find inspiration to understand better the type of mediation in which Ph.D. students are involved.

The circulation of the quasi-object, quasi-subject turns the latter into an object of use for people, *and* a subject of the movement, distributing object and subject positions through its passing. People participating in the circulation become alternatively subjects and objects depending on their position regarding the quasi-object/quasi-subject. With this observation of how objects and people swap properties through exchange and circulation, Michel Serres is not far away from the insights that some anthropologists develop from observing exchange and contract:

> "The transformation of subjects into objects and *vice versa* is, as Munn recognizes, equally central to the Maussian understanding of gift exchange, in which things become 'parts of persons' and persons 'behave in some measure as if they were things' (Mauss 1954:11, see Munn 1970: 142)" (Ingold, 1986, p. 139).

("May I? May I?"), marking those who are close enough to be included, from those who do not deserve to be considered friends. Some of the children playing football will leave the orbit of the ball, attracted by the force of the album – maybe the teams can re-structurate themselves and the game can go on, maybe this desertion will end the match abruptly. Some of the girls chatting will surreptitiously attempt an approximation, under the disdainful look of the boys. The owner of the album might have intended to show it only to closer friends, but once in circulation, who can control the trajectory the album will follow? Indeed, the faster the excitement of the exchange, the less control over who will receive the album next, creating a circuit which will unite those children who managed to have a look at it. This distribution is quite ephemeral, it will soon be interrupted by the bell, or, more commonly, by the teacher on watch, always suspicious of too fast, unidentified, spontaneous formations...

This circulation, then, is a real creation and distribution of subject- and object-positions, of objectivity and subjectivity. This is why we can affirm that mediation is not the circulation of objects among subjects, where actors and acted-upons are defined before the relation, but are effects emerging out of the relation itself, eventful emergences. Hence, Serres' choice of the 'quasi': a not-yet that comes into existence through the relation, a flickering, an ontological doubt that may be decanted, inclined in different directions: either to give birth to objects or to subjects depending on the almost imperceptible interference of the mediation.

Now, to come back to our Ph.D. students: if one has the feeling that to talk of them as mediating factors performs a kind of 'objectification', in the sense that one talks of people not as free agents acting, but as an entity making possible exchange and emerging from it, then one should probably say that yes, Ph.D. students, as well as Lévi-Strauss' women, become more 'objects'. For as soon as they appear as mediators, they perform as quasi-object, quasi-subject. As mediators, they lose some characteristics of subjects to melt again in the muddle of exchange. Maybe not all of their persona are so transformed – as sons and daughters or husbands and wives or friends, their identities may (only may) remain unaltered by the travelling. But as Ph.D. students in an exchange visit, their identities are neither fix nor pre-configured, but ever-changing, dancing at the tune of the relation. Before and after the exchange, when they are working in the IBF, they may be constituted as objects or subjects, but while exchange is on course, they are constituted by mediation as quasi-objects, quasi-subjects.

The complex circulation of members, artefacts, materials and techniques contributes to the creation of the collective. Quasi-subject-quasi-objects circulate, pass from hand to hand, and with this movement mark the lab that receives them temporarily as an 'I', a subject, and the rest as an anonymous mass –'we'. 'We' are, precisely, those who do not have the object, anonymous in a monotonous chain. Those who know that do not have the object, *but have had it and may have it again* –which means that we know that this 'we', this 'I', and the quasi-object are intimately united: only by

receiving the object that makes me 'I' may I participate of the 'we', and only because I participate of this 'we' may I become 'I' for a short while through the temporal holding of the object. We know that any of us can be 'I', marked out of the collective, pointed at like a victim, capturing all the attention. Like an illuminated position, this moment of individuality is a small interruption of the flow of the collective, which must be passed on to be overcome, since "(t)he collective game doesn't need persons, people out for themselves" (Serres, 1982, p. 226). This is why the position of the subject is one of risks: "subject, that is to say, sub‑mitted. Fallen, put beneath, trampled, tackled, thrown about, subjugated, exposed, then substituted, suddenly, by that vicariance. The list is that of the meanings of *subjicere, subjectus*" (ibid, 1982, p. 227). The risk is avoided, then, in that we keep on passing on this ambivalent position –both dangerous and attractive.

And this is precisely where the object is of use: passing the quasi‑object –that which makes us subject- helps exchanging the token of the 'I'. If labs were self-sufficient, we would have a set of labs, but not a scientific community. The circulation of ambiguous artefacts may create the collective, but its blockage stops the passing of the 'I' and with it the emergence of the collective. The essence of the collective, then, is not "a set of 'I's", but rather "the sets of its transmissions": "The 'we' is made by the bursts and occultations of the 'I'. The 'we' is made by passing the 'I'. By exchanging the 'I'. And by substitution and vicariance of the 'I'" (ibid, p. 227). The stabilisation of this 'we', then, is granted by the circulation of the object. For this is precisely the main effect of the circulation of the quasi‑object, quasi‑subject: it unites people around itself, and approaches them or distances them with its flow, distributes them. In other words, it brings people in connection, it weaves the collective. The collective emerges out of the circulation of the quasi object. Around it, the collective 'fluctuates quick as a flame, around it, through it' (Serres, 1997, p. 87):

> "The object here is a quasi‑object insofar as it remains a quasi-us. It is more a
> contract than a thing, it is more a matter of the horde than of the world. Not a
> quasi‑subject but a bond, not a near-*ego* but what Pascal termed a *cord*, Leibniz

a *vinculum*. The social bond would only be fuzzy and unstable if it were not objectified" (ibid, p. 88).

Since to experience the collective entails the passing on of the 'I', there is something of the ecstasy in it: a move outside of oneself, a putting of the centre outside ourselves. If labs were closed upon themselves, without any need to get out of themselves in order to increase their possibilities of being, the weaving of the collective would be impaired. But as soon as they enter into circulation and exchange, they accept passing the 'I', they accept ceding the central, illuminated position. Those labs that do not want to take part in the circulation of the artefact will receive the disdain of colleagues for being too selfish, too 'self-centred', too self-sufficient. Thus, the creation of the collective is in strong relation to a decentralisation: the object remains in the centre, but the person marked by the object as subject must oscillate, this centre being occupied by a different person every time. The oscillation object-subject-object is intimately related to a movement non-centre, centre, decentralisation.

Part III. Space: weaving topologies

> "Ángels mariners, en el vaixell de
> l'aire,
> hissen estrelles a tots els pals"[28]
>   (Miquel Martí i Pol)

1. A global IBF

The head is distributing Ph.D. students through scientific space –in this case, to several laboratories- in an attempt to create collaborations and a field of reciprocity. Even though common sense would have it that these different host laboratories pre-exist to the relation, notice that these laboratories abroad only become possible destinies due to the relation. The scientific space through which IBF members can travel only emerges as effect of collaborations. If we observe the net of collaborations once it is already constituted, the network appears as an unproblematic space: Houston, London, Strasburg, Heidelberg, Munich... they are all points that seem accessible, already there, whether there is a Ph.D. student there or not. However, if we observe this space under construction, we will realise that labs such as those are possible options for them only after heads and Ph.D. students have somehow initiated contact. Paris or Seville are non-existent points yet –they do not appear in the map of science as imagined by the IBF, and they will not do so unless relationships between labs are started. Likewise, the IBF does not appear in the map of science for many other laboratories, unless it initiates somehow a relation: contacts, publications, exchanges.

This field of collaborations, then, is not pre-constituted, Ph.D. students do not move within an already-structured system of positions, with fixed borders[29]. On the

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[28] "Sailing angels, in the ship of the air, / hoist stars in every pole".
[29] The different laboratories where Ph.D. students dwell for a while are not points orienting and defining their movement. On the contrary, one could say that movement define points, that movement looks for those points where to make a halt, just to set out for the next destiny: "In striated space, lines or trajectories tend to be subordinated to points: one goes from one point to another. In the smooth, it is the opposite: the points are subordinated to the trajectory... The dwelling is subordinated to the journey; inside space conforms to outside space: tent, igloo, boat. There are stops and trajectories in

contrary, more similar to smooth space (Deleuze & Guattari, 1988), the very go and return trip of Ph.D. students from the IBF to the host-laboratory contributes to giving shape to this field[30]. Through their continuous deterritorialisation and reterritorialisation -a becoming detached from local place (Kearney, 1995, p. 552), just to attach to another locality- they weave space along time. The configuration of collaborations, then, is emergent, and fluctuates, modelled by the trajectories of Ph.D. students, redimensionated with each trajectory. Thus, time and space are intimately related to create configurations, for, as Massey suggests, "(s)pace is not static, nor is time spaceless" (Massey, 1993, p. 155).

Each new configuration holds for as long as the established relation holds: when the Ph.D. student withdraws, the collaboration comes to an end if nobody or nothing comes to substitute her[31]. In this sense, the circulation of the Ph.D. student creates "a site of sociality, in the here and now, and always for an uncertain length of time" (de Certeau, 1997, p. 119). Being in-between members, with a never-ending movement, they make possible a 'transverse communication', that is, communication which cuts across fields and structures, creating a space of collaborations, a space where new relations become possible, where the borders of different laboratories merge partially to create new strategic, temporary collective identities.

Without establishing these contacts, the group would perhaps be in a more marginal position. But not because the group is located 'in the periphery', as if one could find pre-constituted and predefined areas where good science is impossible, too far away from the centre to be up-dated and equipped. Rather, because of lack of connection that would include it in a local position. Marginality is not related to peripheral

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both the smooth and the striated. But in smooth space, the stops follows from the trajectory; once again, the interval takes all" (Deleuze & Guattari, 1988, p. 478).

[30] Looked at from the perspective of the individual, one can find lines of stratification. The go-return movement of a particular Ph.D. student may resemble more 'fabric', image, in Deleuze & Guattari, of the striated space, than the chaotic waving of felt: "the necessity of a back and forth motion implies a closed space" (1988, p. 475). However, looked at from the perspective of the whole group, movements are much more disordered or, to put it softer, less pattern-like.

[31] For each translation is liable to be further translated, modified without pause or interruption; each translation is an attempt at stabilising the flow of events –"to make verbs behave as if they were nouns" (Law, 1994, p. 103)- that will last as long as relations last.

position, but to isolation. Instead of emigrating to a centre, through their travelling back and forth the IBF group manages to attach to a network of relations, collaborations and resources which allow it to expand a local context into a global one. Instead of being closed onto itself, the laboratory expands its sphere of action through being connected to a global network. If the group 'goes global' is because the movement of Ph.D. students weave a space in which the IBF can connect to other such localities. This is a space woven by Deleuze & Guattari's local operations linking local labs to local labs. A connection of paths constructing globality.

This is a notion of globality à la Serres (1994), that rejects an 'homothetic' understanding of the global –an understanding that conceives the global as a space – vault or dome- empty, transparent, pure structure to be filled by events. A priori space, as if time and space were the scaffold or frame, unrelated to the locality where life performs. The local is small down here, the global is big up there:

> "The homothetic extension –that of the frog bursting on wanting to be as big as an ox- dates back to the time of the empires, when the imperialist universal consisted of a swelling of the local through which the One, swollen, excluded the Other" (Serres, 1994, p. 142, our translation).

But we can imagine a different type of globality if we observe accurately the movement of some of his favourite figures: the fly with its random movement, Hermes playfully and erratically flying among Gods, angels carrying messages, grains of dust in the wind, a drop of water in a stream, the baker kneading the dough. In these cases, one finds a very intimate connection between the local and the global. The simplicity of the global mass is created out of the complexity of local matter, the unity of the global derives from the diversity of the local; university is weaved with singularity; the orderly regularity is constituted through the chaotic movements of irregularity. There may be a change of scale between the micro and the macro, but this change is due to the distribution of the local, and not to a growth or increase of dimensions. The global, then, does not involve big players –like frogs, balloons or institutions (Munro, 1996).

2. A distributed IBF

This group follows a strategy which has not to do with concentration in a centre, with attracting everything to the self, but with distributing the self through a space, distributing places to create a globality. We do find 'action at a distance', but not of a centripetal-introjective type, but centrifuge and projective (Deleuze & Guattari, 1988, p. 395). We are not in front of a centre of translation or centre of calculus. If a centred-strategy would usually involve to annex something and bring it home, to bring 'the exterior' in, here we find that home adjusts itself to its exterior[32]: "The dwelling is subordinated to the journey; inside space conforms to outside space: tent, igloo, boat" (ibid, p. 478). More importantly, by adapting their interior to their exterior, that is, by sending Ph.D. students around, the group expands itself, blurring the difference between outside and inside, between home and away (Urry, 2002, p. 257); Ph.D. students dwell in-between, this is why even when they travel Ph.D. students are still 'at home': they are still scientists doing science: "The abode does not separate an inside from an outside (…) Thanks to this continuum, the exterior does not differ from the interior, nothing is cut off or split off, neither the art in parts, nor things in elements" (Serres, 1994, p. 32-3, our translation).

This dwelling in-between poses the question of 'where' science is produced -or, rather, it challenges attempts to close the question with a fix locality. The physical building, the head, the laboratory room, they all are situated in Bellaterra; but that does not exhaust the location of the group. A part of the it is in the laboratory in London where a co-joint paper is being written; another, in the U.S. laboratory that receives a sample of their sequenced protein; another still in Strasburg, where a complex computer program is working upon the IBF proteins and the IBF Ph.D.

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[32] "For among sedentaries, clothes-fabric and tapestry-fabric tend to annex the body and exterior space, respectively, to the immobile house: fabric integrates the body and the outside into a closed space. On the other hand, the weaving of the nomad indexes clothing and the house itself to the space of the outside, to the open smooth space in which the body moves" (Deleuze & Guattari, 1988, p. 476).

students simultaneously... Indeed, where can one locate the point of 'manufacture' of a piece of knowledge produced with proteins extracted and processed in the IBF, analysed in Munich, with internationally accepted protocols, brought back and forth by IBF-Ph.D. students, who work at the bench at both laboratories? Where is the knowledge produced? In Girona, in Barcelona, in Houston?

We do not stand in front of a fix, local bench: instead, something like a global bench[33] emerges. The expression 'global bench' is less the attempt to create one more catchy compost of 'global' than a try to focus our attention towards the emergence of a distributed laboratory. When a Ph.D. student goes to Heidelberg, with a particular task and aim, is he leaving the IBF? Is he really moving –from inside the IBF to an exterior? They may abandon the building of the IBF, but this does not mean that they leave their laboratory behind: they never lose belonging, never quite abandon the IBF, and in that sense, one could say that they do not move. This is why, in spite of having a lab half empty, heads show no sign of anxiety or restlessness: they know their Ph.D. students are working somewhere else. Working for the group, or even, working in the group, working in the IBF! But this time, an IBF distributed through networks of connection.

The group we were observing, then, mediated by its angel-messengers (Serres, 1994), becomes distributed in many other places, and carried by the Ph.D. students wherever they go. If one can say that the Ph.D. territorialise and deterritorialise, this is not less truth of the IBF itself. Through the deterritorialisation and reterritorialisation of its Ph.D. students, the IBF group itself manages to deterritorialise and reterritorialised in its turn. In a way, one can say that the IBF, constituted by trajectories, is in circulation: it is parcours. Weaving of global space? In this virtual space, in the space of our globality, appears a new question: "We do not have to answer the question 'where to go?' anymore, but this other, 'where are you?) (Serres, 1995a, p. 13, our translation). Which is a nice way to question the taken-for-grantedness of 'position'. The laboratory is not restricted to the physical walls of the building in Bellaterra;

neither to the scientific territory limited by Spanish borders. Paraphrasing bell hooks (in Urry, 2002, p. 258), we could say that the IBF is no longer one place, but locations.

Ph.D. students increase the field of influence of the IBF, its possibilities of action. After all, deterritorialisation is a way to amplify one's territory (p. 378). This amplification can be conceptualised as an extension. The deterritorialisation of Ph.D. students extends the IBF. By bringing points into connection, Ph.D. students are approaching points which, seen in Euclidian space, are still at a distance; as if, within their relational universe, they were folding space (Mol & Law, 1994)[34]. Mixing local point with local point, implicating labs with each other, the weaving movement of our angels is precisely this, an implication, an application, a fold –a clinamen making the difference. A folding of locality to achieve a relational space: "The fold implies volume and starts to construct the place, of course, but through multiplication or multiplicity, its folding will eventually fill up space. (....) Towards the small or in the immense the fold allows the passage from place to space" (Serres, 1994, p. 46, our translation).

If movement performs a fold, and the connection of locality creates a space, this may give us insights into why, while traversing smooth space, one does not move. If the fold brings localities together, one does not need to move in order to go from one to the other –which is a way of saying that it is not a matter of moving but of transformation, "intense *Spatium* instead of *Extensio*" (Deleuze & Guattari, 1988, p. 479). A space crossed by intensities, of relationality and contact, proximities and haptic perception[35]. What in Euclidian space looks like distribution through distances,

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[33] Of course, the expression 'global bench' is here a metonymy, referring to all kinds of work and knowledge production, and not only those which take place on a bench.

[34] For a similar argument discussing 'inter-topological effects', places where the rules of two different spaces clash, see Cooper (1988).

[35] "Where there is close vision, space is not visual, or rather the eye itself has a haptic, nonoptical function: no line separates earth from sky, which are of the same substance; there is neither horizon nor background nor perspective nor limit nor outline or form nor center" (Deleuze & Guattari, 1988, p. 494). These authors prefer the label haptic to tactile, for it does not oppose two senses –vision and tact, and thus implies that the eye can, under certain conditions, work in a more digital, less optic way. In fact, haptic space can be visual, tactile or auditory, as long as perception is not based on distance: in

is an implosion into proximity through the fold in a relational space; what in the first space looks like transmission and transportation becomes in the latter transformation, translation and mediation.

3. Coexistence of topologies

Whereas it remains true that the very first time a Ph.D. student travels to another laboratory she is actually co-constituting the relationship between the IBF group and the host laboratory, sometimes, if collaboratio ns prove productive and beneficial, paths may stabilise: the relation con*solidates*, and a particular laboratory will become a frequent host for IBF-Ph.D. students, as it is the case of the collaboration IBF-Girona-Houston, for instance. Compare the solidit y of a relation based on occasional three-month stays, to a relation based on periodical visits by heads, invitations to congresses, rented flats, a strong history of common publications and the reciprocity of materials, where each laboratory contributes with something that the other laboratory does not have. We abandon the insecurities of haphazard travel for the stabilities of programmed and regular encounters. The rather smooth space of those first contacts gradually acquires more striated characteristics: flow may become a more regular go and return through the same way, points becoming more important than movement per se. This transformation is no surprise: 'Smooth space is constantly being translated, transversed into a striated space; striated space is constantly being reversed, returned to a smooth space" (Deleuze & Guattari, p. 474) [36].

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smooth space one never sees from a distance: "one is never 'in front of', any more than one is 'in' (one is 'on'...)" (Deleuze & Guattari, 1988, p. 493).

[36] Nevertheless, this transformation does not necessarily happen in this direction –no evolutionism establishes an order here. Transformations between smooth and striated space do not have a fixed directionality, as happens with notions of 'growth' or 'progress'. Deleuze & Guattari are clear about their rejection of evolutionism. Thus, one cannot assume that there is a progress between smooth space and striated space, as it was once thought of the relation nomads-sedentary (before studies showing the contrary by Gryaznov). The same paths which take shape from smooth to striated space may later on dissolve again in the magma of smooth space: it may happen that when the Ph.D. finishes the thesis and abandons the IBF, the relationship between the IBF group and the host laboratory where this Ph.D. had been fades away. Or, as we saw before, that the breaking of a protein ruins the stability of the relation.

Once relationships are in place, and the IBF consolidates as part of a circuit of relations, the IBF can be characterised as what Deleuze & Guattari (1988) called a 'city'[37]: a point in a circuit, no matter its nature, regulating the horizontal flow of exchange with other points. This exchange of artefacts among cities is key, since circulation constitutes the city: "The town is the correlate of the road. The town exists only as a function of circulation, and of circuits; it is a remarkable point on the circuits that create it, and which it creates" (ibid, p. 432). Thus, cities cannot be understood as single entities, but own their being to connection: "a phenomenon of *transconsistency*, a *network*" (p. 432, original emphasis). As opposed to the vertical integration of the state[38], the city integrates horizontally: it distributes flow, whether "inert, lively or human", horizontally, from city to city, that is, completely but locally.

Each city becomes a centre, but an intermediary centre, a point in-between mediating between other central points, in coordination with them. Indeed, centrality is a matter of perspective. When one looks at the flow from one particular city, the latter appears as the distribution centre. In the IBF, this is clear: the IBF is still the point from where Ph.D. students depart, and where Ph.D. students return; it constitutes, then, a centre of a kind, regulating movement. Nevertheless, this claim is only possible if one is observing relations from the IBF itself. When one is looking at relationships from, lets us say, London, then the IBF appears as another group, a simple point in a circuit sending Ph.D. students and other scientific products to a prestigious group which can

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[37] Please notice that the argument is not metaphorical here: it is not that the IBF looks like a city, or as if it were a city, but that it is a city, reinterpreted as Deleuze & Guattari understand it.

[38] The circulation proper of cities gives form to a rather striated space, since there is integration. And still, the kind of striated space created by cities cannot be compared to that created by the state, for each of these players performs a different kind of integration. The state integrates vertically, through stratification: a central point, not placed in the middle, but above, subordinates the rest of the space through isolation; every point is linked to the centre, and de-linked from other points; vertical lines cut and cross horizontal ones. Central power is hierarchical. All unconnected points become something 'external'. All connected, internal points resonate with the centre. Unlike the transconsistency of the horizontal connections of the city, the state is a phenomenon of intraconsistency, where circuit is not between, but simply internal. "Each State is a global (not local) integration, a redundancy of resonance (not of frequency), an operation of the stratification of the territory (not of the polarization of the milieu)" (p. 433).

regard itself as the centre –for them, what should the IBF be the centre of? No point emerges as the Centre organising and directing exchange. Centrality would be something much more perspective-dependent, temporal, local, contextual, variable (coexisting at the same time with less centred spaces, and with more centralised structures).

This is why Deleuze and Guattari say that among cities in exchange there is no global resonance, but a connection of local frequency. The connection of the nodes implies a certain polarization, bringing points together. And this operation already involves a certain degree of deterritorialisation: an artefact needs to be deterritorialised enough to enter the urban circulation. Whereas the circulation among cities performs a rather striated space, in some occasions, though, the degree of deterritorialisation achieved by the circulation of a city-circuit sums up its effects to those of decodified flows originating from a overcodification of the state. When this happens, exchange between cities constitutes a deterritorialisation which gives cities a certain autonomy. Cities in connection may create a space of exchange of their own: "Thus towns arose that no longer had a connection to their own land, because they assured the trade between empires, or better, constituted on their own a free commercial network with other towns" (Deleuze & Guattari, 1988, p. 434).

The IBF, participating in exchange with other lab-cities, constitutes itself as a node in a circuit where deterritorialised Ph.D. students, papers, proteins, and other objects flow horizontally from mediating centre to mediating centre. It escapes from a Centre by distributing itself; it constitutes itself in a much more relative centre by securing itself as a point in a flow. Exchanges achieve enough deterritorialisation to overcome the resonance of the national landscape and participate in relations not *beyond* the state (as in the sense of abandoning national space to navigate in international waters), but *ignoring* state divisions. Whereas a part of the IBF group is defined by its belonging to Spanish science, the deterritorialisation achieved provides them with an identification other than the limitations of Spanish science. Moreover, after achieving this certain threshold of deterritorialisation and decentralisation regarding the striated

space of the state, it also reterritorialises in a transnational space, still in relation to the state, but not defined by it anymore (Beck, 1997).

This group of the IBF, then, gives shape and is shaped in its turn by different topologies. The IBF exists as a point localised and fixed in the vertically integrated, striated space of the state, limited by national boundaries. It also exists as a point-circuit in a dynamic global network distributing Ph.D. students and other scientific (quasi)objects in exchange when relations are strong enough, or as a flickering appearance through chaotic trajectories in the local absolute of science when relations are still on the making. Any attempt to reduce the group to one of these topologies is missing an important part of its functioning and being. These configurations are in tension, but they are not necessary opposite. They sometimes complement each other, sometimes interfere with each other. Rather than consider each of these types of circuits or spatial distributions to be different from others and closed to contacts, it may be more interesting to detect how they intermingle and interact[39].

For instance, the IBF group can be distributed because it is also a distribution point. And it may have such a powerful position in Spain because it is able to create and maintain a circuit of exchange with other points regardless of national belonging. Simply territorialised, the IBF would be a point with limited influence on other points; connected and deterritorialised, the group increases their possibilities of being and of action. The circuit-space emerging out of the contact between laboratories is promoted by spontaneous communication among laboratories, and not planned by the state as part of its scientific policy. However, it nevertheless takes place thanks to public funding. Money comes sometimes from scientific organisations, sometimes from the European Union, and others directly from states. This means that one cannot defend a strict opposition between a national space and a space that overcomes states: nation-states find ways to infiltrate again and again in global formations (hence, the

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[39] Not even the striated space integrated by the state has achieved the creation of impermeable boundaries: the State could not avoid that science produced in its 'interior' contributed to a process of global interdependence (Solingen, p. 34). And after all, the tendency towards the establishment of international links has always been related to both national and global economical and political circumstances (Abir-Am, 1997, p. 139).

name transnational, supposed to refer to those constellations that do not make sense from a state logic, but where states nevertheless participate (i.e., European Union).

This is why we should resist the temptation to think that if a group participates of a space ignoring national borders, it automatically escapes from capture by the state. As Deleuze & Guattari warn, states are also able to secrete smooth space, and one should not assume that sedentarisation is the only strategy of the state to keep control on movements. It seems that the state is able to allow and make possible a certain deterritorialisation, while not abandoning the pretensions to capture and reterritorialise all these movements. After all, it is quite often to the state's advantage that people travel abroad: they bring back home knowledge whose production would have been more expensive. Or to mention a more flagrant case in Spain, it is cheaper for the state to give postdoc grants for trained scientists to spend some time abroad, than to produce a post at the university for this same person (the risk being, however, that this person eventually decides to remain abroad –to emigrate: then the state is giving away all the investment that the training and education of this person cost). Through a certain deterritorialisation, then, the state extends its power[40].

However, the risk the state runs is that such vector of smoothing increases possibilities of being of entities that escape the control of the state –that is, that the smoothing it produces is not liable to reterritorialisation, but becomes a milieu for configurations overflowing the state, beyond its reach. As Solingen puts it (1996, p. 41), transnational links may make the scientific community stronger, and the control of the scientists by the state weaker. This is why a state may take actions to impede such links:

> "A state can oppose the mobility of its scientists outside the country, and the 'exportation' of the product of their efforts, especially if this product can favour that another state increases its relative power (…) States can also protect themselves from foreign scientists and filter knowledge coming from abroad if

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[40] Another example of how the state may be able to reaproppriate deterritorialisations is offered by Kearney (p. 553): "But as Basch et al (7) argue, a deterritorialized nation-state may extend its hegemony over its citizens who, as migrants or refugees, reside outside of its national boundaries".

they consider that they will introduce subversive elements with scientific adornments" (Solingen, 1996, p. 39).

This relates to a second temptation one should avoid: imagining that smooth space is emancipating per se. In fact, it is the ideal milieu for war machines, such as the "laboratories without boundaries" of multinational corporations, industrial research by petrochemistry, military research, global market pressures, ONG and transnational scientific associations, both public and private:

> "It is true that this new nomadism accompanies a worldwide war machine whose organization exceeds the State apparatuses and passes into energy, military-industrial, and multinational complexes" (Deleuze & Guattari, 1988, p. 387).

This cuts both ways. To lessen the control of the state on scientific exchanges may have positive resonances when we are thinking, for instance, of transnational ecological organisations[41], or even of transnational political organisations; but not necessarily so positive if we think of multinational corporations. In spite of its relation to resistance and revolutions, movement and deterritorialisation are not emancipatory or positively creative per se. As Deleuze & Guattari admonish, "Never believe that a smooth space will suffice to save us" (1988, p. 500).

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[41] "Finally, world threads to the environment are strengthening world political activities, uniting scientists in transnational network able to exercise political power in a local context" (Solingen, 1996, p. 51).

IV. Closing arguments

1. Knowledge production: between actuality and virtuality

In this chapter we have seen how scientists establish a network of collaborations and reciprocity between laboratories of different countries by exchanging both artefacts and people in a process of collective production of knowledge. Results, papers, materials, proteins, etc. can be sent from lab to lab; but when it was a matter of exchanging techniques, heads of groups needed to set Ph.D. students in circulation, so that the latter could 'incorporate' knowledge and make it flow. Thus, 'sociality between labs' and 'knowledge about the world' were simultaneously constructed out of heterogeneous exchange.

Production of knowledge, then, is intrinsically linked to a process of virtualisation. It is a misunderstanding, Lévy claims, to think that 'virtual' opposes 'real', as if that which is virtual did not exist. If something is located in time and space, as virtuality still is, then this something is real; it has existence. Virtual, he proposes following Deleuze, is not opposed to real, but to actual. Something actual is something present in the here and now. Actualisation, for this author, is the process through which something comes into being, the creative moment in which a new solution, not pre-given in the problematic circumstances, is offered to solve a puzzle. It is never a simple reproduction in reality of given and predetermined possibilities -this would be a process of realisation- but involves the emergence of novelty into the world, the invention of a form. Through actualisation something "arrives" here and now, action in the present is possible. Actualisation is akin to Aristotle's efficient causality.

The contrary process, that of virtualisation, implies an exit or abandonment of the present, of the fixity of the here and now. On the one hand, virtualisation brings time into existence, by extending us into the future and into the past, freeing us from the present, opening possibilities of recollection and remembering as well as of imagining and expectation. On the other, virtualisation also extends us into space,

allowing us to inhabit somewhere else than the 'here': we can be 'not here', as when a virtual company takes place in many points simultaneously, or a hypertext is nowhere and everywhere at the same time. Existence without a stable place of reference –or, in other words, a kind of deterritorialisation. Indeed, Lévy, opening a debate between Heidegger and Serres, proposes us to recognise that existence is so much rooted in the *Dasein* as in the ex-sistere (to be placed outside of) (Lévy, 1999, p. 21). Since not being here and now is also an experience strongly tied to thought and practice, Lévy will defend that virtualisation is a human process per excellence, a vector of humanisation.

The tension between the virtual and the actual is felt within science too. The sequence uniting a potato protein in a field, a potato protein in a test tube and a simulated potato protein does not simply show a change of context, but reveals a degradation from actuality to virtuality, where positions are relative to each other. The tension between these two tendencies can also be noticed in the territorialisation and deterritorialisation, not only of members of scientific groups, but also proper of knowledge production: one could say that proteins are deterritorialised from the bench and reterritorialised on the screen; that research is deterritorialised from one local lab and reterritorialised in journals. This often means to transform the object in a way that it alters its time-space coordinates, challenging actual existence in the search for new possibilities to arrive. Virtualisation problematises, puts stable solutions into question, opens and reopens questions, challenges: if the actualisation solves a problem, the virtualisation makes space for a new one, and consequently, for a new solution, for the arrival of more actuality:

> "In a sense, Hobbes is right: the separation between meaningless facts and explanations is artificial. But is 'being right' the essential problem for Boyle and the experimentalists -that is, to adjust to the real? Isn't his problem instead to prepare a device able to isolate from knowledge a virtual, mobile, reproducible part, independent from people? Even if this is so only in the bosom of the restricted network of laboratories equipped with means to redo the experiences. Here, the mobile, separable, meaningless and circulating

character is the *fact*. The efforts to institute science as a virtualising machine were probably more fertile than the will to adjust to the real, to say what is true"" (Lévy, 1999, p. 82-3, our translation, original emphasis).

2. Stabilities on the move

One day, after having been observing and taking notes for some hours in the IBF, I said bye-bye to people in the corridor. "Already going home?" –they asked me. "No, I'm just going to my office to write up some of the notes". As soon as I had said this, I noticed some sarcastic smiles. I put them down to their scepticism regarding whether I was going to keep on working, and did not pay much more attention to them. However, a similar scene happened some days later, when I mentioned I was going to my office to pick up something: some Ph.D. students were in pains to hide their laughter. One day, while I was going, some of them, shouted at me mockingly "hey, Psycho, are you going to your office? Ha, ha, ha!". By then I had already started to suspect that the expression "my office" was responsible for these reactions. A bit puzzled, I approached one of the Ph.D. students looking for some compassionate explanation. As it seemed, there were several meanings, some more explicit than others, linked to these words. For a start, my confident told me that, as gossip has it, somebody caught some when one of the bosses in the act of having a nap in his office. Since then, every public announcement of going to one's office was welcomed by collective hilarity, so I was wisely advised not to mention my working place too often.

On reflection, I believe that there is more in this anecdote than it seems at first sight; there are, I suspect, other layers of meaning attached to offices that can be appreciated once one realises the status of offices in the IBF. For a start, to have an office is a rare privilege in a centre where space is scarce and it translates directly into money and prestige. Only bosses have private offices, seniors usually share one among several. Postdocs and Ph.D. students can use common tables placed in the

laboratories themselves, and therefore, have no private space at their disposal. Thus, to have an office is, in principle, a sign of status within the IBF; a mark not of one's value, but of one's category, differentiating between bosses and seniors on the one hand and all the rest on the other. But offices are also a symbol for closed space: offices have a door that separates them from the rest –and sometimes, even secretaries barring entrance. They offer privacy, and make social control difficult: nobody knows what it is done behind closed doors. As opposed to the sheltered space of offices, laboratories offer a more open space, where people work in front of others, with the help, but also under the control, of others, without limits separating personal spaces: the whole lab is a huge bench, a collective working space.

Furthermore, offices are not mobile belongings –on the contrary, they tend to be quite immobile[42], a part of the building, of the house, of the institution. This blatant truth is not so meaningless in a group where movement is so primordial: offices are also dividing between those members who move (mobile or, better, movable), and those who are more fixed. Both in the sense of travelling and in the sense of belonging. Bosses and seniors have contracts and positions at the university, and they tend to inhabit the space defined by the laboratory as institution. Postdocs and Ph.D. students have a more unstable, temporally limited affiliation to the laboratory, added to their more frequent movement to other laboratories -almost as if Ph.D. students, on travelling and establishing collaborations with other labs, were expanding the open space of the bench: connecting labs, connecting spaces, creating a global bench where distribution and cooperation are possible.

Thus, offices are not only a sign of status, a marker of category, a premium of privacy and a way to escape the social control suffered by those who work in places reachable by the omnipresent collective look. They also divide between fix people and people 'on the move', or put differently, between those who experience the group as institution, and those who circulate in the group as extitution (Domènech & Tirado,

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[42] That is, in spite of advertisements by virtual technology companies informing us of (threatening us with?) the forthcoming possibility of turning every space into a working place.

1997; Tirado, 1997, 1998; Tirado & Domènech, 1998)[43]. If an institution gains its coherence through concentration in space and time, an extitution does it through regulation of flows. Control of the circulation between places as opposed to the disciplining of closed bodies –haunting instead of inhabiting. An exploitation of movement instead of an antinomadic device.

3. Weaving topologies

At the beginning of this chapter, we have shown some of the problems that, in our opinion, the dichotomy centre-periphery entails; among others a reification of the metaphor in territorial, geographical terms (terrain from where it emerged in the first place). Whereas we would not dare saying that centres and peripheries do not exist, we may find it more interesting to see them as temporal effects, creating an ever-changing landscape, highly dependent on one's perspective. The IBF-group we have been observing can constitute itself in a partial centre, when you consider the traffic coming to and leaving from the IBF, but this centrality disappears as soon as one looks at the circulation from another point of view. Neither is the IBF a peripheral point surrendering to the leading centre; this is avoided not by becoming a centre, through a strategy of appropriation and concentration, but in quite a different way; by dispersion, distribution and extension.

The IBF-group performs a kind of deterritorialisation. By connecting locality to locality, it contributes to the emergence of a new way of relating and working. Our interpretation of the data suggests, then, that the tension centre-periphery should be complemented by the tension local-global. Unlike the former, which works through appropriation and accumulation, the latter is deterritorialising. Globalisation, in

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[43] The notion of extitution, first coined by Serres, has been developed to signal the possible emergence of a different type of power. The old role of the institution is well known: as a centre of reclusion or antinomadic device, this gathering in space and time allows discipline to act at its best; a work upon the body produces knowledge and creates subjectivity. Extitutions regulate circulation through several buildings, accompanying and controlling flux. For an empirical treatment of this notion, see aforementioned references, as well as Domènech, Tirado, Travesset & Vitores (1999) and Vitores (2003).

Serres' sense, constitutes itself into a line of flight. The juxtaposition of both tensions may have different contextual effects. Sometimes both tensions may reverberate together, as when a local point remains isolated and in the periphery, or when the line of flight of globalisation is recaptured by the state creating new centres (i.e., the constitution of the East Asia as a new economical centre thanks to transnational flows of capital and work (Ong, 1999). Sometimes both tensions may interfere, as when a local point connects to a global network of localities escaping from the (possible) marginality of the periphery, or when globalisation achieves a deterritorialisation of flows.

But probably, more than pointing at the importance of the tension local-global, our interpretation brings to the fore the mixture and intermingling of different topologies[44], which are connected without being reduced to each other. We have emphasised the rather smooth, deterritorialising, fast-motion dimension in which the IBF gains life. Next to it, though, a more striated, sedentary, slow-motion dimension is to be found: whilst we can talk of a distributed IBF; the IBF institution also comes to life in the circuits around the bench and the boss' office, where people moves less. Indeed, the boss herself does not have the same range of mobility than Ph.D. students have. Granted, we could often observe her leaving the office to attend to meetings with bureaucrats of the university, and politicians of the government. But these were short, one-day trips, forcing always a return to the centre. Even on holidays did the boss need to come back to the office: movements which do not manage to achieve deterritorialisation[45], like those of other members of the group that were not on the move, but are rather fix to this more local, central IBF.

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[44] For a new way of conceiving organisations that recognises a simultaneous multiplicity of temporalitites, see Tirado, Alcaraz & Domènech, 1999.

[45] This lack of mobility of the boss is not omnipresent. We have actually seen that the boss of a young group managed to take up some visits abroad. However, this is seen as an exception, and a privilege made possible by the youth of the group, and most of the bosses do not consider abandoning the laboratory for long periods (actually, abandoning the laboratory at all). During the months we spent in the IBF, the boss of the group sending Ph.D. students around did abandon neither the laboratory nor the office work at all. We know, however, that he did so some years afterwards, in an attempt to 'change things and dynamics' in the group and the whole centre, in an attempt to send the signal to others that, after a long period of strong dedication to 'politics' he was 'back to science'.

And these different topologies cross and interlink. Local practices, occurring in one lab, on one bench, coexist with global practices, in the sense that they take place *between* localities; that is, the movements of the Ph.D. students orchestrated by the boss is how this group in particular achieves the weaving of the globality without abandoning the local. At the same time, they are able to connect to a space of transnational flows and collaboration without abandoning a space defined by national relations, too. The group produces science within the transnational circuit, but they also produce science in the lab as a point within a national space, with national funding. A Ph.D. student may be working in a thesis to be presented in one's university, while working with national funding in a lab physically placed in another country, or with foreign funding travelling around, publishing in international scientific journals just to contribute to the research of a vaccine which may be patented by a national company.

Some movements draw national boundaries, others weave globality; some resolve into a transnational relation, others maintain them tight to their bench. Central, peripheral, local, global, national, international and transnational at the same time, the group participates of different spatialities, all of which are not isolated and independent from each other, but entangled and entangling. In fact, the movement of Ph.D. students contributes to this partial connection between spaces: mediation is also mixture, and if, as Kearney (1995) suggests, globalisation implies "a shift from two-dimensional Euclidian space with its centers and peripheries and sharp boundaries", it is only to produce, not an abstract, round, empty space like a dome above our heads, but a "multidimensional space", with "interpenetrating subspaces" (p. 549). We find different types of spatial organisation, "many systems of proximity, many practical spaces" (Lévy, 1995, p. 23); different topologies that coexist in partial connection – sometimes reinforcing each other, sometimes limiting each other: a simultaneity and mixture of times and spaces (Massey, 1993; Serres, 1995a).

Each of these topologies is related to a particular type of relations, enabling different ways of being for the group. Thus, the particular group we had been observing exists

in several topologies. One way of putting it is saying that there are multiple spaces, and that each of them participates in the constitution of a different IBF. A multiplicity of spaces, a multiplicity of IBFs. And in principle, so put, this argument echoes postmodern concerns, in the attempt to challenge the univocal and singular character of entities. Nevertheless, this claim poses some difficulties. For a start, it does not explain why we all experience the IBF-institution as a single entity. Or why, although we understand that the IBF is constituted by several groups, each of them is perceived as a unity, and not as a multiplicity.

In other words, to say that we confront multiple IBFs does not help much if we are not able to show how this multiplicity manages to co-ordinate to give rise to an entity which, even if it is not perceived as single and homogeneous, it is neither considered to be 'many'. Perhaps a possible solution to this conundrum is to differentiate between there being multiple entities –as in multiple versions of one entity- and considering this entity as multiplicity: we do not face a multiplicity of IBFs, but the IBF as a multiplicity. Which means: neither plurality nor unity, but parts in partial connection, without reaching unification, where parts remain in tension through relations (Cooper, 1995; Strathern, 1991). Once more, the in-betweenness of relations is what keeps parts together and separate simultaneously[46].

4. Alternative economy

If groups such as the one we have been observing engage in these complicated strategies of connection, reasons for doing so are multiple. Given certain economic limitations that Spanish scientists face, some of them have developed some courses of

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[46] Thus, it could be interesting to think of the IBF as a boundary or liminal organisation, in a different sense, however, to that implied by Guston (2001): a kind of mediating organisation patrolling (facilitating or limiting) negotiation and exchanges between the two sides of the boundary, both of which it depends (like institutions mediating relations between science and politics). Boundary organisations "involve the participation of actors from both sides of the boundary, as well as professionals who serve a mediating role; ... they exist at the frontier of the two relatively different social worlds of politics and science, but they have distinct lines of accountability to each" (Guston, 2001, p. 400-1).

action to try and correct their situation, which they qualify as precarious in comparison to the position of other countries. In these pages we have analysed one such strategy, consisting in the constitution of a network of collaborations whose reach goes well beyond the Spanish borders. And if I say beyond, it is not in the sense that this strategy obliterates or overcomes national borders, as in leaving them behind; rather, in the sense that it achieves the weaving of a tissue of collaborations, resources and funding which is not limited to the constriction of a national space. This network of contacts and common projects allows the creation of a space where competition mixes with collaboration, where local limitations are compensated by other local resources to which one accedes through relations, where proximities and distances are not necessarily to be lived geographically but relationally.

This strategy is still partly relying on money, for the travelling of Ph.D. students requires funding. However, beyond that point, collaborations seem to be based more on exchange of different commodities, a kind of sophisticated barter of collaborations, based on reciprocity and contextual agreements. This creates a space where both, collaboration and calculability are possible. It is always dangerous to resource to sarcasm, and think that collaboration is just a subproduct of a calculative being together -at least, as dangerous as thinking that exchanges take place only out of disinterestedness and scientific curiosity. Both aspects co-exist and need to be harmonised by those engaging in collaborations –which is not easy, since it involves managing a difficult balance between equality and inequality.

Efforts to achieve a total equality may bring us back to a market economy: we saw that the more closed and fixed the interaction is -the faster and fair the retribution- the closest we come to a commercial exchange without links remaining between the two sides afterwards. If, on the contrary, the relation allows a time extension, a postponement of reciprocity, it will resemble a gift economy where partners are caught in relation after exchange. Groups participating in a collaboration need to avoid being classified in one of the two extremes; neither as swapping paper for student out of self-interest, nor as living off the generosity of personal contacts and

favours. The division between economical exchange or reciprocity of favours is not, however, one of interest vs. personal trust: both types of exchange are equally immersed in trust and interestedness, the level of entanglement between partners indicating the type of transaction taking place (Callon, 1998).

In this context, I cannot resist the temptation to offer a suggesting connection. The strategy in which the boss and her group have engaged is not based on the definitions of borders –as in delimiting what piece of land belongs to whom– but on movement and distribution. It is in this sense that it is a rather nomadic strategy, since it does not try to expand the sedentary law of one's polis, but it distributes people in land[47]: "Opposition of the traveller and the homebody, the pastor and the peasant, the right of passage and the right of ownership" (Serres, 1982, p. 84). Indeed, nomad is believed to come from *noumos*, both from the root *Nem*, which indicates distribution and not allocation -the distribution of animals in a non-limited space: "*To take to pasture (nemô) refers not to a parceling out but to a scattering, to a repartition of animals*" (Deleuze & Guattari, 1988, p. 380, n. 51). This pastoral root will eventually give birth to monetary terms such as *nomisma* or *numismatics*, and it is not chance that the word *pecunia* (money, treasure, wealth) derives from *pecu* (cattle & sheep, herd, flock) (Braidotti, 1994; Mauss, 2000). Money would come to signify that which is mobile and distributed, being able to be passed on, *mancipio*. As Lévy (1999, p. 113, our translation) puts it, "the coin is the antithesis of territory", since it is fluid, sharable, anonymous, and not appropriable in the way land is[48]. We will leave it up to the reader, though, to establish certain connections between Ph.D. students, coins and cattle...

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[47] For this argument, see Deleuze & Guattari (1988), Casey (1997) & Braidotti (1994).
[48] Lévy claims: "If everybody kept its money in a case, the contemporary economic game would collapse completely. On the contrary, owners keep their land, and this has no catastrophic consequence for agriculture" (1999, p. 113, our translation).

5. IBF as intermezzo

In relation to movement, there is a tension crossing members of the IBF. Some members belong to the institution in a more or less stable way. Thus, bosses of the groups (lecturers and professors) and those seniors who are lecturers, together with janitors and secretaries have indefinite contracts. Many seniors, technicians and some postdocs have temporal contracts. Many postdocs and all Ph.D. students are members of the IBF only while they enjoy a grants or do the thesis. They know that they will not be able to stay in the IBF, they know that they, like many before them, will simply "pass through". This is not an irrelevant division, but has consequences in how relationships are lived there. Thus, Ph.D. students complain that members of the PAS feel they belong in a more true way: they have a "feeling of superiority" because "they gain more money, and they have a post, they are part of the institution. You are just a thing that passes, comes in here and comes out there. (...) They belong here, and you are just passing through" (EG05-28).

The conceptualisation of Ph.D. students as temporary members that 'pass through' is shared by all members, and impregnates the vision of the very institution. We should not forget that, given its links with the university *and* the impossibility to contract people, one of the main functions of the IBF is, next to research, the training of Ph.D. students: they can't do anything else than capture Ph.D. students, train them and let them go, just in time to attract more Ph.D. students and start the cycle again. They see themselves as a place of production of Ph.D. students, "a passage, between the end of the degree and the search for a job. A training place" (EI16-11). As the director of the IBF says, this is a "passing through" institution (EI01-14). An intermezzo, an in-between belonging.

Ph.D. students know that they will sooner or later have to leave the IBF as well. Movement, then, is not experienced as an exception, but as an activity which may very soon become a necessity –as if they made a virtue out of a necessity: "the movement is not from one point to another, but becomes perpetual, without aim or

destination, without departure or arrival" (Deleuze & Guattari, 1988, p. 353). When Ph.D. students or postdocs decide to leave one's lab to 'search for fortune' somewhere else, they are not so much trying to look for a new home, as if the change from lab to lab equated a jump from home to home. Rather, movements make it possible to remain 'at home', to remain within science; they are not homeless scientists, but scientists travelling with their belongings so that they can turn any place into their home, they can make home everywhere (Braidotti, 1994, p. 16). By moving, scientists stay a bit longer within the domains of science. They move in order not to move –this is the type of movement without movement[49] defining Deleuze & Guattari's nomads:

> "Whereas the migrant leaves behind a milieu that has become amorphous or hostile, the nomad is one who does not depart does not want to depart, who clings to the sooth space left by the receding forest, where the steppe or the desert advances, and who invents nomadism as a response to this challenge" (Deleuze & Guattari, 1988, p. 381).

The constant movement needed to remain in science means that belonging depends on a constant territorialisation-deterritorialisation, a constant attachment and detachment. Belonging, then, is linked to this oscillation, one becomes attached to the movement of detachment, as if they "reterritorialised on deterritorialisation itself (ibid, p. 381). This attachment to detachment maintains the link between people and ground, but not with a place or land, but with a region –again the region of the local absolute, where they can circulate and emerge at any point. The identity of these wandering scientists is not defined anymore by the place where they at present are – they know this place may go, as others before- but it is given rather by the path, the

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[49] "The nomad distributes himself in a smooth space, he occupies, inhabits, holds that space; that is his territorial principle. It is therefore false to define the nomad by movement. Toynbee is profoundly right to suggest that the nomad is on the contrary he who does not move" (Deleuze & Guattari, 1988, p. 381). The notion that things in circulation may nevertheless not move is already to be found in Mauss (2000). He explains, for instance, how *tonga*, that is, goods or property which belong to the mother's side and that at marriage pass from mother to daughter, are considered 'immobile property', not because they do not move, but because they never leave the maternal side: "they are kinds of fixed property –immovable because of their destination" (Mauss, 2000, p. 9). In contrast, the *oloa*, objects and tools belonging to the husband, change from the paternal to the maternal side –this is why they are seen as movable possessions.

trajectory, the sequence of places where they have been (Braidotti, 1994, p. 14), their relocations or shifts of habitations (Ingold, 1986).

6. *Religare*: weaving the collective

Along these sections we have insisted upon the collective meaning of this strategy. Of course, Ph.D. students, as well as the boss, obtain personal gains with it, such as new experiences, new knowledge, and new connections. But above all, the great benefit is obtained by the group, which nurtures itself out of these movements: its curriculum and renown increase. Participation in the strategy of collaborations implies simultaneously participation in an act of feeding. Thus, with their movements, Ph.D. students –who are usually nurtured by the group- have a chance of contributing to collective nurturance. In this sense, these moments of exchange are not so dissimilar from traditional rituals of exchange. Boas translated *potlach* -the name with which some societies have labelled these rituals- as 'feeder' or 'place of being satiated': "the two meanings of potlatch, i.e. 'gift' and 'food' are not mutually exclusive, since at least in theory the essential form of the 'total service' relates to nourishment" (Mauss, 2000, p. 86, n. 13). Exchanges of Ph.D. students, like other ritual of exchanges such as conferences, meetings, speeches, are moments where both, intellectual food ('knowledge') and 'nurturance' are passed on.

Exchange also engenders new connections of relation: the circulation of people and scientific objects –quasi-objects, quasi-subjects- creates new configurations, not pre-given, but emerging with the exchange and its unfolding. Like angels weaving the scientific community by bringing groups in affinity, Ph.D. students unite different localities through their trips, mediations and double belongings. Thus, the scientific community emerges not so much as the place where members share culture, values, perspectives, perceptions...[50] Rather, this collective is rather being created by the circulation of entities, both people and artefacts, that effortfully carve out paths. Paths

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[50] Though this may still be the case, but more as a result than a cause of this 'being together'.

which will eventually dissolve in the sand again, or which will strengthen into highways, transporting and transforming papers and recipes, pipettes and programs, hand movements incorporated in bodies, pulverised proteins, gene gels and frozen samples[51].

Circulation of artefacts brings a new assemblage of possibilities into being: collaborations which allow the laboratory to be part of a circuit, of a circulation in which the scientific 'I' may flow. A circulation which grants a participation in science, the becoming of a member, that is, of a position which takes part in the flow. Through it, the whole group stabilises, while the scientific persona of the boss and the Ph.D. students consolidate more with each trip; if they become a more recognised group, making themselves a name, it is not because they have achieved a distance from the others that makes them more visible (as it would be the case with competition), but, on the contrary, because they have become so entangled with others that they have entered the circuit where they can participate in the flow: by giving and receiving, they take part in the consumption and distribution of 'I's and 'you's (Serres, 1982; 1997)[52].

The circulation of artefacts (and we will understand here quasi-objects, quasi-subjects) creates deterritorialisation, and with it, a certain amount of virtualisation, understood à la Lévy. Through this exchange, the IBF frees from the here and now, to

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[51] The constitutive role of quasi-objects on circulation has already been observed, though differently narrated. Historians of science have shown how, more than a commonality of values, it was the generalisation of instruments, standards, procedures... that created enough affinity among laboratories, so as to allow scientists of a part of the world to work in collaboration with scientists of another. For instance, from the middle of the XIX century onwards, the generalisation of some methods and practices throughout several laboratories created the conditions for an easy communication and contact among laboratories. The focus on experimentation and measurement in physics, chemistry, biology, physiology, biochemistry gave birth to a common way of working that made possible the diffusion of instruments and techniques from laboratory to laboratory. So much so, that already at the end of the XIX century scientists of one part of Europe could adapt and work without problems in a laboratory in another part. In its turn, this favoured the establishment of a standard nomenclature, methods and unites of measure (Crawford, 1996). It seems that the introduction and spread of similar objects, techniques and units favours commonality in a way that does not require the previous existence of values and culture.

[52] The possession of individuality, of an 'I', is conferred on by the connection with the circulation of the object: now you receive it, now you pass it on. It is also in this sense, Serres says, that we can say this circulating object is a quasi-subject: it distributes subjectivity.

attempt new ways of being, attempts to prolong or extend the IBF beyond the limits of certain time-space conjunction. The circulation of the artefact problematises sedentary ways of being together, as well as established notions of belonging and property, and opens a search for new ways of understanding community. The movement of virtualisation and actualisation (deterritorialisation and reterritorialisation) that the IBF suffers with each movement is a witness to this (ontological) search:

> "The object sustains virtuality: deterritorialised, operator of the reciprocal step from the private to the public, or the local to the global, not destroyed by its use, nonexclusive, it traces the situation, it charges the problematic field, the nuclei of tensions or the psychic landscape of the group. This virtuality embedded in an objective support is usually actualised in events, in social processes, in acts or affects of collective will (ball movements, narrative statements, sales, new experiences, new links to the Web" (Lévy, 1995, p. 117, our translation).

7. Political extensions: the dealer

We saw that this type of exchange is collective, since, as we have seen in our data and as Mauss already advanced, it is not moved by individual motivations, but orchestrated by the boss. If exchange achieves mediation it is also mediated by the head: she puts herself in-between, between her Ph.D. students and those laboratories harbouring them. She is the negotiator, the bargainer, the dealer, the trader and, at the same time, she is the translator making the wishes and aims of one laboratory compatible with those of the other. This double value, translator and trader, is not casual, as it is revealed by the figure of Hermes[53], the mediator. This is the position of

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[53] The Greek God Hermes, son of Zeus and Nymph Maya, brother of Apollo and father of Pan, was the protector of paths as well as wanderers, and had as one of his tasks, to accompany souls in their way from this world the next. God of shepherds and herds, he was nevertheless related to music (he created the lyre and improvisation), competition, inspiration and invention (he invented the way to obtain fire by rubbing stones. Given his connection to mental clarity and creation, he was also the god of rhetoric and orators. He was also god of thieves and, with the Romans, god of commerce (equated to Mercury).

the person who describes relations of the sort many-one-many. (Many tongues-one translator- many listeners understanding each other through the translation. Many interests- one dealer- many people coordinating each other through a common interest). Without this narrowing of the flow through one point (a kind of funnel effect, a rotunda (Serres, 1995b)), no relations would be established, chaos of languages or of interests would reign –heterogeneity finds a way to homogeneity ("they ease passage, control it, and relate to the one-to-one" (p. 42). And in this passing point, this moment of translation is also a moment of transformation: flow cannot go through without changing. The ex-changer is a changer, a transformer.

The dealer or translator is put exactly in-between, in the best point: the intersection of relations, controlling the flow, making exchange possible. This being at the crossroads weaves a tissue of relations forming a relatively centred space: the mediator puts herself in the middle, where all decisions and circulation must go through. This is a weaving of the collective –a weaving, as we saw, of collaboration networks and community, and therefore, automatically a political weaving:

> "It is at the knots of regulation, and suddenly, it relates to the collective. The one who succeeds in the relation of many-one, forms and makes it work, is the politician and has found power. As is often said, he has the power of decision: of course, since he is at the crossings, the intercuttings: here, the intersections" (Serres, 1982, p. 43).

When members of the IBF say that their bosses sometimes do more politics than science, they are intuitively grasping the nature of the bosses' positions; when they say that bosses 'move' people like pieces of a game to weave a group strategy, they are recognising their being in-between: bosses are interesting Ph.D. students, they are interesting laboratories, they are being interested in its turn, creating an assemblage in which they all become entangled (or "inextricably entwined", to use Law's

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Magician, he was in possession of a magic wand which brought dreams, blessing and wealth. His nimbleness and elegance turned him into the messenger of gods, mediating between gods and gods and humans, represented with winged shoes and a hat (Enzyklopädie Brock Haus, vol. 9, p. 710).

expression appropriated by Cooper, 1995). A political entanglement, in the sense that it affects people's life, it constitutes the collective.

But being in-between is often a matter of perspective. If one can say that the boss is between the Ph.D. students and the welcoming laboratories, one can also claim that Ph.D. students are between two bosses, two groups. The way they are in-between, and the effects this between-ness has, are nevertheless different. For instance, the mediation of the boss is intimately linked to a central position where decisions are taken and exchanges is regulated; the mediation of the Ph.D. students stimulates exchanges and its productivity more than controls them (de Certau, p. 120). Positions of mediation, then, cannot be simply equated: mediation is " a generalized practice of singularity" (de Certeau, p. 119; see also Serres), a matter of particular circumstances, of concreteness.

8. Mixtures

Until now, we have interpreted the travelling of Ph.D. students under several perspectives. Our reader may be now as lost as at the beginning: what is the conclusion? Are we in front of a case of economical exchange? A barter of favours? A renting of working labour? Is this an interested or a disinterested transaction? Scientific collaboration or strategic business? A way to improve resources? A strategy to expand a network of friends and collaborators? A circulation of money, of commodities, of goods, of quasi-objects and quasi-subjects? A way to extend the boss of a group beyond its limits? An appropriation by and for the self of other's hospitality? A way to spread belongings to extend belonging? A flight of devoted angels weaving the collective? Lost highways centring and decentring marginalities? A tension between deterritorialisation and reterritorialisation? Nation-states forcing scientists to emigrate while profiting from the capture of the results of their movements? Scientific centres extending colonisation? Margins rendering themselves to conquest?

If we do not risk to privilege any of these possible interpretations and exclude the rest is because members of the IBF are the first to play with this indefiniteness. In the field, the decision about the nature of this exchange is continuously postponed. Its effectiveness lies precisely on the fact that it cannot be properly defined and cut to limits. These moments of exchange are highly ambivale nt: they share characteristics proper of reciprocity and gift exchange, but also the bargaining and the gain possibilities of economical transactional; they are moments of production and obtaining of 'nurturance' and also 'knowledge' (both substances interact with and transform into each other: a paper is made possible by knowledge and gives way to nurturance, while nurturance is needed to produce knowledge); they show qualities of rituals, where links and relations are built and renewed and cancelled (where the collective is weaved, hence *religare*); they entail negotiations proper of economics, and proper of politics. And if they are ambivalent in the sense that they share traits from several orders, it is also ambiguous since it fulfils none of them completely. It is neither quite a matter of currency transactions, nor of barter, nor of solidarity and support, nor of symbolic interactions, nor of feeding, etc. These moments of exchange do not let themselves be captured by an identity label, but rather make ambiguity circulate, and while uncertainty leaves open doors, these mixing moments enable things to come together, and groups to find new expanding possibilities.

9. Transforming movement

Throughout this chapter, we have shown how movement could lead to creation, the emergence of something new. And this not because of movement per se, but thanks to the amount of mediation which movement makes possible, a mediation which allows the new to interrupt the old. This is another way of saying that when we are dealing with mediation or translation the emphasis falls not on movement as displacement, but on movement as transformation. As Robert Cooper (1995) observes, we are culturally prepared to perceive what Whitehead called the principle of simple

location, that is, to perceive that "clear-cut, definite things occupy clear-cut, definite places in space and time" (p. 1), and that the thing reminds unaltered when moving from place/time A, to place/time B. This perception makes us blind to a more radical type of movement, the movement of transformation: "For Whitehead, movement resides in the unfinished and heterogeneous character of 'mutual relatedness'; movement is the action of the 'between'" (ibid).

To recognise all the transformations entailed in movement, to understand that movement through space or time involves metamorphosis, means to accept the incompleteness, unfinished nature of life. Every time Ph.D. students move, every time a paper changes hands, a new thread is being woven: the collective is slightly modified, a clinamen. Whether this modification will impose a new configuration or will adapt itself to the previous one is not predictable before hand, but the important point is to understand that even when things appear as stable and not moving, this is because there is work done somewhere to keep things the same [54]. These thoughts, indebted to the insights of ANT and ethnomethodology, bring about awareness of the fact that permanence and change are interrelated (id, p. 3); there is always textuality and tectonics (Curt, 1994), stability and fluidity (Lee, 1996).

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[54] This work remains often unacknowledged, hence multiple and diverse efforts at representation of invisible figures: Morrison, 1987; Shapin, 1991; Star, 1991; Webster, 1998; Wilkinson & Kitzinger, 1996.

1. Liminal moments

> "A boundary is not that at which something stops but, as
> the Greeks recognized, the boundary is that from which
> something begins its presencing"
>
> (Heidegger, 1971, p. 154).

We have reached the conclusion, point at which we are supposed to offer a balance of
the proposals so far made. This can be hard work, though, having into account the
heterogeneity that these pages have gathered: ethnographic extravagations, scientific
specificities suggesting a possible demarcation based on risk, skirmishes between
science and politics crossing laboratory life, work of coordination for the emergence
of a scientific fact, angels flying around weaving globality while mixing objects and
subjects... It may be now difficult to convince the reader that all these topics are
united by a common problematic -and yet, this is precisely what I want to suggest.
All the situations we have discussed along these pages refer us to what we could call
'liminal moments', encounters between things that are perceived as different, that
pose problems of commensurability and relation. Moments, to put it with other
words, when belongings are at play, and we inhabit the boundary[1].

Understood like this, the notion of 'boundary' has had the interesting effect of
bringing relations to the fore within several fields of social sciences (Álvarez, 1995;
Lamont & Molnár, 2002). However, this concern with 'being on the boundary' often
suffers subtle (and not that subtle) transformations. One of the risks of these notions,
when they are interpreted through geographic images, is to end up reifying limits as
already existing zones, as well as those territories or entities assumed as existing on
each side of the border. That is, we end up believing that those entities allegedly
negotiated at the boundary pre-exist liminal relations (Abbot, 1995; Strathern, 1997),
forgetting the radical constitution of those 'fundamental encounters (Smith, 1996).
And this risk remains even when we claim that boundaries are permeable or when we

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[1] We ignore, to our own peril, Deleuze & Guattari's differentiation between limit and threshold: "the
limit designates the penultimate marking a necessary rebeginning, and the threshold the ultimate
marking an inevitable change" (1988, p. 438). Our use of liminality is closer to their 'threshold', even

complain against the unequal distribution of the right to cross them. If anything, boundaries are effects (Mendiola, 2001; Pallí, 2000).

But if reification is a risk, there is another effect of this conception which is much more difficult to avoid: exclusion. Along these pages we have found several instances of this peculiar way of drawing limits one must surpass: either you are in or out of the scientific community; either you work as a scientist or you become a politician; either is the protein constructed or real, single or multiple; a gift of hospitality or an investment. Moments, then, when an entity is defined as self or other. These dangers are well exemplified by discourses on immigration and cultural contact suggest with their imaginary of cultures as contained entities of closed borders that collide against each other in destabilising moments of contact (see Stolke's (1995) sharp critique). If you are on this side of the line then you are in; if you are on the other, you are out and crossings are problematic -either if they are conceptualised as dangerous or as challenging.

However, in this work we have made space for moments in which limits were differently imagined, allowing a coordination of heterogeneity which the previous view would prescribe. Moments in which boundaries are not boundaries *of* (Abbot, 1995), avoiding too fast exclusions on behalf of the dichotomy sameness vs. otherness, since it does not rely on the geographical understanding of limits as a line to cross. Alternatives might be difficult to think, since, as Smith (1996, p. 34) reading Deleuze suggests, empirically limits are inaccessible and unimaginable, whereas from a transcendental point of view, limits are that which can *only* be imagined. However, the effort is worth, since new ways of conceptualising liminality could help avoid what Serres calls "the evil in the world" (1997, p. 148): belonging intimately linked to exclusion.

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though nothing impedes the transformation becoming an old rebiginning.

Throughout the thesis, I have tried to present 'limits' as moments of constitution[2] in which something new enters the world, a process of becoming. We have opened this work showing a moment of negotiation of belongings and constitution in which the ethnographer felt moved by something other than herself, emerging out of this encounter as a new position partially connected to the old one: partly the same, partly different. We have followed the progressive constitution of scientists as competent members of their labs, as well as moments when attachments blurring parts and wholes create groups and personae partially connected. We have analysed how certain figures are placed in a liminal zone between politics and science, and are made to carry the burden of this differentiation while simultaneously betraying the work necessary to sustain the difference. We have also seen how several proteins are overlapped and engineered into one, made to belong to each other in a way which is not understandable through relations such as prototype vs. subtype, abstract model vs. concrete exemplar, or integration of multiplicity into one. And how movement translates communities, constituting a patchwork-like globality. Moments in which homes shake, belonging must be worked upon and knowledge experiences the tension between virtuality and actuality.

I have resourced to several authors to bring to the fore some characteristics which, to my view, all these situations share. To make my point I have threaded different arguments together, some borrowed from authors who would not have appreciated the juxtaposition, sometimes at the expense of a certain work of magnification of apparent similitudes[3] that has created a tense, impossible mosaic. If I have dared to

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[2] Anthropology has been well aware of the transformation that becoming involves, clearly expressed in 'rites of passage' and other liminal moments: "To 'grow' a girl into a woman is to effect an ontological transformation; it is not merely to convey an unchanging substance from one position to another by a quasi-mechanical force" (Turner, 1967, p. 101-2). The ethnographer's attempts to belong have also been so conceptualised: "The concept of becoming implies that one gives in to an alien reality and allows oneself to change in the process"(Hastrup, 1990, p. 19).

[3] For instance, I suspect that neither Bachelard nor Stengers would have particularly liked to have Heidegger as neighbour. In my defence I can claim that had I not read Bachelard, I would not have done this Heideggerian reading, and the other way round: without Heidegger, Bachelardian homes would have remained more stable and closed. Likewise, without reading proposals of exile as existence I would not have appreciated Bachelard's 'phenomenology of those words that start with ex', and would not have understood Heidegger's attempts to challenge the closure of the circle, and so on. Connections multiply, links become intimate.

mix so different positions is because I do not claim homogeneity: I do not pretend that these authors say the same or have the same concerns as those discussed here. And still, I have found useful arguments in all of them to discuss liminal situations, drawing a landscape crossed by no return journeys of intensive affections that move us beyond ourselves in an exile of a sort, a kind of 'exit' of one's self, or better, in a movement which weaves selves and belongings as an exit; a spiralling movement that carries us to and fro never reaching an inexistent centre of concentration, not going far enough to go astray; but above all, a journey through ontological changes or metamorphosis. In short, the experience that is repeated in different ways and through different means is one of 'becoming other'.

Liminal moments are those in which clear categories are suspended for a split second, not to dissolve into a field of brotherhood and sameness, simply to enter a more ambiguous field of forces that set previous positionings on the move. A space of exchanges and intimacies, a space of prepositions, if you want, tracing the movement of relations (Serres & Latour, 1995). Seconds before and seconds after, some positions will be strengthened again in more stable categories –psychologists and biologists, analyser and analysed- but when observed in this in-between moment, we perceive movement -a movement that triggers off work upon belongings and affiliations. Liminal moments are those in which ontologies are in the making. Which means, of course, that they are also moments in which power is at its best. Not a negative power that excludes through clear boundaries, rather a positive, creative power that constitutes us. Full with new possibilities, liminality is not an innocent space.

This is, in a way, the space in which thought moves. In his reflections on Kant's essay *Was ist Aufklärung?*, Foucault proposes us to consider the philosophical ethos to be a sort of limit or limiting attitude: a type of critique that escapes the dichotomy between being inside or outside, and places itself on the boundary. But not simply to state where limits are, or, as the Kantian critique wanted it, to detect those limits that knowledge should not overcome. Rather, "it is a matter of transforming the critique

exercised in the form of the necessary limitation into a critique put into praxis in the shape of a possible crossing" (Foucault, 1994b, p. 16). This is a type of critique which "will not deduce from how we are what it is impossible for us to do or know; but it will derive, from the contingency which has made us be what we are, the possibility to stop being, doing and thinking what we are, do or think" (ibid, p. 16). In other words, thought inhabits liminality so as to help us participate in, and fight for, what we are, what we don't want to be and what we would like -a folding and unfolding of ourselves which requires "a work of ourselves upon ourselves as free beings" (ibid, p. 16; Deleuze, 1989).

In this sense, a liminal moment implies an opening, a problematisation that can contribute to the developing of autonomy, in Castoriadis' sense (1988). The intense ambivalence and ambiguity that impregnates liminality (Bhabha, 1994; Mendiola, 2001) opens new possibilities of being that allow us to question our own world and our own being, to alter our system of knowledge and organisation in order to create another 'ontological eidos', another self in another world –without forgetting, though, that the same ambiguity which opens possibilities also maximises power and its subjections (Munro, 1998, 1999). Thus, work on the boundary helps us break open the enclosure or cognitive circle through which living beings, according to Castoriadis reading Varela, organise their world and constitute themselves. The emergence of autonomy would be, then, an ontological opening –an event.

2. Connecting partially

Liminality has usually been related with moments of contact –encounters between selves and others in transformation, and this tension has appeared under different appearances in several moments: biologists vs. psychologists, natural vs. social scientists, subject vs. object, individual vs. group, singularity vs. multiplicity, locality vs. globality, actuality vs. virtuality, institutions vs. extitutions, etc. One tool has become relevant to make sense of this oscillation: Strathern's partial connection. I

have put this notion to use so as to think differently about those encounters between sameness and otherness -that is, assuming neither that they are complete entities in themselves which enter into relation *a posteriory* in encounters which simply bring them closer, nor reducing difference to the dictatorship of the same that can only imagine fusion into oneness.

Indeed, from this partial perspective, relata are not previous to the relation, since they emerge as such thanks to these particular ontological transactions. But neither were they inexistent previous to them, and this is precisely what allows this flickering perspective: now we see two parts, now we see two parts-in-connection. The link between relata and relation is also partial, whereby the emergence of a 'third entity' mediates and displaces parts, without total substitution, since a partial-connection argument challenges a clear distinction between here and there, now and then, same and other, object and subject.

This double perception makes space for the tense coexistence of characteristics which, differently thought, could not be brought together. Thus, my self is me *and* not me; your extensions belong to you *and* are beyond you; scientific personae are individual *and* collective; scientific practices exclude *and* include politics; groups and knowledge reproduce *and* regenerate themselves; proteins can be regarded as real *and* as constructed, multiple *and* single; science can be imagined as local *and* global; transactions exchange profits *and* gifts; Ph.D. students change on moving places *and* transform without moving; the IBF is an institution *and* an extitution. Already-gone members are present *and* absent in their labs. What we could dismiss as contradiction appears here as creative tension. Creative not only because difference can coexist without cancelling itself, but because images-in-tension strengthen each other.

We have seen a good example of this in our analysis of the process through which a protein is progressively brought into the world; not only are 'reality' and 'construction' not opposite to each other, but they promote each other, it is precisely because it is constructed, that objects become real. But it is also because they are real

that we can construct. Likewise, only because the IBF is a solid institution well rooted in the here and now of the Spanish science can it deterritorialise in circulation. And only because the virtual IBF works for belonging in international science can the local IBF be so well rooted 'at home'. Only because newcomers in a lab attach to objects and old members can they become scientists, just like objects can take part in science only when attached to scientists. Thanks to its anchorage in society –and not in spite of it- can science develop new possibilities of being, just as society extends through science. Thus, each figure reinforces the other, sustains the other, in a way that emphasises dependencies.

Now, both perceptions cannot take place simultaneously. We cannot see it all, and our perception is dependent on position. But this double perception is not produced by a movement, as if we changed position in order to see different things. In chapter 3 we have seen that the change from a macro to a micro approach on a phenomena gives the impression of a change in position or in perspective: from this point we see the whole, from this we see the parts; we look up and we see the collective, we look down and we see individuals. However, partial connections are not elicited by movement, but by transformation: "not two perspectives as it were, but a perspective seen twice, ground as another figure, figure as another ground" (Strathern, 1991, p. 113). A switching of perspectives where each one is making the other possible –going to the background, transforming into ground for the other to emerge or establish (for a while: until the next switching). This changing movement should not be imagined as much as a clear substitution of one perspective for the other, since the invisible perspective remains there, "ducked down", as it were. Rather, it is more like a displacement, temporary and partial.

Displacement conveys better the flickering involved in these connections. In a substitution we imagine the second entity taking the place of a first one, which then disappears. The new one is present, the second is absent. However, in a partial connection, none of the two perspectives can be regarded as absent –or if you want, it is an absence which has effects upon what is present. As we have seen in the case of

the boss entering -through her Ph.D. students- a lab where she is not, extensions as partial connections challenge the opposition between absence and presence. After all, extension as effect is a vector of displacement, a relation with a nonrelation; a moment of union that keeps us separate, a moment of eternal separation which joins us; a game of absence and presence that informs life[4]. Maybe this is why Gabilondo suggests another reading of 'absence': absence (*ab-sens*, from *ab-sum*) does not only reflect separation and deferral, but origin or point of departure, it shows a remoteness which appears as another proximity (Gabilondo, 2001, p. 68). It is as if one perspective could carry an absence that constitutes it.

Now, a displacement always leaves a gap –a split second, a pause, a void- without which the very switching would not be possible at all. This switching of perspectives makes such a gap visible –and twice: the gap between parts in connection (say, between science and politics), and the gap between perspectives (the gap between a partially connected perception where science and politics are compatible and a merographic one where they are distinct). In all these cases we perceive a gap which is already there, so to speak, that calls for its bridging: it is precisely this gap, and the need to cross it, that makes relation possible. As Serres (1982) has put it on analysing communication, if there is no difference between sender and receiver there is no channel, and no relation. At the same time, though, when we see parts in connection, the relation is already there, and it is this connection that allows a perception of a possible cut -out of which two different parts emerge allowing exchange –without two groups being imagined as different, exchange does not make sense. This has been illustrated, for instance, through the process by which scientific groups come into being: the cut of flow between an old senior-about-to-become-a-boss and her old boss must be performed, even if this is only in order to unite them with new collaborations right afterwards. It is as if the gap came before and after the relation, challenging cause-effect logic.

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[4] Only so can we understand that, according to Levinas, in Blanchot death is lived as an event of life (Arranz, 2001).

A partial extension is not a material juxtaposition (elements next to elements that enter in transaction without intimacy); neither a continuity of one entity into an other without separation (from same to same, an homogeneous whole). Neither identity nor difference, then: a partial connection that performs both a cut that opens the perception of the two entities in exchange as different, and an attachment, since this difference is precisely an occasion for connection: "As Vygotsky would say, 'the bridging of the 'gap' is the making of the link" (Shotter, 1993b, p. 45). "Always and ever differently the bridge escorts the lingering and hastening ways of men to and fro, so that they may get to other banks... The bridge gathers as a passage that crosses" (Heidegger, 1971, p. 152-3). The attachment obtained through a partial connection is this third moment that contructs both, the gap and its bridging: "What absent and white culture constructs the separation and then the contact between two chromatic cultures?" (Serres, 1995a, p. 32, our translation).

The switching of perception that the gap enables makes space for transformation. We have just mentioned the 'displacement effect' that this switching provokes. But as we have shown in chapter 5, movement also implies change. Thus, displacement and transformation go hand in hand. The argument, then, is not only that knowledge is situated because one's perspective depends on one's position ('knowledge changes depending on the position of the knower in relation to the object'); but also that each perspective makes the knower change. In other words, the switching does not only alter the object but also the subject. A mutual alteration, if you want, whereby the subject's position may alter the object and the latter affects the former. An alteration which, as the word itself suggests, is a becoming other.

This becoming other can be understood as an intimate type of exchange. We have seen that exchange is not simply a system for making things change hands, but also for things to make people shake hands. Now it is time to insist that exchange is also a passing on of properties and therefore, an ontological process - not a mere give-and-take, but an intimate relation that links two parts in a mutual becoming. Throughout these pages and with several examples we have emphasised the entanglement

bonding entities in exchange, so much so that it is sometimes difficult to claim where one starts and the other finishes. United by a reciprocal transaction that obliges them to each other, this exchange disturbs boundaries, since it gives way to a circulation between figures that swaps properties: a movement without return or centre that keeps on distributing one relata into the other. I have something other in me, you have something mine, objects that take subject positions and subjects that are objectified, quasi-objects quasi-subjects. A possession that brings transformation: a becoming other. Thus, exchange is a *relation* that links them intimately, with constitutive effects that disturb the clear division between self and other. Exchange performs relation: neither one nor two, entities in exchange remain partially connected.

3. Being affected

To emphasise liminal encounters as moments of constitution is to talk of *affectivity*. Before I am told off for mentioning such a dirty word, I should hasten to add that I am not proposing we include in our explanations feelings understood as individual, intrapyschic emotion separated from cognition and action. And to anticipate criticism, neither is affectivity 'outside' language, on the contrary, several authors have shown the affective, moving character of language: "besides (and prior to) their referential-representational function, words also work in a noncognitive, formative way to 'shape' our unreflective, embodied or sensuous ways of looking and acting - in short, to 'move' us. Vygotsky says that 'every idea contains a transmuted affective attitude toward the bit of reality to which it refers'" (Shotter, 1993b, p. 41-2). Which means neither that affects are simply 'a way of talking', nor that their analysis can be exhausted by discursivity. More efforts would be needed to make sense of this 'being moved' by others that we keep on detecting in these encounters with otherness –and which I was forced to acknowledge in field work. Relations mould us into particular configurations: we become a constituting part of an ethos as an organised practical-moral setting (Shotter, 1993a, b), a structure of feeling (Williams, 1973).

Affective moments of relation can be conceptualised, with Spinoza, as encounters which bring us further or that constrain us; in any case, moments which do not leave us indifferent, but affect our power. Or, with Heidegger, as moments that make us vibrate, entering in a kind of harmony with something other than ourselves: a mutual modulation of being[5] (see also Serres (1995b) on music). Thus, feelings can be understood as those changes we undergo in encounters with others, as effects of external forces that do not originate in 'our-selves' (least of all in our hearts) –a way of being moved and attuned to something other, a way to enter in vibration with something other, either to sum up forces or to create interference. Thus, affectivity blurs - in the intensive way Bachelard has suggested in the first chapter (extension without movement)- the division between intimate and external space. And if affects do it, it is in part because they affect us bodily too, it is in this locus that they manage to have their 'moving effects'[6] (Serres, 1995b). This is why, when John Law (2002) analyses the multiple constitution of subject-object positions through story-forms that coexist, coalesce and interfere, he can claim:

> "[The body] is a detector, a finely tuned detector, a detector of narrative diffraction patterns. It is an exquisite and finely-honed instrument which both performs and detects patterns of interference, those places where the peaks peak together and there is extra light. And those ... where there is dark, where there is something wrong, where the energies cancel one another out. Where multiplicity is not reduced". (...) "The body is a site an important site, where subjectivities and interpellations produce effects that are strange and beautiful

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[5] A personal anecdote may serve as illustration. After spending some years abroad, I have repeatedly corroborated a peculiar phenomenon. I sometimes hear somebody making a joke I do not understand, but I laugh nevertheless. Nothing surprising here, you'll complain, you simply pretend to understand so as not to appear stupid and be left aside. The problem is, however, that when this happens I laugh honestly; I mean, I do not feel I am pretending, but I truly feel 'funniness', as if the laughter of the group was enough to make me laugh sincerely without knowing what I was laughing at.

[6] So much so, that we often take information about our emotional state through observation of our own reactions, not only when we are sad because we cry, à la Williams James, but also when we deduce how much we love a person because we miss her a lot. As Freud already said, through emotions we often learn our own position regarding inner and outer events –they are informative. For these arguments, see Hochschild (1983), an original Marxist analysis within social psychology on alienation of emotions and emotion-labour.

–indeed sometimes terrible. And these are effects which might make a difference if were able to attend to their intertextualities" (Law, 2002, p. 59).

It is this constitutive effect that links affects to power, as Spinoza clearly saw, intimately linked to discursive mechanisms of subjection and regulation (Gil, 1999; Hochschild, 1983).

We have seen examples of such alterations when we were discussing the ontological changes that the figure of the boss undergoes according to the situation and the perspective that the group privileges: now a politician, now a bureaucrat, now a scientist. Or when we analysed the modifications that the biocomputing PCI undergoes when interacting with the experimental PCI, or the way newcomers learn to vibrate with crucial experiments. But exactly so well can one notice these ontological modifications in our pathetic transformations during fieldwork. If I felt moved and touched and affected is because, in the most literal sense, I was being moved, touched and affected. Altered not only at a cognitive level –as if all changes were due to an understanding of 'their norms, values, and attitudes'[7]- but also at the level of being.

The relation between affectivity and being connects liminality with the fold. The latter is a moment of folding and unfolding, a moment in which the encounter with something other than our-selves moves us towards a new *becoming*. Liminal moments which preserve what Deleuze calls 'the category of the possible', the opening of the being towards new virtualities (Deleuze, in Domènech, Tirado & Gómez, 2001, p. 35). The fold is the (Foucauldian) notion re-created by Deleuze (1989; see Foucault, 1984a, 1984b) to understand exactly these liminal moments, in which old subjections transform into new ones; processes of subjectification (Foucault, 1976, 1990, 1994a) moments in which an element of an assemblage may connect with another one, moments of intersection of vectors, of crossing of speeds and circuits and heterogeneities:

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[7] In this sense, it is interesting to remind the affective character of which attitudes participated in their first formulation by Thomas & Znaniecki in 1918 (see Pallí & Martínez, 2003).

"The *fold*. This figure refers to processes, relations of movements and rests, capacities to affect and be affected, it defines, then, modes of individuation which do not correspond to a subject and that, therefore, do not require resource to psychological or linguistic metatheories" (Domènech, Tirado & Gómez, 2001, p. 31, our translation).

Liminal moments, then, reveal a different way to conceive subjects –not as a self bounded by a skin separating an interior and an exterior, but as an effect of connectio ns or assemblages; a fold of exteriority (Deleuze, 1989; Foucault, 2000; Rose, 1996). A border itself (Domènech, Tirado & Gómez, 2001), where reflexivity can nevertheless take place as one more fold that allows that the subject creates from its condition of created (Mendiola, 2001; Rose, 1999). The fold offers a way to inhabit the border, avoiding the picture of a world populated by closed entities, with definite limits defining either-or options, self-others that leave no space for ambiguous zones where novelty emerges. If there is no inside or outside, then, we live in the limit, in the outside-line which, through folding, constitutes habitats: life in folds (Serres, 1995a). Fold upon fold upon fold, we inhabit implications and explications.

4. Unfolding virtualities: the doubling

We have said that the oscillation of perspectives performs an exchange of properties, in which each of the parts extends and distributes into the other, as when we see a politician and a scientist intermingling ontologically. When we ourselves are part of the exchange, we gain a feeling of movement and transformation, a kind of doubling (Lévy, 1999), since we participate of both positions: we can take our interlocutor's position –be in her shoes, so to say, while feeling we are also in ours. I can be here and there[8] -neither here nor there, inhabiting the relations between these two

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[8] Can we say, without ambiguities, 'I am here', while we are in Barcelona chatting with colleagues in London preparing a conference for Berlin? Can we say 'I am here' when one works in two different cities at the same time? Can we say 'I am here' when our memory has transported us some years and

positions. I can talk with my self of yesterday, and feel it is both I and not I anymore, just as my future 'I' maintains a partial connection with my present. A challenge to the logic of the excluded third in favour of inclusion (Serres, 1995a)[9]. These type of transactions, which are enabled by the virtualising capacity of language and symbolism, are the basis for our process of constitution, which is always a heterogenesis (Lévy, 1999):

> "The beings, influenced by the dialectic process, double themselves[10]: on the one hand, they keep on being the same; on the other, they are vectors of another being. Therefore, they are not themselves any more, even if their identity is the ground on which they are able to signify. The self and the other place themselves in a spiral in which the interior and the exterior change sides continuously, like in a Moebius' ring (Lévy, 1999, p. 85, our translation).

We are never stuck in one position, since we participate of many others, multiple exchanges constituting us. These are partial substitutions –or, as we have called them before, displacements- thanks to which we can incorporate the point of view of others, signify each other in relation, take share in the other's life. Not as a cognitive apprehension –as if one captured the other's life, her wishes, her thoughts, her dids, everything devoured in a mouthful to be digested and integrated in a huge stomach. Rather, as a partial participation: as much as we are here, we also take place there, in the other: "who am I? The third. The included third. What is the sense of this word? That I am intimately associated to an other and to many more. Yes, I am legion: a countless set of others. Replaceable" (Serres, 1995a, p. 78)[11].

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kilometres away? Or our work and worries are projecting us into a bothersome future? Can we say 'I am here' when we meditate? Can we say 'I am here' when we are suffering together with the hero of the movie? The adverb 'here' loses its force when we take into consideration the fluid character of our identity. "I am here because of my *Horla*, present in the so-called real space for my absences in one hundred places called virtual" (Serres, 1995, p. 78, our translation).

[9] "The subject of the logic obeys these two principles, third excluded or contradiction; but why should personal identity not differ from logic identity? My self is the same, of course, there is something identical in my identity, but there is not only identity, so that I am the same and not the same. Why to confuse *idem* and *ipse*, *self* and *same*? I am neither a geometrical point nor a localised place in a metric space, neither a hard ball in a solid box, nor the steersman in his boat, nor a hard stone to write" (Serres, 1995, p. 78, our translation).

[10] In Spanish, *doblarse*: to double oneself, to unfold in two, where the double is and is not the same.

[11] "To want to explain such a differentiated man like Harry with the puerile division between wolf and man is a desperate try. Harry is not composed of two beings, but of hundreds, of thousands. His life

The ethnographic relation uses precisely this potential of virtualisation. On entering in exchange with others, on being moved by the relation, one is still "here", in one's community, the old self that has nothing in common with these others, but already starts being "there", a new fictional self in a new community constituted by those others that offer hospitality. Constituted, as Serres would put it, with them, in them and through them: "The spirit becomes flesh: vampire or leech, that with her mouth on mine, drinks my life between my lips. This is the return of the parasite" (Serres, 1995a, p. 76; 1982). A constitution of a persona that is born and dies with the relation[12].

Cristina the ethnographer is a self that I partially gained and that I partially lost, me and not me simultaneously, a true fiction that extended, if temporarily, my experiences, my possibilities of being. And it is precisely by exploring and explicating these processes of heterogenesis to which the ethnographic self is subject, that the ethnographer can learn about the other: "As a distinct field of scholarship, anthropology invests itself in the present not only to document cultures but to experience the processes of their making" (Hastrup, 1995, p. 21). Knowledge through a giving-in: the rendering to an other with which to entangle intimately, a mutual becoming other, the acceptance of the gift: new ways of being to extend your self.

Lévy insists that this process of heterogenesis is not an exchange only between humans. Rather, it is a process threaded by the tendencies of subjectification and objectification, which are complementary movements of virtualisation. Lévy (1999, p. 121) defines subjectification as the implication of technological, semiotic and

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(like all men's lives) oscillates not between two poles, for instance, but between countless pairs of poles" (Hesse, Steppenwolf, p. 45).

[12] "The concept of becoming implies that one gives in to an alien reality and allows oneself to change in the process. One is not completely absorbed in the other world, but one is also no longer the same. The change often is so fundamental that it is difficult to see how the fieldworker has any identity with her former self. Fieldwork, therefore, escapes our ordinary historical categories. The space discovered has neither a firm future nor a distinct past, because intentions and memories are transformed as definitions, categories and meanings shift. Participant observation today implies an observation of participation itself (cf. Tedlock 1991); "it is not self-evident that what we participate in is the real life of the others" (Hastrup, 1995, p. 19).

social devices in the individual physical and somatic functioning, whereas objectification would be the mutual implication of subjective acts in the process of constitution of a common world. With these definitions, neither the subject nor the object can be regarded as substances, but as fluctuating nodes of events that intercalate and wrap reciprocally. As Serres (1995a, p. 50ss) wittingly implies when looking for a definition of man (sic) that eliminates superfluous characteristics and preserves intimate properties, the subject's minimal expression already contains the object, a minimal property, a protective fold that no human can do without: Diogenes the Cynical's barrel, Jesus' tunic, the poor's rags and tatters.

Artefacts (things, abilities, concepts) are not only things we use, but things we appropriate. We appropriate them; we make them ours, that is, we make them part of us. We turn an artefact into our belonging. But at the same time, by appropriating the object, we appropriate the relations it carries and displays and we become engaged in them in a particular way –artefacts make us their possession, their belonging. Mutual appropriation, mutual engagement in relationships. This process of heterogenesis combining actualisation and virtualisation is like the move that extension makes possible: we come into being through extensions in the other, this is why "we are always in extension. Indeed, extension is all we are ever 'in'" (Munro, 1996, p. 264)". Which also helps us understand Heidegger esoteric thoughts about being and loving. If our self is never completed, but must be made through the other, then our being is not a project that we can finish on our own (or finish at all), we are always co-constituting each other's projects, always problematising each other's stabilities to create new ways of being[13]. For this author, "(e)xistence is then 'letting-be and making-be'... something other than oneself" (Duque, 2002, p. 55).

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[13] The extent to which we are constituted by the possibilities others give us is manifest in those moments in which we lose somebody we love: when we lose somebody, we also lose the possibilities of being that this person used to give us. This person takes with her a part of me that could only come into existence thanks to her. Our being-together (*Miteinandersein*) loses existence, a piece of world becomes indifferent to us. We regret not only the loss of the other, but also the loss of the self that we, through them, had been.

From the moment we imagine object and subject as partially connected we can leave aside an antagonistic vision of their relation. Object and subject do not oppose each other, but each is part of the conditions of possibility of the other. Without subjects it would not make sense to talk of objects, but simultaneously we can emerge as subjects precisely because objects sustain us. Subject –thrown under; object –minimal property: each works as the ground where the other can set foot and stand. Object and subject go hand in hand. The subject is *of* the object; the object is *of* the subject –a mutual belonging and a mutual generation:

> This 'of' (...) is the last root of all language and of all generation, simultaneously a linguistic form (a 'case') and a becoming which appropriates and propitiates the *conjunction* of what is counterpoised, and, therefore, antithetically, creatively interpenetrated: this genitive (not Being, nor man, but the genetic co-belonging of both) is the *Ereignis* [event]" (Duque, 2002, p. 90, our translation, original emphasis).

5. Ex, metamorphosis and other transformations

> "(T)he caracola [see snail] encloses the see and it so becomes an emblem of universal life, of its perpetual dying and rebirthing. At the same time, the see snail does not contain but air, it is nothing. This duality ... transforms it simultaneously in a crock and a ritual object. (...) The see snail is the point of intersection of all the lines of force and the place of their metamorphosis. It is in itself metamorphosis"
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> (Paz, 1971, p. 56, 57).

The flickering between perspectives is experienced as a kind of movement or journey (Strathern, 1991, p. 108), a kind of being moved that we can experience as if it was a two-way displacement. Despite efforts of evolutionism, we do not feel we follow a straight line[14], but have often the feeling of 'return': we imagine ourselves coming back to the house where we were born, to the streets that saw us playing, to our

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[14] Although we will not touch upon such a topic here, our reader can find a discussion about other

school friends of our childhood, to the first love we have not quite overcome or to the community that sent us in a mission. But as anybody who has seriously attempted one of these returns must painfully discover, such come backs never take place. Hence, those feelings of displacement when one does not feel in place –a place one should recognise but one does not, when the house seems inhabited by disquieting absences, and it does not separate interior and exterior anymore (Serres, 1995a, p. 32).

Experience is never a return-journey, because there is no resolution into a central figure, or no position that remains fixed, as if an identity-core remained in spite of extensions. Each extension towards otherness changes our being in a radical way – that is, not even 'roots' are free from alteration. Thus, the circle never closes. At occasions we live ourselves moving towards the future, at others we image the movement backwards towards our past, but we always arrive somewhere else. What remains is this circulation or movement of which we are part: "So movement is never a travelling to and from a core self, but always movement around the 'surplus' of a rim: we move endlessly from 'figure' to 'figure', not so much inchoate as undecided and undecidable" (Munro, 1996, p. 270-1).

If circular or oscillating, this is not a movement that closes upon itself. In the first chapter we have encountered several authors helping us imagine this openness: Bachelard's spiralled surface that vibrates with an attuning of its two sides –two which is nevertheless one; Heidegger's openness through which the potentiality of Being actualises or sets foot in the world; Lévy's Moebius ring that swaps sides; Blanchot's helicoid thought turning around an impossible, absent centre of exteriority; Deleuze's Foucauldian folds, entangling labyrinths of exchange, our no-return journeys which nevertheless pretend again and again to come back home...

All these images suggest the impossibility of a closed circularity out of which the self would feed and sets in its place an openness that crosses radically the being; a being moved that prevents the self from exhausting in itself, and that pushes it towards the

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nonlinear ways to conceive time in Serres & Latour (1995).

outside; an exposure that extends the self through intensity and metamorphosis; a continuous game of implication and explication, of folding and unfolding, of hiding and making explicit, of virtualisation and actualisation, of subjectification and objectification, of extricating and entangling, of cutting and bridging. A nonlinear movement of exit or expedition exhibited in the ex of experience, of exile, of existence, in "the phenomenology of those words that start with ex" (Bachelard, 1994)[15]:

> "nothing except at a distance from itself, this is why experience, existence and ecstasy are expressed by the same word *exposure*, which says distance to equivalence" (Serres, 1997, p. 28, original emphasis).

Now, we are not making a moral, ethical claim, of the type 'we have to open ourselves to the other', as if this was an act of generosity, a conscious, redemptory self that gives in to the other to pay a debt or correct an injustice (see the last paragraphs of Law, 1997). Indeed, the debt has been accumulated (Lee & Stenner, 1999), and we must do our best to repair unfair relations, but my point here is different, it tries to emphasise that the self, understood as effect (Foucault, 1976, 1984a, 1990, 1994a), is always already open, like a fold, like a spiral:

> "the self, porous, blended, accumulates presence and absence, it connects and saws what is close and what is far, the real and the virtual, it separates and makes come near the *hors* and the *là*" (Serres, 1995a, p.80, our translation).

Imagine a caracola –a spiral, a see snail. Fold upon fold upon fold, the line turns and turns, but never returns to "the absence of a central point" from which it started (Laporte, 2001). As if moved by an affection, the line suffers small deviations in trajectory, the clinamen, enough to avoid the closure of circularity. Always heading to the outside, moving without pause in one single direction: never to one-self but to the exterior. But not an exterior that is foreign to us, but an exterior that also constitutes us. Follow the spiral: the more you think you approach the centre, the more emptiness you find. Fold upon fold upon fold, just a surface separating and connecting intimate

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[15] To which we can add, among many, the Spanish words e*xtranjero* (stranger, foreinger), *extraño*

and exterior space, an interior and exterior which are just the same, made different by a partial connection. Thus, thought can be grasped in reference to the eternal movement of the infinite questioning. As if moving in spiral or helix, thought is always already leaving itself, always already an exit, without ever coming back, always out of place, either lost in the past that is still to come or the future that just passed –"hope of a past, accomplished cycle of a future"[16].

6. Taking part

Liminal transformations happen at those moments and places of connection. Those moments when parts come together, creating novelty, those moments which philosophers such as Whitehead and Deleuze call 'events'. There have been many attempts to understand these connections, and Cooper (1995) summarises some for us: assemblages, articulations, heterogeneous networks, partial connections, fractals, cyborgs. Moments when parts come together, without summing up into a totality. A multiplicity (which is not a multiple of one), a mixture of parts that intermingle, arranged in such a way that one can say that they both join and separate. And if their connection gives birth to a novelty, it is not simply because parts come next to each other, but because this connection brings out "the alternation or otherness that inheres within their relationships" (Cooper, 1995, p. 4). It is a matter of mutual relatedness, a becoming.

Participation in a collective, to take part in it, then, has nothing to do with division or repartition, but with this shift or migration of the 'I' by way of intimate exchange. It is a matter of abandoning 'my individual', 'my being' in a circulating quasi-object weaving relations –"the abandonment of the 'I''s on the tissue of relations". Or, as

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(strange, stranger) and *extrañar* (to find strange or surprised and significantly, to miss).
[16] "Given a past, given a future, without anything that allows the move from one to the other, so that the demarcation line would demarcate them even more because it remains invisible: hope of a past, accomplished cycle of a future. Of time there would only remain this line to cross, always-already crossed, uncrossable nevertheless and, not positioned regarding 'me'. The impossibility to place this line is perhaps the only we could denominate 'present'" (Blanchot, *Le pas au-delà*, quoted in Herrera,

Serres also puts it, this is the renunciation of the being in favour of the relation – a "transubstantiation of being into relation" (Serres, 1982, p.228). Being in relation, that is, being in the middle, being in connection. Not being at the beginning (the cause), not being at the end (the effect), but mediating in-between: being in the muddle.

This mixture or muddle emerges out of a radical intimacy between humans and nonhumans, an intermingling, as Mauss (2000, p. 26) put it, of "things, values, contracts and men":

> "Souls are mixed with things; things with souls. Lives are mingled together, and this is how, among persons and things so intermingled, each emerges from their own sphere and mixes together. This is precisely what contract and exchange are" (id, p. 20).

And now, after Serres, we can read Mauss' words 'contract and exchange' with more hues: contract and exchange not simply as an accord among people, but also as Pascal's 'cord' or Leibniz's 'vinculum' (Serres, 1997, p. 88), the cord constituted by the passing on of the quasi-object, quasi-subject, giving substance to the social bond - a different type of contract[17]. An accord which does not necessarily involve the conscious moment of agreeing, but the vibrating together –hence, Heidegger's *Gestimmtsein* (Duque, 2001)[18], and that gives way to a mutual constitution of all elements participating in assemblages.

2001, p. 54, our translation).

[17] There is also another side to the notion of contract, linked to the operation of capture by the State, "a juridical expression" of a "proceeding of subjectification, the outcome of which is subjection. And the contrast must be pushed to the extreme; in other words, it is no longer concluded between two people but between self and self, within the same person –*Ich=Ich*- as subjected and sovereign. The extreme perversion of the contract, reinstating the purest of knots. The knot, bond, capture.... A *nexum*" (Deleuze & Guattari, 1988, p. 460, 461).

[18] The word *gestimmt* (to be in tune), according to the unfolding suggested by one of our more poetic explorers of etymological treasure-troves (Duque, 2001, p. 103-4), derives from *Stimme*, that can be understood as 'voice' in the political sense, that is, as metonymy for 'vote'. From *Stimme* come words related to 'agreement', all of which were then figuratively projected to the world of musical instruments. *Gestimmt sein* is to be in accordance, to vibrate together, to be in tune. Not by chance, in Spanish the word *voz* (voice) is also to be found in words like *convocar* –summon, gather, convoke. Summons where 'voices' had to be heard and united happened in the assembly or thing, from where *Ding* (thing) comes from.

And 'participate' is probably quite a well chosen word here. Interestingly enough, when Mauss describes for us the intermingling that contracts and exchange involve, he adds, in a footnote, that Lévy-Bruhl used to call this mixture 'participation' [19], a name having origin in 'confusion' and 'muddle'. One is immediately reminded of Serres' tohuwabohu or Cooper's middle as muddle, the inextricably entwined.... Participation, then, is to take part in this intermingling, an intermingling where things and souls mix –where people and things become each other's parts (Mauss, 2000), making appropriation and mutual constitution possible (Ingold, 1986). We find Ph.D. students and bosses united in a special articulation which is neither one nor two, but something else. Ph.D. students attaching groups with their being in-between. Proteins, students, bosses, papers, making the 'we' circulate around, allowing small interruptions of the collective in the shape of an 'I' which needs to be passed on next, lest one stops the flow. That is, which we must pass if we want to take part in this movement: to be a part, to participate, to be a member.

Belonging, then, does not necessary refer to being included in an entity that encompasses us completely, for this is not the only way to offer shelter or a supporting ground. Belonging can be understood, more generally, as being intermingled -'inextricably entwined'- with something other, to take part in something other that simultaneously sustains us –a belonging together (Duque, 2002; Gasché, 1999). Belonging, to be in longing, longing to be: an intimate entanglement with other entities which constitutes us anew as part of each other –as mutual belongings, so to speak, as parts which do not need to resolve in a whole, but that are the condition of possibility for each other, reciprocating, as it were, gifts of existence.

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[19] "Here one might use the term normally employed by Lévy-Bruhl: 'participation'. But the term has in fact its origin in confusion and muddle, and especially in legal identifications and communal procedures of the kind that we have now to describe" (Mauss, 2000, p. 104, note 54).

Performing the IBF: the Fondue

> "This coincidence of opposite processes and notions in
> a single representation characterizes the peculiar unity
> of the liminal: that which is neither this nor that, and yet
> is both"
>
> (Turner, 1967, p. 99).

> "For the door is an entire cosmos of the Half-open (....)
> If one were to give an account of all the doors one has
> closed and opened, of all the doors one would like to re-
> open, one would have to tell the story of one's entire
> life. But is he who opens a door and he who closes it the
> same being? ... there are two 'beings' in a door ... And
> then, onto what, toward what, do doors open?"
>
> (Bachelard, 1994, p. 222, 224).

Christmas is just round the corner; one can feel it in the air. Not that people are mesmerised by the Christmas spirit, but they do seem seized by a peculiar excitement. It is not rare to find small groups of Ph.D. students secretly whispering at the bench, with naughty eyes and malicious smiles on their faces. While work goes on as usual, at some moments a couple of labs are empty, and their members (head included), hide somewhere beyond curious looks articulating conspiracies. Our Psycho is slightly disoriented, not daring to ask, even though she believes that this time this sudden arousal has nothing to do with her –she's clean: mouth shut, eyes open, discreet flights, epistemic concerns in the pocket. Her wandering presence has been absorbed in the normal IBF rhythm, no cause for surprise any more. And still, she has caught a couple of amused looks directed at her that make her suspicious. What is going on there? Until she finds the answer... in a notice hanging at the coffee machine: the yearly Christmas party is being organised.

Members of the IBF meet and eat together in several dinners, lunches and other meetings during the year, most of them organised spontaneously. They also celebrate each thesis with a nibbling lunch in the corridor of the IBF. Other events are repeated regularly every year, as is the case with the "piggy lunch", an out-door lunch (a pick

nick) that takes place in August, (in Spain the typical month of holidays) among those who nevertheless come to work. But there is no doubt that the most expected events and celebrations of all is "the Fondue", the IBF's Christmas party. The psycholo gists had often heard of it before, it is impossible to spend some time in the IBF and not know about it. During the whole year anecdotes of the previous fondues circulate in conversations, and some of them have turned into mythical accounts told by veteran members thus bonding in a unique story old colleagues -who have already some fondues on their shoulders- and newcomers -who still have to live their first one.

So, the big party has finally arrived -together with the end of the year and the last moments of our psychologists' ethnography. Psycho reads the note advertising the Fondue and smiles. After hearing about it for so long she is curious. But she will not be for long, because at the end of the public note there is an invitation for them: psychologists are welcome! As long as they participate in the even as others do. This does not mean that they need to collaborate with the food preparations, though: organised every year by two different Ph.D. students and postdocs, whose names are announced in the previous Fondue, the rest of the members just have to pay "their whack" towards the Christmas bash. But the party is more than just this. Every laboratory must prepare a kind of representation on a proposed theme, which is recorded in video and projected after lunch. And here the organisers' instructions are clear –and adamant: psychologists are "strongly recommended" to take part in the video session, needless to say with their own production!!

The day finally comes; our psychologists are excited, almost more than the rest, who keep on working until the last minute. But soon something happens which marks the day as special. Old members of the IBF keep on arriving in order to join the rest in their celebration. Some of them come from nearby, others come from abroad; some still work in science, others have already abandoned the scientific career, mostly out of necessity and not conviction. They all gather around the coffee machine, great each other, repeat old jokes and remember old times.

The IBF shelters an interesting mixture, then: present members who know they will remain part of the IBF for a long time; members who already know that their membership will some day expire; ex-members who had to leave, either in order to secure their belonging to science in some other laboratory (even in some other country), or to abandon research definitely. Thus, this meeting brings two extreme phases in contact: those who have just arrived at the IBF with those who had to leave it; those who start their scientific career with gloomy perspectives and still have to face decisions, with those who have already made some decisions so that they can at least have gloomy perspectives. Psycho looks at them all. They all seem to feel at home, entering through the door as members in their own right.

Since she arrived Psycho has heard stories going the rounds about old members. Some of them were said to be unbelievably good and skilful; others made such a funny mistake that it is still told among laughter to newcomers so that they learn to avoid it. For newcomers (and she secretly almost includes herself in this group now) it is somehow surprising to see how some of the myths, legends and ghosts of the IBF gain status of reality, even if only for one day: characters which until now have remained narrative incarnate now in real scientists, most of which still work in science. The past becomes present bringing hope for the future, strengthening collective bonds that spread in time in spite of limitations. Recollections that re-member, memories that reconstitute the collective. *Recordatio* as a re-enactment of the cord that bonds us together.

But the psychologists are also told of absences. Of people who could not come this year, of people who cannot come any more. Names that contributed to the constitution of this lab, and that are somehow still attached to it by memories, stories and anecdotes. Absences which are nevertheless present and that sustain the actuality of those who are now there. Members who are part and not, absences that remain present in memory.

These vague reflections are soon interrupted, because the organisers give the signal and people slowly go downstairs (including workaholics who reluctantly abandon the bench, followed at last by the heads) to the corridor-turned-dinning room full with tables lavishly supplied, where a triple fondue -cheese, meat, chocolate- is awaiting them. They all stand around the improvised tables, ready to start the collective meal. Sharing food from the same plate, as it were, people gather around the kettle where cheese and oil are boiling, taking turns to both talk and dunk the bread and meat in this concoction –symbolically passing each other the turn: "after you, please; after you, thank you".

Our psychologists are busy trying not to lose their piece of bread in the cheese, lest they are punished as Romans were in Switzerland [1]: lashed for cutting the flow. Food and word turning around and making them part of the same endeavour. Members of different laboratories within the institute, who do not usually spend much time together, meet in one space and time, partially dissolving boundaries between groups: (a performance of) communion at the table. The Fondue binds all laboratories and all members, creating a feeling of connection beyond the belonging to one particular group. It performs with intensity the perception that the IBF as institution is a multiplicity, multiple groups attached to each other in a rather peaceful, sometimes tense and always creative way. Groups translating each other, tolerating each other, sustaining each other.

Food and conversation is after a while followed by entertainment: video session accompanied by laughter and more laughter. Each group, psychologists included, presents a recording about a funny episode, revolving around science or IBF topics, which have the added bonus of showing people fancy-dressed. For our psychologists, it is impressive to see the level of humour, implication and lack of inhibition with which members participate in this elaboration (but they are not left behind, you see, their video on cannibals and sorcery causes a sensation). In all these videos scientists

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[1] See *Astérix chez les Helvetes*.

transform laboratories into landscapes inhabited by fairy tales, sleeping beauties, wild women, witches, absent-minded professors...

Like celebrations such as carnivals (Bajtin, 1987), this event manages to recreate a world parallel to the every day world of the institution. Whereas during weekdays the IBF is dedicated to work in scientific tasks, the Fondue permits that this same space shelters entertainment and merry; next to a 'serious world' the Fondue opens up a 'world of laughter': jokes and mocking go on the whole daylong. Arguments mock science and topics that are usually invested with transcendence –such as narratives of vocation and effort, of implication and discovery. If hypothesis, objectivity and impartiality fill their justifications of the daily work, money, chance, prejudices and interests feature without censorship in their videos. This parody performs a kind of degradation. They choose topics whose nature is thought to lay in the world of the abstract, the spiritual and ideal, and root them in a more banal, material world –they bring them 'down to earth', in a rejection of the division between the abstract and the material, an admission of mixture. It is not by chance that jokes, anecdotes and funny performances are those moments in which it comes easier for members of the IBF to admit the complexity and hybridism of science[2].

They all have not stopped laughing since the party started, and the videos add to general hilarity. The laughter arisen by the videos is quite ambivalent: happy and joyful, but simultaneously mocking and sarcastic, it is a laughter that affirms and denies at the same time. Still, this is not the laughter of the easy criticism that makes fun from outside, but the ambivalent laughter of those who laugh of something they know themselves part of. This is the ambivalence proper of parodies, since this is what they do, they parody themselves: scientists all year long, clowns for a day: they reconstruct their world but upside down. Nevertheless, 'upside down' does not mean 'falsely'. This world is not an impossible world in which they all engage knowing

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[2] In many works it has been found that humour is an important ingredient of many scientific laboratories (i.e., Gilbert & Mulkay, 1994, last chapter; Mulkay & Gilbert, 1982; Editorial in Science, 1990). It has been interpreted that humour emerges in moments of tension between interpretative repertoires: the purified narrative clashes against hybridism, and this contradiction is expressed through laughter.

they are acting, none of the people taking part in the Fondue feel they are feigning. On the contrary, it is as if the day allowed the pure expression of affinity feelings. Even the psychologists allow themselves to be carried away, enjoying the feeling that they are somehow a part of them.

But just for a while. Tomorrow things will go 'back to normal'. This is not the atmosphere the institution recreates every day, and they all know it. This event, then, is placed on the overlapping of performance and life, a mixture between game and veracity, neither 'theatre' ('acting') nor 'reality' ('real life'). Psychologists must admit that this nuance is not unimportant; people do not witness the day as spectators (or actors) of a representation; neither do they let the day go by unreflectively as if nothing special was happening. On the contrary, reflection and performance seem to go hand in hand. Actually, the rest of the year they talk about the Fondue as something very real, and, at the same time, very far away from the daily, regular activities. People live and experience the extraordinary –they participate, they take part, they *are* part. The extraordinary appears as a dimension which does not substitute daily life, but as a split that infiltrates within and accompanies it –a liminal event. Yes, the psychologists know that tomorrow they may not have much to do with the IBF any more, but today, even if only for a while, they take part in these others.

They party from lunch until dark: food, drink, entertainment, conversations, jokes, dance... Almost everything seems to take place that day; even 'small mischief' seems to be more or less allowed. The psychologists are told about water wars flooding the whole building and of fire extinguishers putting an abrupt end to the party; of some naughtiness tolerated by the heads of the institute and of some prohibited –and even sanctioned. But they can witness it by themselves: the atmosphere of the party allows unusual things to take place. Implicit norms of weekdays soften or become inappropriate; contact among members is more familiar and wider.

But, above all, members of the IBF emphasise that "anything may happen" because that day "the hierarchy is broken and you leave everything aside" (EI11-28). Not only

members of different groups but also Ph.D. students, postdocs seniors and heads mix in the same space-time. Heads are said to "forget everything and participate"; they sacrifice temporally the distance associated to their status, and accept, up to a certain point, to have their authority challenged. Jokes cross and blur the pyramid, bonding them all in a common laughter that erases positions -laughter that does not allow superiority to be established. In this day everything seems directed to the celebration of sameness; at the end of the day, they are all scientists, united in their always incomplete search for knowledge.

She feels slightly emotional, kind of moved and this would make her feel a bit embarrassed had she not realised that this is happening more or less to all of them. As if they all felt attuned, played by a same accord -altered by the company. Stories about how this party changes people and their relation to the group abound. They say that "the Fondue sometimes allows the alter ego of people to emerge" (EI11-27) and helps people experience other sides of their personality: "Some people, who would only work and work, discovered thanks to the Fondue that they had more to offer to others at a personal level... They felt more integrated in society, they became more 'human'" (EI11-29,30). As if reinforcing Durkheimian pacts, people feel in them the renewal of their collective, of which they are made a part. A collective that emerges in the fest as a Bataillian thing-for-itself or as a Girardian mirror point where to read and create themselves.

Affected, then, by the community and its centripetal forces. This is the kind side: the reinforcement of attachments. But this is also a configuration that brings their selves more in line with collective requirements. Through these ceremonies members are impregnated and configured by the ethos of the community; they are made to feel what is expected from them -what actions, thoughts and feelings would be proper or improper in the community. This is why 'integration' should not be read simply through its positive undertones (as in happy and together ever after), but also as pressure to conform. After all, mischief is allowed up to a certain extent, and people

who overdo it will be called to order. This party is one of those Turnerian symbolic events that convert the obligatory into the desirable.

But now that she thinks about it, the image of integration does not quite fit here, since scientists do not feel totally encompassed by the IBF. First, because their being 'attached together' does not demand reductions to the same. Partially united by their knowledge practices and their will to pose questions, they contribute -each in their own way- to invigorate the IBF, without for all that having to conform to fixed patterns. No identity is needed to create and maintain bonds. Integration is a bad choice also for a second reason; the institute is just one of their belongings, and not even one that lasts forever. They arrive, they dwell, they leave, and the group has grown used to seeing temporary members passing by.

But again, this precariousness is not detrimental: students doing practices, newcomers, stable staff and old members: they all feel they belong as soon as they are there. As if the impossibility to belong completely had as an interesting counterpart the generous offer of a partial acceptance of all those who attach to the group. Everything happens as if there was no symbolic boundary being crossed in order to affirm belonging, no criteria one needs to comply with to be accepted. Just attachments and detachments altering the physiognomy of the IBF and of its members. Pleasing encounters that invigorate mutual conatus.

As if all angels were coming back to heaven for Christmas, IBF members gather in the building –official members, Ph.D. students sent around, old members, some collaborators: people spread throughout the world in the complex net of science. By joining scientists from all over the place, the IBF extends its links beyond its physical borders, performing connections to a wider collective, the scientific community, which extends beyond the space-time limits of a particular institution.

This movement towards a 'home' that it is not quite so actualises attachments in a way that makes the IBF pulsate. Yesterday the IBF was extended by a network of

collaborations and contacts, today all the messengers have come home, concentrated in a point -the local IBF magnifies. Tomorrow they will be gone again, the IBF will spread extensions configuring international science and the local institute will diminish; the house will become quiet again, detachments will cut connections, some of which will have to wait for another special occasion to be renovated. A movement of oscillation that never quite returns to the same image, a pulsation out of which two different images alternate, in tension but still supporting each other (magnification followed by diminishment); expansion, contraction; locality that turns global, globality that becomes localised; an actualisation in the here and now of memberships which are virtualised in space and time.

Observing this transit come and go, our Psycho can't help wondering about her future. She must soon leave the IBF behind too. Will she ever come back? As a scientist or as an outsider? From nearby or from abroad? But she is surely not the only one struggling with these questions –so must do the other Ph.D. students, for this meeting produces its own symbolism. On the one hand, this encounter makes them all feel the maintenance and continuity of belonging. Even those people who left the IBF years ago can come back and be welcomed as part of it (some seem even to feel more at home than those who have an institutional link with it). Attachments remain –once there, forever there- and they enjoy a type of membership which is not easy to efface, whose resonance lasts longer than official links.

But on the other hand, these people that come back for a day do not quite belong there any more, which reminds them all of the impossibility to remain in this institution, and in science in general; belonging will come to an end, only in some exceptional cases will some of them manage to turn it into an uninterrupted attachment. Meeting old members has this sweet-and-sour flavour: an ambivalent feeling mixing permanence and disappearance –an affirmation of life and the creative possibilities it offers but also a reminder of death, a destructive moment where opportunities fade away. All mixed with the fear, almost certainty for some, that this moment can (will) happen at some point; their personal growth can be cut, their

scientific career may come to a halt. Thus this encounter makes the ambiguity of belonging clear: belonging is reaffirmed and declared as impossible at the same time.

There are some melancholic tones in this party, but Psycho does not quite know whether the sad tones are in the air, or she's being particularly sentimental because she is leaving. The truth is, though, that generally speaking, merry wins the battle, as if the duality between life and death was won beforehand. She has read somewhere that Bataille said that the party performs the destruction of the community, but it cancels this danger just in time. It seems so in this case; the Fondue proclaims the perpetuation of belonging to science in a ritual which affirms its continuity in different ways. For instance, in its relation with a cyclic time. The Fondue can be seen as a celebration of the cyclic character of belonging, resolved in favour of continuity: groups grow and diminish, old members go, new ones come in. But the IBF, renovated, remains. Individual destinies lose significance in front of this awareness: the meeting dissolves the individuality; it is the continuity of the collective that matters. The constant giving in and passing on the 'I' so that the 'we' is never cut.

But the Fondue is never caught in a closed circle of reproduction, in a cyclic repetition of the same. The institution as such, with all its up and downs, starts cycles again and again, but never quite the same. The IBF is in movement, in a spiralling movement that creates a feeling of return which nevertheless never takes place. Its trajectory never steps twice on the same line, on the same self, but progression is not linear (and maybe it is not progression at all). This open spiral is perceived by many, who reject a static description of the centre. As an interviewee told her once, "the IBF is not the IBF, I mean, as an institution, as walls, it has no meaning to me. For me the IBF is its people. And members evolve –both, old members evolve and new members come. … In my opinion the IBF is not static, it is something in movement" (EI03-12).

Everything happens as if the IBF had a double message –transformative like a ritual, confirmatory like a ceremony[3]. On the one hand it marks and allows a change, a continuous and incomplete metamorphosis of the IBF, always in process, a renovation. On the other, it allows this thing-in-change to behave as a state, to become a shelter within science, for some in a stable way, for some more temporarily. This duality does not go unnoticed by those who party, as if they all participated in the performance of a liminal IBF: caught in a difficult balance between the IBF that passes away and the IBF that remains; between the IBF in movement as extitution, the settled IBF as institution. Simultaneously a problematisation of their being together and a solution to remain connected, this party enacts the unstable relation between the virtual and the actual.

The Fondue opens up an ambiguous space, a split between the every-day practices and the extraordinary, a split that allows the experience of ambivalence. A disturbing laughter suspends hierarchy -even though they all know hierarchy remains- and performs equality -even though they know inequality remains. The party performs unity to counteract the awareness of internal division, and it also performs division from every day life in order to recreate the continuity of an institution. An event where members and non-members can celebrate together partial and incomplete belonging, where the IBF renovates itself, accepting its multiplicity and celebrating its belonging to the extended scientific community.

The psychologists can understand much better now why members of the IBF imbue this Christmas party with such significance (so much so, that almost all of them mentioned it as a special feature of the laboratory). A festival of ambivalence, the Fondue opens a space or state of reflection, in which the IBF –its values, its identity, its definitions- are negotiated and performed: they come into being, as if they all gathered to play for themselves how they are and how they want to be. Psycho smiles while she remembers what one senior said: "the Fondue is the IBF" (EI02-38).

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[3] Turner (1994, p. 95).

Indeed, this party opens space not so much for reproduction as for regeneratio n, a renewal of their links.

After a while, those who still want to party all night long leave the IBF to go somewhere else. Our Psycho goes along, but first she must pack her things. The end of the Fondue is also the end of their fieldwork, even though she knows she will have to come back again after Christmas to complete some details and say bye-bye properly –starting to suspect, but not quite, that this will take her some more days than expected. She goes to the corner where her things lie and picks up her notebook, after sweeping away the crumbs of tenderness spread all over. Unobservantly she frowns, but hastens to correct this gesture with a soft smile, brushing her hair back with her hand, as if wanting to dispel an unspoken grief.

"But I'll surely come back, maybe next Fondue". Or maybe the next one or the next one, she cannot know, and neither can we. But she shows convincement, as if trying to justify this weird feeling that she does not go for good, that she does not go completely. After all, thr ough her thesis at least, she has somehow become one of those attachments through which the IBF performs itself, and the IBF has become intimately entwined with her professional self –a belonging.

She walks to the door, where others are already waiting –the night is young. As if approaching Alice's mirror, she crosses the threshold, knowing she will step in another world, in another self –with a sensation she cannot quite describe: as if she was accompanied by a ghostly absence in herself, or even as if she was a ghostly presence to herself. She is tempted to look over her shoulder, but she forces herself not to. She does not need to turn around to know that behind her Psycho remains, looking at her while she goes away.

She ignores how often members of the IBF will remember that 'once upon a time' social psychologists stayed with them and were moved by them extravagantly –not very often, she guesses. She does not know whether the psychologists' ghostly

presence will melt into nothingness or will become one of those absences around which jokes are woven in their yearly parties. But she can already feel the anticipation of a sorrow floating in the air, a kind of echo of the future that brings notes of nostalgia. As if she could already advance that she will miss not only those scientists that have affected her beyond expectation, not only this IBF of which she cannot but wish to be a part, but also the Psycho that she, through them, partially became. Gifts of existence, a no return journey indeed.

"Yo soy uno y muchos y tampoco sé quien soy. Sólo sé que ayer volví a caminar, repetí el paseo del jueves. (...) Ficciones que brincan y se expanden más allá de mí y de ellos, más allá incluso de la Arabia feliz y de lo que fue mi vida, por la que yo esta noche, sin razón aparente, lloro como se debería llorar al final de todos los libros, de todas las historias que vamos a abandonar" (Vila-matas, *Recuerdos inventados*)[4].

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[4] "I am one and many and neither do I know who I am. I only know that yesterday I repeated the walk of Thursday. (…) Fictions that jump and expand beyond me and beyond them, even beyond the happy Arabia and what my life had been, a life for which tonight, without apparent reason, I cry as one should cry at the end of all books, of all stories we are about to abandon" (Vila-matas, *Recuerdos inventados*).

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