Mechanismic explanation in ecology

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MECHANISMIC EXPLANATION IN ECOLOGY

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DEDICATION

I lovingly dedicate this dissertation to my wife, Carolina, without whose selfless support and (utterly unscientific) understanding this dissertation (and I myself) would have never took form, and to my children, Jerónimo, Manuel, and Joaquín, for making me a better human being.

ABSTRACT

Ecology is a science of practical and theoretical importance that has recently begun to appeal to professional philosophers. Yet, work on the philosophical foundations of ecology, particularly on its explanatory practices, is still scarce, even though ecologists perceive the debate on ecological explanation as an important one. In this dissertation, I contrast the main theses of three different philosophical projects that attempt to account for scientific explanation in terms of mechanisms descriptions with two cases of ecological explanation based on mechanisms, as ecologists understand the term: the mechanisms of ecological facilitation and competition. The examples I study come from the subfield of ecological succession, though both facilitation and competition are widespread along the whole of ecology. Based on my analysis of those cases I argue that those projects have contributed important elements to the ontology and epistemology of scientific explanation, but that there is still room for improvement towards an adequate characterization of the precise nature of ecological mechanisms and mechanismic explanation in ecology. Following the lead of previous work by systemist philosopher Mario Bunge, I suggest that ecological mechanisms are specific processes in systems, and that, even though they may take different forms, mechanismic explanations consist in descriptions of those processes in the context of a description of the system of interest.

Key words: ecology, scientific explanation, mechanistic explanation, mechanismic explanation, mechanism, new mechanistic philosophy, contemporary mechanismic philosophy, philosophy of ecology, philosophy of science.

RESUMEN

La ecología es una ciencia importante, tanto desde el punto de vista práctico como desde el teórico, que recientemente a comenzado a atraer la atención de los filósofos profesionales. Con todo, la investigación sobre los fundamentos filosóficos de la ecología, en particular sobre sus prácticas explicativas, está aún poco desarrollada; y ello pese a que los propios ecólogos perciben que el debate sobre la explicación ecológica es importante. En esta tesis doctoral comparo las principales tesis ontológicas y epistemológicas de tres proyectos filosóficos que ofrecen un análisis de la explicación científica en términos de mecanismos, con la descripción de dos casos de explicación en ecología basados en mecanismos, tal como los entienden los ecólogos, los mecanismos de facilitación y la competencia ecológicas. Los ejemplos que analizo aquí provienen del campo de la sucesión ecológica, aunque tanto la facilitación como la competencia son interacciones muy extendidas en todo el ámbito de la ecología. Sobre la base de mi análisis, sostengo que si bien las contribuciones epistemológicas que los proyectos filosóficos estudiados han realizado al debate de la explicación científica son importantes, pero que aún hay mucho espacio para mejorar la caracterización de la naturaleza de los mecanismos ecológicos y de la explicación mecanísmica en ecología. Basado en el trabajo previo del filósofo sistemista Mario Bunge, propongo que los mecanismos ecológicos son procesos específicos que ocurren en sistemas y que las explicaciones mecanísmicas en ecología pueden asumir diversas formas, pero que consisten en descripciones de esos procesos en el marco de la descripción más general del sistema de interés.

Palabras clave: ecología, explicación científica, explicación mecanísmica, mecanismo, nueva filosofía mecanicista, filosofía mecanísmica

contemporánea, filosofía de la ecología, filosofía de la ciencia.

CONTENTS

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My former colleague ecologist and always-dear friend Luis Marone taught me a fascinating perspective on the science of ecology and encouraged me to pursue my philosophical studies with the rigor of scientific thinking. To him and to the members of ECODES I am grateful for intellectual partnership, as well as for endless passionate discussions on the philosophical issues raised by ecology in the lab and the field, sometimes with *mate*, other times with a golden glass of *torrontito* as a further source of inspiration.

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Last but certainly not least, I want to thank Mario Bunge for his generous guidance and inspiration through the tempestuous ocean of philosophical ideas. I hope I have learned.

Barcelona, November the 30th, 2015.

PART I

MECHANISMS IN ECOLOGY

The subject before me is so inexhaustible and so varied, that I fear either to fall into the superficiality of the encyclopedist, or to weary the mind of my reader by aphorisms consisting of mere generalities clothed in dry and dogmatical forms.

Alexander von Humboldt, *Kosmos* (1945).

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INTRODUCTION

Motivation—My justification for the subject of my dissertation project is threefold: (a) the importance of ecological science for society, (b) the importance of a philosophical analysis of ecological practices and (c) the recent philosophical interest in mechanisms.

Importance of ecology for society— In the last two and a half decades, ecology and other environmental sciences have come to the fore in the public opinion and the media. A similar phenomenon took place for the first time in the 1960s and 1970s, during a period that has aptly called the "Environmental Age" by a historian of ecology (Hagen 2008) because of the importance that environmental issues had acquired, especially in American culture.

In our days, public interest in environmental issues has taken the form of a widespread concern over the impact of climate change and global warming on natural systems and, especially, its consequences on human societies (e.g., Fagan 2001, 2004; Diamond 2005, Gore 2006, Hagen 2008). Naturally enough, when environmental issues reach societal concern, decision makers turn to environmental sciences and technologies, and to ecology among them, in search of understanding, counseling, and efficient practical responses.

However, the simplified and sometimes apocalyptic tone with which these matters are usually treated in political debates, science popularizations and the media contrasts with the sober tone of scientific reports. The sweeping generalizations of the former differ strikingly from the cautious statements often associated to the high theoretical and methodological complexity of the problems tackled by ecology and the other cognitive enterprises that are supposed to provide support to such generalizations[1].

Importance of a philosophical analysis of ecology— The ethical and political dimensions of ecological knowledge alone would suffice to justify a philosophical interest in the source of such knowledge. Indeed, those were the first issues to attract philosophers' attention. However, there is also the necessity to understand the reaches and limits of ecological research as a source of environmental knowledge; and ecology poses all kind of fascinating foundational problems, both ontological and methodological. It is only reasonable that this problematics is more and more appealing to philosophers of science.

Publications on the philosophical challenges posed by ecological research started to appear in the last decades of the twentieth century. However, that was mainly the work of ecologists interested in the epistemology of their science (e.g., Levins & Lewontin 1980, Simberloff 1980, Peters 1991). Many of these attempts treated methodological matters and where characterized by their recourse to particular philosophical schools as conceptual backing for their own favorite research methodology (Cooper 2003). Interesting and

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[1] Due to the complexity of the subject matter and the diversity of cognitive and practical goals involved, there is a whole array of disciplines devoted to the production of environmental knowledge and to environmental problem solving. Among them there are a variety of natural sciences, such as geology, geography, oceanography, climate science, and many biological disciplines, including palinology, paleobiology, systematics, and ecology with all its different subdisciplines. Social science is of course also important for facing environmental issues, especially disciplines such as sociology, anthropology, human ecology, and political science, which may shed light on the patterns and causes of anthropogenic environmental impact and the potential avenues for ameliorating them. Finally, there are the varied technologies designed to deal with environmental problems from a practical standpoint, among them environmental engineering, environmental management, environmental law, and some portions of conservation biology and ecological economics.

useful as they were, such explorations did not benefit, in general, from the panoply of philosophical tools available to the professional philosophers of science of the day. Moreover, some ecologists did not pay much attention to possible inconsistencies among the diverse philosophical ideas they advocated in defense of their own methodological programs (Cooper 2003).

In spite of the practical importance and the conceptual interest of the field, philosophers of science took their time to show a professional interest in ecological science. The first signs of such interest appeared in the 1990s in the form articles in collections devoted to the philosophy of biology. In fact, save for a few isolated publications dealing with methodological issues (e.g., Shrader-Frechette & McCoy 1993, 1994, Cooper 1998, Sterenly & Griffiths 1999), professional work on ecology's philosophical problems only started to appear with certain regularity in the twenty first century, and the interest in the field keeps growing (e.g., Keller & Golley 2000, Colyvan et al. 2009, Brown et. al 2011).

There is one more aspect that should serve as an incentive for philosophical research on ecology, namely that it might prove beneficial for the general philosophy of science itself. The reason is that the ontological, methodological, praxiological, and axiological problems elicited by the special sciences —i.e., all science but physics— are sometimes quite different to those that occur in the philosophy of physics, the main inspiration of traditional philosophy of science. For instance, the philosophy of biology has already put into question a set of assumptions that once were the core of general philosophy of science. Some of those assumptions now called into doubt relate to central methodological issues such as the structure of theories, the nature of general laws and their role in scientific research, and the nature of scientific explanations. The latter has been, precisely, an important point of debate between ecologists with a philosophical leaning, who tried to answer what is an ecological explanation.

Recent philosophical interest in mechanisms— The present essay explores the question of scientific explanation with an emphasis in the role that the description of mechanisms plays in explaining ecological facts. Indeed, the philosophy of the life sciences has revived the idea that in science to explain a fact is identical to describe the mechanism that produces that fact. However, there is no consensus either about the precise nature of mechanisms or about the exact form of explanations based in mechanisms.

Aim and structure—The aim of the present dissertation is to provide an account of ecological explanation based on mechanism description that is free of some of the problems that affect similar philosophical projects. The general strategy I apply is to contrast the main theses of the contemporary mechanismic philosophy with two cases of ecological explanation by mechanisms in the subfield of vegetation dynamics, more precisely in ecological succession.

I begin by putting my project in context within the philosophy of ecology and the philosophy of scientific explanation, and showing the importance of addressing the problem of ecological explanation (Chapter 1).

In Chapter 2, I examine two examples of ecological explanation put forth to account for different aspects ecological succession. Succession theory was one of the first theoretical frameworks of ecology and is still important as a portion of vegetation dynamics. Mechanisms invoked to explain successional phenomena are varied and they include both positive and negative ecological interactions. I devote Section 2.1.1 of this chapter to examine the former type of interaction, also known as facilitation. Among negative interactions, I chose to investigate competition because more often than not it is considered one of the central mechanisms (or THE central mechanism) of community structuring (Section 2.1.2).

In Chapter 3 I review the origins of the philosophy of explanation, especially the covering-law model of explanation, in order to provide a context for a general assessment of the project of grounding scientific explanation in the description of mechanisms. This review continues in Chapter 4, where the models of explanation discussed revolve around the ontological notion of cause.

In Chapter 5 I describe and analyze the four proposals of explanation by mechanism description I deem the most promissory, namely those of mechanisms as systems (Stuart Glennan), mechanisms as entities-and-activitites (Peter Machamer, Lindley Darden, and Carl Craver), mechanisms as ephemeral processes (Glennan), and mechanisms as specific processes in systems (Bunge).

In Chapter 6 I put to test each of the views described in the previous chapter. My strategy consists in contrasting the main ontological and epistemological theses of the philosophical projects discussed in Chapter 5 with the corresponding theses implied by real cases of scientific explanation in the field of community ecology. In this chapter I also begin to take stock of the position of the contemporary mechanismic philosophy with regards to explanation in ecology.

Finally, in Chapter 7, I continue the analysis started in the previous chapter and concentrate in some problematic aspects of mechanisms and mechanismic explanation. My conclusion is that ecological mechanisms are best understood as specific processes in systems and, in consequence, that contemporary mechanismic philosophy would benefit from taking into account Bunge's systemic approach, particularly of his mechanismic model of explanation.

2

EXPLAINING ECOLOGICAL SUCCESSION

The notion of succession is foundational to ecological science (McIntosh, 1985) and was one of the first theoretical developments aimed at providing not only descriptions of ecological systems, but also explanations and even predictions of their origins and behavior. That of succession is still an important theoretical framework for research on more specific ecological matters, both of practical and theoretical interest. This importance has at least two sources. One of them is that disturbances are so common that successional processes are now recognized as ubiquitous (Meiners & Pickett 2011). The second source is that some successional mechanisms —e.g., facilitation, inhibition, competition, etc.— are also usually invoked for accounting for aspects of biological invasions, biodiversity, ecosystem regulation and many other important ecological phenomena.

Briefly put, succession theory[2] concerns itself with the changes that ecological systems undergo in their history, thence the name of *dynamic ecology* that one of its founders, Frederick S. Clements, gave it.

In its original guise —that developed by Henry Cowles (1899) and, especially, Clements (1916)— the main referent of succession theory is the ideal successional process. This process starts in areas that either are denuded of all vegetation (primary succession) or have suffered a strong disturbance so that plant cover has been drastically reduced (secondary succession). In both

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[2] While succession “theory” may not be considered a genuine theory by some standards —e.g., because it is not hypothetic-deductive system— it is nevertheless a genuine theoretical framework or it supplies many of the goods that theories are often assumed to provide, e.g., descriptions, explanations and predictive ability.

cases succession is a sequence of stages (each of them called a *sere*) through which sites affected by some radical disturbance —such as large fires, volcanic eruptions, floods, or human activity— change their vegetation structure and composition. The process includes the successive arrival, establishment, and ulterior replacement (or permanence, in the case of the climax) of *specific* types of plants in a *specific* order. Thus, an ideal succession would proceed through a number of *seral stages*, each with its own name, until a stable final stage or *climax* is reached. Clements grouped the multiplicity of factors that determine such events into four kinds of "causes" —the arrival, establishment, and replacement or stabilization of specific types of plants in a particular area—, and attributed the role of general constraint to the climate of the region, so that according to him the climax of a seres is mainly controlled by climate.

Now, the foregoing briefly describes the ideal process known as primary succession, but the theory also attempts to account for succession when a particular sere suffers disruptions —human activities, for instance— so that it deviates in one of a variety of possible ways from the path towards the climax. Once the effects of the disrupting factor have disappeared, the sere would return to its original path towards the climax. In this case, secondary succession and a profuse diversity of seral stages —each with its own name provided by Clements (1916)— ensue.

As a consequence of this most commentators —whether ecologists, historians, or philosophers— have it that the core of Clementsian succession theory was a deterministic law binding vegetation dynamics to climate, as a result of which the surface of the Earth would end up covered by discrete,

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homogeneous vegetation units whose characteristics depend on the climate of the region. In that view, climaxes are self-maintaining wholes characterized by the fact that the vegetation structure has reached its highest point of complexity and stability in relation to its habitat, and is “in balance with the climate” (Barbour 1996: 34) of the region. This interpretation receives support from Clements’s own frequent comparison of vegetation units to “organic entities”, a fact that has gained Clementsian metaphysics the label of the “organismic” or “superorganism” view of vegetation. According to this reading, Clements held that vegetation units were organisms, or like organisms, and that the succession process was comparable to the development of an organism. In other words, Clements's theory has canonically been deemed as committed to ontological holism (e.g., McIntosh 1985, 2011; Kingsland 2005). Whether one accepts this construal or not (see Eliot 2007), what is true is that Clements’s conceptual framework was soon challenged, at least in its methodological aspect, by Henry Gleason (1926), who thought that successional processes were to a great extent a result of chance and individual plants characteristics (for a detailed comparison between Clements’s and Gleason’s ontology and methodology see Eliot 2007). Further developments have attempted to integrate aspects of these two approaches paying attention to the particular mechanisms involved (Connell & Slatyer 1977).

According to current successional theory, succession is just a special case of vegetation dynamics —the compositional and structural changes of plant cover— and that it is not always a directional, predictable process nor has a clear endpoint or climax (Meiners et al. 2015). Indeed, succession frequently ends up in a community able to regenerate itself, but such stage is by no means stable, and there are successional processes that exhibit essentially cyclical dynamics, with successive stages continually replacing one another

(Meiners & Pickett 2011).

In general, successional dynamics proceeds along several axes that include area available for colonization (which usually decreases as succession proceeds), species diversity, biomass accumulation, nutrient retention, plant height, and seed size of plants (all of which generally increase along successional time) among others. As seed size increase, dispersal changes from abiotic dispersers, such as wind and water, to biotic ones such as ants, birds, and mammals. Ecologists group all factors affecting the successional process in three general classes of drivers or “causes”, namely *site availability*, *species availability*, and *species performance*. Each of these three kinds of drivers includes specific mechanisms (and factors) that produce the patterns of succession observed by ecologists.

As is the case with many ecological mechanisms, successional mechanisms are rather local and the particular combination producing a given successional pattern depend heavily on contingent initial conditions on the site such as type of soil, microclimate, time of the disturbance, differential availability of species at the time of the disturbance (which is connected with their dispersal syndrome), etc. In fact, according to succession specialists Joseph Connell and Ralph Slatyer, the mechanisms that produce the sequence of species in successional processes had not been identified before as late as the end of the 1970s, mainly due to methodological reasons. One of the reasons is that some mechanisms only start to work in later successional stages and most ecological studies are too short to capture them. Another reason is that the focus of successional studies used to be on plants (which are not only producer organisms but also account for most of the biomass and structural characteristics of successional systems) and their interactions with the physical environment, thus missing the effects of non vegetal living organisms on the process. A result of this emphasis on plants and abiotic factors has been a focus on plant competition and physical stresses as mechanisms of succession in detriment of other processes such as predation and parasitism. A third reason for the lack of knowledge of successional mechanisms until the 1980s was the dearth of hypothesis testable by controlled field experiments (Connell & Slatyer 1977). From those times on, research on vegetation dynamics has increasingly focused on mechanisms (Meiners et al. 2015). In particular, Connell and Slatyer proposed three types of successional processes, each of them characterized by one principal mechanism (Fig. 2.1).

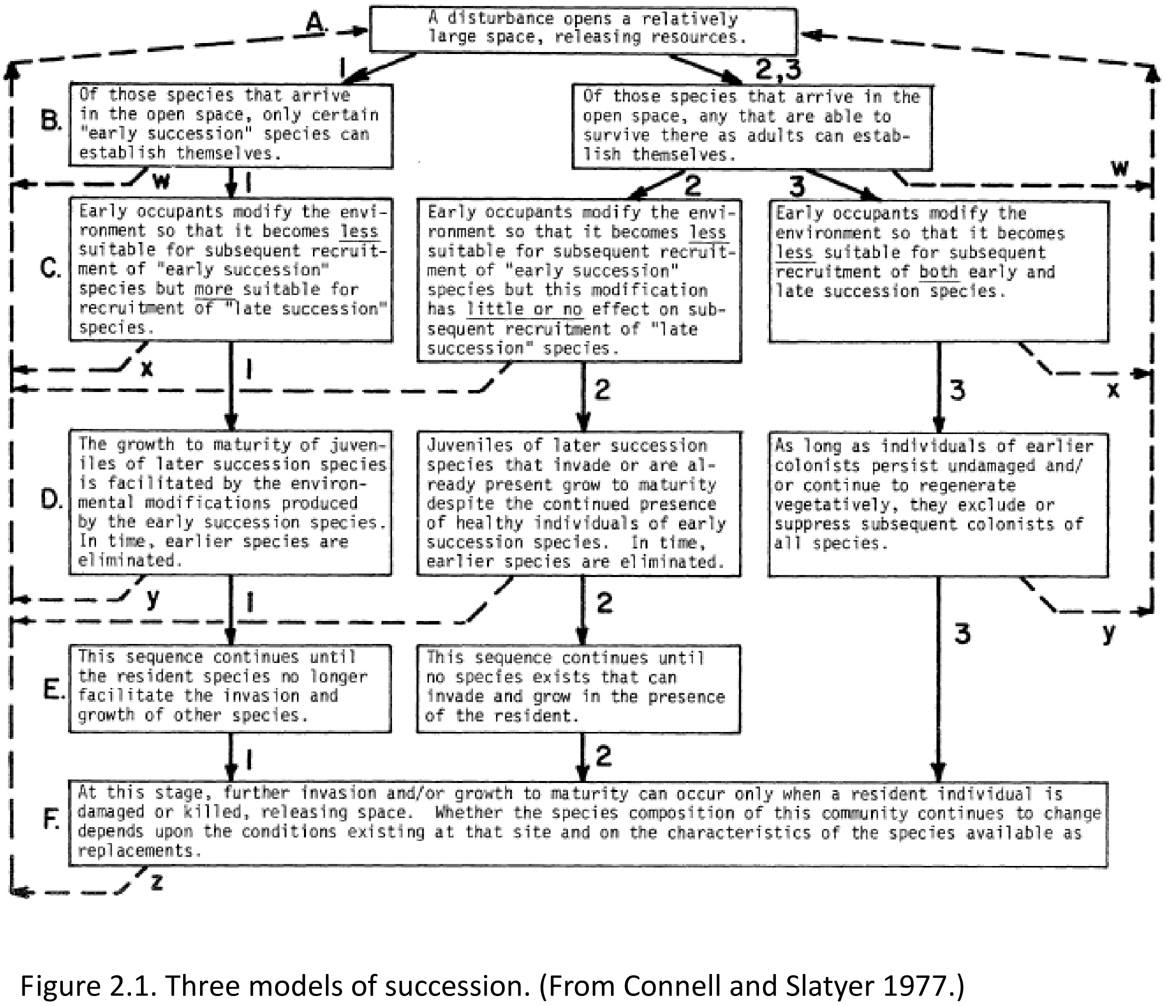

Figure 2.1. Three models of succession. (From Connell and Slatyer 1977.)

In the first type, captured in the *facilitation model*, the space opened by the disturbance is colonized by early successional plants that modify the habitat in ways that make it favorable for plants not capable of colonizing the site at early successional stages. In other words, the main mechanism producing this type of successional pattern is facilitation. Another result of those changes is that the habitat grows less suitable for early successionals, which eventually are eliminated.

In the second type, represented by the *tolerance model*, changes brought about by early successionals also alter the habitat in ways that are less favorable to early successionals. However, in this model later successionals are not affected in neither favorable nor negative ways. Thus, if later successionals available, they invade the site, coexist for a time with the colonizers, start to compete with them, and eventually eliminate them. In this case, the mechanism involved is resource competition.

Finally, in the third type of succession, pictured by the *inhibition model*, early successional plants alter the environment in a fashion that excludes other early and precludes invasion from later successional organisms.

Once the main mechanism is operating, the first two types of successional processes will iterate until a new perturbation damages or eliminates the resident plants or maturity is reached. In the facilitation model, for example, community composition and structure may change, but the main process at work, that is facilitation, will continue to produce the replacement of a given community by another one. In the tolerance model it will be competition the process that will keep the community successional dynamics. Finally, in the inhibition model, after colonization only further disturbance will allow invasion.

2.1 Mechanisms of succession

According to the present understanding of succession, there are three main mechanisms that drive successional processes: facilitation, competition, and inhibition.

2.1.1 Facilitation

By facilitation ecologists denote a collection of positive ecological interactions that play an important role in communities strongly shaped by environmental conditions, including successional communities. Facilitation interactions are well documented and have been characterized as “encounters between organisms that benefit at least one of the participants and cause harm to neither” (Stachowicz 2001: 235). Strictly speaking, however, facilitation interactions do not need to be real encounters (individuals are not required to actually meet), not even interactions (the effects of facilitation may be unidirectional). For facilitation to occur it suffices that there is a causal process that connects, either directly or indirectly, some individual organisms and have a beneficial effect at least on one of them. Thus, facilitation processes can be direct, as when stress-tolerant neighbors ameliorate the physical environment for less hardy species. Stressful environmental conditions can be excessively high or low temperatures, high salinity, low water, soil oxygen, or nutrients availability. Positive ecological interactions also can be indirect, in which case individuals of one species interact with individuals of a second species indirectly benefiting a individuals of a third species, as when the presence of one species deters predators from the site, increase pollinators visits to the site, or enhance the effect of mycorrhizae and soil microbes (Baumeister & Callaway 2006, Callaway 2007).

In positive interactions, the benefit can be mutual (*mutualism*, a genuine interaction) or unidirectional (*commensalism*). Moreover, different positive interactions are known to combine among themselves and with negative interactions (another case of genuine interactions), as when shading combines with competition, making it important to unravel the relative contribution of each kind of process in community structuring (Callaway 2007).

Although facilitation processes are ubiquitous in nature and were important for early ecologists, ecological theory has paid much less attention to them than to negative interactions, especially to competition. As a consequence, theorization on positive ecological interactions is a rather novel scientific enterprise (Bruno et al. 2003) and is not as developed as competition theory.

A common form of facilitation occurs through habitat modification, in which an organism produces changes in its environment that result in a less stressful habitat for other organisms. A *stress* here is “any extrinsic force that reduces fitness of an individual or population” (Stachowicz 2001: 235) and it can be of biotic (competition, predation, parasitism, etc.) or abiotic (temperature, humidity, mechanical impacts, etc.) origin.

A classic example of facilitation is that of taller plants providing shade to seedlings and less tolerant, shorter plants. Shading modifies the environment mainly by reducing the amount of sunlight reaching subcanopy microhabitats. Beneficial effects of shading on benefited plants comprise reduction of respiration costs, ultraviolet radiation and transpirational demands (the latter by decreasing the vapor pressure difference between leaves and air); maintenance of tissues below lethal temperatures, and increasing soil moisture through lower evaporative demand (Callaway 2007).

Baumeister and Callaway (2006) provide a recent study on the relative importance of different facilitation (and competitive) mechanisms in a prairie-forest ecotone[3] in the northern Rocky Mountains, in Montana (USA). The forest component of the system is dominated by the stress-tolerant limber pine (*Pinus flexilis*), which is an early successional in species after fire and the only tree species that initially colonizes prairie grassland. The prairie component of the ecotone is dominated by two species of fescue grasses, *Festuca cabrella* and *F. idahoensis*, with interspersed shrubs patches, limber pine stands, aspen (*Populus tremuloides*) groves, and riparian corridors.

Numerous species of plants grow under the crowns of *P. flexilis* but not in the open grassland, suggesting the occurrence of some sort of facilitation by the limber pine. Plants associated to subcrown sites under *P. flexilis* are the Douglas fir (*Pseudtsuga menziesii*), a conifer (Figure 2.2), and the deciduous evergreen shrub known as wax currant (*Ribes cereum*). The area presents rather harsh climatic conditions, including “extraordinarily high” warming catabatic (or downwards) winds that blow from the mountains onto the prairie, predominantly from the west-southwest and very high thermal amplitude (from -40º to 37ºC). Annual precipitations average 70 cm.

Using both observational and experimental methods, the authors attempted to answer three questions (for details of experimental design see the *Material and Methods* section of Baumeister & Callaway 2006):

(a) Do patterns of association between *P. flexilis* and other plant species suggest facilitation interactions?

(b) What are the mechanisms of facilitation and do these mechanisms interact and/or vary in importance with the severity of environmental

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[3] An ecotone is a transitional zone between two adjacent plant communities or biomes characterized by a rapid turnover of species along a spatial transect or ecological gradient (Ricklefs 2008).

conditions?

(c) Do the importance of and the mechanisms of facilitation vary among benefactor species?

Figure 2.2 Two young individuals of Douglas fir (*Pseudotsuga enziesii*) growing under the crown, on to leeward of one individual of limber pine (*Pinus flexilis*). Drawing by the author, adapted from a photo in Baumeister and Callaway (2006: 1817).

In order to answer question a), the authors located 50 plots (450 m^2 each) at random along a 15 km section, containing at least 15% canopy cover of *P. flexilis* and at least one individual of *Pseudotsuga*, and then registered the location (beneath *P. flexilis* crown, beneath *Pseudotsuga* crown, or in the open) of all *Pseudotsuga* and *Ribes* seedlings (individuals less than 5 cm height). For *Pseudotsuga*, the authors also registered seedlings position (leeward, “neutral”, and windward) with respect to wind direction and the nearest *P. flexilis* individual. In order to know the order of appearance in the site, the age of both *Pseudotsuga* and *P. flexilis individuals* was estimated.

Observational results provided a firm basis to answer question (a) in the affirmative, as the authors found that significantly higher proportions of *Pseudotsuga* (69,3%) and *Ribes* (91,0%) individuals grew beneath *P. flexilis* even though the latter covered only 39,0% of the study area. Location of *Pseudotsuga* individuals regarding wind direction indicated that protection from the wind could be one of the facilitation mechanisms at work in the area, for a percentage significantly higher of them were located to leeward with respect to the nearest *P. flexilis* tree and (64,2% under *P. flexilis* trees; 47,9% in the open). It is important to note that in almost all instances of co-occurrence (97,4%) *P. flexilis* trees were older than the associated *Pseudotsuga* and that 98,3% of *Ribes* individuals occurred beneath *P. flexilis* trees older than 60 years, confirming the temporal precedence of the latter in the site.

To explore the abiotic conditions both under the crown of *P. flexilis* and in the open grassland, Baumeister and Callaway chose two stands of the dominant *P. flexilis* located at opposite sides of a hill, which shared the type of substrate but differed in exposure to the wind: the “windward site” was fully exposed to the strong Chinook catabatic winds that sweep the area, while the “leeward site” was located on a protected slope. In the latter site, *Pseudotsuga* occurred naturally in the understory. Then, the authors randomly chose 10 individuals of *P. flexilis* in the windward site and 10 in the leeward site, and measured soil moisture, nutrients (available phosphorus and nitrogen), light (photosynthetically active radiation), litter depth, and thickness of the A and O horizons under the canopy (see details in Baumeister & Callaway 2006).

The significant difference in mean size (height, trunk diameter, and crown radius) between *P. flexilis* individuals of approximately the same age in the windward site and those in the leeward site (much larger) suggested that the growing conditions in the former were much more stressful. Likewise, under canopy A soil horizons in the windward site proved to be 65% shallower than those in the leeward site, and both A and O soil horizons were thinnest in the open windward site and thickest in the open at the leeward site.

Photosynthetically active radiation (PAR) under *P. flexilis* crowns was 40,2 % lower than PAR in the open grassland, and reduction did not differ significantly between the windward and leeward sites. On the other hand, litter depth resulted significantly greater beneath the trees than in the open at both sites. Availability of phosphorus and nitrogen (measured by presence of P, NO3-N, or NH4-N) in soils did not differed significantly between under canopy and open grassland measurements in the leeward site. Another potential facilitation mechanism showed to be unlikely was conservation of soil moisture, for soil moisture was lower beneath *P. flexilis* than in the open grassland both in the windward and in the leeward sites.

As for the more complex question about which facilitation mechanisms are at work in the area —i.e., question (b)— the authors used an experimental approach to attempt to answer it.

In order to know seedling survival under different conditions, the authors chose 25 *P*. *flexilis* individuals older than sixty years and planted 4 one-year-old *Pseudotsuga* seedlings under each *P*. *flexilis* canopy and other 100 seedlings in the open. Then, they compared seedling growth and survival rates, location and treatment (leeward/windward site, beneath/beyond crown, and protected/unprotected from herbivory) during three years. Moreover, using the same 25 *P. flexilis* trees of the foregoing experiment, the authors planted 2 one-year-old *Ribes cereum* seedlings under each individual of *P. flexilis* and similarly monitored their growth and survival rates during two years.

For *Pseudotsuga* seedlings, survival was greatest beneath at the leeward site (37%) and lowest (2%) in the open at the windward site. Survival was also higher at the leeward site, both under the trees and in the open, than the corresponding locations at the windward site, with a total survival of 20% for the former and of 11% for the latter. At the windward site the difference in survival beneath *P. flexilis* (19%) vs. in the open (2%) amounted to 10 times, while the same difference was only 3 times at the leeward site, suggesting that facilitation is more important in the abiotically stressful windward site. Further data analysis showed that wind amelioration by *P. flexilis* was an important facilitation mechanism for *Pseudotsuga* and *Ribes* in the area.

In order to separate the effects of the different likely aboveground mechanisms (snow accumulation, wind amelioration, and shade) by which *P. flexilis* facilitates the survival and growth of *Pseudotsuga* and *Ribes*, Baumeister and Callaway (2006) designed a three-way, fully factorial, blocked experiment at a level plateau dominated by *Festuca* grasses with scattered *P. flexilis*. The experimental area (35 x 15 m) was encircled with a 2.5m tall wire fence to keep herbivores out. Inside this rectangle, 1 x 1 m plots were established with different treatments. Shade treatments (“shade”, “shade + drift”, “shade + no wind”, and “shade + no wind + drift”) consisted in each plot being covered by 1.5 x 1.5 m green propylene shad cloth (which produced a 48% reduction in PAR). Snow accumulation (“drift”) treatments (“drift”, “shade + drift”, “drift + no wind”, and “shade + no wind + drift”) were established using plastic snow fences directly windward of the plots from October to April each year. Wind was blocked with U-shaped polycarbonate fences, which blocked more than 80% of the wind without increasing snow accumulation. Soil moisture was measured at the center of each plot.

The authors planted 5 *Ribes* and 3 *Pseudotsuga* seedlings in each of the replicate quadrats for each of the nine treatment combinations and then assessed treatment effects on their survival and growth. Analysis of the resulting data showed that shade, not wind, was the most important effect. Without shade, no other treatments produced significant differences either for *Pseudotsuga* or for *Ribes*. Once shade was provided, *Pseudotsuga* treatments did show significant different among them, suggesting that mechanisms operated in a hierarchical manner, with shade on top of all of them. Curiously enough, a separate two-way ANOVA showed that "shade" and "no wind" treatments alone were significant, while their interaction was not. The same statistical analysis detected significant treatments when "shade" and "drift" treatments were assessed without "no wind". This further stressed the hierarchical effect of treatments.

Within the shaded plots, *Pseudotsuga* survival was lowest without wind protection and without enhanced snow accumulation, and was highest with drift fences. Mortality peaked at the first winter (84% in plots with no shade, 35% with shade, 16.7% with shade and wind barrier, and 2.4% in plots with shade and drift barrier). Once shade was provided, wind reduction had a significantly positive effect on seedling survival during the first winter, but not later. Shade did not enhance soil moisture conservation. The highest positive effect of shade on seedling survival occurred in the first winter (17.5% mortality with shade vs. 84.1% without shade). For *Ribes*, on the other hand, the positive effect of shade occurred in the first summer (2.4% mortality with shade vs. 55.2% without shade).

Both species were taller in the shade treatments than in treatments without shade, and root mass and root-to-shoot ratios followed the same pattern, which further corroborated the positive effect of shade on seedlings.

The overall reading of these observational and experimental findings indicates that *P. flexilis* has strong facilitation effects on both *Pseudotsuga* and *Ribes* plants.

In the case of *Pseudotsuga* those positive effects are due mainly to *P. flexilis* protecting the associated plants from the wind, while in the case of *Ribes* the seasonal patterns of mortality suggest that shade is the main facilitation mechanism and wind protection has a secondary role. As is usual in ecology, things are not that simple and results also showed that facilitation mechanisms interact so that the positive effect of some of them depends on the occurrence of another. For example, the experiments showed that the presence of wind barriers and drift only produced positive effects in the presence of the shade treatment.

As for the precise way in which shade helps seedlings, according to the authors, there are at least two processes involved.

One of them is moderating under crown temperatures. In summer, as in the case of *Ribes*, shade lowers under-crown temperatures, reducing soil moisture evaporation and leaf evapotranspiration. Soil moisture reduction does not seem to agree with the authors' results, but they explain this fact invoking the particular technique they used to assess soil moisture (frequency domain reflectometry), which measures soil relative water content and not water potential (an indicator of water availability for plants). Though the two variables are correlated, the latter varies a lot with soil texture, so at equal relative water content values different water potentials may occur so this particular research may have underestimated the importance of crown facilitation of soil water. In winter, as in the case of *Pseudotsuga*, shade helps keeping temperatures higher than in he open through insulation, preventing damages from cold and from the effect of low temperatures on photoinhibition (see below).

A second facilitation process that brings about the shade effect is light reduction. This would be the case of *Pseudotsuga*, whose highest mortality in the absence of shade took place in winter. High reflectance from snow would raise photoinhibition rate in seedlings. Photoinhibition is a light-induced depression of photosynthetic rate produced by leaves absorbing and amount of electromagnetic radiation —especially in the blue wavelengths— higher than that they can effectively use. Though the precise molecular mechanisms of photoinhibition are still under study (Tyystjärvi 2008), it is known that cold temperatures increase the rate of photoinhibition (Greer 1988, Germino & Smith 2005). Thus, in winter, *P. flexilis* would facilitate *Pseudotsuga* seedlings by protecting them both from PAR and low temperatures. The relative importance of shading as a facilitation mechanism is likely to be related to the high percentage (81%) of sunny days in the area.

Another facilitation mechanism at play in the study area was wind amelioration. The precise processes by which *P. flexilis* trees protect seedlings from adverse wind effects are also diverse, but all follow from the tree acting as a windbreaker that mechanically slows down wind speeds. Since *P. flexilis* usually present a crown shape similar to krummoltz[4], with numerous low-lying branches, both the tree trunk and its crown may work as a barrier against wind for the seedlings located on the leeward side. This, in turn,

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[4] Subarctic or subalpine vegetation stunted and contorted by continual exposure strong winds. Their shape, which includes high near ground growth, partially relates to rocks or snow protecting the lower parts of the plants from the wind (see Holtmeier 1981)).

brings about other changes that may be beneficial for seedlings. First, a reduction of wind velocity decreases the mechanical abrasion from soil particles and ice, which has been shown to reduce the cuticular wax of leaves that reduces evapotranspiration. Second, wind speed reduction decreases soil desiccation and evapotranspiration in seedlings. Third, catabatic winds, which are warm, may increase above ground winter temperatures stimulating photosynthesis activity in plants growing on frozen ground. This would increase leaf temperature and evapotranspiration without the possibility of water replenishment by roots, favoring leaf desiccation.

A further positive effect of *P. flexilis* on understory is drift, that is, the accumulation of winter snow, which would act as an insulator and a protective barrier for seedlings against excessive irradiation and wind.

All the foregoing indicates that *P. flexilis* stands alter the immediate environment of *Pseudotsuga* and *Ribes* seedlings in ways that are beneficial for their survival and growth. Benefits comprise the possibility for *Pseudotsuga* and *Ribes* seedlings to occur in sites and times outside their corresponding fundamental niches. According to this, through facilitation *P. flexilis* would expand the realized niches of *Pseudotsuga* and *Ribes* (Figure 2.3).

Several facilitation mechanisms that operate simultaneously explain enhanced survival and growth of young *Pseudotsuga* and *Ribes* under the crown of *P. flexilis*, and their relative importance depends not only on the particular species involved but also on environmental conditions and season of the year. Moreover, facilitation processes interact among themselves and they do it in a hierarchical manner so that some mechanisms only operate in the presence of another mechanism. This finding suggests the methodological consequence that studies that investigate isolated facilitation mechanisms may lead to error.

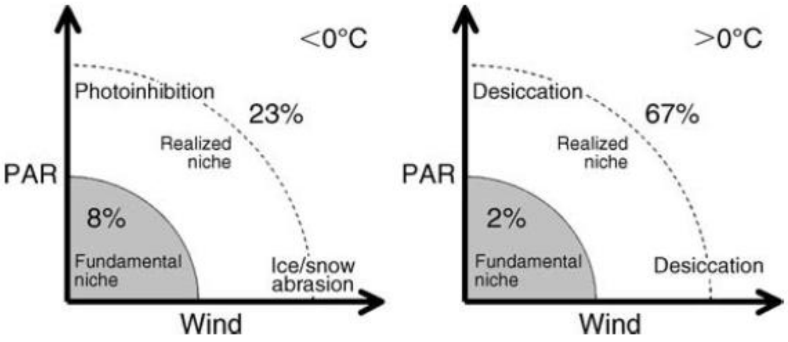

Figure 2.3 Expansion of *Pseudotsuga*'s and *Ribes*' niches by *Pinus flexilis*. Quantification is by days per annum with conditions within the fundamental niche of each species (Bumeister & Callaway 2006: 1827).

2.1.2 Competition

Ecological competition is one of the negative interactions that are essential to successional processes (Clements 1929, Connell & Slatyer 1977). Furthermore, for a long time ecologists have considered competition the central force structuring ecological communities and driving the “struggle for existence”, perhaps because a number of authors identified Darwin’s “struggle for existence” with “competition” (Keddy 2001). Be this as it may, what is true is that at least during its coming of age as a science, ecology revolved around the notion of individuals competing for resources of some kind (McIntosh 1985, Cooper 2003).

The emphasis on competition has now diminished as other ecological interactions, positive and negative, have gained prominence as explanatory factors in community dynamics. Yet, competitive mechanisms continue to be key components of ecological theory, both within and without the theoretical framework of ecological succession.

2.1.2.1 Resource competition

Individual organisms exist in an environment that includes other organisms of the same and of different species, as well as abiotic factors such as soil, water, and climatic conditions. Inevitably, organisms interact with their immediate environment in a variety of ways that affect both the individual organism and its environment in definite fashions. The study of those interactions is one of the main objects of ecological research.

Moreover, the environment is not homogeneous; it comprises a diversity of entities that are likely to have different values for the survival and reproduction of the individual. Ecologists usually call ‘resources’ the items in the environment that have a certain positive value for the survival and/or the reproductive ability of an organism.

More precisely, in ecology a *resource* can be characterized as follows: “A factor, R, is defined as being a resource for species i if increases and decreases in R lead to increases and decreases, respectively, in the specific growth rate, $fi(R)$, of the species and if the species consumes the factor (i.e. $\delta R / \delta Bi < 0$). [...] with Bi being the abundance, or biomass, of species i per area” (Tilman 2007: 88). Resources may be of different nature, from nutrients, light and water, to mates, space and shelter and since resources are sometimes relatively scarce, individual organisms may compete for them under certain circumstances. For example, in an early successional stage, individual plants may compete for water and space (Bazzaz 1990). Likewise, male red deer, *Cervus elaphus*, usually compete for females during rut (Clutton-Brock et al. 1979) and there is evidence that damsel fishes of two species —*Dascyllus flavicaudatus* and *D. trimaculatus*— compete between each other for the branches of corals or anemones they use as shelter (Holbrook & Schmitt 2002).

Ecologists usually consider competition a mechanism and this is why some of them defend a mechanistic approach to the study of competition. David Tilman, for example, one of the most preeminent students of competition defines "the study of competition as mechanistic if it includes both the direct processes by which competition occurs and information on physiology, morphology, and/or behavior of individual species or functional groups relevant to that direct process. […] A major goal of the mechanistic approach is to use information on the physiology, morphology, and/or behavior of individual species to predict the outcome of pairwise or multispecies interactions" (Tilman 1987: 771.)

Competition is one of the mechanisms of Natural Selection, since competing individuals reduce each other's fitness. Besides, competition is central to some of the most important theoretical developments in ecological science, namely succession and niche theories. In the former, competitive processes are invoked as a one of the mechanisms that explain certain successional patterns. In niche theory, competition is used to explain niche contraction and niche displacement.

In a very elemental rendition, the resource-competition hypothesis states that under conditions of relative scarcity of resources, organisms will affect each other in a negative way[5] by making use of those resources, so that fecundity, growth or survival, that is some aspects of biological fitness, are reduced. One consequence of this hypothesis is that organisms with different efficiency in

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[5] For Keddy (2001: 12-28 ff), the mutual negative effect is just a limiting case in continuum whose extremes are total symmetry and total asymmetry. Most known examples show at least slightly asymmetric competition, but there are cases in which the negative effect of one of the organisms on the other is so small that is impossible to measure.

using resources will fare differently.

Now, there can be competition both between individuals that belong in the same species (*intraspecific* competition) and between individuals of different species (*interspecific* competition). In this work I shall be concerned with the latter as my second exemplar of an ecological mechanism for ulterior philosophical analysis.

Ecologists recur to interspecific competition as a mechanism for explaining a variety of patterns of species abundance and diversity in ecological communities. In particular, interspecific resource competition has been proposed as a major explanation for multispecies coexistence, an especially vexing problem in community ecology with implications both for theory and for conservation (Tilman 2007).

2.1.2.2. Interspecific competition

Interspecific competition is a general term encompassing a number of specific negative interactions. One of them, *interference competition*, consists in individuals of different species competing by directly attacking, physically or chemically, each other. For example, Jiménez et al. (1996) offered direct physical aggression as a mechanism to explain the patchy distribution of two species of canids in southern Chile. According to Jiménez and collaborators, culpeo foxes (*Lycalopex culpaeus*) would actively attack and thus exclude its lesser relative, the South American grey fox (*L. griseus*), from the patches with higher densities of the small mammals that both species preferred as food. Another example of interference competition is that experimentally demonstrated for two aquatic plants present in North American, the autochthonous *Ludwigia rapens* and the exotic *Hygrophila polysperma* (Doyle et al. 2003). In this case, individuals of the former species facing physical interference by individuals of the latter showed reduced growth in speed, length and number of sprigs, while there was no negative effect of *L. rapens* on the growth of *H. polysperma*. This result suggests that even when populations of both species may colonize unvegetated habitats, *H. polysperma* will invade *L. rapens*.

Alternatively, organisms of different species may compete indirectly by making some limiting resource –such as water, nutrients, or light– less available to each other in such a way that a negative effect on their biological fitness obtains. The latter interaction is usually called *exploitative competition*. Returning to South American foxes, the coexistence of *L. culpaeus* (~ 13 kg) and *L. griseus* (~ 4.5 kg) in Central Chile has been explained invoking a causal chain triggered by exploitative competition processes. According to this hypothesis, in Central Chile, populations of those two species of foxes coexist because of the competitive relaxation favored by niche differentiation[6]. Niche differentiation in this case would consist in the differential use of the available food resources and would be favored by character displacement —body size, in the case of those two South American foxes (Fuentes & Jaksiç 1979, González del Solar & Rau, 2004). In this evolutionary hypothesis, resource competition is the main selective pressure for character displacement and, thus for coexistence. Thus, coexistence and niche differentiation are the phenomena in need of an explanation, and competition (plus its long-term consequences) is the mechanism that explains those phenomena. In the following pages I describe one more example and a model for this latter kind of interspecific competition, i.e., interspecific exploitative competition.

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[6] Or niche partitioning, or niche segregation, or niche separation.

2.1.2.3 Discovering interspecific exploitative competition (IEC)

I have chosen two examples of competition research for philosophical analysis. One of them is an already classical study that explored competitive interactions between distantly related taxa, ants (Insecta) and rodents (Mammalia), in the Sonoran desert (Brown & Davidson 1977). The other one is quite different because it is an attempt to model the competitive mechanism.

2.1.2.3.1 Interspecific exploitative competition between ants and rodents

The study of interest (Brown & Davidson 1977) was part of a larger project aimed at investigating granivory —i.e., seed predation— in a xeric environment (Brown et al. 1979). Granivory is important because seeds play a major role in arid zones, both as a source of new plants in dispersal processes and as a rich food resource for consumers of different taxa. In areas usually characterized by resource scarcity such as deserts, competition is likely to arise between organisms with similar utilization of resources (usually ants, birds, and rodents). This was thought to be the case with ants and rodents in the Sonoran desert scrubland, near Portal (Arizona, USA), at the time of the study. The hypothesis about strong competition between these two kinds of granivores was based on previous experiments conducted with comercial seeds. Those studies had shown that rodents and ants took most of the seeds offered to them, “harvested same sizes and species and collected them from the same microhabitats” (Brown & Davidson 1977: 881); besides, both ants and rodents foraged native seeds of overlapping sizes and species.

The Portal study was designed to explore “the significance of competition among distantly related organisms” (Brown & Davidson 1977: 880) by measuring the effect of seed foraging by ants on rodents and vice versa, as well as the effect of both consumers on seed abundance. The species involved were specialized granivores ants belonging to the genera *Pogonomyrmex* and *Veromessor* (but also to *Novomessor* and *Solenopsis*, both with a more omnivorous diet) and the granivorous rodents of the genera *Dipodomys* (kangaroo rats) and *Perognathus* (pocket mice), but also the more omnivorous *Peromyscus* (deer mice) and *Reithrodontomys* (harvest mice). The experimental set up consisted in eight circular plots, 36 m in diameter each. Pairs of plots were assigned each of the following four treatments. *Rodent exclusion* (No rodents): seed-eating rodents were excluded from the plot by means of mesh fences and individuals within the circle were removed by trapping. *Ant exclusion* (No ants): seed-eating ant colonies were identified and eliminated using insecticide. Ants without the exclusion were fenced off with insecticide. *Ant and rodent exclusion*: both ants and rodents were removed and fenced off combining the previous treatments. *Control* (C): the plot was not manipulated. The authors measured regularly the number of rodents (by means of live traps) and ant colonies within the plots. Rodents' biomass and soil seed content within the plots were also measured. (For details on the experimental design and the measuring techniques, see Brown & Davidson 1977). Results indicated that the negative effect of rodents on ants was much stronger than vice versa, suggesting an asymmetric competitive interaction between the two groups of organisms (Table 2.1).

Table 2.1 Effects of seed removal by ants and rodents from experimental fenced plots in an arid scrubland. Increase percentage is calculated relative to Control. (see Brown & Davidson 1977).

	Control	No rodents	No ants	Increase (%)
Ant colonies	318	543	_	71
Rodents (numbers)	126	_	151	20
Rodents (kg biomass)	4.2	_	5.4	29

2.1.2.2 Modeling interspecific exploitative competition

There is a diversity of models representing exploitative competition and all of them include three kinds of elements: resources, mechanisms, and organisms (Keddy 2001). Most of them make use of the equations proposed by the biophysicist Alfred J. Lotka (1925) and Vito Volterra (1926) originally and independently in the first third of the twentieth century. Curiously enough, Raymond Pearl and L. J. Reeds developed and published similar equations by more or less the same time (Pearl & Reed 1920). These equations turn to be so popular among the students of ecology that studying them is sometimes considered research (Keddy 2001) and they all derive from previous mathematical work in demography by Pierre-François Verhulst (1838).

Here I describe the Lotka-Volterra equations following Keddy (2001). The basic idea is that in the absence of constraints the growth rate ($\mathrm{d}N/\mathrm{d}t$) of a biological population is proportional to the size (N) of that population. This can be expressed in the form of a differential equation such as

$$\mathrm{d}N/\mathrm{d}t = rN\ , \qquad [1]$$

where r is the intrinsic growth rate of the population. Differential equations are adequate to represent dynamic relations of dependence. Constraints, however, are essential features of real systems of any sort, so if one want to represent biological populations, one should introduce limitations to the population growth rate. As Malthus saw with clarity in his study of human populations, one such limitation is resource availability, which (partially) determines the number of individual organisms viable in a given environment. The Lotka-Volterra equations set an upper limit to population size including the notion of carrying capacity (K), that is the maximum number of individuals that a certain habitat can support.

$$dN/dt = rN\,[K - N)/K] \qquad [2]$$

According to these equations, when the population size (N) approaches zero, the rate of population growth (dN/dt) approaches is exponential, and when N approaches its carrying capacity (K), dN/dt approaches zero.

In order to explore competition, two equations should be studied simultaneously: one representing the negative effect of each individual of population 2 on population 1 and another representing the negative effect of each individual of population 1 on population 2. A useful way to measure such effects is comparing the per capita competitive effect of each population relative to the other using the relevant competition coefficients (α_{ij}). By definition, the competition coefficient of a species upon itself is 1.

Now, the equations are:

$$dN_1/dt = r_1N_1\,[K_1 - \alpha_{11}N_1 - \alpha_{12}N_2)/K_1] \qquad [3]$$

$$dN_2/dt = r_2N_2\,[K_2 - \alpha_{22}N_1 - \alpha_{21}N_1)/K_2] \qquad [4]$$

There are two possible outcomes when competitive populations grow in the same area, (a) one of the populations becomes extinct and the numbers of the other population increase until it reaches its carrying capacity; (b) populations coexist. Usually the aim of studying the Lotka-Volterra equations is to know what factors determine that populations coexist and which population will persist when coexistence does not occur. The answer to the relevant questions will be one of the following factors: the size of each population of interest (N_1, N_2), their intrinsic growth rate (r_1, r_2,), the carrying capacity for each of the populations (K_1, K_2), and their corresponding competition coefficients (α_{12}, α_{21}). The interaction between two populations may be pictured by means of isoclines, that is the lines representing all possible combinations of conditions that result in null population growth. Thus, the isocline for population 1 is calculated by setting population growth equal to zero:

$$dN_1/dt = r_1N_1\ [K_1 - \alpha_{11}N_1 - \alpha_{12}N_2)/K_1] = 0 \qquad [5]$$

For obvious mathematical reasons, $dN_1/dt = 0$ when one of the following three conditions obtains: $r_1 = 0$; $N_1 = 0$; or $K_1 - \alpha_{11}N_1 - \alpha_{12}N_2 = 0$. These conditions provide the trivial solutions to the equation, but the third condition allows graphically plotting the isocline by finding the intercepts of the axes and joining them with a straight line. The intercept with the N_1 axis when N_2 is zero will be:

$$K_1 - \alpha_{11}N_1 - \alpha_{12}0 = 0$$
$$K_1 - \alpha_{11}N_1 = 0$$
$$K_1 = \alpha_{11}N_1$$
$$N_1 = K_1 / \alpha_{11} \qquad [6]$$

The intercept with the N_2-axis when N_2 equals zero is calculated similarly and the result is analogous to the foregoing: $N_2 = K_2 / \alpha_{22}$ (Figure 2.4).

At any point of the isocline, population growth is zero, i.e. the population is at "equilibrium". When N_1 exceeds the carrying capacity of the environment (K_1) size N_1 tends to decrease with time because there are too much individuals for the habitat to support them all. Likewise, when N_1 is lower than K_1, the population numbers tend to increase because there are relatively plenty of resources. A similar graphic can be constructed for N_2.

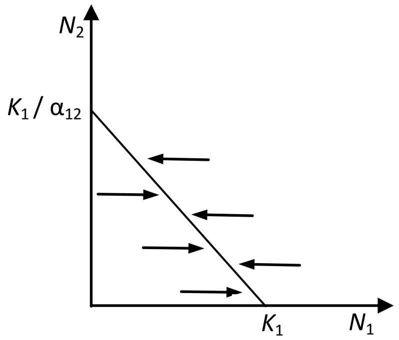
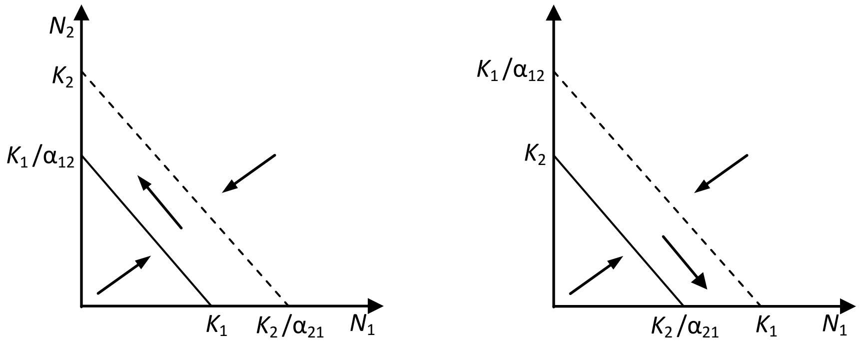


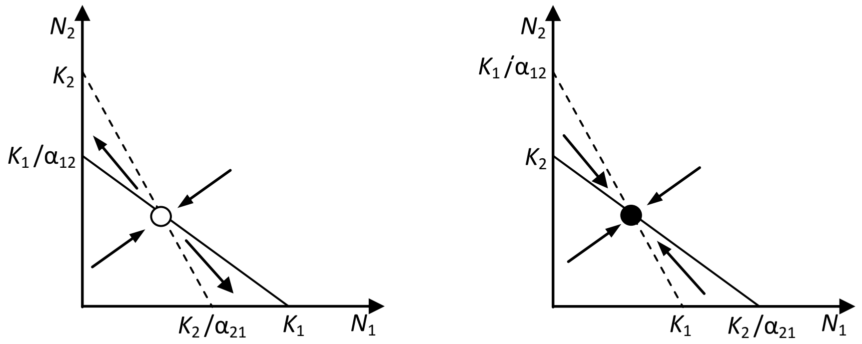
Figure 2.4. Zero-growth isocline for population 1 plotted in a two-dimensional space. The horizontal arrows show the tendency of the population to increase or decrease its numbers when its size (N_1) assumes values above or below, respectively, of the carrying capacity (K_1) of the environment for that population.

Now, if one wants to explore the possible outcomes of the interactions between two populations, then their two isoclines can be plotted in the same graphic. This exercise reveals four interesting possibilities: three exclusions of one of the populations and one equilibrium-coexistence (Figure 2.5).



(a) Exclusion

(b) Exclusion



(c) Exclusion

(d) Equilibrium coexistence

Figure 2.5. Four possible combinations of zero-growth isoclines for competing

populations of species 1 and 2 plotted in a two-dimensional space. The arrows represent changes in population size with time. The dots symbolize the equilibrium points of these pairwise interactions.

In the end, according to these models the outcome of interspecific exploitative competition depends on both (a) the effect that changes in resource availability have on the fitness of individuals of each species and (b) the per capita effect of individuals of each species on resource availability (Chase & Leibold 2003). The former is measured by the carrying capacity of the environment for each species, K_i while the latter is measured by the competition coefficients, α_{ij}. This is why one can specify the outcomes of the models introduced in Figure 2.5 in terms of carrying capacities and competition coefficients. Another way for studying the models is to set $K_1 = K_2$ and see what the outcomes are for different combinations of α_{ij}. In the first place, those corresponding to (a) and (b) in Figure 2.5, that is exclusion due to competitive dominance:

$$\begin{gathered}\alpha_{11}/\alpha_{12} < 1 \quad \text{and} \quad \alpha_{12}/\alpha_{22} < 1 \\ \alpha_{11}/\alpha_{21} > 1 \quad \text{and} \quad \alpha_{12}/\alpha_{22} > 1\end{gathered} \tag{6}$$

Secondly, the case represented by (c) in Figure 2.5, that is exclusion due to contingent exclusion:

$$\alpha_{12}/\alpha_{22} > 1 > \alpha_{11}/\alpha_{21} \tag{7}$$

Finally, the case represented by (d) in Figure 2.5, that is equilibrium coexistence:

$$\alpha_{12}/\alpha_{22} < 1 < \alpha_{11}/\alpha_{21} \tag{8}$$

Because of their simplicity, the Lotka-Volterra models are sometimes conceived of as mainly exploratory models that represent natural systems only to a minimal extent (Keddy 2001).

PART II

GENERAL PHILOSOPHICAL CONSIDERATIONS ON SCIENTIFIC EXPLANATION

3

THE PHILOSOPHY OF SCIENTIFIC EXPLANATION

One of the main goals of science is to offer explanations of the facts that observation and experiment reveal. Explanatory efforts stretch to comprise explanations of scientific laws and less general regularities of scientific interest once they have been found.

Being explanations a central aspect of the scientific enterprise, explanatory tactics have been one of the central topics in the philosophy of science during the twentieth century.

In this chapter I review the classical treatments of scientific explanation performed by philosophers during the second half of the twentieth century. My aim is to lay a general context for assessing whether the contemporary mechanismic movement provides solutions to the problems raised during the classical stage of the philosophical study of scientific explanation.

3.1 General theoretical framework: Explanations in everyday life and in science

In everyday life everybody asks for and offers explanations about a variety of matters. A mother may ask her child why he did not arrive home at the expected time; a technician may “explain” the CEO of her company how a certain task is usually performed at her department or why certain processes did not result as expected; a kid may ask his friend what to do when he is before the girl he likes; a teacher may ask a student why dinosaurs went extinct; and so on and so forth. In ordinary life, all the answers to those requirements will be deemed explanations and explanations are important because they work as guides for understanding and behaving. Indeed, in everyday life, we call "explanation" a rather varied collection of conceptual objects as long as they are useful for "understanding" in one way or another. In other words, everyday explanations provide reasons for understanding behavior: why certain states of affairs occur in certain given circumstances, why someone did or omitted a deed, or the rules one should follow to accomplish a given task.

So, in ordinary life, providing instructions, motives, or causes are usually accepted as explanations.

At first blush then an everyday explanation consists in a set of ideas —some kind of *conceptual* device— that provides an *understanding* of some sort about something. Consequently, it would seem that we can define an explanation as something that provides understanding. While the latter notion is certainly related to that of explanation —at least when ordinary knowledge is concerned— understanding is not a notion clear enough to ground a general concept of explanation, that is one that comprises scientific explanation. To begin with, understanding is a psychological notion, not an epistemological one, and while the psychobiological processes behind the phenomenon of understanding might be general for the human species, the results of such processes are strongly contingent on the conceptual —and I might add emotional— framework involved. One person's understanding may be another person's confusion, bewilderment or misunderstanding. An obvious example of this is scientific knowledge. A scientific proposition such as Hutchinson's definition of the ecological niche as an *n*-dimensional hypervolume (Hutchinson 1957) may be rather clear for the trained ecologist, while quite confusing for the nonprofessional. Of course, part of the beauty of science is the assumption that anybody with due training will be able to understand scientific knowledge[7], so there is no in principle obstacle for the nonprofessional's *learning* to understand a given scientific proposition. Yet, the condition of adequate training is what matters here. The reason is that understanding depends on how information is interpreted and this, in turn, depends on the conceptual framework used to interpret that information.

Specific scientific training is one way to endow people with (adequate) constraints for their interpretive frameworks. Among other things, that kind of training is supposed to provide meanings (or, rather, definitions) for a number of terms that the trainee will meet during her years as a student and beyond. More importantly, training is supposed to offer a range of tools for assigning meaning to the new terms that the future scientist is bound to find during her career, and even to create new meaningful terms! Consequently, when looking for intuitions about scientific explanation, we should probably leave everyday explanations and common sense understanding at rest and talk about *scientific* understanding —that is, *scientifically trained understanding*. Of course, this will not erase all the ambiguity in the problem, but it will certainly reduce its scope.

Philosophers of science also have found a diverse collection of scientific conceptual devices to which they ascribe explanatory power. Depending on the particular science and philosopher involved, motives, narratives, causes, functions, laws and/or arguments —though not instructions, in general— have been considered among the various candidates for explaining scientific facts. Indeed, available characterizations of scientific explanations are so varied that one might ask whether each of them provides a different type of understanding and, further, whether those kinds of understanding are all

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[7] At least on a Baconian view of science.

scientific. In any case, the sheer diversity of conceptual structures designated by the word 'explanation' in science would suffice to motivate a philosophical exploration of the notion of a scientific explanation, but the central role of explanation in scientific research makes its philosophical analysis necessary.

3.2. Why scientists explain

Among other motivations, scientists attempt to explain facts out of sheer curiosity. This seems to be the case of distinguished pieces of scientific knowledge such as the theories of evolution and general relativity, which were inspired by their authors' desire for understanding the corresponding portions of the world. Yet, a lot of science is made on the assumption that the resulting knowledge will eventually bear some practical relevance. This does not mean that explaining with an eye on utility is devoid of curiosity, nor that certain explanations of facts inspired by purely intellectual motives may not end up being the basis of useful artifacts[8]. Furthermore, the history of science and technology teaches us that successful explanations have inspired both important conceptual breakthroughs and useful practical applications. For example, a portion of the already mentioned evolutionary theory, that is the theory of natural selection, is nowadays the conceptual basis of several medical norms related to antibiotic resistance, such as the one prescribing not to stop taking an antibiotic before the treatment is completed, and of the prediction that present antibiotics will eventually loose effectiveness (Davies & Davies 2010).

Other examples of putatively explanatory knowledge used for practical

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[8] By 'artifact' I mean any kind of man-made device, either of material or conceptual nature. An example of the former is a mist net for trapping birds, while a case of the latter is an action plan to prevent the Iberian wolf (*Canis lupus signatus*) from becoming extinct (Grilo et al. 2002).

applications, these ones within the realm of ecology, are the species-area relationship and the theory of island biogeography (McArthur & Wilson 1967). The former has been used to set targets for conservation (Desmer & Cowling 2004) while the latter has served as a conceptual framework for studies on the impact of habitat fragmentation on biological diversity, as well as for research in conservation biology in general (Wu & Vankat 1995).

Science can boast having explained the motion of falling bodies, the movements of the planets, ocean tides, the occurrence of new biological species, the regulation of temperature in a variety of organisms, and a long list of other significant facts in all the scientific disciplines. However, there is still no consensus among scientists or philosophers of science with regard to *what exactly a scientific explanation is*. In fact, there is a tendency, nowadays, to admit that different scientific explanations may have different structures (e.g., González 2002), even though different kinds of explanation may not always be of equal value to the scientist (Bunge 1998).

3.3. The philosophy of scientific explanation

The nature of scientific explanation is a subject that occurs in scientific literature only rarely[9], but philosophers of science have paid a lot of attention to the problem, especially as of the beginnings of the second half of the twentieth century. Indeed, the philosophical literature has been the arena of lively debates concerning the role of descriptions of motives, functions, laws, arguments, and causes in scientific explanations attempting to answer questions such as “What is a scientific explanation?” or “Where does explanatory power come from?”

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[9] Ecology, however, is an exception. See Cooper (2003).

A venerable tradition holds that the answer to the first question is that good explanations are always deductive arguments, that causal laws occur among its premises, and that the conclusion of the argument describes the fact to be explained. This is how Aristotle, for example, conceived of explanation (Jeffreys 1971, Losee 2001). However, later authors have emphasized one of the main two aspects of Aristotelian explanation. On the one hand, there is the *deductivist view*, championed, among others, by John Stuart Mill (1843), Karl R. Popper (1935), Carl G. Hempel (1965), and Phillip Kitcher (1981). On the other hand, there is the *causal view* of explanation, championed, for example, by Michael Scriven (1962), Wesley S. Salmon (1984), and more recently, by the so-called new mechanistic philosophers and James Woodward (2003).

Table 3.1 The four submodels of the covering-law model of explanation. (Slightly modified from Salmon 1989: 9.)

Laws in the explanans	*Explanandum*	
	Particular facts	General regularities
Universal	Deductive-Nomological	Deductive-Nomological
Statistical	Inductive-Statistical	Deductive-Statistical

An obvious example of the deductive view of explanation that leaves almost no room for causes is Hempel and Oppenheim's account of explanation, arguably the most influential treatment of scientific explanation so far. Hempel and Oppenheim’s approach illustrated the philosophical tradition known as *logical empiricism*, a contemporary and successor of logical positivism with which shared many views, even though it was critical of some of its theses.

Among the conceptions that logical positivists and logical empiricists shared were the following methodological theses:

(a) *logicism*, i.e., an emphasis in the use of formal tools —especially those of logic and mathematics— for clarifying the meaning of obscure linguistic expressions;

(b) *empiricism*, i.e., the position that deems experience as the main or even the only source of genuine knowledge; and

(c) *antimetaphysicalism*, i.e., the stance that metaphysics needs to be eliminated from good philosophical and scientific discourse.

In a nutshell, the covering-law (C-L) models of scientific explanation —first provided by Hempel and Oppenheim (1948) and further developed by Hempel (1962, 1965)— state that the basic pattern of scientific explanation consists in an argument (*explanans*) whose conclusion is the description of the fact in need of explanation (*explanandum*). The premises of the explanatory argument must include the relevant data and general laws that make it possible to infer —either deductively or inductively— the explanandum (Table 3.1).

Hempel and Oppenheim provide the following example. When one looks at the extreme of an oar submerged in water, it appears to be bent upwards with respect to the shaft[10]. Why is that? The relevant explanation —that is,

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[10] Hempel and Oppenheim's choice of this example is fit for commenting on an interesting aspect of logical empiricism. The authors use the word 'phenomenon' for referring to the fact to be explained (and sometimes use the word 'fact' referring to the datum that describes the fact). Logical empiricism is known for its Kantian inspiration and in Kantian terms, the phenomenon is all we can aspire to know. Yet, this example shows two levels of knowledge, one provided by senses alone (the appearance of the oar being bend) and another provided by the scientific explanation of the appearance. One may ask, then, to what does the explanation refer? To a phenomenon or to a fact? Of course, I am here introducing one of the central debates in philosophy, that between epistemological realism and its anti-realist counterpart.

the answer to the question of why that part of the oar seems to be bent— is obtained by considering some laws and data as premises. Among such premises will be the law of refraction and the law stating that water is a medium denser than air, as well as descriptions of certain antecedent conditions, beginning with the datum describing that the oar was partially submerged.

Summing up, according to Hempel and Oppenheim (1948):

(a) Explanations are answers to "Why" questions and

(b) explanations consist in *subsuming* the fact —the datum describing the fact— in a general law (or a collection thereof), paying attention to the background conditions of occurrence of such fact.

(c) The logical form of the explanatory argument is deductive, whence the name of *deductive-nomological* (N-D) model of explanation usually applied to this submodel of the general covering-law model of scientific explanation (Figure 3.1).

Put differently according to this approach, scientific explanations are answers to questions like this: "According to what general laws and by virtue of what antecedent conditions does the phenomenon occur?" (Hempel & Oppenheim 1948: 136).

Hempel and Oppenheim set several requirements for an explanation to be of the D-N kind and one of them was that the explanandum must be a logical consequence of the explanans. This is important because general laws have no exceptions and the deductive form of the argument guarantee that the truth of its premises is carried over to the explanandum. In Hempel's words:

> Deductive-nomological explanations satisfy the requirement of

> explanatory relevance in the strongest possible sense: the explanatory information they provide implies the explanandum sentence deductively and thus offers logically conclusive grounds why the explanandum phenomenon is to be expected.
>
> (Hempel 1966.)

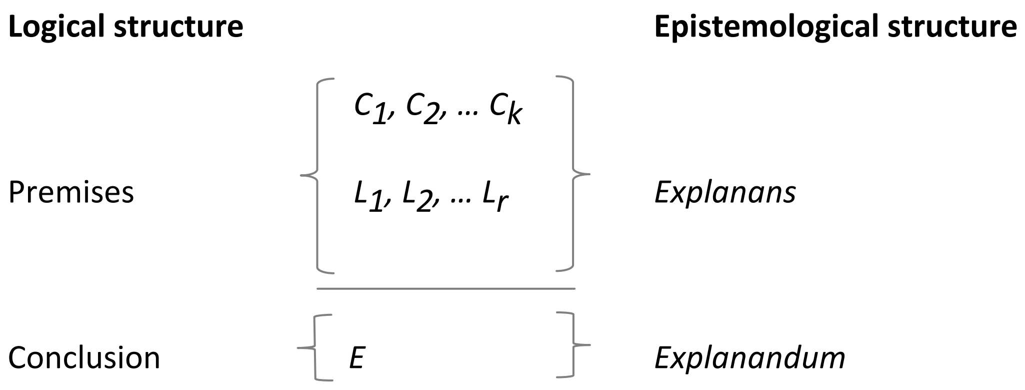

Figure 3.1 A schema of a scientific explanation, according to the deductive-nomological submodel of explanation. C_i stand for the relevant antecedent conditions, L_i represent the relevant general laws, and E symbolizes the description of the phenomenon to be explained. The line that separates the conclusion from the premises represents the deductive jump from the latter to the former. (From Hempel and Oppenheim 1948: 138.)

Thus, the D-N model of scientific explanation identifies the explanatory relation with that of *nomic expectability*, or expectability according to certain general laws. This is the root of the much discussed thesis of the formal symmetry between explanation and prediction: both would have the logical structure of a deductive argument. According to this thesis the only difference between explanations and predictions would be exclusively one of pragmatic character, namely that in an explanation the fact described in the conclusion is known to have occurred, while in a prediction (as well as in retrodictions) the occurrence of that event is still unknown.

The symmetry thesis has an important methodological consequence for research; it provides a powerful justification for theory development because it connects the cultural beauty of explanation with the more mundane interest in prediction[11]. Under this view, the same conceptual device is powerful enough to provide the two key goals of science: understanding and control:

> *It is this potential predictive force which gives scientific explanation its importance*: only to the extent that we are able to explain empirical facts can we attain the major objective of scientific research, namely not merely to record the phenomena of our experience, but to learn from them, by basing upon them theoretical generalizations which enable us to anticipate new occurrences and to control, at least to some extent, the changes in our environment.
>
> (Hempel & Oppenheim 1948: 138, my italics.)

Put in other words, should a theory (understood as a hypothetico-deductive system including laws) be true, when conjoined with the relevant true data it would supply not only an explanation of the phenomenon of interest, but also predictions about it, and both with logical certainty. This, of course, would amount to epistemological heaven and there would be no tension between explanation and prediction in sciences that, like ecology, teem with methodological debates regarding the lack of predictive power of their explanatory models and theories. Pitifully, in spite of its fundamental importance as a philosophical development, the C-L model —especially the D-N submodel— did not fare as well as its proponents thought it would, especially in the special sciences.

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[11] I hasten to add that scientific prediction can play at least two different roles in scientific research: (a) prediction is the main and most powerful tool for testing scientific hypotheses; (b) prediction may suggest courses of action for controlling the behavior of the system under study. Indeed, when combined with the relevant descriptions of goals, prediction can ground technological forecasts and prescriptions.

3.3.1. Classical criticism to the C-L model

Criticism to the C-L model is important because it set the theoretical framework for further discussion of the problem of scientific explanation (Salmon 1984, 1989).

Classical criticism to the D-N submodel was directed mainly to four of its aspects, usually presented as issues related to the necessity (of laws and inferential form) and the sufficiency (for not including temporal order or relevance constraints) of its requirements for a good scientific explanation. Those attacks came principally from philosophers who approach explanation from a different point of view, namely one that considered causes to be central to explanation.

3.3.1.1. On the necessity of laws

Critics questioned not only the N-D submodel of explanation but the whole covering-law model, for not requiring the description of causes. In their view, it is not the property of nomic expectability what makes a model or theory explanatory, but the description of the causal processes that lead to the fact to be explained.

One proverbial counterexample against the need for laws in the explanans offers a causal narrative as an alternative for explaining singular facts: "The curtains brushed against the vase, *thus* knocking it over." (Scriven 1962: 53). This example was designed to show that laws are not necessary and that Hempel and Oppenheim’s contention that explanations need to be something else than descriptions —that is, arguments— is not correct. Scriven believed that not only are laws not included in his explanation of the fall of the vase, but also that if the statement is explanatory at all the reason is that it *is* a description, more precisely a description of the cause of the fact that to be explained. Accordingly, from this point of view, the relevant question regarding the role of description in explanation is not *if* descriptions can have explanatory power, but *when* and *why* descriptions explain. The short answer is that explanatory descriptions are characterized by including descriptions of the causes that bring about the fact to be explained.

Hempel’s answer to the foregoing criticism was based on the notion of an *explanation sketch* and dealt with D-N explanations (Hempel 1942, 1965), but its validity carries over to the more general C-L model as well. While it is true that real scientific explanations do not always have the form proposed by the C-L model, this is because those explanations are sometimes formulated in an elliptical or incomplete manner. This is not a problem for the C-L model if one recognizes that those are explanation sketches that once fulfilled would become ideal C-L explanations. According to him, the N-D submodel suggests an ideal that could function as a criterion of perfection or maturity for real scientific explanations.

Thus, while Scriven's example of the curtains and the vase does not *explicitly* include any law and, consequently, it cannot have the form of a deductive argument, it would include laws in an *implicit* manner. The fact that we understand the falling of the vase *as a consequence* of the curtains brushing them is due to *the implicit assumption that this is a particular case of a regular type of events*[12]. Ultimately, the argument goes on, Scriven's counterexample assumes the lawful principle that “Same cause, same effect”. This assumption then, not the effective knowledge of the relevant law, would be a requirement for explaining. In Hempel's own words: "[t]o say that an explanation rests on general laws is not to say that its discovery required the discovery of the laws" (Hempel 1966: 243). Similarly, other examples leave unmentioned certain assumptions regarding the prevailing physical conditions in which the phenomenon takes place. This is the case in Hempel's (1966) popular example of the explanation of puerperal fever by the unfortunate Austrian physician Ignaz Semmelweis, the "savior of mothers".

Semmelweis explained the unusually high proportion of deaths occurring in the First Clinic of the Vienna General Hospital as an effect of blood-stream poisoning by decomposed animal matter through open wounded surfaces[13]. This explanation does not explicitly invoke any law, but the fact that contamination *generally* causes puerperal fever was taken for granted by the Austrian physician, besides of what the generalization is needed to account for the deaths. In sum, Hempel's answer was that "[a]s the preceding example illustrates, corresponding general laws are always presupposed by an explanatory statement to the effect that a particular event of a certain kind *G* […] was *caused* by an event of another kind *F*" (Hempel 1966: 243).

Yet, as I have already mentioned above, the need for general laws has

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[12] In fact, there is much more at play than the assumption that particular causal statements presuppose general causal statements. Hempel's rejoinder is the tip of the iceberg regarding the problem of the nature of causes. I will discuss this topic later in the present chapter. For the time being, suffice it to say that Scriven's criticisms presuppose a notion of cause that the logical empiricists did not share.

continued to be challenged by different authors, especially in connection to two important events: the rising of quantum theories and a change of mind in the philosophy of science, which started to pay attention to sciences different from physics with a fresh eye.

The adequacy of the D-N submodel for disciplines where laws (or less general generalizations) are statistical or even irreducibly probabilistic was also put into question.

3.3.1.2 On the sufficiency of the C-L conditions for an explanation

Attacks on the sufficiency of the conditions for an adequate explanation provided by Hempel and Oppenheim include several counterexamples attempting to show problems related mainly to temporal order and explanatory relevance that the C-L model cannot handle while causal explanations do not seem to face.

Problems of temporal order relate to the symmetry thesis associated to the three C-L submodels of explanation that have a deductive structure (see Table), which imposes no temporal constraints to scientific explanations. A familiar counterexample to the symmetry between explanation and prediction is that of predicting a storm on the grounds of a sharp drop in the reading of a barometer and some generalizations to the effect that sharp

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[13] After discarding a priori several alternative hypotheses, the dead of a colleague who had cut his finger with a (contaminated) scalpel allowed Semmelweis to discover a tragic difference between the First and the Second Obstetrical Clinics of the Vienna General Hospital. While the First Clinic was a training ground for medical residents who often performed dissections of dead bodies before examining patients, in the Second Clinic examination was in charge of midwives who did not practice dissections. Semmelweis conjectured that poisoning of the blood was the cause of the deaths that worried him so much. (For more on Semmelweis's investigations and ill fate see, e.g., Hempel 1966; Carter & Carter 2005.)

drops in barometer readings are followed by storms (Salmon 1989: 47). The issue here is that C-L explanations do not provide any guidance for not interpreting them the other way round. In the example, there is nothing in the C-L model that prevents explaining the storm invoking the change in the barometer. Of course, we know that neither the storm explains the drop of the reading in the barometer nor the latter explains the former. Rather, atmospheric conditions cause —and thus their description explain— both the barometer reading and the storm. The easy way to solve this problem is, of course, to recur to a causal form of explanation, at least if one accepts the *principle of antecedence* (Bunge 1959) usually assumed to be built in the notion of causality, that is that causes always precede their effects.[14] Thus, once again, it seems that causal explanation can handle cases that are problematic for the N-D model.

A different kind of criticism points to the possibility of including nonrelevant premises in the explanans, as shown in the following counterexample due to Henry Kyburg (Salmon 1989: 59). A tablet of salt put in water dissolves a few moments after a sorcerer casts a dissolving spell upon the tablet. Then the event is explained by saying that all hexed samples of salt dissolve when placed in water. Adequately organized, the knowledge about this fact can be given the form of a D-N explanation. So, why did the salt dissolved? Scientist

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[14] A safer claim, though, is that effects are never prior to their causes, a statement that allows for the possibility of effects and their causes being simultaneous. There are reasons to be cautious about the direction of causation (see, e.g., Schaffer 2008), though I don't think that backwards causation arguments hold any water. A more plausible possibility is that of simultaneous causation, as proposed by the argument in which an iron ball depresses a cushion as the former sits in the latter. Yet, I believe that this argument relies in a misdescription of the event, and that our identification of causes and effects is likely to be too coarse-grained to be reliable. Incidentally, grain is one of the reasons why I favor the mechanismic approach to explanation. Being descriptions of processes, mechanismic hypotheses are not as ambiguous as merely causal ones. But, again, I don't want to jump the gun. I will discuss the metaphysics of causation in Chapter 4.

are likely to answer that all salt samples, *whether hexed or not*, dissolve when placed in water. Consequently, hexing does not seem to be relevant for explaining the dissolution of the salt tablet.

A number of critics of the C-L model of explanation believed that the answer to the philosophical problems of scientific explanation was in some form of causal model. Even though Hempel answered to some of his critics that many scientific explanations of particular facts do not involve universal laws among their premises but statistical laws. Thus, Hempel introduced his inductive-statistical (I-S) submodel of scientific explanation, distinguished from the other three submodels of the C-L by its inductive structure.

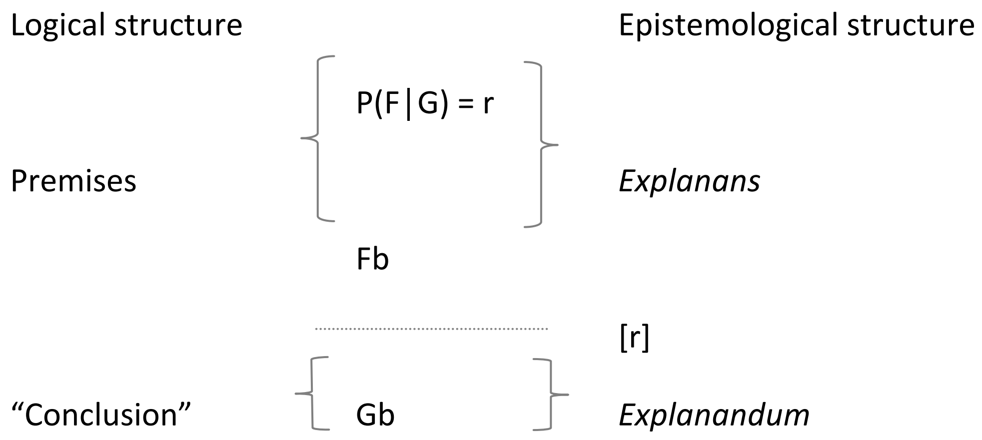

Figure 3.7 Schema of a scientific explanation according to inductive-statistical version of the C-L model. P(F|G) = r is a statistical law asserting that the relative frequency of Gs among Fs is r, where r is fairly close to 1, while Fb is description of the conditions. The dotted line that separates the explanandum from the explanans represents the inductive jump from the latter to the former. [r], at the right of the dotted line, stands for the probability assigned to the “conclusion” on the premises.

The I-S submodel faced problems different to those affecting the deductive versions of the C-L model. One of these issues is what Hempel (1962) called the *epistemic ambiguity* of I-S explanations, and it stems from the fact that inductive arguments lack certain important properties that deductive arguments possess. One of such properties is that of *transitivity*, i.e., the property of a deductive argument of maintaining its validity when new premises are added to it (and none of the original premises is deleted). In a deductive argument, if *A* entails *B*, then *A.C* entails *B*, whatever *C* stands for. This is not the case with inductive arguments. Since the latter are content-dependent, the addition of new premises may affect the strength with which the conclusion is drawn, and may even turn the conclusion upside down. In other words, inductive arguments that strongly support one "conclusion" can be transformed by the addition of just one more premise into inductive arguments strongly undermining the original "conclusion".

To make his point Hempel (1962: 124) uses the following example. John Jones is certain to recover from a streptococcus disease once he has received a penicillin shot, because in *almost all* such cases infections subside rather quickly upon penicillin administration[15]. Here, of course, the statistical law is hidden behind the "almost all" generalization, which refers to the high effectiveness of penicillin to clear the disease in those cases. This means that there are cases of persons infected with streptococcus in which penicillin administration is not followed by the recovery of the patient. Hempel draws attention to the collection of characteristics (properties) of John Jones, such as his age, sex, blood pressure, etc. The list can be very long. Should one of this features be associated with non recovery in an "almost all manner", one could build an explanation with the same form of that used to explain (and predict) John Jone's recovery. However, the "conclusion" of such an argument would be the negation of the "conclusion" of the former. For clarification, we

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[15] Note that this example, besides not referring to a scientific fact, is not really about explanation, but about anticipation and thus presupposes the symmetry thesis. In fact, this case presupposes subthesis (ii) described in Subsection 2.2.2, that is that all predictions are C-L explanations.

can turn to Salmon's discussion of the example.

Salmon (1989: 54) adds one more characteristic to the list of John Jones’s features, namely that of being infected with a penicillin-resistant strain of streptococcus. It is clear that now the probability of Jones's recovery is very different from that of his recovery in Hempel's example. The reason is that *almost all* patients with infections of penicillin-resistant strains of streptococcus do not improve upon receiving penicillin treatment. So, what can we do to protect I-S explanations from this epistemic ambiguity infection?

Hempel's attempt to cure his I-S model took the form of a new requisite, namely that the *reference class* of all C-L explanations be maximally homogeneous[16]. This condition, that he called *requirement of maximal specificity* (RMS), is a weaker version of the requirement of total inductive evidence that logicians had devised for solving the same problem in inductive arguments. Hempel does not require total evidence because, in order to satisfy such a condition, the premises of an explanatory argument should include all known facts, among them the *explanandum*. This, of course, would leave us in the awkward position of having to explain a datum invoking that very same datum as a premise.

RMS is automatically satisfied by all deductive explanations thanks to their universal laws, whose reference classes are always homogeneous, since they include all possible (past, present, future) instances. In contrast, in I-S explanations, RMS prevents us to lump together cases such as an infection by streptococcus vulnerable to penicillin treatment and an infection by penicillin-

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[16] I will discuss the problem of reference classes in statistical explanations in Section 3.3.2, when I discuss Salmon's statistical-relevance model of explanation

resistant streptococcus. This dependence on knowledge makes I-S explanations essentially relative to the chosen reference class. Hempel called this characteristic of I-S explanations their *epistemic relativity*, a feature that is absent from deductive explanations, but is unavoidable in inductive ones.

The I-S submodel is also vulnerable to the problem of explanatory relevance we have already discussed for the N-D submodel. A couple of examples provided by Salmon in the early sixties should suffice to show how. In both examples the proverbial John Jones sacrifices on behalf of the philosophy of science. One of the examples is this: “John Jones experienced significant remission of his neurotic symptoms because he underwent psychotherapy, and a sizable percentage of people who undergo psychotherapy experience significant remission of neurotic symptoms” (Salmon 1989: 58-59). The problem with this case is that the explanation invokes a reason that may well be irrelevant for accounting for the *explanandum*. Neurotic symptoms are known to subside without treatment, hence the uncertainty as to the relevance of psychotherapy, the element allegedly contributing the explanatory power. In other words, when offered an explanation similar to the previous example one may reasonably doubt that the reason put forth as explanatory actually made any difference. This was precisely Salmon’s line of thought: what matters for explanation is not nomic expectability: “What is crucial for statistical explanation [...] is not how probable the explanans renders the explanandum, but rather, whether the facts cited in the explanans make a difference to the probability of the explanandum” (Salmon 1989: 59).

3.3.1.3 On the requirement of high probability of the *explanandum*

Still another problem with the I-S model is Hempel's requirement that the explanandum be obtained with high probability. As Salmon (1989) points out, this condition seems natural when I-S explanations are seen as only slightly different from deductive ones, with an eye on nomic expectability. However, imposing this new requisite to I-S explanations carries with it a new problem, for it puts a limit to the possibility of explaining events with low or extremely low probabilities. Let us imagine with Salmon (1971: 9) the need of explaining the case of an alpha particle not tunneling through the potential barrier of the nucleus of an atom undergoing radioactive decay. This is an event with an associated probability as low as 10^{-38}, so it is clear that the requirement of high probability is of no help in this case.

The difficulties of the I-S submodel in accounting for statistical explanations —especially, with its high probability requirement— were one of the most serious limitations of the Hempelian general model leading to further developments in the theory of explanation. In the following section, I will review Salmon's own response to this problem in the form of an account of explanation dispensing with one of the most essential commitments of the received view of explanation, one that Salmon called "the third dogma of empiricism", namely the thesis that scientific explanations are arguments of sorts.

3.3.2 Salmon's statistical-relevance model of scientific explanation

The motivation behind the requirement for a high probability of the explanandum was Hempel's construal of the explanatory relation as a relation of nomic expectability, an approach seriously threatened by admitting *explananda* whose nomic expectability is low or very low as a consequence of including in the *explanans* probabilistic laws that assign low or very low probabilities to the events they "govern". Moreover, statistical claims abound in the sciences where "variability" —as biologists usually call the diversity in the properties of the entities and processes they study— is important. And there seem to be too many facts in the sciences that obtain with low or very low probabilities.

In the previous section I described the case of an alpha particle escaping tunneling, but there are further examples belonging in disciplines other than quantum physics, and some of them have become standard in philosophical literature on explanation since their introduction. One of them is Salmon's example of troubled John Jones attributing the remission neurotic symptoms to psychotherapy because sizable percentage of people who undergo psychotherapy experience significant remission of neurotic symptoms (Salmon 1989: 59). This case was designed to illustrate two of the drawbacks of the Hemepelian treatment of explanation, the one concerning the explanatory relevance of the premises and the other posed by low probability events. Since neurotic symptoms are known to subside spontaneously, the treatment invoked as accounting for Jones recovery may not be the true explanation of the event described in the *explanandum*. In the first place, as we may recall from Kyburg's “hexed salt” counterexample described in Section 3.3.1.2, the problem of explanatory relevance was already present in the D-N model of explanation. Besides this, the example illustrates the problem of epistemic ambiguity of statistical explanations alluded by Hempel. Both, Jones recovery and his lack thereof can be “explained” by the same set of premises including a statistical law describing the number of cases that, in general, experience improvements after psychotherapy treatment[17].

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[17] However, there is a deeper problem here, one related to the meaning of such statistical claims and to how one construes probability statements. Certain quantum facts are assumed to be irreducibly probabilistic by quantum theories, but statistical claims such as the one used to explain Jones' recovery from his neurotic symptoms, do not need to be construed as irreducibly probabilistic. Besides, there is nothing cogently pointing to probabilistic interpretations in the psychotherapy framework. Thus, a further question I want to ask is “Where does the statistical nature of the claim come from?” My favored answer is that statistics here plays a methodological role: it allows us to deal with complex facts whose occurrence can take place through varied paths. In any event, this is one of the ways statistics enter ecological knowledge and I will be saying more on this in Chapter 6.

In consonance with the problem of explanatory relevance, the case of the alpha particle not tunneling through an atomic nucleus identifies another problem. It shows that the crucial question for an explanation is not if the explanandum obtains with high probability, but whether the facts described in the *explanans* make a difference for the probability of the event described in the *explanandum* (Salmon 1989: 59). We already saw this problem in Scriven's paresis counterexample against the symmetry thesis (Section 3.1.2 of this chapter).

Most scientists know very well the practical aspects of the problem of relevance. It boils down to eliminating the so-called confounding factors in order to identify the one responsible for the fact under study. In ideal circumstances, the most powerful single way to sort out the irrelevant factors in order to identify the relevant ones is to conduct experiments (for the different roles of experiment in ecology see, for example, González del Solar & Marone 2010). The most simple form of this research tactic involves establishing two types of randomized samples: one in which the factor —represented by the “independent variable”— that researchers suspect is responsible for the fact under study —represented by the “dependent variable”— is manipulated and another in which such factor is not manipulated, but is otherwise identical to the former. The rationale behind experiments is the maxim we already encountered while discussing Scriven’s criticisms to the D-N model: “Same cause, same effect” and in the fact that the researcher produces the cause by manipulation (or intervention). This can be stated in a more cautious form saying that most experiments are based on the assumption that certain regular, manipulable metaphysical dependences exist between certain facts. Of course, the assumed metaphysical dependence is more often than not a causal relation, but stochastic mechanisms can also be studied by means of experiment. Yet, should all objects and facts be identical there would be no need to use statistics. Statistical tools come into the scene because of two aspects of real-life experiments. In the first place, when quantitative variables are involved, one may ask how different treatment and control have to be to consider them different. In the second place, when experimental results are not exactly the same, that is when there is “variability” (variation) the researcher typically resorts to statistical comparisons. Thus, when experimental error and “variability” are taken into account, statistics enters the scene. Results in treatment samples are compared to those of control samples through some statistical test —e.g., a chi square goodness of fit test— in search for statistically significant differences. If found, such differences are usually interpreted as indicative of the explanatory relevance of the factor tried in the treatment. It is important notice the role of different kinds of errors in experimental (and all empirical) work. Among them, statistical errors associated to the acceptance (or rejection) of the null hypothesis are especially significant, because they work as terminals or knobs that the researcher can manipulate to different purposes (see Shrader-Frechette and McCoy 1993 for examples in ecology).

Experiments, however, are not always possible or ethically acceptable, circumstances that are frequently the case in ecological studies. More importantly, practical solutions at the scientific level may certainly guide philosophical thinking and even suggest solutions for some philosophical problems, but they do not solve all of the latter. Thus, in the case of explanatory relevance, there is philosophical work still to be done. This is precisely what Salmon (1971) attempted to accomplish with his statistical-relevance (S-R) model of explanation.

As Salmon notes, a high probability in obtaining the explanandum is neither necessary nor sufficient for establishing the explanatory relevance of an item in the *explanans*. It is not necessary because there are cases that need explanation —like that of the alpha particle— in which low probabilities are involved. It is not sufficient because —as John Jone's recovery from cold upon having taken vitamin C shows— there are cases in which more than one factor can account for the explanandum with high probability. Thus, the corresponding criterion must be some other. Salmon puts forth statistical-relevance as such criterion and builds a completely new model of explanation upon it. This is the model we are about to discuss now.

Let us imagine with Salmon that an Australian, 30 year old man has a high probability of surviving to age 35 and that an Australian, 30 year old man that suffers from lung cancer has a low probability of surviving to age 35. In this case, as in those of John Jones's paresis and streptococcus infection, it is easy to note that the reference class of the explanation is not homogeneous. It is possible to partition the class of Australian, 30 year old men (A30) into two subclasses, or cells, one containing A30 that do not suffer from lung cancer and another class containing those A30 that do suffer from lung cancer. In Hempel's terms, A30 is not a maximally specified class. Once we make a relevant partition —i.e., a partition allowing for the factor relevant to the probability of the explanandum— we understand why some events obtain with high probability and why some other do not. In other words, applying Hempel's RMS shows why the relevant fact was to be expected with high probability. Salmon's contribution is a method for performing relevant partitions of the explanans and the explanandum such that (i) all relevant factors, but no irrelevant one, are included in the explanation and (ii) the key explanatory relation is the difference between the prior probability —that of the original reference class— and the posterior probability —the one of the maximally specified reference class after all relevant partitions have been made until achieving an subjectively homogeneous reference class. Besides, Salmon's proposal differs in important ways to that of Hempel's. In the first place, Salmon's approach departs from the view that statistical explanations are arguments. Once the relevant partitions have been made, the explanatory relation is seen to consist in the difference between prior and posterior probabilities, not in the nomic expectability of the explanandum. Once nomic expectability is out of the game, there is no need for a statistical explanation to be an argument.

Yet, as Salmon himself admitted some time later, statistical relevance did not provide answers for a number of issues arising from scientific explanations. In particular, since S-R “explanations” do not tell us *why* or *how* something is the case they do not supply a true model of *explanation*, but a useful criterion for identifying relevance classes. Besides, S-R relations cannot be used to develop an account of theoretical explanations. Thus, the S-R account leaves the task of building a theory on explanation unconcluded, a task that Salmon and other philosophers of science attempted to fulfill by paying attention to the metaphysics of explanation. Such attempts are the subject of the next chapter.

3.3.3 Explanation as unification

Our next general account of scientific explanation is that offered by Philip Kitcher, which goes in precisely the opposite direction to that we have been discussing in the preceding subsections. The unificationist approach concurs with the Hempelian one —more precisely with the deductive-nomological account— in considering that explanations are (i) always deductive, (ii) arguments, and (iii) that a theory of explanation should not make metaphysical commitments.

In Kitcher’s view, the main task of a theory of explanation is to provide *a characterization of genuine relevance relations*, thus delimiting the class of genuine why-questions. Such genuine relevance relations are, according to Kitcher, those derivations that best systematize the available knowledge involved in the answer to a certain why-question. The proposed criterion for systematization is, in turn, unification, that is, an explanation must provide “the best trade-off between minimizing the number of patterns of derivation employed and maximizing the number of conclusions generated...” (Kitcher 1989: 431). In Salmon’s terms, then, the unificationist account is an “epistemic” view of explanation, just as Hempel and Oppenheim’s D-N account. The main difference between them is that while in the D-N arguments are viewed as pairs of elements (i.e., premises and conclusion), Kitcher views an explanatory argument as “a sequence of statements whose status (as a premise or as following from previous members in accordance with some specified rules) is clearly specified” (Kitcher 1989: 431).

Now, prior to delivering his analysis of explanation, Kitcher investigates the pros and cons of what he deems to be its main rival. With C-L models under heavy philosophical fire and being itself a deductivst account, the rival of the unificationist view is of course the causal view. Kitcher assess of Salmon's causal account of explanation finds that while it has obvious merits — especially at handling problems of asymmetry and irrelevance— there are some serious objections that causal accounts have still to overcome if they are to provide the best account of an ideal explanation. We have already studied Salmon's efforts and some of the problems related to the nature of the causal relation, so let us now list those objections that Kitcher deems paramount.

(i) The first objection is, of course, the lack of an adequate (empiricist) analysis of the causal relation. We have already seen that the theory of mark transmission went through so much trouble that Salmon eventually resigned it in favor of Dowe's theory of conserved quantities. The latter, however, has not managed to convince those who claim that causation involves no necessary physical connection (e.g., Schaffer 2004).

(ii) A second problem relates to the causal account being restricted to events that have causal histories, a feature that excludes from the domain of scientific explanation all explanations offered by the formal sciences.

(iii) A further limitation of the causal approach is that it is restricted to explanations of singular events. Theoretical explanation places the challenge to show how some laws are more fundamental than others. Furthermore, causal explanation theorists are likely to answer that explaining generalizations should involve “the identification of mechanisms that are at work in all the cases covered by the regularity”, so they should provide more precise characterizations of “what is for a mechanism to be at work in an event, state, or process” (Kitcher 1989: 429). We will have to wait for Chapter 4 in order study the main attempts to make the notion of a mechanism and the role of mechanisms in explanation more precise. For now, we will continue to expound the unificationist account, which according to Kitcher (1981) had been an “unofficial” conception of explanation (the “official” one being Hempel’s C-L model) that empiricists such as Feigl (1970) and, especially, Friedman (1974) had promoted or defended occasionally. Kitcher (1981, 1985, 1989) intends not only to solve the four main objections to the Hempelian C-L model without parting with the empiricist tradition in philosophy of science —in particular, taking seriously the results of Humean analyses of causation (see Chapter 4, Section 4.2)— but one that is superior to causal approaches too.

Taking lead from previous work done by Michael Friedman, Kitcher (1989: 432) states that “Science advances our understanding of nature by showing us how to derive descriptions of many phenomena, using the same patterns of derivation again and again, and, in demonstrating this, it teaches us how to reduce the number of types of facts we have to accept as ultimate (or *brute*)”.

Kitcher’s proposal, then, illustrates what he has called the top-down approach to explanation, a type of account that considers theoretical explanation, instead of singular explanation, as primary. Thus, in Kitcher's account the analysis of theoretical explanation comes first, and it is then used “as a basis for underwriting talk about ‘fundamental mechanisms’ and so proceed toward the identification of causes in particular cases” (Kitcher 1989: 430.) In contrast, the bottom-up strategy adopted by causal explanation theorists deems causal explanation of singular cases as the source from which theoretical explanation springs. In sum, while the causal approach implies to ground explanation on causal claims, the unification approach proposes to ground causal claims on explanation.

Kitcher asserts that ideal explanations are derivations. More precisely, an acceptable ideal explanation consists in a derivation that must belong to the explanatory store —or “reserve of explanatory arguments” (Kitcher 1981: 332)— over the set of statements endorsed by the scientific community. In order to be able to follow Kitcher’s account, with a few more notions besides those of “explanatory store” —or $E(K)$— and “set of statements endorsed by scientists” —or K—. Let us list them here.

(i) *Schematic sentences* are sentences in which certain terms have been replaced by dummy letters.

(ii) A *schematic argument* consists in a set of schematic sentences.

(iii) *Filling instructions* for a schematic sentence are directions indicating what term each dummy letter stands for.

(iv) A *classification* for a schematic argument consists of a set of statements describing the inferential characteristics of the argument, especially which statements are to be considered as premises and which one is the conclusion.

Now we are sufficiently equipped to characterize Kitcher's account of explanation, for a *general argument pattern* (or *explanatory schema)* is characterized by a triple composed by a schematic argument, a set of sets of filling instructions, and a classification for a schematic argument. We should prefer those argument patterns that are applicable in a larger number of cases. For example, the reader may remember the case of the wizard “hexing” a salt tablet, putting it in water and then explaining the tablet's dissolving as a result of the fact that it was hexed. According to Kitcher (1989: 482), if we wanted to assess the explanatory power of this explanatory schema, we should compare it with, for example, that offered by the molecular view of solubility. What we would find is that the latter pattern of explanation applies to much more cases than the “hexing” one. In other words, while all cases of “hexed” solubility would also be explained by the molecular approach, the “hexing” pattern would not explain all those cases of solubility involving material that has not previously “hexed”. Besides, should we be confronted with a “hexed plus molecular view” pattern, we would also have reason to prefer the molecular view schema alone, for it is more economic. Then, in this particular example, the molecular view pattern offers the best systematization of the knowledge available. In other words, the molecular view pattern provides the best trade off between a large number of consequences and a small number of explanatory principles.

Similarly, in van Fraassen's example where the height of a tower is explained by the length of its shadow —an example similar to that of a flagpole and its shadow— the shadow pattern fares worse than, for example patterns that Kitcher called “of origin and development”. One reason is that origin and development patterns are applicable also to objects that do not cast a shadow, as in the case of transparent ones.

3.4 Concluding remarks

With the C-L model of scientific explanation first published in 1948, Carl Hempel provided the backbone of the discussion on explanation for no less than the three subsequent decades (Salmon 1989). The C-L model is actually a supermodel that comprises four models or submodels, whose central epistemological thesis is that facts are explained when their descriptions are shown to be the conclusion of a correct argument. Such explanatory argument is deductive in the case of D-N and D-S explanations, while it is inductive in I-S ones. In all four versions of the C-L, the premises of the argument (jointly called *explanans*) must include at least one true law of nature. This law is a universal law of nature in the case of the D-N submodel and statistical (probabilistic) law in the I-S one. In the case of the explanation of “particular facts”, further premises must be included describing the conditions in which the fact to be explained occurs. It is a requisite for a good C-L explanation that both laws and data in the premises must be true. I-S explanations have two further requisites: (i) the “conclusion” of the argument must obtain with high probability and (ii) I-S explanations must conform to the requirement of maximal specificity.

While C-L models have arguably been an important stimulus for research on the philosophy of scientific explanation, there are certain problems that are not generally accepted as solved, especially those of symmetry and explanatory relevance. Besides, some critics attacked the C-L model at a more fundamental level, the role of causation and pragmatics in explanation being two of such criticisms. I will devote the largest part of Chapter 4 to the study of causal explanation and causation. Causation will still be important in Chapter 5, where I will describe a type of explanation that is akin to causal explanation in many aspects and has gained popularity in the last decades, the so-called mechanistic type of explanation.

4

A JOURNEY TO THE METAPHYSICS OF EXPLANATION. THE CAUSAL STRUCTURE OF THE WORLD

> *The great phenomenon of nature, the revolutions of the heavenly bodies, eclipses, comets; thunder and lightning, and other extraordinary meteors; the generation, the life, growth, and dissolution of plants and animals; are objects which, as they necessarily excite the wonder, so they naturally call forth the curiosity of mankind to inquire into their causes.*
>
> Adam Smith (*The Wealth of Nations*, 1776)

4.1 Introduction

In Chapter 3 I decribed the first formal model of scientific explanation ever, the so-called covering law (C-L) account originally proposed by Hempel and Oppenheim (1948). In C-L explanations, explanatory power would ultimately come from the laws in the *explanans*, which are the main basis of nomic expectability, the trade mark of C-L explanations. In Chapter 3, I also reviewed the main objections to the different versions of the Hempelian model of explanation. As the reader will recall, the main targets of the attacks were the sufficiency and/or necessity of the requirements for C-L explanations established by Hempel and Oppenheim (1948) first and then by Hempel (1962, 1965). According to such criticism, explanations would not need to be arguments, because they need not predict their *explananda* —neither with certainty nor high probability. The main upshot of the debate is that nomic expectability may well be a relation of predictive relevance, but it is not a relation of explanatory relevance.

A further challenge to the C-L model relates to the need for the truth of the premises in the *explanans*. Nowadays it is widely admitted that, in general, theories and models are largely false —ecological theories and models certainly are false to an important extent. This circumstance, however, does not necessarily prevent those theories and models to be explanatory (see, for example, Frigg & Hartmann 2012), a fact that suggests the question of how false theories and models can possibly explain. For example, how can a conceptual device such as the Lotka-Volterra model for biological interactions, with all its abstractions and idealizations —or, in other words, with all its distance from truth— still explain, say, predator-prey behavior?

Returning to criticism to the C-L account of explanation, the other group of objections was directed to showing that being an argument with true premises, general laws, empirical content, and a true conclusion, is not sufficient to be an acceptable scientific explanation. In other words, something relevant to explanatory power seems to be missing in the C-L model. Not surprisingly, the strongest alternative to the C-L model of explanation held that all that explanations need to be is *descriptions of causes*, more precisely, descriptions of the causes that bring about the fact to be explained.

If this were so, causes would solve all, or at least most of, the more serious problems encountered by the C-L account of explanation. Why, then, did Hempel, Oppenheim, and other deductivists resist including descriptions of causes in their explications of scientific explanations? The answer lies in the work of the English empiricist philosopher David Hume, so it will be convenient to look at what he had to say about causes.

4.2 Of Humean bondage[18]

David Hume's analyses of a number of philosophical notions are now an integral part of the history of Western thought. Without them, a substantial portion of the views developed by such important thinkers as Immanuel Kant, Bertrand Russell, and the logical empiricists would be difficult to understand. One of the notions Hume analyzed, central to both everyday and scientific thinking, is that of causation or the relation between a cause and its effect. Much of the philosophical work on causation done in the twentieth century is an attempt to improve or rebut Hume's arguments on the subject.

While there is some controversy on the precise interpretation of Hume's results on the problem of causation, I will not undertake here the extremely difficult task of investigating this topic[19]. Rather, I will take for granted the most accepted view on Hume's analysis of causation because that is the one the logical empiricists, particularly Hempel, adopted making it relevant for our study of scientific explanation.

Causes had been invoked as explanatory items ever since Antiquity, and perhaps before that. Aristotle himself rooted all understanding in the knowledge of four kinds of causes, but things started to change when British

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[18] I took this wonderful title that paraphrases that of Somerset Maugham’s novel —*Of Human Bondage* (1915)— from a paper by Christopher Hitchcock (2003) on causation.

[19] One of the reasons of the diversity of ontological interpretations of Hume's results is linked to the motives that led him to investigate the notion of a cause. According to some authors, Hume's was not an ontological investigation, even though it had ontological import. His was an epistemological and psychological exploration of the way human beings justify their beliefs. Hume's analysis has been interpreted in at least three alternative ways. According to the logical positivists and their Berlin counterpart, the logical empiricists, Hume's viewed causality as perceived-constant conjunction; this is the (perceived-) regularity view of causality. A second interpretation is that of the so-called skeptical realists, who stress that Hume recognized the necessity of causal connections, but since such connections are not observable, he was skeptical about them. For a general review on this topic, see, e.g., De Pierris and Friedman (2008).

empiricists attempted to elucidate the notion of a cause and found no purely logical or purely empirical basis to ground judgments of existence of such a thing as a causal relation.

In particular, Hume's skeptical solution to the problem of causation provides the framework of the notion of causation as nothing else than *regular conjunction* embedded in the Hempelian model of “causal” explanation, as Hempel and Oppenheim named originally their C-L account of scientific explanation.

In the first edition of his *A Treatise of Human Nature*, published in 1739, Hume investigates causal reasoning as a task for developing his theory of human nature. He is concerned with how human beings acquire the capacity to infer causes from effects and vice versa (Bell 2009). Hume's proverbial example for this analysis involves two billiard balls and a table. One of the balls is moving across the table and the other one is static until the former hits it and the second ball acquires movement. Thus, one can say that the first ball makes the second move or that the first one *causes* the movement of the second.

As we have just said, Hume was interested in the grounds for causal inference. Now, one of the results of Hume's analysis is that formal reasoning alone cannot be the basis of causal inference. In the first place, effects cannot be deduced from their causes. From a purely logical point of view, anything can follow from the datum that a ball moves across the table towards a second ball and hits it. Indeed, while we may expect that the second ball starts moving when hit by the first one (assuming that the latter moves at certain speed), it could be the case that the second remains unmoved. It would suffice to have it fixed to the table —by means of a screw, for example— for this case to obtain. Furthermore, if this was the case and we did not know it was, the second ball will not move and, after hitting the fixed one, the first ball could move back against our expectations.

In the second place, the nature of the cause cannot be inferred from its effects either, as is shown by the fact that we can assign several alternative causes to one and the same effect. A standard example is that of a fire in a building. We may speculate that the fire was started by a short circuit or by a lit cigarette carelessly thrown into the paper bin, or by many other *possible* causes. However, we will not learn what the cause was unless we empirically investigate the source of the fire. This is, in fact, one of the motivations underlying scientific as well as other kinds of factual research: when we want to know the causes of a certain phenomenon, we conduct empirical research. We know there are several possibilities, but deduction alone cannot provide us with the actual cause of a phenomenon, for logic itself does not pose any constraint on possibility; therefore, formal thinking alone cannot reveal the nature of causation. In sum, causal inferences *if* based on a connection between cause and effect, are not ground in logical necessity.

Still, we do make causal inferences once experience has shown us that there is a regular conjunction of two events. Thus, Hume turns to the empirical approach in search of the nature of the causal connection. Could the causal relation be an observable physical connection? Hume's answer is that it is not. Returning to the example of the billiard balls, there is no observable connection between the two impacting balls, nothing observable apart from the table, the stick, and the balls themselves —moving or not. Yet, we want to say that one ball moving after being hit by another is a case of causation. What, then, is causation? It is not a certain kind of logical relation, nor is it an observable connection. But still is some sort of connection. What is then the "mysterious connection" between a cause and its effect?

In his "empirical"[20] investigation of causal relations, Hume finds three features that are always present when causation occurs, namely spatio-temporal contiguity, temporal priority, and constant conjunction, nothing else. Why, then, do human beings expect certain effects from certain causes? The reason, Hume answers, lies in induction, a habit of the human mind that consists in expecting the future to be similar to the past when the latter has been regular. Thus, the only constrain to our attribution of causal relations is a psychological one: habit derived from custom, i.e., repeated experience of spatio-temporal contiguity, temporal priority, and constant conjunction of two events.

In sum, from a Humean point of view, one that has dominated empiricist philosophy of science since its beginnings, causes are nothing but regularities. This explains the reluctance of most "scientifically minded philosophers" (Salmon 1998: 9), and among them Bertrand Russell and the logical empiricists, to introduce causal considerations beyond causal *laws* in their analyses of explanation.

> The law of causality, I believe, like much that passes muster among philosophers, is a relic of a bygone age, surviving like the monarchy, only because it is erroneously supposed to do no harm.
>
> (Russell 1917: 132.)

4.3 The return of the cause: the causal approach to explanation

We have already noted that arguments and causes have traditionally been

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[20] For all I know, Hume's "empirical" argument is just another thought "experiment" (thought observation, rather).

deemed to possess explanatory virtues. Once all the flaws of the C-L model — a representative of the explanation as arguments tradition— were exposed, it was only natural for a causal model of explanation to take the lead. Yet, there was no generally accepted account of causation that could be used to ground such a model, so any author who wanted to develop a causal model of explanation should first provide a viable account of causation. This is precisely what Wesley Salmon attempted to do after having convinced himself that statistical relevance alone was not enough a basis for explanatory power.

The background of Salmon's causal model of explanation includes two revolutionary changes in physical science that took place during the first third of the twentieth century (Salmon 1989, 1998). One of these fundamental developments was quantum theory, which under its standard interpretation opened the possibility of irreducibly probabilistic laws. Salmon was sensitive to this possibility even before he attempted to build a causal account of explanation for that was one of the motivations behind the S-R approach. The other scientific breakthrough that influenced Salmon’s work consisted in Einstein's relativity theories, one of whose results put a limit to the speed of signals propagating in the vacuum.

Quantum theory strongly suggested the need for an account of explanation that allowed for irreducibly probabilistic laws. Relativity theory, on the other hand, provided Salmon with an objective criterion for distinguishing between causal and non causal processes, two central concepts of his account of causal explanation. Let us see the details.

4.3.1 Salmon’s project

Shortly after publishing his S-R “model” of explanation, Salmon changed his mind about the relevance of statistical relevance for scientific understanding.

Consequently, he set out to build a new theory of explanation that could incorporate the possibility of irreducibly probabilistic facts and, at the same time, deal with the standard counterexamples offered against the Hempelian models of explanation (Salmon 1989). The new theory relied on the description of causal relations as the explanatory element of scientific explanations. Salmon's project to offer a novel account of causal explanation was constrained by three goals:

(i) To provide an *account of causality* that showed what was the nature of the “mysterious connection” sought —and not found— by Hume;

(ii) to provide criteria that could satisfy interlocutors of strongly empiricist inclined minds; and

(iii) to provide a causal model of explanation that could explain irreducibly statistical events.

Goals (i) and (ii) were to be satisfied by Salmon's construction of the notions of *causal process* and *transmission of causal influence*. The need to cope with irreducibly random processes —goal (iii)— was to be fulfilled by the notion of *probabilistic cause*.

According to Salmon's explication of the notion of a causal relation, the world consists of processes. A *process*, for him, is any sequence of events occurring in a continuous spatiotemporal region. However, not all processes are equal; some of them can enter causal relations and some of them cannot. The reason is given both by the need that causal processes transmit signals of some sort and by relativity theory, which states that no signal can travel faster than light.

In Salmon’s theory, a causal process is one that can transmit what Salmon calls causal influence from one part of spacetime to the other. The causal influence that causal processes can transmit must be some kind information. Thus, causal processes must be able to transmit information. Accordingly, causal processes must be constrained by the law stating that no particle carrying a signal can exceed the speed of light. We find, however, that some processes do move faster than light. Those particular processes that can move faster than light cannot carry any kind of information; therefore, they cannot transmit causal influence.

Thus, Salmon is in a position to distinguish between processes with the ability of transmitting causal influence and processes that lack such capacity[21]. In other words, Salmon has found a conceptual criterion for telling processes capable of being causes from processes that cannot be causes. The next task to be done consists in providing an *empirical criterion* for distinguishing between the former —that Salmon calls *genuine causal processes*— and the latter —labeled *pseudo-processes*. In the first version of Salmon's theory, a causal process is characterized by it being capable of transmitting a mark upon interacting with another process. When a process transmits a mark trough its interaction with another process, a causal interaction takes place. Causal processes and causal interactions are, according to Salmon (1984b: 297) the two fundamental causal mechanisms.

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[21] In other words, the ontological basis for telling processes from pseudo-processes is to be found in relativity theories (Salmon 1984: 141). A Minkowski diagram offers a convenient representation of relativistic space-time. At any given point in a Minkowski spacetime, two specular cones —i.e., one two sheeted cone— can be built, along the Y axis, joint by their apexes into a point E_0, which represents an event. The inverted "upper" cone represents all light pulses diverging out from E_0, while the "lower" cone represents all light pulses converging to E_0. The former cone, or future light cone, contains all possible events upon which E_0 can have causal influence. And the other one, or past light cone, contains all possible events that can exert causal influence upon E_0. Thus, Salmon (1998: 120) calls this two sheeted cone "causal relevance cone". The set of all light cones provides the spatiotemporal structure of the world.

4.3.2 The mark transmission criterion

Now, we have here two concepts that need elucidation, namely those of *mark transmission* and *process interaction*. Salmon identifies as a *mark* any modification of a process that *persists in time, without further intervention,* upon an interaction with another process; and two processes interact when their spatiotemporal trajectories intersect. Let us review the famous example of the beacon that Salmon (1984, 1989) took from Reichenbach's book *The Direction of Time* (1956), which is also the source of Salmon's analysis of causal interactions.

The light emitted by the beacon of a lighthouse can be forced to change its color —or, in other words, the light beam can be marked— by putting a piece of red cellophane in any point of the beam's trajectory. From that point on, the beam will be red and will remain red unless further interventions occur (e.g., removing the filter). In other words, the light beam can transmit a mark (the change in color) imposed on its trajectory.

Let us now imagine the spot of light projected by the beacon on, say, a group of clouds. The color of such luminous spot can be changed too, for example by placing a red filter just on the spot. In contrast to the light beam, the luminous spot will not maintain the red color along its trajectory unless the filter moves with the spot of light. In other words while the beam transmits the mark, the luminous spot does not. Consequently, according to Salmon, the former qualifies as a genuine causal process and the latter as a pseudo-process. Similarly, the image on TV of a horse running on a green field is a pseudo-process, though the light rays coming from the screen are genuine causal processes. Should an enthusiastic member of the audience draw her gun and shoot the horse on the screen, the screen, but not the image of the horse, would receive the mark, which will persist until a new (and probably expensive) intervention. It is important to note that a process need not transmit all marks to qualify as causal, nor must it actually transmit a mark for being thus considered. It suffices that it has the ability to transmit some mark.

As we have said before, the relevance of mark transmission is that it provides an empirical criterion for recognizing the transmission of causal influence: since such transmission requires some sort of signal, it must be subject to the constraint that relativity theory puts on the speed of signals in the vacuum. Salmon shows that while some "processes" can surpass the speed of light in the vacuum, they cannot transmit "information". Those are pseudo-processes precisely because of their incapability for transmitting signals, thus marks, thus causal influence.

Now, that of transmission is a causal notion itself, so if one wishes to use it in an explication of the relation of causation, one also needs to elucidate it in non-causal terms. In order to explicate how a mark can be transmitted, without recurring to causal notions, Salmon appeals to Bertrand Russell's "at-at theory" of motion. Here is why.

In the first place, the reader will probably remember Hume's objection to the effect that in a causal chain there is still a problem with the relation between the links. In other words, appealing to ever finer-grain descriptions of causal processes may end in an infinite regress, so that the precise nature of the relation between the links of the chain remains unexplained. Should one invoke a finer-grain causal relation to explain the nature of this linkage, a further horizon would appear at a still finer grain of explanation. Salmon's solution originates in the fact that such infinite regression reminds that of the well-known Zeno's paradox of the movement of an arrow. The "paradox" goes approximately like this. A flying arrow moves across the air. How is it possible for the arrow to go from point *A* to point *B*? The arrow, Zeno notes, is at every intervening position between A and B, so it must be always at rest. If this is so, it does not move, hence the paradox.

Russell's original response to Zeno's paradox was his “at-at theory” of motion, which consists in considering motion as a functional relation between spatial points and temporal points. There is no difference between being at rest and being in motion, as far as only one spatial point is considered. An object moves from point *A* to point *B* being *at* each one of the different intervening positions *at* different times.

Since transmission is a type of motion, Salmon applies Russell's at-at theory to its analysis with the following general result:

> A mark that is imposed at point A in a process is transmitted to point B in that same process if, *without additional interventions*, the mark is present at each intervening stage in the process. The difference between a process transmitting a mark and not transmitting that mark is that in the latter case the mark is present at the later stages in the process only if additional interventions occur reimposing that mark...
>
> (Salmon 1989: 110, his italics.)

As for the notion of a causal interaction, Salmon's appeals again to Hans Reichenbach’s analysis of temporal intersections (Reichenbach 1956) in order to supply a non-causal explication of the notion of an interaction. This is possible because, in contrast to the notion of an interaction, that of an intersection is a purely geometrical concept that can be applied to the trajectories of processes, whether causal or not.

Now, a particular feature of modern science is that it contains a high number of statistical correlations. In fact, in many branches of science researchers actively search for correlations between the variables they believe may be causally relevant for the phenomena they study and use those correlations as an indication of causal connections. The stronger the correlation is the better grounds for suspecting a causal relation. This is a usual practice in ecological research, for example, one better understood as exploratory rather than confirmatory of the occurrence of a causal relationship, because not all correlations are indicative of a causal relationship. Of course, from the point of view of a philosopher, both the practice of looking for correlations and its exploratory character must be accounted for.

Returning to our study of causality, Reichenbach, for example, suggests that there is good reason for suspecting that strong correlations are not mere coincidences; behind a strong correlation, it is *likely* there is a causal process. Sometimes, however, even when there is some sort of causal relationship behind a correlation, it may be not clear what the relata of the causal relation are. The indicated causal relation may not exist between the two correlated events, but between each of them and a third one —a common cause— that independently causes the two correlated events. In other words, this *principle of common cause* provides a causal explanation of strong statistical correlations of variables that are not mutually related in a causal fashion. Those statistical correlations are screened off once the common cause is known. The principle was originally explicated by Reichenbach (1956) in terms of conjunctive forks. A *conjunctive fork* —or Y-type causal interaction— consists in an interaction where certain background conditions give rise to two correlated events, often in a non-lawful manner (Figure 4.1).

Salmon (1984) provides the example of a teacher that receives two identical essays. In this case, coincidence is deemed highly unlikely, a circumstance that leaves the teacher with two options for explaining the fact of interest. One of those possibilities is that one of the students copied the other's essay. The other possibility is that both students copied the essay from a third source, perhaps a file from the internet[22]. The latter option can be characterized as a conjunctive fork. Formally, a conjunctive fork is defined by the following four conditions:

$$P\,(A\,.\,B\,|C) = P\,(A\,|\,C) \times P\,(B\,|\,C) \qquad [1]$$

$$P\,(A\,.\,B\,|\hat{C}) = P\,(A\,|\,\hat{C}) \times P\,(B\,|\,\hat{C}) \qquad [2]$$

$$P\,(A\,|C) > P\,(A\,|\,\hat{C}) \qquad [3]$$

$$P\,(B\,|C) > P\,(B\,|\,\hat{C}) \qquad [4]$$

where $\hat{C}$ stands for the absence of *C*, and whose probabilities are neither 0 nor 1. The foregoing conditions entail

$$P\,(A\,.\,B) > P\,(A) \times P\,(B\,), \qquad [5]$$

that is, that the two events are not mutually independent.23 In the example, the impression of lack of independence of events A and B (submission of two identical essays by two students) is explained by a common source from which the two students copied the each essay. Then, *A* and *B* are independent (equations [1] and [2]), but the presence of *C* (the common cause) acts as a positive cause of *A* and *B* (equations [3] and [4]) because their corresponding probabilities of occurrence is higher when *C* is present than when *C* is absent. Now, applying the multiplication theorem to equations [1] and [2], and

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[22] Of course, the internet part is my modest contribution to Salmon's example.

[23] Two events A and B are mutually independent if their joint probability of occurrence equals the product of their respective independent probabilities of occurrence: P (A . B) = P (A) x P (B).

convening that $P(A \mid C)$, the result is that equation [1] entails: $P(B \mid C) = P(B \mid A.C)$. In words: *C* screens off *A* regarding *B*. A similar argument shows that $\hat{C}$ screens off *B* with respect to *A*. Returning to the example, the conclusion is that there is no physical relation between the two events *A* and *B*, though there is one between each of them and a common cause.

Another example of a conjunctive fork explaining a strong correlation is that of lung cancer and yellow teeth. We know that yellow teeth do not cause cancer. Nor lung cancer is known to cause teeth to become yellow. However, we know there is one factor that can explain both effects, namely smoking. Smoking, then, is the common cause behind the high correlation between lung cancer and yellow teeth. Still another case of a conjunctive fork is that involving a low reading in a barometer, a storm, and a steep decline in atmospheric pressure. While the barometer reading and the storm are correlated, the decline in atmospheric pressure is the common cause (see Chapter 3, Section 3.3.1.2). In a conjunctive fork –or Y-type interaction— then, one process splits into two marked processes, as when a unicellular organism, such as an amoeba, reproduces by binary fission.

Besides conjunctive forks, Salmon distinguishes interactive forks, which encompass χ-type interactions and λ-type interactions, where two independent processes intersect and are marked by the interaction (Figure 4.1). Two billiard balls that collide exemplify an interactive fork, while a snake eating of a mouse is a case of λ-type interactions.

Among other characteristics, interactive forks are interesting because they are "governed" by conservation laws and the statistical correlation between the two correlated causal processes involved is not screened off once the cause is known, as in the case of conjunctive forks (Salmon 1984: 170).

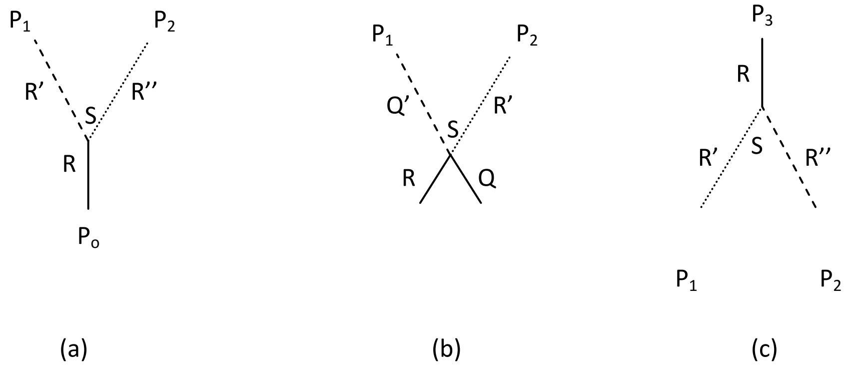

Figure 4.1 The three main types of causal interactions. (a) Y-type interaction, in which one process splits giving rise to two processes; (b) χ-type interaction or mutual modification; and (c) λ-type interaction, where the P_i are processes; Q and Q' as well as R and R' are characteristics of the P_i before and after modification, respectively. S represents the intersection point.

Salmon's theory of causal explanation, then, considers that an explanation consists of three elements (Salmon 1984: 170-171, 179; 1998). In the first place, it includes *descriptions of causal processes* (that are the means by which order and structure propagate and are, in turn, characterized by their ability to transmit a mark (or conserved quantities, see Section 4.3.4). Then, there are descriptions of *causal interactions* (which transmit conserved quantities and act as interactive forks). Finally, there are descriptions of conjunctive common causes (which have a fundamental role in the production of structure and order, and act as conjunctive forks). A causal connection is one that involves a succession of interacting causal processes.

Interestingly, Salmon (1984: 179) notes that while causal processes and interactions seem to be governed by the fundamental laws of nature, this is not the case of conjunctive forks, which depend critically on factual background conditions. The example of the two identical essays illustrates this point, since it exhibits, according to Salmon, a non-lawful fact.

4.3.3 The indeterminacy of the world

Another important feature of Salmon's theory of explanation is that it does not consider that a cause is sufficient to produce its effect. In this too, Salmon departs from Hume, because Salmon does not deem that constant conjunction is a regular feature of causation. An example of a causal relation without constant conjunction is that of smoking and lung cancer. While we have knowledge that allows us to state that smoking and cancer are causally related, we must admit that smoking is not a sufficient condition for contracting lung cancer.

Salmon does not consider causes necessary for their effects either. Indeed, the same event may be caused by a variety of alternative causes, a fact that for the author is a strong indication that causal relations must be understood in terms of probabilities and the characterization of causal relations must be done in terms of statistical relevance. The example of smoking and cancer fits also this opinion well, for while we consider that smoking is one cause of cancer, we know that smoking is not necessary for contracting cancer. Furthermore, as we have already said, modern physics provides a picture of a world where probabilities are not merely a measure of our ignorance —something that is possible in the case of the smoking-cancer relation— but a measure of an irreducible feature of the world. For Salmon the world is largely indeterministic, therefore, scientific explanation should not be characterized in terms of the sufficiency and necessity of certain conditions.

As an illustration of his views about the importance of probabilities, Salmon (1971) provides the following three examples.

(a) According to classical kinetics, there is a very low —but not null— probability that an ice cube placed in tepid water gets colder —and the water warmer— instead of the ice cube getting warmer and melting while the water gets colder. Placing an ice cube in tepid water is neither a sufficient nor a necessary condition for its melting.

(b) Bernoulli's principle explains the lift on an airfoil (or any similar object) with a certain shape and moving at definite speed through a fluid in statistical terms.

(c) The atoms that make up a laser device have a definite probability of decaying, emitting radiation. When the device is irradiated, the probability of a larger number of atoms decaying gets higher, but the probability that all the atoms in the artifact make their transition to a lower energy state spontaneously, i.e., without the need of irradiating the material, is never null.

While examples (a) and (b) may be reduced to causal explanations, (c) shows an irreducibly probabilistic physical law. Salmon (1984: 187) harmonizes this fact considering that a causal relation that can be characterized in terms of sufficiency and necessity is just a limit case of a probabilistic cause in which the probability of the events equals one. While philosophical treatment in terms of the sufficiency and necessity of certain conditions is possible in a large number of cases, Salmon claims that a general explication of the notion of a cause in non-probabilistic terms would be unnecessarily loaded with metaphysical ballast. His objection then is not aimed at the metaphysical quality of the load, but at its lack of necessity.

Causal explanations have two important aspects. One of them consists in the causal narrative that describes the causal chain or chains involved in the production of the fact to be explained. Following Larry Wright, Salmon (1984b) calls these explanations “etiological”. The other aspect of causal explanations consists in the causal analysis that shows how a certain fact is brought about. The latter Salmon calls the “constitutive” aspect of causal explanation. An example of the etiological aspect is an explanation of the presence of a worked mammoth bone, radiocarbon-dated 30,000 years from present, in an Alaskan archaeological site. It is well known that the oldest evidence of human presence in America is, in the best of cases, 10,000 years younger, so an adequate explanation of the existence of the mammoth-bone tool must account for the that fact. One possible hypothesis is that the raw material used for carving the artifact predated in several thousand years the time of it being worked by human beings. Perhaps the freezing conditions of the Alaskan environment helped preserve the bone until a human artisan found it.

In turn, an example of the constitutive aspect of causal explanation is an explanation of the pressure exerted by a gas in a container in terms of the collisions of gas molecules against the walls of the container. There is no causal narrative here, just the description of the aggregated effect of molecule collisions.

According to Salmon, both aspects occur together in a large number of explanations. He offers as an example the tragic destruction of Hiroshima by a nuclear bomb, for whose scientific understanding we need both the causal story that describes the chain of events ending up with the bomb exploding over Hiroshima and the mechanism that explains nuclear chain reactions as well as —we may add— enormous their destructive power.

I find Salmon’s distinction most useful, in particular for understanding the different types of explanations occurring in discipline with a strong historical component, such as ecology. These two aspects account for a great deal of methodological discussion in the ecological literature defending now the preeminence of the etiological, now that of the constitutive aspect of ecological explanation. While etiological explanations provide understanding through describing causal chains often made up by historically contingent, unique events, constitutive explanations rely on regular mechanisms[24].

Salmon’s acknowledgement of the indeterminacy inherent in the world played a fundamental role in his understanding of scientific explanation. In fact, it led him to distinguish three different views of explanation, which he called respectively the *epistemic* conception, the *modal* conception, and the *ontic* conception of scientific explanation (Salmon 1984b). According to Salmon, the *epistemic conception* was inspired by a deterministic —or Laplacean— worldview, and consists in the perspective that explanation rests on logical relations between propositions. The modal conception is a necessitarian one, and states that explanations show “that what did happen had to happen” (Salmon 1984b: 293) in virtue of some laws of nature. Finally, the ontic conception takes explanations to show that the fact to be explained fit into natural patterns or regularities, especially causal regularities and “physical”[25] mechanisms.

Now, in the light of a deterministic worldview, all three conceptions seem too similar to be worth distinguishing. The indeterminacy of the physical world, however, alters the scenario in such a way that the distinction becomes almost necessary. With the new significance acquired by probabilistic

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[24] Chapter 5 includes a discussion of the nature of mechanisms, including their degree of regularity.

explanation, the differences between epistemic, modal, and ontic accounts of explanation are now clearly relevant, not less because the modal conception seems tenable no more[26] and because the ontic conception does not necessarily impose the form of an argument to explanations. Besides, if there are irreducibly probabilistic laws —as the standard interpretation of quantum mechanics suggests— then not all explanations will be causal, a fact that seems to limit the reach of the ontic approach. Yet, while the latter is committed to the discovery of the mechanisms that produce the fact to be explained, it does not restrict itself to the discovery of *causal* mechanisms. Perhaps there are different, non-causal mechanisms in the world, at least in the quantum realm. If this is so, Salmon conjectures, the following consequences ensue. First, what he calls the criterion of adequacy of scientific explanation —which states tat the same set of conditions that explains a certain fact cannot be used for explaining a different fact— does not hold. As we have suggested above, both the epistemic and the modal conceptions of scientific explanation assume this criterion of adequacy. Second, and perhaps more importantly, maybe not all scientific domains explain facts in the same way. In other words, there is no universal logic of explanation, such as the one sought, for instance, by Hempel and other defenders of the C-L model. For Salmon (1984b: 299), "[w]hat constitutes an adequate explanation depends crucially […] on the kind of world in which we live; moreover, what constitutes an adequate explanation may differ from one domain to another in the actual world." Therefore, "[t]he ontic conception

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[25] The scare quotes here intend to issue a caveat for the reader: for many authors the term 'physical' amounts to "material", even when it is used to refer to entities or processes whose study is well beyond the reach of physics, such as organisms and the societies they sometimes form. In other words, the use of "physical" here does not imply the reductionistic approach usually known as physicalism.

[26] This is not the only alternative available, for one may stick to the modal conception of explanation and simply admit that genuine chance facts cannot be explained (Salmon 1984a, 1984b).

mandates attention to the mechanisms that actually operate in the domain in which explanation is sought". Interestingly enough, this perspective opens the door to the possibility of fresh views on scientific explanation in tight connection to the particular branch of science the philosopher is concerned with. Salmon's motivation for noting this is the possibility of irreducible probabilistic laws, which represents a change from the traditional view in philosophy of science, which sought a theory of explanation that would fit all the sciences[27]. In Salmon's words:

> To shift from the epistemic to the ontic conception involves a radical gestalt-switch. It involves relinquishing rational expectability as a hallmark of successful scientific explanation. Instead of asking whether we have found reasons to have expected the event-to-be-explained had the explanatory information been available in advance, we focus on the question of physical mechanisms. *Scientific understanding, according to this conception, involves laying bare the mechanisms--etiological or constitutive, causal or noncausal--that bring about the fact-to-be-explained. If there is a stochastic process that produces one outcome with high probability and another with low probability, then we have an explanation of either outcome when we cite the stochastic process and the fact that it gives rise to the outcome at hand in a certain percentage of cases. The same circumstances —the fact that this particular stochastic process was operating— explains the one outcome on one occasion and an alternative on another occasion*.
>
> (Salmon 1984b: 301-2; my italics.)

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[27] The interest in a general theory of explanation valid for all the sciences is, in turn, connected to two important themes in the philosophy of science, namely the unity of science and reductionism, or the thesis that such unity is to be attained by reducing knowledge in all branches of science to a few physical laws. The logical empiricists held both theses, that is, the one stating that the scientific endeavor is one and methodologically homogeneous, and the thesis that one of the important features of that scientific methodology is reductionism. The advance of science has shown us differently regarding reduction. Complete reduction does not seem possible in the majority of the cases and the unity of science, if there is something like that at all, is a very different thing to that proposed by the logical empiricists.

Of the three conceptions of explanation, the epistemic and the ontic ones have been developed in more ample and deeper fashion.

Three types of theories of explanation belong in the epistemic conception: the inferential view, the information theoretic view, and the erotetic view. We have already studied the inferential conception under the name of the received view. Clearly, Salmon labeled it “inferential” because in the light of this view explanations are arguments (deductive or inductive) and to explain a fact amounts to subsume its description under a generalization, so as to show that the fact was to be rationally (logically) expected —with certainty in the deductive models, with high probability in the inductive one. Thus, according to this view, the explanatory relation is one of nomic expectability, which is logical in nature. An interesting aspect of this theory is that it does not respond to a common sense intuition. People do not go invoking laws for explaining facts, unless they have a certain scientific education. Thus, the inferential conception of explanation corresponds to a scientific worldview that assigns fundamental epistemological roles to scientific laws and theories (Salmon 1998: 127).

Salmon's S-R model departed from the epistemic tradition in the sense that S-R explanations need not be arguments and the explanatory relation is not equated with nomic expectability but with statistical relevance. According to this model, an explanation consists in showing the factors that are statistically relevant —i.e., those that make a difference in the probability of occurrence of the fact to be explained.

4.3.4 From mark transmission to conserved quantities

An early and important criticism that Salmon’s theory of explanation received after its inception had to do with the mark transmission criterion (Kitcher 1989, Salmon 1994, Dowe 1992, 2008). Salmon (1994: 302) recounts that a problem presented to him by Nancy Cartwright led Salmon to reluctantly add a counterfactual clause to his characterization of causal processes. Cartwright's objection took the form of a slight modification to the classical example of the beacon (see Section 4.3.2). Now, for convenience, the putative pseudo-process, the spot of light, is projected on a wall. If we attach a red glass filter to the beacon a few nanoseconds after imposing a mark (with a red cellophane paper) to the luminous spot on the wall, we would be in trouble to tell whether the red spot of light that persists on the wall is due to the cellophane or to the glass filter. So Salmon added a counterfactual condition to his theory of causality:

> Roughly speaking, this condition says that two intersecting processes, each of which would have proceeded without modification in the absence of an intersection, interact causally if and only if both are modified at the intersection in ways that persist beyond the locus of intersection.
>
> (Salmon 1998: 18).

Salmon's discomfort with counterfactuals was due to the difficulties for interpreting counterfactual claims. His concept of causation referred to a physical relation, not to one between sentences. Salmon was in search of “completely objective causal concepts; counterfactuals are notoriously context dependent” (Salmon 1997: 470). Besides, what is the precise ontological status of a counterfactual?

Other issues raised by the mark transmission criterion (MTC) can be lumped into two groups. One of these groups we may call *collateral damage group*, and comprises claims to the effects that MTC excludes some genuine causal processes. The other group, that I shall call the *failing target group*, comprises claims about MTC failing to discard some pseudo-processes.

The collateral damage-group of issues focuses on two main problems associated with the requirements imposed by Salmon. One of them relates to the uniformity requirement and the other to the precise meaning or viability of the requirement of “absence of further interactions”. The former requirement states that a causal process exhibits a certain degree of uniformity over a time period. Yet, this time period is not precisely defined. Besides, there are causal processes that are extremely short lived, such as the case of some subatomic particles. So, we are entitled to ask “How long the regularity must persist in order for a process to be deemed causal?” Salmon also requires that uniformity occurs “in the absence of further interactions”. However, it is by no means clear that in real life there is anything as a circumstance to which one could attribute an “absence of interactions”. In the first place, interactions are very frequent in real life processes. Some causal processes such as sound waves are not even conceivable “in the absence of interactions”. Furthermore, unless one constrains the type of interactions, spatial interactions, which are constant, would render the condition meaningless. And of course, we cannot ask that those interactions be causal under risk of circularity. Another problem of the “absence of interactions” requirement is that it takes self propagation for granted, even though it is well known that such processes as falling bodies and electric currents are not self propagated, but their propagation depends on their respective fields (Dowe 2008).

The failing target group comprises criticisms related to MTC failing to exclude pseudo-processes. A central problem concerns the vagueness of the notion of a “characteristic” that lies behind Salmon's characterizations of “production” and “propagation”. Indeed, since no constrains are imposed on what might be understood as a characteristic, MTC is open to counterexamples where shadows and other “pseudo-processes” transmit marks. For example, the shadow of a car may change, and remain changed, as a consequence of one passenger's raising a flag through one of the car windows (Kitcher 1989). Another counterexample is the following, offered by Phil Dowe (1992, 2008). In Sidney, the top edge of the shadow of the Opera House is closer to the Opera House than to Harbour Bridge until a certain hour of the day. From that time on, the upper edge of the shadow is closer to Harbour Bridge than to the Opera House. So, according to the characteristic “being closer to Harbour Bridge”, the shadow of the Opera Hose should be considered a causal process. Not all counterexamples involve shadows. Elliot Sober (1987: 254) has provided one in which the chosen property is “occurring after a certain time”.

4.3.4.1 The exchange of conserved quantities criterion

Criticism to the mark transmission criterion seemed well grounded, so when Phillip Dowe (1992, 1995) published his critique on Salmon's theory of causation proposing the *exchange of a conserved quantity* criterion (CQC) for telling causal processes from non causal ones, Salmon (1997) was ready to side with him. The magnitude to which such quantity corresponds needs not be always the same. It may be linear momentum, mass, electrical charge, or what have you, but in any case, the new criterion allowed Salmon to dispense with uncomfortable counterfactuals.

Let us start with some examples of conserved quantities. When two billiard balls collide, linear momentum is conserved; when a hen lays an egg, total mass is conserved, and the same is the case when a snake eats a mouse. According to Dowe's criterion, all these are examples of causal processes just because they involve intersections of processes in which quantities (of certain magnitudes) are conserved. By contrast, when the shadows of two planes intersect, no quantity is conserved.

Salmon integrated Dowe's (1995) CQC with some modifications into his own theory of causation and proposed the following definitions:

(a) *“A causal interaction is an intersection of world lines which involves exchange of a conserved quantity”.*

(b) *“A causal process is the world-line of an object that transmits a non- zero amount of a conserved quantity at each moment of its history (each spacetime point of its trajectory)”* (Salmon 1997: 68).

(c) *“A process transmits a conserved quantity between* A *and* B *(*A ≠ B*) if and only if it possesses [a fixed amount of] this quantity at A and at B and at every stage of the process between A and B without any interactions in the open interval (*A*,*B*) that involve an exchange of that particular conserved quantity”* (Salmon 1997: 62).

The concepts involved in these definitions are in turn, defined thus (Salmon 1984; Dowe 2008):

(i) A *process* is the world line of an object, i.e., a “worm” in space time. Yet, not all worms are processes. Kitcher (1989) calls worms that fail to be processes “spatiotemporal junk”.

(ii) An *object* is anything pertaining to the ontology of science or common sense.

(iii) A *world line* is a succession of points in a Minkowsky diagram.

The conserved quantity criterion (CQC) answered some of the charges leveled to the Salmon-Dowe theory of causality. However, as is always the case, new criticism arouse, though at a deeper ontological level.

4.3.4.2 Criticism to CQC

The most challenging criticisms to CQC are, perhaps, those aiming not at CQC itself, but to more basic features of the Salmon-Dowe theory of causal explanation, namely the problem of explanatory relevance and the necessity of a physical connection between the cause and its effect. Both are as important as difficult to solve, but I think that the former turns the balance against the theory. A simple example will show why. Let us return, in our imagination, to the proverbial billiard table and balls. This time we want to explain the falling of the eight ball in the corner pocket. The setting is as usual, but now, just before the player takes her chance, she puts some blue chalk in the cue stick so when the stick touches the cue ball the latter is marked with a blue spot. Now, if we attempt to explain the sinking of the eight in Salmon’s terms, we find a problem of relevance. Indeed, the causal nexus in which the sunken eight ball should fit comprises two causal interactions, one between the stick and the cue ball and the other between the cue ball and the eight ball, as well as at least three causal processes: the moving balls, the cue and the eight. Surely we would attribute causal relevance to the linear momentum of the stick and the balls, and not to the blue spots in the balls, but Salmon’s theory does not provide a tool for excluding the blue mark from the causal story, and this amounts to a problem of explanatory relevance. In fact, it is a problem similar to that Salmon and other critics of the C-L model found in the Hempelian account of scientific explanation (Hitchcock 1995: 310).

As for the second serious problem with Salmon’s theory, it consists in the kind of challenge issued by cases of the so called “negative causation” of which more anon.

4.3.5 The challenge of negative "causation"

The Salmon-Dowe theory of causation is based on causal processes transmitting some sort of physical mark or conserved quantity. This transmission implies a physical connection between the cause and its effect. Consequently, if there were cases of *genuine causation exhibiting no physical connection*, the aforementioned theory should have to be profoundly redesigned, so much so as to end up being a different kind of theory, faraway from Salmon's or Dowe's desideratum of an ontic theory of causation and explanation. Another consequence of this criticism is that it may affect other theories of causation that rely on a physical connection between the relata of the causal relationship, especially Glennan's mechanistic theory of causation (but see Chapter 5).

Expressed in a nutshell, the Salmon-Dowe theory of causation would not be able to cope with *genuine* instances (if there were any) of so-called negative causation, namely alleged cases of causation by omission, absence, prevention, double prevention, and so on. The ontological dilemma behind this debate is, of course, whether physical connection is necessary for causation or not. Let us review some examples.

Helen Bebee (quoted in Dowe 2004) provides the following example of an omission "causing" an event: "I killed the plant by not watering it". Here, there is clearly no *positive* physical connection between not watering the plant and its death. Not watering is an omission —i.e., a "non event"—, so no physical process can possibly connect such a "non fact" with the fact (or event or process) of the death of the plant. Without a physical connection there is no mark transmission nor conserved quantity transmission between the relata of the proposed causal relation: not watering the plant and its death.

Clearly, by accepting this example as a case of causation —more precisely, as one of negative causation— the Salmon-Dow theory of causation (as well as all other theories relying on the necessity of some sort of physical connection for causation) would be in deep trouble. Of course, there are a number of theories of causation that can accommodate Bebee's example, namely those considering causation as constant conjunction or regularity (objective or subjective).

Let us see another example: A terrorist presses a detonator button and the bomb explodes. This seems a pretty straight example of causation. Jonathan Schaffer (2004: 198) suggests that this is indeed an instance of causation, *even though the precise way in which the detonator is wired has not been disclosed*. But there is a trick. Let me clarify a bit this claim.

The wiring of the detonator can be done in several ways. One possibility is that the detonator sends some kind of signal —e.g., an electromagnetic wave— to the bomb, which explodes (Figure 4.2a). This is a clear case of "positive" causation because there is an apparent physical connection between the pressing of the button and the explosion. An alternative to the former, however, consists in the detonator button being connected to a device that *prevents* the bomb from exploding by blocking the source of electrical power. The terrorist's pressing of the button prevents the inhibitor from blocking the power that now reaches the explosive and the bomb explodes. In this second wiring, a blocking of a blocker case, there is no apparent physical connection between the relata of the proposed relation of causation, namely the pressing of the button and the explosion of the bomb (Figre 4.2b).

Schaffer (2004) then asks "Does it really matter, causally, which way the detonator is wired?” If one answers 'no' —i.e., if one admits the second alternative as a case of genuine causation— one commits himself to negative causation and, consequently, to the view that physical connections are not necessary for causation. If physical connections are not essential for causation, then what is causation? Just a way of speaking? Nothing more than a habit of our minds?

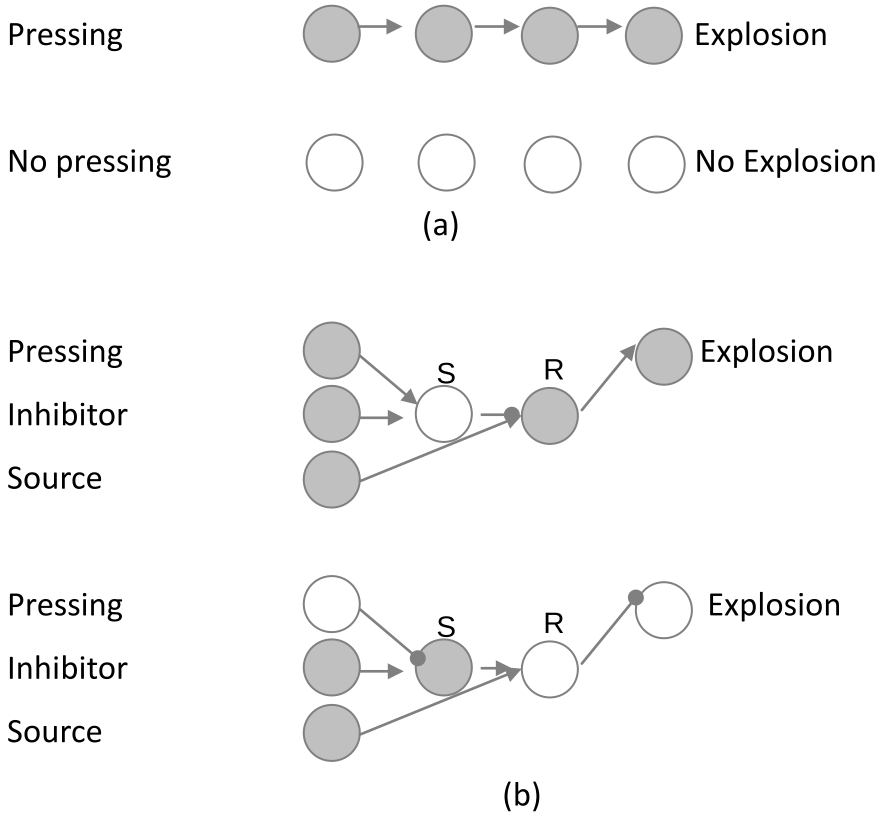

Figure 4.2 Two alternative ways of wiring an explosive device. Filled circles represent events, while unfilled circles represent absences. Arrows stand for physical connection. (a) Positive causation: Pressing the detonator button sends a signal that physically connects the detonator button with the bomb and brings about the explosion. No pressing of the button is followed by no signal emission and, consequently, by no explosion. (b) Negative causation: Pressing the button sends a signal that blocks an inhibitory device, a fact that releases the input from the source, which brings about the explosion. No pressing of the button implies no blocking input into the inhibitory device, which continues to prevent the signal from the source from reaching the explosive, so no explosion follows. (From Schaffer 2004: 197-8.)

Schaffer's argument in favor of considering these examples as genuine causation consists in showing that negative causation, as exhibited by the examples, possess all the functional features usually attributed to the concept of causation. These features are being statistically relevant and being an agential means; providing predictive evidence (from the negative “cause” to the effect) as well as retrodictive evidence (from the effect to the negative “cause”); helping explain —causally— the explosion; and providing grounds for the attribution of moral responsibility.

More precisely, Schaffer (2004: 198-199) claims that, in the case of the terrorist and the bomb, whichever way the detonator is wired, the following hold:

(i) The pressing of the button is *statistically relevant* for the explosion event —i.e., $P(E|B) > P(E|\text{-}B)$— where P stands for probability, E means explosion, and B and $\text{-}B$ represent the pressing of the button and the omission of this act, respectively.

(ii) The explosion is *counterfactually dependent* on the pressing of the button.

(iii) The pressing of the button serves as an *agential means* to achieve the explosion;

(iv) and provides *predictive evidence* of the explosion. Conversely, the explosion provides *retrodictive evidence* of the pressing of the button.

(v) The pressing of the button may *help explain* (causally) why the bomb exploded; and

(vi) makes the terrorist *morally responsible* for the explosion of the bomb.

Schaffer complements his case stating that there are three ways for identifying the reference of a term when the former is assumed to be a natural kind. The first —or Kripkean— view, consists in specifying the *original, dominant kind* that was targeted at when the term of interest was created. The second —or Putnamian— view, advices to specify the natural kind targeted by the *community of experts* in using the term. Finally, the third —or Kaplanesque— view suggests identifying *the actual extension of the functional definition* of the term. According to Shaffer, all three ways of detecting the natural kind targeted by a particular term —in this case 'cause'— suggest that negative causation is usually referred as causation. Of course, Schaffer admits that there are differences between negative and positive causes, but he deems this another story.

Personally, I find hard to accept that there is such a thing as negative “causation”. In the first place, I tend to believe that causation is objective and, furthermore, that it amounts to more than just constant conjunction. These intuitions, I think, come from my training as a biologist, which in general seems to presuppose that causes are physically connected to their effects. However, as Dowe (2004) suggests in his response to Schaffer, unchallenged intuitions are not to be taken as the sole or more important criterion for evaluating a philosophical theory —especially when there are conflicting intuitions involved. Therefore, we should look for better arguments to reject (or accept) negative causation.

Dowe's contribution to the problem takes the form of the theory of “quasi-causation”, a term intended to encompass all those relations —characterized by omissions, absences, preventions, etc.— that exhibit *functional* similarities with genuine causation but lack physical connection between the relata.

Dowe's first step, however, is to dismiss common sense intuitions as a criterion for judging causation. He points to past intuitions, that of the sun rising at dawn for example, that were later shown to be wrong by new scientific theories. To make his case stronger, the author provides a handful of examples of negative “causation” in which, the author assumes, intuition is to offer inconsistent advice (Dowe 2004: 191). Here are some of them:

(i) I caused her death by holding her head under the water for five minutes.

(ii) The hospital administration caused the death of an elderly patient by refusing to release funds to ship expensive equipment from the USA and thereby allowing her to die by “natural causes.”

(iii) My not throwing the rock caused the window not to break.

While causation seems to be absent in (i) and (iii), it is arguably present in (ii). This notwithstanding, the *negative-friendly* theories deem them *all* to be cases of causation. According to Dowe, this suggests a problem in negative-friendly theories of causation, for a theory of causation should explain those varied intuitions.

Dowe's second step is to review Mill's theory of “pragmatic considerations”, according to which “the scientific 'total' cause of an event is sufficient for its effect, but we can consider any part of the total cause that is necessary for the effect a *partial cause*, and we could call partial causes 'causes' if we wish” (Dowe 2004: 192). Pragmatic considerations —a context of human interests— is what determines our intuitions. Since some partial causes are more interesting to us than others, our intuition picks them as causes and leave to the other the role of conditions. Dowe dismisses Mill's theory on the grounds that while it provides a response to negative-friendly theories of causation, it does not provide a criterion useful for negative-excluding ones. Thus, Dowe sets out to provide an account of the “near relative to causation, quasi-causation", useful both for negative-friendly and negative-excluding theories, a theory that "presumes nothing about causation itself" and is furthermore given in terms of causation, a reason why it is compatible with any theory of causation. Dowe defines first the case of quasi-causation usually called *prevention by omission*:

> Prevention by omission: not-*A* quasi-caused not-*B* if neither *A* nor *B* occurred, and (1) *A* had occurred, *A* would have caused *B*, where *A* and *B* name positive events or facts.
>
> (Dowe 2004: 192.)

According to the counterfactual theory of quasi-causation, then, an omission prevents something *simply by not causing it*. "So quasi-causation is essentially the mere possibility of genuine causation." (Dowe 2004: 192.) Thus, turning now to quasi-causation by *prevention,* Dowe provides the following definition:

> *A* prevented *B* if *A* occurred and *B* did not, and there occurred an *x* such that (P1) there was a causal interaction between *A* and the process due to *x*, and (P2) if *A* had not occurred, *x* would have caused *B*, where *A* and *B* name positive events or facts, and *x* is a variable ranging over events and/or facts.
>
> (Dowe 2004: 192-3.)

Prevention can also obtain from genuine causal processes, as in Dowe's example of a subject who prevents a terrorist attack by blowing up the terrorist's van. But this theory does not account for the different and changing intuitions we have in different cases of causation and negative causation. Such varied intuitions Dowe attributes to an "epistemic blur" that occurs between genuine causation and quasi-causation, a cognitive phenomenon that makes it difficult to tell which one of them is the case. For example, maintaining someone's head under the water until she dies seems to be a case of genuine causation. However, death occurs by oxygen being prevented to fill the lungs and thus from entering the blood stream and reaching the brain. So, it is a case of prevention that involves quasi-causation.

A similar "epistemic blur" takes place when we claim that smoking causes cancer. But smoking, according to Dowe, quasi-causes cancer by preventing "normal processes from impacting certain cells in a certain way, so that, in the absence of those processes, diseased cells prosper (causation by omission)" (Dowe 2004: 19). The uncertainty in the identification of cases pertaining to genuine causation or to quasi-causation shows, according to Dowe, why it is useful from a practical point of view to treat causation and quasi-causation as if they were the same thing. But there is more:

> … causation and quasi-causation play very similar practical roles. Negatives (negative facts or events), when they figure in quasi-causation, can be ends and means and can rise chances. As well as serving as means and ends, since they raise chances they can be evidence for their quasi-effects and quasi-causes, and they can also feature in explanation. Arguably, quasi causation may also, subject to "pragmatic" considerations, track moral responsibility in just the way causation does. This is why it does not matter that for practical purposes we don't bother to, or can't, distinguish quasi-causation from causation. The distinction only becomes important theoretically, in metaphysics.
>
> (Dowe 2004: 194.)

Dowe's theory of quasi-causation introduces a clear theoretical distinction between genuine causation and quasi-causation, but does not explain why we have conflicting intuitions in telling between positive and "negative" events. The role of the notions of "epistemic blur" and "practical equivalence" are precisely to explain such conflicting intuitions. The former consists in the difficulty to tell between positive and "negative" events; the latter, in the usefulness of treating all negatives as positives, as in the case of killing a person by drowning her.

Dowe's theory then distinguishes between genuine causation and quasi-causation on the basis of the presence of a physical connection. In genuine causation the relata are related by a causal process, while in quasi-causation that is not the case. Further, Dowe invokes an epistemic blur between causation and quasi-causation as explaining the consequent practical convenience of taking both as the same, namely causation.

In his theory, Dowe uses the presence of a physical connection as a criterion for genuine causation, while deeming the absence of such a physical bond a criterion for quasi-causation. In order to explain our mixed intuitions, Dowe submits his thesis of the epistemic blur, which somehow results in a justification of the practical equivalence of positive and negative causation.

I mostly concur with Dowe. Human life is organized around *expectations* so it comes as no surprise that intuitions on causation (and in many other things, especially non-observable ones) are mainly guided by their practical aspects. This view explains the fact that we may have an intuition of causation when a “negative event” is the case but its practical results are equivalent to those of positive causation. This is the case of Dowe's example of “causing” someone to die by keeping their head under the water for five minutes, in which a positive event (keeping the head of the victim under the water) prevents another event (oxygen reaching the cells) and “causes” still another event (dying). However, causal chains do not seem enough to characterize such complex events as that in the previous example. It seems that a network of processes is needed to account for the death and, especially, that there is the need to take into account that the world is not a static object, but a dynamic one.

For example, someone may believe that not throwing a stone causes a window not to shatter (Dowe 2004), but this is a wrong intuition resulting from epistemic blur and practical equivalence. You may not throw a stone to a certain window and it may still shatter because of vibrations produced by an earthquake. Am I causing the Big Ben to stay on its feet because I am not blowing up its grounds? Am I causing some of the zillions of processes operating in the world right now just by not interfering with them? I certainly do not think so. The key, in this case, are the expectations of the knower regarding the regular behavior of the world. Practical (functional) equivalence should not be an obstacle for recognizing different kinds of processes, and while scientists do value practical equivalence and functional accounts in many fields of knowledge, they usually see them as a one more step towards a deeper understanding of the world.

In sum, practical equivalence blurs our epistemic understanding and overrides all possible metaphysical differences between causation and "quasi-causation". The problem, as Dowe rightly notes, lies in the opposite intuition, that is, when we take the case not to be one of causation.

But my favorite is this one:

> A man is engrossed in the view at a lookout, and doesn't see a small girl (who he doesn't know) playing nearby. He sees her just as she is about to slip off the cliff, and runs as fast as he can to the edge, hurls himself headlong across the rocks, gets just a finger to the child's shirt as she slips off the edge, but is unable to prevent her from hurtling to her death. He caused her death by omission – had he not been so engrossed, he would have seen her earlier and been able to save her.
>
> (Dowe 2004: 191.)

I think this is a rather telling example for it shows how sometimes our attributions of “causality” are in actuality attributions of *responsibility.* And while responsibility and causality may be related notions, I do not think we can conflate them. It is my opinion that faced to the previous example most people would not consider this a case of causation. The reason is the unclear responsibility of the man in the death of the girl. As I see it, the key point here is that the man *was not supposed* to be looking after the girl. Thus, there is no moral connection between the man's behavior and the death of the girl. Had the man been related to the girl, the case would turn out to be one of responsibility: we expect a relative, a friend or even of any adult aware of the presence of the girl to protect her from foreseeable perils. Since the man in the example was none of this, he cannot be held responsible for the death of the girl.

Since a materialist worldview cannot countenance negative —i.e., non existing— events affecting events —the only ones that exist— the materialist seems to need a different interpretation of the examples of negative causation. However, as Shaffer (2004) suggests, every time a need is not satisfied we face a case of “negative causation”.

We understand quite well the event of the plant's death because of its lack of water, as well as the explosion of the bomb because of the inhibition of the inhibitory mechanism. We might be tempted to attribute causal powers to such non-events and yet the lack of water is simply one intervention (among millions) that does not take place and thus does not change a process that is already taking place: the changes in the plant due to desiccation. Desiccation is a positive event involving a host of metabolic processes, among them evaporation and transpiration. Thus, the death of the plant is not “caused” by someone not watering it, but by those processes comprised under the label of "desiccation", a positive fact. I think that similar explanations can be offered in all cases "negative causation". Then, insufficient grain in explanations can account for the "practical equivalence" and the "epistemic blur". In sum, there seem to be no such a thing as "negative causation", and "quasi-causation" is a pragmatic term.

4.4 Concluding remarks

Philosophy has attempted to capture the structure of good explanations since ancient times. Three main themes of analysis may be recognized in those attempts: explanations are (i) descriptions of causes, (ii) deductive arguments, and (iii) answers to certain questions.

Of course, in general terms (i), (ii), and (ii) are not incompatible. Explanations *are* a certain type of answers to certain questions and they may respond to both or one of two powerful, ancient intuitions. The first intuition is that the description of the causal chain(s) producing or bringing about a given fact has explanatory power. The second intuition is that explanations need to be subsumptive, especially deductive arguments. Both intuitions, I submit, are quite natural in both the man of science and the man in the street, but perhaps the causal aspect of explanation is more evident to them, while the inferential aspect of explanation is more obvious to philosophers and to scientists with a strong formal leaning.

Intuitions, however, become more complex and offer much less clear guidance after two revolutions: the scientific revolution and Hume's revolution. The former provided a variety of conceptual structures that while being different, at least at first blush, scientists take to be explanatory. The second rendered suspect the notion of causality: after Hume's analysis of causality, many scientists and philosophers of an empiricist persuasion displaced the explanatory power traditionally attributed to causes to laws of nature, more precisely, to scientific laws embedded in scientific arguments. This view had its heyday in the first third of the twentieth century with the logical empiricist school, but it is still quite influential in contemporary analytic philosophy of science. One of its early champions, Carl G. Hempel, conceived of an explanation as a deductive argument whose explanatory power came from law statements acting as premises of the argument (Hempel and Oppenheim 1948; Hempel 1965). The nomological-deductive view dominated the scene of philosophical analysis of scientific explanation for some three decades, until the fact that certain events could be irreducibly random imposed itself with intensity equal to the fantastic predictive success of quantum theories, which seem to be irreducibly probabilistic in nature.

Thus, statistical accounts of explanation were offered, sometimes within the explanations-as-arguments view (Hempel 1965), sometimes combined with causal regularities by means of refurbished analyses of causation in terms of interactive forks and transmission of marks or conserved quantities, which had no use for argument structures (Salmon 1984; Dowe 2008).

The deductivist ideal, however, was not abandoned, but took the form of unificationist models of explanation (Friedman 1974; Kitcher 1989), which locate the source of explanatory power in net of just a few general principles from which all other generalizations derive. Nowadays, the causal approach and the unificationist approach are the most accepted general accounts of scientific explanation.

The causal approach is usually threatened on two grounds. First, there is the issue of the notion of a cause itself. Second, there is the alleged limitation of causal explanations to the so called “singular facts” or in other words the inability of the causal approach to account for theoretical explanation unless the nature of mechanisms is further elucidated. Let us then turn to the philosophy of mechanisms in order to assess if the answers it provides help strengthen the causal theory of explanation.

5

MECHANISM REDUX?
SECOND JOURNEY TO THE METAPHYSICS OF EXPLANATION

5.1. Introduction

As we saw in Chapter 3, the problems of explanatory relevance and causal symmetry are among the most important difficulties encountered by Hempelian models of explanation. Besides, serious objections to the I-S model led some philosophers to search for new approaches to the problem of scientific explanation. According to Salmon (1989: 61), those new approaches can be grouped in three main categories. Philosophers whose work falls in the first category attempt to preserve the inductive-statistical model by giving answer to the main technical objections it had raised. Then there are those who undertake the task of building new theories of statistical explanation. Finally there are those thinkers who discard the very possibility of statistical explanations of particular facts and, consequently, stick to deductive models. Attempts to maintain the I-S model never succeeded in solving the problems described in Chapter 3, Section 3.3.1.3. Attempts to build new explications of scientific explanation, such as Salmon’s statistical-relevance model, did not find general acceptance and, furthermore, Salmon himself found it wanting after a time. Finally, attempts to keep the deductivist approach persisted with different success, and the same can be said of those attempts based on the causal approach to explanation.

Thus, the views on scientific explanation that currently concentrate the attention of philosophers belong either to the deductivist camp, especially different versions of Kitcher’s unificationist view, or to the causal camp. The latter, part of whose foundations Salmon contributed to lay, have incorporated systemic thinking elements and have become the presently in vogue mechanisms approach. To the role of mechanisms in explanation, then, I will devote the fifth chapter of this dissertation.

As the reader shall recall from Chapter 4, the causal approach faced two important challenges. One of them relates to the difficulties to provide an analysis of the notion of a cause that satisfies thinkers of empiricist leaning (Chapter 4, Section 4.3.1). The other challenge consists in the obstacles of the Salmon-Dowe theory of causal explanation for supplying theoretical explanations, i.e., explanations of laws. As we also saw in Chapter 4, one avenue for dealing with this issue is to offer a more developed theory of causal mechanisms, one that provides more precise characterizations of "what is for a mechanism to be at work in an event, state, or process" (Kitcher 1989: 429). Perhaps more important, the mechanisms approach holds promise for solving at least some of the issues raised by causation, especially outside the realm of fundamental physics. Moreover, according to some authors, mechanisms are the answer even for the explanation of probabilistic events.

In this second journey to the metaphysics of explanation, I shall discuss the various aspects of the nature and roles of mechanisms in scientific research as viewed by philosophers who share Salmon's and Dowe's interest in causal mechanisms, but are not satisfied with their particular treatment of the latter. Let us proceed.

5.2 Mechanism: old and new

In the last two decades, there has been a remarkable increase in philosophical studies focusing on mechanisms —and the number of papers on the subject keeps growing. So much so, that a commentator recently called this trend a "mania of mechanisms" (Weiskopf 2011). While this statement is rather hyperbolic, the truth is that the philosophical study of mechanisms has taken off with the new century to the extent that some works on the subject —especially Machamer et al. (2000)— are among the most cited articles in the realm of the philosophy of science. These data are significant because mechanisms have not always been so popular among philosophers of science, especially during the twentieth century.

In the first place, mechanisms used to be understood as machine-like things, that is, exclusively as mechanical objects or processes just as classical seventeenth mechanism conceptualized them. In the second place, mechanisms have been construed, more often than not, as *causal* mechanisms and were thus burdened with all the problems of causation, at least in the view of the prevailing strong version of philosophical empiricism. In the third place, not long ago, in fact in the early 1990s, the term 'mechanism' was being criticized, even by some authors who were somewhat sympathetic to the approach, for being "an unanalyzed" term (Schaffner 1993: 287). Nowadays, however, there is a thriving philosophical movement centered on mechanisms.

Indeed, the main feature of what I shall call the contemporary mechanismic philosophy[28] is its emphasis on the central role that mechanisms play in a

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[28] Part of the mechanismic movement has also been called "new mechanistic philosophy "(Skipper and Milstein 2005). I find two problems with this label. One is that there have been philosophers defending the importance of mechanisms in scientific research for at least two or three decades before the new mechanists. Besides, the term 'mechanistic' is still charged with the ballast of physicalism and reductionism, a good reason for preferring 'mechanismic', the adjective Mario Bunge usually uses precisely to that effect. As we shall see below, the contemporary view of mechanisms admits mechanisms of diverse kinds, from physical and chemical, to biological, psychological, and social in nature.

variety of fundamental scientific practices, especially in the discovery, explanation, unification, and prediction of scientific facts. Though contemporary mechanismic philosophy covers the philosophy of all the sciences (see e.g., Bunge 1964, Pickel 2004; Gerring 2007; Glennan 2010a), it is particularly strong in the philosophy of biological sciences (Wimsatt 1972; Bechtel & Richardson 1993; Glennan 1996, 2002; Mahner & Bunge 1997, Machamer et al. 2000). According to William Wimsatt in a much quoted statement "[a]t least in biology, most scientists see their work as explaining types of phenomena by discovering mechanisms" (Wimsatt 1972: 67). In fact, biology has a venerable tradition in the mechanisms approach. Indeed, mechanism (or, rather, the mechanismic view together with the materialist one) was the uncontested victor in the mechanism vs. vitalism controversy (Mayr 1982; Bechtel & Richardson 1998). Furthermore, two of the most important theoretical developments in biology in the last two centuries, namely evolution by natural selection (Marone et al. 2002, Skipper & Millstein 2005) and molecular biology (Burian 2005), are usually considered mechanismic. Moreover, as we shall see in the following pages, some authors hold that mechanism is the ruling scientific philosophy in neurobiology and molecular biology, and that those disciplines are better understood in terms of mechanisms (Bechtel & Richardson 1993; Machamer et al. 2000).

What makes the study of mechanisms relevant to the present dissertation is that, according to some philosophers and at least in some disciplines (and perhaps in all of them), to scientifically explain the behavior of an object — usually a complex object— amounts to describe the mechanism that *produces* or *underlies* such behavior. Yet, the noun 'mechanism' and the adjective usually associated with it in the literature —i.e., 'mechanistic'— do not always mean exactly the same in the referred contexts. Sometimes they point to chains of causal *processes*, but sometimes they refer to certain *systems* that display a regular or systematic behavior, and still other times it is the materialist, reductionist nature of the *approach* —associated to classical mechanism— that is emphasized in using the term.

Research on mechanisms is hardly new to natural science. It has always played a role in scientific research, and mechanism was a central aspect of the Scientific Revolution. In point of truth, the roots of mechanism can be traced back to classical culture and the work of the atomist and stoic philosophers. However, the mechanistic view was developed by natural scientists such as Galileo Galilei, Christiaan Huygens, and Robert Boyle, who thought of the world as an immense machine, an enormous “mechanism” composed of inert particles moved by mechanical forces. Thus, for the classical mechanists, a mechanism was a thing, more precisely a machine. Later, this approach became widespread as the “mechanical philosophy” usually associated with René Descartes, Thomas Hobbes and other seventeenth-century French philosophers (see, e.g., Machamer 1998), a philosophical perspective that has also been considered as the first thoroughly scientific view of the world (Bunge 2000a: 37) [29].

Classical mechanism revolves around two main philosophical theses to which mechanisms are central: an ontological thesis and a methodological one[30]. The former states that the world is a huge machine or like a huge machine composed by corpuscles of different size and shape. Such constituent parts are related to one another in exclusively physical ways, more precisely

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[29] Also according to Bunge (2006a: 41) the contenders of classical mechanism were the magical view of the world in its different varieties, ordinary knowledge, and Aristotelianism. The same author holds that mechanism, considered as a general philosophy, declined and died in the second half of the nineteenth century, as a result of the introduction of the concepts of force field, energy, chance, and the micro-macro distinction in physics (Bunge 2010: 31)

mechanically (i.e., by friction and collision). In other words, classically conceived of, a mechanism is a complex, machine-like object consisting of individual corpuscles that interact by means of mechanical connections. As is widely known, the paradigmatic metaphor for classical mechanism is the analog clock, with all its springs and bolts and gears moving by virtue of mechanical interactions.

In short, from an ontological point of view, classical mechanism may be characterized by its machinism and reductive physicalism, and by considering that mechanisms are things.

The central methodological thesis of seventeenth century mechanism is (methodological) reductionism, the view that in order to understand a phenomenon it is necessary and sufficient to break it down into its component parts and determine the interactions among such parts. A classical mechanical explanation then is provided by describing the size and shape of the corpuscles, as well as the physical interactions that occur among them.

Mechanistic philosophy, did not remain unchanged in the light of ulterior scientific and philosophical developments. While machinism heavily receded during the nineteenth century (Hedström & Swedberg 1998), reductionism — both ontological (physicalism) and methodological— is still with us in different degrees, from mild to radical, and with diverse fates in different disciplines.

Here a caveat is in place, for reduction and reductionism are not the same concepts. In the contexts of science and philosophy of science, reduction is a

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[30] For an authoritative account of classical mechanism, see Des Chene (2005).

conceptual operation that consists in recurring to (descriptions of) lower level entities and interactions either to describe the nature an object (ontological reduction) or to explain its behavior (methodological reduction). Being a conceptual operation, reduction is always performed on ideas not on things, especially on theories (see van Riel & van Gulick 2015).

Reductionism, on the other hand, is the philosophical position stating that reduction is the main or only means to achieve some of the main goals of science, especially to unify and explain scientific facts.

Ontological and methodological reductions are mutually independent. For example, the former has been successfully applied in the case of the psychoneural identity hypothesis presupposed by current neuroscience, but not many ecologists would accept that biosystems are just collections of atoms (though some ecologists claim that communities are just collections of individual organisms, McIntosh 1985). Methodological reduction, on the other hand, is nowadays in particularly good shape in almost all the sciences[31] and there is a lively philosophical debate as to its relation to explanation (van Riel & van Gluck 2015).

Radical ontological reductionism, the thesis that physical and chemical objects and interactions are the only real entities (as well as its epistemological counterpart consisting in explaining every possible phenomenon in terms of physical and chemical concepts) is sometimes known as physicalism, but this term is not consistently used in the philosophy of science and is sometimes equated to materialism.

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[31] Richard Looijen has studied reduction in ecology within the context of the holism-individualism debates (Looijen 2000).

In any case, the search for mechanisms, and usually some form of reduction —especially of the methodological kind—, became part and parcel with the methodology of scientific research in the seventeenth century.

However, it is important to emphasize that contemporary mechanismic philosophy is quite different from its seventeenth-century counterpart. Perhaps, the main difference is that such conception explicitly admits mechanisms other than those purely physical (i.e., those studied by physics). In general, authors in the contemporary mechanismic philosophy accept that the nature of mechanisms can be physical (e.g., friction), chemical (e.g., inhibitory competition), molecular (e.g., DNA replication), biological (e.g., natural selection), social (e.g., self-fulfilling prophecy) or even biosocial (e.g., the inhibition of incestuous impulses by a shared childhood). (See Bunge 1979, 2003; Glennan 1996, 2010a; Machamer et al. 2000, Craver 2007). Indeed, the majority of the philosophers in question assume that the world is organized in layers or “levels of organization” and are not committed to the theses of *radical* reductionism. These are the ontological theses that (i) the only entities with real existence are physical entities and all complex objects are but aggregates of physical individuals; and (ii) those aggregates do not possess any real global properties. Besides, radical reductionism defends the methodological thesis that (ii) in order to scientifically explain a phenomenon, models and theories must *invariably* recur to lower-level entities and their interactions, preferably to those at a purely physical level.

While this brand of radical reductionism is not frequent, milder forms of reductionism are the norm in the sciences of life. For example, there is a handful of research approaches in biology and ecology that are frankly reductionistic. This is the case of the “selfish gene” evolutionary perspective and that of some speculative brands of evolutionary psychology (both of which, incidentally, have been charged with being pseudoscientific, e.g., Bunge 1999, 2003).

In sum, the main difference between contemporary and classical mechanism is that the former is committed neither to the theses of machinism nor physicalism (or radical ontological reductionism), nor (in general) to those of radical epistemological/methodological reductionism. Though related to seventeenth century mechanism, contemporary mechanismic turn is a deep re-elaboration of the original idea that mechanisms are a central feature of the world, in a clear attempt to come closer to the research methods and results of contemporary science, especially to those of the so called special sciences, more particularly o the methodology of biological science.

5.3. Contemporary views on mechanisms

The contemporary philosophical movement centered in the study of mechanisms gained momentum in the course of the last two decades, especially after Peter Machamer and collaborators (MDC) published their article on the subject (Machamer et al. 2000), which soon became a landmark on the subject. However, there have been philosophical voices insisting on the importance of mechanisms for science and its philosophy —in particular for scientific explanation— long before the emergence of the new mechanistic philosophy. Indeed, different authors (e.g., Bunge 1964, Jeffreys 1971, Wimsatt 1972, Baskhar 1978, Railton 1978, Salmon 1984) have emphasized the centrality of mechanisms to explanation since as early as the mid-twentieth century. For example, Richard Jeffreys and Peter Railton provided some of the basic ideas to be developed by the new mechanistic philosophers (Glennan 2002), and Mario Bunge and Roy Bashkar approached mechanisms from an explicit realist stance, a fact that allowed them to advance their ideas without being troubled by empiricist objections on causation[32]. In particular, I find Bunge's investigations remarkable as an early defense of a style of explanation by means of mechanisms (Bunge 1959, 1964). This is in part because Bunge has built a whole philosophical system, around "the twin notions of system and mechanism" (Bunge 2004) but also because he has continued to develop his views on mechanisms until recently (Bunge 1997, 2004, 2010)[33].

As I have said, contemporary mechanismic philosophy revolves around the notion of mechanism. Actually, I should write 'notions' of mechanism, since the literature provides a number of characterizations of what mechanisms are and the roles they play in scientific research. In a review article, for example, John Gerring (2007) distinguishes at least nine concepts of mechanism in social science and philosophy, some of them ontological (conceived as things or processes) and other epistemological (as parts of explanations). We do not need to study all these concepts here, but only those philosophical accounts of mechanisms generally accepted to be the most articulated. In each case, I shall discuss the ontological aspects —i.e., what the nature of a mechanism is— of a particular view on mechanisms in the first place. Then, I shall discuss the epistemology of mechanisms, with an accent in their methodological applications.

5.3.1 The ontology of mechanisms

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[32] This is particularly true of Bashkar, who appears to have resigned empiricism altogether (Creaven 2010).

[33] Bunge's work has had a stronger influence in social science (e.g., Hedström & Swedberg 1998, Pickel 2004, Gerring 2007, Wan 2011) and neuroscience (Mountcastle 1998) than in biology or ecology (but see Marone & González del Solar 2000, González del Solar & Marone 2010; Gonzalez del Solar et al. 2014), though this is, in my opinion, a problem for the latter disciplines.

The online Merriam-Webster Dictionary[34] defines the noun 'mechanism' as

> 1a**:** a piece of machinery
> b**:** a process, technique, or system for achieving a result
> 2**:** mechanical operation or action**:** working 2
> 3**:** a doctrine that holds natural processes (as of life) to be mechanically determined and capable of complete explanation by the laws of physics and chemistry
> 4**:** the fundamental processes involved in or responsible for an action, reaction, or other natural phenomenon.

Acceptations 1a and 3 are in consonance with classical mechanism, as we have already seen, while acceptations 1b, 2, and 4 correspond to concepts in the contemporary mechanismic philosophy. Indeed, authors in this movement conceive of mechanisms either as *things* (Bechtel and Richardson 1993; Glennan 1996), as dual objects made up by *entities and activities (*Machamer et al. 2000), or as *processes* (Bunge 1959; Salmon 1984; Glennan 2002, 2010a). Bechtel and Abrahamsen (2005) speak of mechanisms as structures. Variety is present even within these groups. Take the mechanisms-as-processes view as an example. Wesley Salmon writes about causal processes within a process-ontology framework where a billiard ball does not engage in processes but *is* a process. Stuart Glennan, in turn, focuses on unique causal chains (Glennan 2002) or on ephemeral mechanisms (Glennan 2010a) assuming that processes are interactions among ontologically stable things or parts of things. Finally, Mario Bunge concerns himself about the specific processes that make certain concrete (real) systems "tick", and understands processes as sequences of changes of state —i.e., events— in

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[34] URL: http://www.merriam-webster.com/. Accessed: 04/02/2012.

things (Bunge 1979).

Let us now take a deeper look at the different contemporary conceptions of mechanisms.

5.3.1.1 Mechanisms as things

The mechanisms-as-things, mechanism-as systems, system-mechanisms, or “mechanistic systems approach” (Glennan 2010a)— reminds that of classical mechanism in that both of them conceive of mechanisms as complex *objects* (or things). In its more recent guise, that of the systems decomposition approach, this view was introduced by Bechtel and Richardson (1993), and was lately elaborated by Stuart Glennan (1996, 2002, 2005), who provides the following definition of a mechanism:

A mechanism for a behavior is a complex system that produces that behavior by the interaction of a number of parts, where the interactions between parts can be characterized by direct, invariant, change-relating generalizations. (Glennan 2005: 445.)

In other words, a mechanism is a system whose parts interact in a regular manner so that they jointly produce the regular behavior of the system.

The following ontological theses can be recognized in this version of the mechanisms-as-systems view of mechanisms.

(i) Mechanisms are complex systems. According to Glennan (2005: 446), being systems —i.e., being objects made up of a number of component parts that interact— is the most important characteristic of mechanisms, most likely because

(ii) the mechanism’s behavior is brought about (*produced*) by the interactions among its component parts. Such behavior can be input driven (like that of a Coke machine) or autodriven (like that of a heart), but a constant aspect of them is that

(iii) the interactions that produce the phenomenon —i.e., the mechanism’s behavior— are *causal and regular*. In other words, a change in a property of one or more of the mechanism’s parts regularly brings about a change in certain properties of other parts of the mechanism.

Mechanisms underlie a large quantity of phenomena, but

(iv) not every event is the product of a mechanism because there are also “genuinely singular events” (Glennan 2002: 348). Finally,

(v) the component parts of mechanisms may be simple objects or complex ones, mechanisms themselves, that make up a *hierarchy of nested mechanisms*.

Note the following points. First, according to (i), mechanisms are a special type of system, namely complex systems. By “complex system” Glennan surely means an object composed by parts. Yet, systems are usually defined as complex objects of sorts —that is, objects composed by interacting parts—, this raises the question of what a *simple* system might be and what are the reasons to restrict the notion of a mechanism to systems that are complex. Whether there is a certain complexity gradient for systems, Glennan does not say, but his assertion that mechanisms are complex systems reasonably presupposes such gradient and a reliable way to assess the degree of complexity of a system. However, it is difficult to see why only *complex* systems should be deemed mechanisms. Furthermore, Glennan’s examples of mechanisms include some systems that are hard to consider as complex except for in the most trivial way of being composed of parts. Those examples include vending machines and toilet flushing devices, which arguably are rather simple systems, at least compared with, for instance, organisms and social groups, two other examples mentioned by Glennan (2005: S345)[35]. In sum, if having parts is an essential characteristic of systems, it seems to me that a more consistent definition of mechanisms as things would begin by characterizing them just as systems (i.e., as a certain kind of complex objects) rather than as *complex* systems.

The second point to note concerns the behavior of a mechanism. On the one hand, mechanisms are “for a behavior”, and not mechanisms *simpliciter*; then what counts as a mechanism depends on the identification of the behavior the mechanism underlies: “one cannot even identify a mechanism without saying what it is that the mechanism does” (Glennan 1996: 52). This strikes me as a reasonable claim: if a mechanism is conceptualized as a system, the description of what the system does is an integral part of the description of the mechanism. Such description will guide the system’s decomposition (Glennan 1996: 52).

One reason for the importance of the identifying and describing the behavior of interest is that systems, especially complex systems, may behave in many different ways, and different behaviors may result from the interactions of different groups of components. In other words, those different behaviors will be the product of different mechanisms.

To take one of Glennan’s own examples, while at work, a vending machine not only delivers certain products —the global behavior used to demarcate the

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[35] In an earlier paper, Glennan (1996) offers a belt as an example of mechanism for stopping bullets. As I see it, a belt does not qualify as a complex system in Glennan’s sense, though perhaps the system composed by a human wielding a belt does.

mechanism in the example— it also makes some noise and liberates some heat. Noise and heat production are also behaviors of the vending machine (a thing), but none of them is the *intended* behavior of the vending machine, so they cannot be used to delimit the mechanism (i.e., the system) of interest. In this sense, heat and noise production are not essential properties for being a vending machine and may be considered “side effects” of the mechanism’s operation. This is because vending machines are artifacts designed to dispense products (drink cans, snacks, earphones, etc.) and if we wanted to explain their behavior (or operation), we should describe how such behavior results from the interactions of the component parts of the machine. For example, one of the interactions that explain how the desired product, say a cold drink can, reaches the dispensing outlet includes a gyratory coil and the desired drink can that is drawn to the outlet by the coils’ movement. Clearly, heat and noise production are not part of the equation. However, we might want to explain, instead, why vending machines produce heat while working. In this case the behavior of interest would be different to the one of the vending machine as a whole and so would be the relevant mechanism involved, as well as its decomposition. The desired mechanistic explanation will describe heat production as a result of a variety of processes (e.g., friction of moving pieces) that take place in several parts of the vending machine. Thus, heat production –like noise production— is not a specific global behavior of the vending machine, but a behavior of some of its subsystems, that is, of some mechanisms that are component parts of the vending machine.

All this said, when dealing with natural systems, we cannot recur to the “side effect” solution unless the mechanism of interest is an organism and we assume that all its behavior is adaptive. Where there is neither purpose nor natural selection, as in the case of non-adaptive conducts of organisms, we should refrain from setting up a hierarchy of behaviors.

A third characteristic of Glennan's definition of a mechanism as a thing is that its behavior is the result of causal interactions among the mechanism's component parts. The behavior of a mechanism is regular and such regularity results from the regularity of the causal interactions among the components of the mechanism (thesis iii, above). Behavioral regularity and stability of its configuration are important features of Glennan's mechanisms. The former consists in the mechanism behaving in similar ways at more than one time and place. Put in other ways, the mechanism's behavior is repeatable in virtue of the stable configuration of its parts (Glennan 2002: S345). Another aspect of mechanisms that makes for generality is that there are types of mechanisms that comprise a number of tokens or there are, in Glennan's words, "many tokens of the same type of mechanism" (Glennan 2002: S345). This regularity in behavior and configuration justifies one of Glennan's requisites for a system to be a mechanism: that its behavior and the components interactions be describable by invariant, change-relating generalizations.

Finally, Glennan puts forth his "mechanical theory of causation" (1996: 50) as a response to Hume's problem of the "secret connexion" in a causal relation. Glennan suggests that if we pay attention to the way causal claims are best tested and supported, we will find that causal claims are deemed true when the corresponding mechanism that connects the causal relata has been shown. Thus the connection between cause and effect is not "secret" any more, but consists in the productive interactions among the mechanism's parts. In Glennan's own terms: "two events are causally connected when and only when there is a mechanism connecting them" (Glennan 1996: 64)."

While this approach has its merits, it cannot be applied to every field of science. In particular, fundamental physics poses a challenge to the mechanistic approach, because fundamental laws cannot be analyzed in terms of mechanisms, at least for the time being.

Another problem seems to be circularity. Since interactions among the parts of a mechanism are causal and connections between events are causal —always outside the realm of fundamental physics— only when they are connected by a mechanism and an essential aspect of a mechanism are causal interactions. Since that of causal interaction is a causal notion itself, it is not clear how mechanisms could be the basis of theory of causation. Glennan's answer to this objection consists in distinguishing between different kinds of causal relations, namely that in fundamental physics and that in other fields. Whereas the laws of fundamental physics are not "mechanically explicable", the laws in other levels of organization are. In sum, for Glennan, outside the realm of fundamental laws, causation amounts to the existence of a mechanism connecting the causal relata.

5.3.1.2 Mechanisms as entities and activities

In what constitutes one of the most cited articles in the history of the journal *Philosophy of Science*, Peter Machamer, Lindlay Darden and Carl Craver (2000) offer a number of arguments that aim at showing how and why thinking about mechanisms makes up a good approach to the history and philosophy of science. Their manifest goal is "to sketch a mechanistic approach for analyzing neurobiology and molecular biology that is grounded in the details of scientific practice, an approach that may well apply to other scientific fields" (Machamer et al. 2000: 2). Machamer and collaborators (hereafter MDC) consider that the focus on mechanisms is also useful in the history of science, for much of it "has been well written, albeit unwittingly, by tracing the discoveries of new entities and activities that mark changes in a discipline" (Machamer et al. 2000: 14).

Their definition of a mechanism is the following:

> Mechanisms are entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions.
>
> (Machamer et al. 2000: 3.)

This definition states or implies at least the following ontological theses:

(i) Mechanisms "are composed of both *entities* (with their properties) and *activities*" (Machamer et al. 2000: 3; their italics). In other words, a mechanism is an ontologically dual object, because entities and activities are correlative and interdependent.

(ii) Activities, which "are the producers of change" (Machamer et al. 2000: 3), are "types of causes", depend on the properties of entities, and determine what kind of entities can be the basis for a given mechanism (*ibid*., p. 6).

(iii) Entities must be *organized* in certain ways —i.e., they must be appropriately located, structured, oriented, etc.— in order to engage in activities; and

(iv) activities must be organized too —that is, they must have a particular temporal order, rate, duration, etc.

(v) Mechanisms are regular. What makes them "regular is the *productive continuity* between stages" (Machamer et al. 2000: 3, their italics). While I deem this statement rather obscure, because to me there is no obvious way in which productive continuity makes mechanisms regular, I agree with the authors that such regularity does not seem accidental. They go a step farther and claim that, together with counterfactual support, regularity implies some kind of *necessity* in the behavior of the mechanism.

In a later paper, Machamer (2004) expressed doubts as to the need of regularity for mechanisms and suggested, following Bogen (2004), that there can be mechanisms that operate only once or at least once in a while (*ibid*., n. 1). This is a possibility that Bunge (1979) and Glennan (2010a) have also suggested.

(vi) Mechanisms occur in nested, part-whole hierarchies, where components of one mechanism are “lower level entities, properties, and activities [...] that produce higher level phenomena” (Machamer et al. 2000: 13).

According to MDC, the main “ontic” justification[36] for thinking about mechanisms in terms of entities and activities, that is, in a dualistic fashion, is to assign activities the basic ontological status they deserve and they elaborate at some length on the metaphysics of activities. In contrast, they do not develop the ontology of the entities that can be considered as components of mechanisms[37]. While they label their analysis of mechanisms as “dualistic”, along their paper the emphasis is on activities, that is, what the relevant entities do. Indeed, they claim that “[a] mechanism *is a series of activities of entities* that bring about the finish or termination conditions in a regular way” (Machamer et al. 2000: 7; my emphasis). With this, the authors attempt to correct what they perceive as the excesses of the “substantivalist”

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[36] They offer “descriptive” and “epistemic” justifications too, but I shall leave their treatment for Section 5.3.2.2, when I deal with the epistemology of mechanisms.

stance, which they reject as "artificial and impoverished to describe mechanisms in terms of entities, properties, interactions, inputs-outputs, and state changes over time" (Machamer et al. 2000: 5). MDC explicitly reject Salmon's (1984) and Glennan's (1996) characterizations of mechanisms in terms of decomposition of systems into their components and interactions. They claim that the substantivalist view —with its talk about entities and capacities, dispositions, or propensities— misses the basic status of activities as an ontological category, a status as basic as that of being an entity. One hint of such basic status of activities, they state, is that activities have to be identified prior to any attribution of capacities, dispositions, or propensities to a given entity. Moreover, MDC also reject "process ontology" (e.g., Rescher 1996) because they deem it a reification of activities, which do not exist separately from their entities —at least in the fields studied in MDC's paper and certainly not in their preferred examples of a mechanism, namely those of protein synthesis and neuronal depolarization (but see Machamer 2004: 29-30)—. All this said, Machamer entertains the possibility that entities are "onticly asymmetric with activities in one sense" (*ibid*., p. 29), since "most often" entities are the things that act. However, the author hasten to counterbalance his admission of asymmetry —that there are no activities without entities— by emphasizing that activities do not "belong" to the entities that act. Furthermore, he is uncertain whether forces, fields, or energy are entities and not "'substantial' activities" (*ibid*., p. 29). Machamer even speculates whether one needs entities in order to understand activities such as equilibrating or reaching stasis (*ibid*., p. 29). I have to say that, while I agree with Machamer in considering both entities and activities as

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[37] Interestingly, though, I asked Professor Machamer about the ontology of entities some years ago, during a summer course at Valencia —"Perspectivas Actuales en Filosofía de la Ciencia y Metodología", UIMP, 2005— and he told me that any kind of activity could be a part of a mechanism, even ideas! This, of course, runs against the grain of the distinction

ontologically basic, I find his some of his speculations about the independence of activities rather weird. For example, without some entity that “moves” (changes) towards, or from, equilibrium or stasis, how could the concepts of equilibrating or reaching stasis possibly be understood? As to Machamer’s other candidates for independent activities, namely forces, fields, and energy, Machamer offers as an unproblematic example the first law of thermodynamics: $dU = dQ - dW$; with dU = internal energy change. From this he concludes that “it is the change itself that is ‘substantial’” (*ibid*., p. 30). There are a few things to be said about this example. In the first place, Machamer only provides the interpretation of dU (as internal change), but not those of dQ and dW. In order to be attributed any meaning, surely the formula has to be further interpreted and that interpretation involves the entities that Machamer has not found in his reading. Second, the referent of dU is not change, but *energy* change. This means that if energy is considered an activity, as Machamer seems to do, and change is an activity in Machamer’s sense, dU would be interpreted as “change change”, a rather uncomfortable ontological situation. However, if energy is understood as a property of things, then its changes are just modulations of a property: no ontological puzzle. In sum, we can admit MDC’s talk of entities and activities because when we analyze real objects we may distinguish aspects that do not really occur one apart from the other. However, just as there are no static entities, there are no activities in themselves; what we have is acting entities: moving electrons, falling bodies, reacting substances, digesting stomachs, running legs, minding brains, whole organisms reaching homeostasis, ecological systems equilibrating (or not), and so on and so forth.

A final but very important aspect of MDC’s view of mechanisms is that it has

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between real things and their models.

been offered as a solution to the problem of causation in general and of "negative" causation in particular. Indeed, following Anscombe (1971) MDC claim that "activities are types of causes" (Machamer et al. 2000: 6), and that 'cause' is a generic, abstract term, like 'interact' and 'organism'. The word 'cause' "only becomes meaningful when filled out by other, more specific, causal verbs, e.g., scrape, push, dry..." (*ibid*., p. 6). According to this view, the relevant problem is not to provide a full-fledged ontological theory of causation, a "general and adequate ontological or stipulative definition [of the notion of cause], but a problem of finding out, in any given case, what are the possible, plausible, and actual causes at work in any given mechanism" (Machamer 2004: 27-28). In other words, the central problem with causes, as MDC see it, is to find *how things work*, that is, to "discover the entities and activities that make up the mechanism" (*ibid*., p. 28). A corollary of this stance is that causes are not things, but "doings": "It is not the penicillin that causes the pneumonia to disappear, but what penicillin does" (Machamer et al. 2000: 6). Once again, the emphasis is in activities and the causal relation occurs between events ("doings"), not just between entities. As for "negative" causation, Machamer's argument begins by making clear that activities are positive, not negative; active instead of passive. In his own words, "... we might say that activities are ways of acting, processes, or behaviors; they are active rather than passive; dynamic rather than static"; thus, "[n]on-existent activities cannot cause anything" (Machamer 2004: 29), even though the lack of occurrence of an activity —i.e., failures and absences— can be *relevant* for the production of a given effect. Absences are not genuinely effective in the production of the effect. In this, Machamer's solution to the problem of the so-called negative causation resembles Dowe's (2004) general point behind his theory of quasi-causation (see Chapter 4, Section 4.3.5), because the former also distinguishes between causally relevant and causally efficacious factors. Machamer, though, admits that the use of the subjunctive mode of speech may have an explanatory role. For example, a physician who lets her patient die is —other circumstances concurring— guilty of that death, but she did not *cause* the death of the patient; she just let the mechanisms already at work in the patient's body to continue to operate. It is those mechanisms that are causally efficacious of the death of the patient, not the physician's omission, which is only causally —and thus legally— relevant because of the possibility of her intervention.

5.3.1.3 Mechanisms as processes

Currently, the mechanisms-as-processes view is mainly represented by the work of two authors: Mario Bunge and Stuart Glennan. Let us take a look at what they have to say.

5.3.1.3.1 Mechanisms as ephemeral processes

Glennan's view of mechanisms as an essential aspect of research has been evolving since his first papers in the 1990s and, as of late, he has come up with an extension of the mechanistic view in the form of what he calls "ephemeral mechanisms". This development springs from Glennan's attempt to link historical explanation —i.e., explanations of unique events— and systems-mechanistic explanation, which may be viewed as ahistorical because it deals with generalizations, but that also possesses a historical dimension consisting in the description of sequences of (repeatable) events.

Glennan starts by showing the narrative character of historical explanation. For him, historical explanation consists in a description of a causally linked sequence of events along a temporal dimension. Its main characteristic, however, lies in the ephemeral configuration of the entities that interact and give rise to each particular event. This contrasts with the stable configuration of system-mechanisms, which gives rise to repeatable events. More precisely, Glennan characterizes ephemeral mechanisms in these terms:

> I take an ephemeral mechanism to be a collection of interacting parts where:
> 1. the interactions between parts can be characterized by direct, invariant, change- relating generalizations
> 2. the configuration of parts may be the product of chance or exogenous factors
> 3. the configuration of parts is short-lived and non-stable, and is not an instance of a multiply-realized type.
>
> (Glennan 2010a: 260.)

A first thing to note in this characterization of ephemeral mechanisms is that while they are generally conceived as ephemeral *processes*, here they are characterized as "a collection of parts". The refinement of the author's analyses prevents us to construe this duality as a mere lack of conceptual clarity. I think that a more plausible interpretation of such duality is that, just like MDC, Glennan acknowledges the ontological interdependence of entities and processes, though he emphasizes the latter as the key element of analysis in ephemeral mechanisms.

As for condition 1, we have already discussed it when reviewing Glennan's account of mechanisms as systems. Such generalizations have the lawlike property of supporting counterfactuals. Conditions 2 and 3 exhibit the distance between ephemeral mechanisms and "their more robust cousins", that is system-mechanisms.

If Glennan's examples of system-mechanisms included toilet flushing devices and vending machines, his examples of ephemeral mechanisms comprise the death of the French literary critic Roland Barthes, the stock market crash of

2008, and the outbreak of Worl War I. Again, while toilet float-valves and Coke machines can be made to repeat a type of event, Barthes's death, the 2008 financial crash and WWI are taken to be unique, non-repeatable events.

The interesting part is that Glennan manages to connect the explanations typical to both forms of events, namely repeatable and non-repeatable. The connection is made by showing first that there is a historical dimension to systems-mechanistic explanations, which involve descriptions of sequences of events. The relevant characteristic of such descriptions is that they are generalized (see Section 5.3.1.1). In contrast, what Glennan calls historical explanations do not involve such generalized descriptions, but descriptions of singular events. Glennan uses the outbreak of World War I as an example of how both kinds of descriptions may be used to explain the same event, but here I will use his main example of an ephemeral mechanism, that is, the demise of Roland Barthes —a famous literary critic and semioticist—, to the same purpose.

The sequence of events that led to the Barthes's death was the following. While returning from a luncheon with France's president François Mitterrand, Barthes was struck by a laundry truck, as a consequence of which he passed away. Barthes's death was a result of the interaction of a rather contingent assembly of factors (parts) acting together in a short-lived and non-stable configuration (condition 2). Glennan considers the following factors as parts of the ephemeral mechanism at work in the example: Barthes, the laundry truck, and President Mitterrand, among others. The "manner these parts come together is chance or unpredictable, how they will interact with each other is not" (*ibid*., p. 261). This aspect of ephemeral mechanisms, that is, the robustness of the interactions among parts is, according to Glennan, the key reason for calling them 'mechanisms'. In other words, for Glennan, the main connection between systems mechanisms and ephemeral-process mechanisms lies in condition 1 above, that is, in that both kinds of mechanisms involve robust interactions *between parts*. More precisely, the reason Glennan offers for calling those ephemeral processes mechanisms is that, like systems mechanisms, the interactions among parts can be described by invariant, change-relating generalizations. The main difference, always according to Glennan, is that ephemeral mechanisms are not robust and their global behavior is not regular. Put in other words, while the parts of a systems mechanism are stably configured, the configuration of an ephemeral mechanism is non-stable.

5.3.1.3.2 Mechanisms as specific processes in systems

Mario Bunge has been vocally defending the central role of the “twin concepts of system and mechanism” (Bunge 2004: 190) in scientific research since mid twentieth century (e.g. Bunge 1964). Though he states that “systems” and “mechanisms” are twin concepts, he does not conceive of mechanisms as types of systems, but as specific processes occurring in concrete (or real) systems, and only in them. In Bunge’s view, there are no mechanisms in conceptual (or ideal) systems, such as classifications, models or theories. (Of course, models and theories may include descriptions of mechanisms, but this is a different thing altogether.)

Bunge’s work differs from that of other philosophers interested in mechanisms in a variety of aspects. One important difference is that while Bunge consider descriptions of mechanisms as the gist of explanation and a powerful aggregated value for prediction, the basic unit of his philosophical analysis is systems, not mechanisms. Furthermore, Bunge’s original philosophy, the one expounded in his *Treatise on Basic Philosophy* (1974-1989), is itself intended to be a (conceptual) system, an instance of Bunge’s particular version of the systemic approach to research applied to philosophical thinking.

Bunge's systemic philosophy is rooted in an ontological systemism of sorts [see thesis (i) below] and the well from which this systemic view springs is science. According to Bunge, philosophy must be informed by science, both in content and methodologically[38], and scientific theories and findings show that the world is made up by systems, thence his ontological systemism, which in turn inspires his epistemological systemism: the study of the world should be systemic, that is, it should pay attention to all aspects of systems and in doing so it becomes a system itself.

A further important difference between Bunge's philosophy and that of the "new mechanists" is that, in general, the former is much more ontologically committed. Indeed, Bunge advocates a rather strong form of scientific realism in which the referents of scientific knowledge are not merely mental phenomena, but real things and processes (except in the case of the scientific study of the mind, where mental phenomena are objectively studied with the methods of science). Bunge then states that science is objective, more precisely that science does describe the independent reality that studies. Yet, these descriptions are not pictures of reality, but have a sui generis, symbolic

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[38] Bunge calls this methodological stance 'scientism', the view that the best way to know the world is scientific research. It has to be said, however, that, Bunge's scientism is quite different from the one popularized by A. J. Ayer (1977), for example. In fact, Bunge explicitly rejects Ayer's notion because of its positivist commitments. Moreover, Bunge's scientism is not exclusivist, for he admits of other kinds of valuable knowledge, such as common knowledge. Bunge (1998b: 10) emphasizes that "science has not the monopoly of truth, but only the monopoly of the means for checking truth and enhancing it". This is why should science conflict with other ways of knowing the world, preeminence should be given to the former. The reason of the epistemic superiority of science compared to other epistemic sources lies in the application of a general research strategy that he identifies with the scientific method to answer scientifically posed questions (See Bunge 1998ab).

nature, mainly characterized by being indirect (i.e., mediated by idealized models and theories) and partial (i.e., only certain aspects of an immensely rich and complex reality are selected and incorporated to scientific models and theories). As a consequence, it is important to distinguish facts —among them mechanisms— from the models that scientists use for studying them.

Returning to mechanisms, the main ontological theses in Bunge's systemic ontology that are relevant to our study of mechanisms are the follwing[39]:

(i) All that exists is a system, a component in a system, or is about to be captured by a system.

(ii) A system is a complex object such that (a) is composed of a number of mutually connected parts that make up the *composition* of the system; (b) its component parts are variously connected to other things —most of them systems themselves— that do not belong to the system of interest and constitute the *environment* of the system. (c) All relations among the system's component parts and between these and the environment make up the *structure* of the system —the *endo*structure in the former case and the *exo*structure in the latter one—. (d) The system emerges, persists, changes, and eventually breaks down as a result of a group of processes specific of each one of the system's behaviors —that Bunge calls *mechanism*— that connect the component parts among themselves and with some elements in the system's environment.

(iii) As a consequence of the operation of mechanisms, systems possess global (emergent) properties that their components lack. Emergent properties

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[39] The main source of these theses is to be found in Bunge's *Treatise on Basic Philosophy*, especially in volume 4, *A World of Systems* (Bunge 1979). However, Bunge has elaborated on his systemism or applied it to different fields of knowledge in a number of articles and books. Among the latter, *Foundations of Biophilosophy* (Mahner and Bunge 1997) and *Emergence and Convergence* (Bunge 2003) are especially relevant for the ideas discussed in this dissertation.

imply the emergence of qualitative ontological novelty in a system relative to its precursors, and the very emergence (or eventual submergence) of the system is the emergence (or eventual submergence) of its (essential) global properties.

(iv) Systems' component parts may be —and usually are— systems themselves, that is, subsystems, so mechanisms (of systems and subsystems) also occur in a nested array.

(v) Processes are sequences of events; events, in turn, are changes of state in (real) things.

(vi) Moreover, processes can be causal or random, so mechanisms can be causal, random, or mixed, and the latter type of mechanism is the most common.

In sum, for Bunge a mechanism is a specific process —thesis (ii)— that gives rise to a concrete (real) system, keeps it together and working, and makes it change or break down (ii). In other words, mechanisms are the specific ways in which certain properties of a system —and the system itself— change. Thus, Bunge's ontology has no use for processes —i.e., chains of events— in themselves: there is no change without a changing thing; nor are there immutable things. In fact, for Bunge, real things are always (essentially) changing things, be they systems or their component parts. Furthermore, in Bunge's "to be is to become" (Bunge 1979). In this, Bunge's metaphysics of mechanisms is roughly similar to that of Machamer and collaborators for both recognize that things (entities) and their doings (activities) are inseparable. Yet, Bunge does not feel the need to take up a metaphysically dualistic stance —or a "quadruplistic", for Bunge also distinguishes the environment and the structure of a system, remember thesis (ii)—, the way MDC do. In fact, Bunge emphasizes in numerous places of his work (e.g., Bunge 1959, 1977, 1979, 2003) the basic ontological unity of real systems and the convenience of analyzing them in terms of their composition (*C*), environment (*E*), structure (*S*), and mechanism (*M*), all of them at a given level of organization, that is, by means of the *CESM* approach. In relation to systems, Bunge recognizes two types of mechanisms, those that are peculiar of a given kind of system (essential mechanisms) and those that are shared by systems of other kinds (non-essential mechanisms). More precisely, "[a]n essential mechanism of a system is its peculiar funcionting acivity […] the specific function of a system" (Bunge 2004: 193)[40]. In a previous work, Bunge defines an essential mechanism thus:

> Definition 1: If σ denotes a system of kind Σ, then (1) the totality of processes (or functions) in σ over the period *T* is π(σ) = the ordered sequence of states of σ over *T*; (2) the essential mechanism (or specific function) of σ over the period T, that is, $M(\sigma) = \pi s(\sigma) \subseteq \pi(\sigma)$, is the totality of processes that occur exclusively in σ and its conspecifics during *T*.
>
> (Bunge 1979.)

The previous definition, conflates two different notions, namely those of mechanism and function, a decision that may raise confusion in the cases when the same function may be performed by different mechanisms. Indeed, the "function-mechanisms relation is one-to-many", as Bunge (2004: 194) himself recognizes.

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[40] The labels 'essential' and 'non-essential' may be slightly confusing in certain cases, especially if one does not hold ontological views as strong as Bunge's, and when the description of the systems and their mechanisms is not made at the lowest possible level of organization. For example, a (type of) mechanism may be essential for a system in the sense that it is inherent to it, even though systems of other kinds share (different versions of) that mechanism. A case in point is the mechanism of competition, which in its general form occurs in all kinds of systems, from chemical ones —e.g., inhibitory competition in enzymes— to social —e.g., economic competition— and ecological ones —e.g., interspecific exploitative competition—. (See Keddy 2001).

According to Bunge "[a]ll mechanisms are stuff-dependent and system-specific." and they "... can be grouped in natural kinds, such as those of fusion or fission, aggregation and dispersion, cooperation and competition, stimulation and inhibition, blocking and facilitating..." Classing mechanisms is important because "[t]he formal analogies among mechanisms involving substrates or stuffs of different kinds facilitate the task of mathematical modeling, since one and the same equation, or system of equations, may be used to describe mechanisms involving matter of different kinds" (Bunge: 2004: 195). The notions of function and mechanism are complementary and functional explanations are useful though they lack the depth of mechanismic ones.

Another important aspect of Bungean mechanisms is that they are always particular. More precisely, real mechanisms are always particular, even though we may group them in types, classes, and even natural kinds[41], as ecologists do with facilitation and competition among organisms but this is a different thing altogether. In Bunge's metaphysics, classifications are conceptual (or ideal) objects, not concrete (or real) ones; consequently, they may be useful devices for introducing order in our descriptions of the world and they may even represent the world to a varied degree of success, but in the end, they are just ideas.

According to Bunge's conception, mechanisms are always particular, even when they are usually highly complex sequences of events that more often than not take place at different levels of organization. Take, for example, the case of ecological succession. As we saw in Chapter 2, ecologists consider

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[41] Bunge (1979) conceives of natural kinds as classes composed by things that share some or all their essential properties.

succession as a mechanism by which certain ecosystems emerge and change in areas whose vegetation has suffered strong perturbations. Now, when a finer-grain description of the mechanism is supplied, 'succession' is seen to refer to a complex net of sequences of varied sorts of events, some causal, some chance, that take place either synchronically or diachronically. For instance, succession may take place through processes such as facilitation and competition, separately or combined. Facilitation and competition, then, are mechanisms of succession. In turn, each one of these two mechanisms may take place through different lower level processes, such as the alteration of the soil and the modification of available light radiation. Soil, in fact, may be altered in several different ways, among them modifying its water content or the proportion of certain nutrients in the soil. Of course, each of these mechanisms may take place through still lower level mechanisms. Indeed, as in the case of the other contemporary mechanismic philosophers, Bunge notes that mechanisms generally occur in nested arrays (v). This is because systems occur in nested arrays and systems and mechanisms go together. Indeed, more often than not, the components parts of a system are systems themselves and emerge (persist, change and break down) as a result of certain mechanisms. Thus, in Bunge, nested mechanisms correspond to nested systems, which in turn correspond (or, better, give rise) to different levels of organization[42]. However, Bunge (1969) takes a more etymological perspective on the notion of hierarchy than the new mechanistic philosophers, for he rejects the use of the term for characterizing the relations between different levels of organization. The reason is that the relation of dominion implied by a strict hierarchy does not occur between levels of organization. For Bunge, "lower" levels *precede* and provide the raw

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[42] For more on Bunge's levels ontology see Bunge (1969; 1977: 47-48; 1979: 13; 2003: 10-17, 78-80, 133-136). For an assessment of Bunge's view of levels associated to the concept of emergence see Blitz (1990).

materials for the emergence of “higher” levels of organization, but in no sense dominate them; that is, “lower” levels are not lower at all than “higher” levels, which are not higher at all. In other words, while levels are not “independent” or “autonomous” one from another, all levels of organization are in principle equally important. Thus, the main relationship between levels of organization is that of precedence, which —contrary to a number of philosophies that admit of supervenience— does not imply any sort of ontological privilege of the lower levels with regards to the higher ones. Consequently, in Bunge’s view no ontological reduction is justified by the existence of levels of organization, each of which is characterized by the occurrence of emergent (or global) properties, that is, properties that the component parts of the corresponding systems lack.

As point (v) above states, according to Bunge, mechanisms can be causal, random or mixed. A causal mechanism —or, rather, the causal portion of a mechanism— is, of course, a process in which events are linked by the cause-effect connection. Indeed, for Bunge, causation is defined for events, not for things or properties. “In other words, causation is a mode of becoming, not of being” (Mahner & Bunge 1997: 37) and it is a special mode of determination (other being probabilistic determination)[43]. A cause is not a thing but an event —a change of state in a thing— that produces another event —a change of state in another thing— and it does so by means of energy transfer.

The particulars of such energy transfer determine an important difference between two varieties of mechanisms distinguished by Bunge, one

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[43] For Bunge’s central work on causation and determinism the interested reader may consult Bunge (1959), a book-length treatment of the causal relation, as well as Bunge (1977: 210-11, 320-27; 1982; 2006: 88ff).

characterized by a strong transfer of energy[44] and another one characterized by a weak transfer of energy. In the first case —which Bunge calls also *complete event generation*— the energy transferred makes all the work in producing the effect. One example of this is when wind moves the seeds of anemophilous plants, disseminating them faraway from the parental plant (a seed dispersal mechanism, Nathan et al. 2002). In this case all the effect (seed movement) is due the cause (the wind *pushing* the seeds). Another example is that of a zebra bone breaking under the pressure of a lion's jaws. All the work is done by the jaws of the lion *pressing* (cause) on the bone and *breaking* it (effect).

In the second case, that of *event triggering*, the quantity of energy transferred can be very small, just enough for it to act as a signal that triggers a different mechanism in the thing where the effect takes place. Bunge's example of this type of causal mechanism is a zebra that flees as soon as it catches sight of a lion within critical distance (Mahner & Bunge 1997). Detecting the lion is the causal event that constitutes the stimulus that triggers the fleeing mechanism that brings about the zebra's escape (effect). A different example, one more in tune with the context of this dissertation, is that of seed germination triggered by light. Seed dormancy can be broken by different biotic or abiotic mechanisms, one of them being exposition to certain light wavelengths during more or less specific time intervals[45]. The effect of light on the seed is to trigger a chain of molecular processes that either eliminate inhibitory factors in the seed or stimulate the germination process.

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[44] Incidentally, the author discusses his philosophical notion of energy in Bunge (2000).

For Bunge mechanisms not only may come in nested arrays and co-occur in a given system, but also usually intertwine with each other: “Highly complex systems, such as living cells and schools, have several concurrent mechanisms. That is, they undergo several more or less intertwined processes at the same time and on different levels. For example, a cell does not cease to metabolize during the process of division…” and “[t]he coexistence of parallel mechanisms is particularly noticeable in biosystems and social systems” (Bunge 2004: 193).

Bungean mechanisms are also characterized by being lawful, that is, they behave according to some law(s). However, one needs to be careful with Bunge’s notion of a law of nature, because it does not coincide with the notion scientific lawlike proposition of the received view. Bunge distinguishes four meanings of the term ‘law’, each of them marked by a subscript number from 1 to 4. A law_1, or law of nature, the kind of law that concerns us here, is “an objective pattern of being and becoming”. A law_2 is law *statement*, that is, a statement that represents a law_1. A law_3 is a nomopragmatice statement, or law-based rule (characteristic of technology), while a law_4 is a metanomological statement, or law about other law(s) (Mahner & Bunge 1997: 13)[46].

In Bunge’s terms, then, a law of nature (or law_1) is an objective pattern best

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[45] The mechanism for breaking seed dormancy triggered by light is much more complex than this, on the one hand, because light interacts with other environmental factors, such as temperature and humidity, on the other because a finer description of the mechanism involves rather complex processes occurring mainly at molecular level, from light absorption by phytochrome to the triggering of seed germination processes (Attridge 1990).

[46] The author gives a rather complete treatment of his notion of scientific laws in Bunge (1998a, Ch. 6).

conceptualized as a property of properties. More precisely, for the author two properties are lawfully related just in case that the scope of one of them is included in the scope of the other (Bunge 1977: 77). Bunge's view on laws admits the possibility of laws_1 with very restricted scopes, which consequently do not posses high predictive power. For example, some developmental laws_1 of organisms are possessed by just one biological species. An extreme instance of this is a law_1 possessed by only one individual, as in the cases of plate tectonics (laws of planet Earth) and the specific laws of the last individual of an almost extinct species.

5.3.2 The epistemology of mechanisms

It is time, now, to discuss the epistemological aspects of mechanisms, that is, the role of a description of a mechanism in scientific research, especially in scientific explanation. In this matter, the authors we studied in the preceding sections have a lot in common, though their accounts of explanation present some interesting differences. Following the order in Section 4.4, let us begin our study with the epistemology of mechanisms understood as things (in contrast to entities and activities, or processes).

5.3.2.1 Explanation by description of mechanisms considered as things

Glennan (2002: 346) states that "[t]o mechanistically explain a regularity, *one describes a mechanism whose behavior is characterized by that regularity*." Let us identify the epistemological theses in Glennan's view:

(i) Descriptions of mechanisms (e.g., mechanismic models) have *explanatory power*.

(ii) The *explanandum* of a mechanistic explanation is a robust *regularity*. In other words, explanations that invoke system-mechanisms account for *types* of mechanisms and explain particular cases as instances of a type. From Glennan's examples, type identification seem to rely on some sort of physical similarity, since he justifies neurons being considered a type of mechanism like this: “For instance, the human central nervous system contains around a trillion neurons. There are lots of human beings, as well as lots of other organisms, that have neurons whose structure is similar to that of human neurons. Consequently, one can develop a general model of neurons that subsumes countless neural events” (Glennan 2002: S345). However, Glennan does not elaborate on the matter nor provides a way to assess how similar two systems must be to be included in the same type. In his example with neurons, it is true that nerve cells have a similar internal and external structure that allows for similar functions (similar internal activities and similar roles of neurons in organisms) but this similarity admits a certain degree of variation —number of dendrites, length of axons, etc.— for which Glennan does not offer a metric. Glennan does discuss the type-token relation in his 1996 article, where he states that mechanically explicable laws are token reducible, but not type reducible and that type mechanisms admit of multiple realizability.

According to Glennan (2010a), this is an important difference between the view of mechanisms as systems and its less robust relative, the view of mechanisms as ephemeral processes. Moreover, regularity confers mechanistic explanations properties somewhat similar to those of covering-law explanations (Glennan 2002: S348). The difference is, of course, that the former require the description of a mechanism to be considered and explanation, while the C-L model requires scientific laws in its more traditional sense, that is, as universal, unbounded generalizations. Mechanistic explanation relies on “direct, invariant, change-relating generalizations”, which are robust, but not exceptionless. In his first formulation of the definition of a mechanism, Glennan (1996: 55) recurred to laws[47] as a guarantee of such regularity. However, in later publications he modified the definition in order to avoid the problems associated with traditional notions of scientific laws, which more often than not are too demanding for the sort of generalizations established by the so-called special sciences and even in important areas of physics. According to Glennan, then, generalizations describing the interactions among parts of a mechanism must be *direct*, that is, with no intervening parts between the main relata of the interaction. Those generalizations must also be *invariant*, that is, invariant under a certain range of ideal interventions on the corresponding variable. Glennan borrows this notion of an invariant generalization from James Woodward (2000), who has developed a counterfactual, manipulative account of causation.

(iii) More precisely, a mechanistic explanation consists of a "mechanistic model" made up by two main elements: a behavioral description and a mechanical description. The *behavioral description* consists in a generalization describing the mechanism's overall behavior. The key feature of such generalizations is that they are "mechanically explicable" (Glennan 2005: 446) or, in other words, amenable to be explained by describing the relevant mechanism. Consequently, for Glennan, an explanation is a kind of description, more precisely a *mechanical description*: a conceptual device that provides an understanding of the behavioral description by showing how the parts of the mechanism and their operation bring about the behavior in question. Thus, the mechanical description includes a description of the (stable) spatial and temporal configuration of the relevant parts, as well as a description of their mutual interactions. According to Glennan, most laws (and, I may add, invariant generalizations) are mechanically explicable, but

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[47] More precisely, Glennan (1996) recurs to a Goodmanian notion of a causal law, whose main characteristic is to be a generalization that provides counterfactual support.

"inevitably, there must be some laws that are not". The laws that are not amenable to mechanical explanation are those of fundamental physics, such as Maxwell's equations, for which no mechanisms are known (Glennan 2002: S348).

(iv) An important aspect of mechanistic models is that there is no univocal relationship between the behavioral and the mechanical description. Rather, the relation is one to many, that is, one behavioral description may be explained by different mechanical descriptions.

According to Glennan, one of the problems of Salmon's causal mechanical theory of explanation is that it fails to provide the criteria for solving problems of explanatory relevance (Ch. 3, Sec. 3.5). In Glennan's view, mechanismic explanations do not face such a problem, because they comprise "counterfactual supporting generalizations" describing how interactions between entities bring about the changes we want to explain (Glennan 2010a: 261).

Moreover, invariant generalizations do not only provide the basis for explanatory relevance, but they make mechanismic explanations somewhat similar to Hempelian explanations. The reason is that in both types of explanation the description of the event to be explained is subsumed under a generalization describing a general pattern of occurrence of such type of events. The difference is, of course, that mechanisTic explanation adds a further explanatory device, namely the mechanical description.

5.3.2.2 Explanation by description of mechanisms considered as entities and activities

Now for the epistemological theses in MDC's view of mechanisms:

(i) According to MDC, "Mechanisms are sought to explain how a phenomenon comes about or how some significant process works" (Machamer et al. 2000: 2). Thus, explanation (at least in molecular biology and neurobiology, but maybe in other sciences too) consists in the description of a mechanism.

(ii) Such description may be provided verbally or graphically, among other ways, and may consist in a schema (i.e., an abstract description of a *type* of mechanism) or a sketch (i.e., an abstraction lacking descriptions of bottom out entities and activities or important gaps in its stages).

(iii) A description of a mechanism includes *set-up conditions* (descriptions of entities and their properties, as well as enabling conditions), *intermediate stages* (intervening entities and activities that produce the end from the beginning), and *termination conditions*. Termination conditions are "privileged" states —such as rest, equilibrium, emergence of a new product— of interest for the researcher, and are thus identified by means of practical considerations.

(iv) All such descriptions are general and idealized, that is, they refer to types of mechanisms and to normal conditions and/or situations simplified by (often implicit) *ceteris paribus* assumptions.

(v) In contrast to covering law models, intelligibility is not achieved by invoking a regularity, but in virtue of the (description of the) *productive continuity* of the connections between stages, from set up to termination conditions. In other words, descriptions of some entities and their properties "are crucial for showing how the next step will go" (Machamer et al. 2000: 11). Indeed, (descriptions of) intermediate activities show "how the actions of one stage affect and effect those of successive stages" (*ibid*., p. 12). Moreover, while our representation of intermediate activities is in terms of stages, MDC state, the latter "are more accurately viewed as continuous processes" (*ibid*., p. 13).

(vi) Since mechanisms occur in nested hierarchies —i.e., they are composed of entities and activities belonging in different levels of organization— *description of mechanisms is often multilevel* and bottom out in the lowest-level mechanism, which is that considered relatively fundamental or unproblematic for a particular research.

(vii) Explaining with mechanisms is not necessarily a reductive operation, and it is certainly not in neurobiology and molecular biology. Explanation may consist in exhibiting how a certain phenomenon results from the activities of entities that belong *either in lower or in higher levels of organization* and the integration of both levels may be essential for rendering certain phenomena, such as neural depolarization, intelligible.

(viii) Regularities of mechanisms support counterfactuals.

Explaining with mechanisms, in sum, "involves revealing the *productive* relation" (Machamer et al. 2000: 22, their italics) between entities and activities, on the one hand, and the phenomenon of interest, on the other.

Interestingly, MDC consider that descriptions of mechanisms render phenomena intelligible independently of the correction of the understanding provided by the explanation. The reason is that intelligibility arises "from an elucidative relation between the explanans (the set-up conditions and intermediate entities and activities) and the explanandum (the termination condition or the phenomenon to be explained)" (Machamer et al. 2000: 21).

MDC find that mechanism schemata are used in many ways similar to theories, since they are used to describe, explain, and predict phenomena, as well as to design experiments and interpret experimental results. They also state that "mechanism schemata may also be specified to yield predictions" (Machamer et al. 2000: 17). Another related use MDC assign to mechanism schemata is that of being utilized as "blue prints" for designing research protocols, because they can guide research into mechanisms.

The fate of a sketch may be to become a schema, but it also may be substantially modified or even replaced. Like schemata, sketches are useful for designing observations and experiments, since they point to tasks that are still to be done (see Chapter 6, Section 6.1.2).

5.3.2.3 Explaining by description of mechanisms considered as ephemeral processes

As described in Section 5.3.1.3.1, Glennan puts forth his mechanisms-as-ephemeral-processes view as a means to understand the explanatory power of historical explanations. By historical explanations, he means those explanations that attempt to account for facts that are unique, such as the extinction of dinosaurs or the death of a particular human being, as opposed to repeatable facts, such as the firing of neurons. In other words, the explanandum in a historical explanation is different from that in a system-mechanisms explanation. While in the latter the explanandum is a mechanically explicable generalization, in the former the explanandum is a description of a unique event.

According to Glennan, however, both types of explanations share the form of the explanans, which is a mechanical description that includes the spatial and temporal configuration of the parts of the mechanism, as well as the interactions among those component parts. Expressed differently, in contrast to explanations of robust systems, whose behavior is regular, explanations of ephemeral mechanisms do not explain regularities but particular events. The similarity between both types of mechanisms lies in the regularities of the interactions that bring about the behavior of the system.

According Glennan, while it is true that the spatial and temporal configuration of the parts of an ephemeral mechanism is not stable, the causal interactions among those parts are stable, that is, they are as regular as in the case of system mechanisms.

5.3.2.4 Explanation by description of mechanisms considered as specific processes in systems

Bunge's view of explanation is a rather strong one, both in terms of the importance of explanations in science and in terms of the role that derivation and descriptions of mechanisms play in scientific explanations. Indeed, for Bunge explaining facts and their patterns is the main rationale for the invention and test of hypotheses, laws, and theories (Bunge 1998b).

As for the precise form of a scientific explanation, Bunge provides the following preliminary definition:

> A *scientific explanation* of a formula *q* is an answer to a well-stated scientific problem of the why-kind, consisting in an argument showing that *q* follows logically from a scientific theory (or a fragment of scientific theory, or a set of scientific theories), auxiliary hypotheses, and scientific data, not containing *q*.
>
> (Bunge 1998b: 19.)

Note, in the first place, that according to this definition scientific explanation is not about facts, but about formulas, that is, about scientific propositions. Thus, facts are explained only indirectly, through explaining the explanandum. This is consistent with Bunge's clear-cut distinction between facts and their descriptions, between real systems and conceptual systems. The formulas in the explanandum do not constitute a complete description of the fact of interest, but they are descriptions of selected aspects of the fact under study.

This characteristic of scientific data accounts for the property of all scientific knowledge of being partial. In other words, scientific data describe —frequently in quantitative terms— only certain selected aspects of real facts.

A second aspect of Bunge's definition, an aspect of pragmatic character, is that scientific explanations are answers to why-questions, more precisely to well stated, scientific why-questions. A question —or problem generator— elicits the search for a scientific explanation just in case it is a scientific question, that is, a well-stated question formulated in the context of some scientific conceptual system. This requirement eliminates part of the ambiguity of the question and is designed to protect against the use of scientific resources in attempts to explain largely non-confirmed or even pseudoscientific data or generalizations[48].

A third characteristic of a scientific explanation, according to this definition, is that it consists in a deductive argument. Naturally enough, Bunge also requires that the explanandum does not occur among the premises for this would render the explanation powerless. In a certain way, this view is similar of the traditional nomological-deductive model. The reader may recall from Chapter 3 that Hempel originally conceived of explanations as deductive arguments whose premises include laws, preferably causal laws. Similarly, Friedman and Kitcher took explanations to be derivations of sorts, but in their explications further items were included, namely those aiming at the identification of the derivation patterns that provide the best systematization of the knowledge available —i.e., those that realize the best trade off between minimizing the number of premises and maximizing the number of

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[48] Bunge is well known for his unwavering crusade against pseudoscience, whose philosophical aspects he has treated in several articles (e.g., Bunge 2006b).

conclusions (Chapter 3). Well, Bunge deems that systematization is the basic characteristic of all rational explanation, whether scientific or not. In other words, for Bunge, every rational explanation consists in a deductive argument where the *explanandum* (or problem generator) is subsumed under a set of premises that includes law statements and data. One may provide rational explanations of a variety of items, among them facts, patterns, rules, precepts, and theories. In turn, different types of explanation may include different kinds of generalizations in the *explanans*, such as laws in the case of formal and factual science, and rules in the case of technology (Bunge 1998b). Thus, being nomological and deductive, scientific explanation is a subtype of rational explanation as defined at the beginning of this section. Yet, while mathematical explanations and all explanations in formal science are fully characterized just by being rational, a factual explanation requires something else for being genuinely scientific. In the first place, the context in which the explanation is supplied has to be a scientific conceptual system, that is, a scientific theory or model. In the second place, while a correct rational explanation *accounts* for its explanandum, it does not really *explain* it, unless it complies with one more requirement. This further requirement consists in a description of how the fact described in the explanandum is brought about by the things and the interactions described in the explanans.

Now, descriptions of mechanisms are characteristic of a sort of theories (or models) that Bunge calls representational theories (or models) as opposed to non-representational or phenomenological theories (or models). Thus, since a scientific explanation proper requires a mechanismic law among the premises of the argument, mechanismic explanations can only be supplied by mechanismic (or representational) theories or models (Bunge 1998b). Bunge also uses the term ‘translucid’ to refer to mechanismic theories and this because they are the counterpart of black-box theories, that is, theories that state that two (or more) variables are connected (correlated), but that do not include descriptions of the processes that connect them, whence the comparison with black boxes (Bunge 1964, 1979). Black-box— and translucid theories are the extremes of a gradient[49] and translucidity is always relative. Indeed, since systems and their mechanisms are frequently composed by nested arrays of subsystems and submechanisms, their behavior may be given different explanations according to the needs of research.

Note that the requirement of mechanismic laws in the explanans is very different from that of “causal” laws occurring in the D-N model. In the first place, Bunge does not require *causal* laws, but a *mechanismic* ones, which may be causal or not. Moreover, the concepts of causation involved in each of these two theories of explanation differ from each other. To be sure, the mechanism described by a mechanismic law$_2$ may have causal portions or even be causal throughout, but the requisite of it being mechanismic is not aimed at including causes only but any form of determination, including probabilistic determination.[50] Certain mechanisms or portions of them need to be described by probabilistic law statements and certain mechanismic explanations must include probabilistic mechanismic premises. Yet, whether probabilistic laws occur in the explanans or not, the logical form of the explanatory argument is always deductive. This goes against the view that there is a kind of explanation that consists not in a deductive argument, but in some sort of probabilistic or statistical inductive argument. Indeed, Bunge

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[49] The reference is to translucid instead of transparent boxes because human knowledge is limited in so many ways that even scientific theories, being partial and relative (though not subjective) can only be conceived of as translucid in the best of cases.

[50] In Bunge’s ontology, causes are not propensities, though probabilities are; consequently, they cannot be elucidated in terms of probabilities. Moreover, while Bunge admits of irreducible chance, he conceives of it as a mode of determination because there are probabilistic laws. Thence, either causal, random or mixed, mechanisms are always deterministic —i.e., lawful— in Bunge’s sense of the word (see Bunge 1959, 2003).

rejects the possibility of inductive-statistical explanations based on his view that probabilities are predicated of facts, not of propositions (e.g., Bunge 2006a). Thus, explananda —which are descriptions of facts, hence propositions— cannot be made more or less probable by their premises —which are propositions too—; only the facts described by propositions can be probable to a certain degree.

It is also important to note that Bunge's notion of causation does not boil down to that of regularity as in Hempel's empiricist view. In other words, Bunge is not a causal regularist. As we saw in Section 5.3.1.3.2, Bunge understands the causal relation as a productive connection between events effected through some sort of energy transfer.

In sum, in factual science, explanatory power comes from the description of the *mechanism* that brings about the fact described in the *explanandum*, a fact that frequently consists in the emergence of a system or some change in it. This is why while scientific explanations may rightfully be considered as answers to (scientific) why-questions, they are more precisely characterized as answers to scientific questions of the "how does it work?" type (Bunge 2004).

The central role Bunge assigns to explanation in scientific research comes from the seminal power he attributes to the practice of scientifically searching for an explanation of a fact. Indeed, while an explanation is a deductive argument, the process of searching for an explanation proceeds inversely to deduction. In the latter we go from a set of known premises to the conclusion of a deductive argument, but in the former, the direction of the procedure goes in the opposite direction, from the *explanandum* to some set of propositions with explanatory potential in the case in question. More often than not, one or more of such premises —especially the mechanismic laws— are unknown and must be conjectured and tested before they can become part of the *explanans*. This epistemological aspect gives scientific explanations —rather, the search for them— part of their heuristic power and stimulates scientific research. In Bunge's words, "[t]his is why problems and particularly why-problems are the spring of science [...] and this is why the demand to stop explaining concentrating on description or remain content with what has been explained lead to killing science" (Bunge 1998b: 8).

Besides its epistemological aspect, all rational explanation possesses other important aspects, namely pragmatic (being answers to why questions), semantical (referring to propositions), syntactical (being deductive arguments), psychological (providing understanding), and ontological. About the latter, Bunge points out that "we may say that to explain a fact expressed by an explanandum is to fit the said fact into a nomic pattern expressed by the law(s) or the rule(s) involved in the explanans—i.e. to locate the fact in a system of lawfully interrelated items" (Bunge 1998b: 7). We should not let this phrasing full us. The system of laws performing the explaining of the proposition in the explanandum does not gain its explanatory power from some kind of nomic expectablity. Fitting the fact to be explained into a nomic pattern provides the rational quality of the explanation and a certain degree of intelligibility, but the root of the explanatory power of scientific explanation lies somewhere else; more precisely, in the premises describing the mechanism —or *modus operandi*— responsible for the occurrence of the fact to be explained. In other words, the explanans must include at least one mechanismic law, a generalization describing how the interactions between certain entities bring about the fact to be explained.

Let us elaborate on one of the examples that Bunge provides in order to compare merely subsumptive explanation with mechanismic explanation. In Bunge’s example, the problem generator is the death of a man upon having ingested a certain (relatively high) dose of strychnine. My version of the example will be the death of a Turkey vulture (*Cathartes aura*) found a few meters away of a carcass of a dead grey fox (*Lycalopex griseus*). The question to be answered by an investigation is “why did the vulture die?” Autopsies practiced on both individuals revealed that the fox died a few hours before the vulture, that the vulture’s stomach contained undigested remains fox coming from the fox carcass, and the presence of high contents of strychnine in the bodies of the two animals.

A subsumptive explanation of the fact of the vulture’s death may be supplied by stating that strychnine is lethal at certain (high) doses with the aid of data such as the quantity of strychnine found in the vulture’s tissues. Put in a slightly different fashion in order to show the pattern of the explanation: Relatively high doses of strychnine are lethal (generalization) and the vulture in the example took a dose of strychnine high enough to be lethal by eating from the carcass of the fox (datum), then the vulture died (explanandum) by strychnine intoxication. While this type of explanation may be useful in certain spheres —e.g., in conservation biology—, in Bunge’s view it is a limited sort of explanation that will usually leave scientists unsatisfied for the question remains of why the ingestion of strychnine (at certain doses) brought about the vulture’s death. Should we remain content with the previous explanation, Bunge goes on, we would be indulging in some sort of labeling or name calling, an operation similar to that rightly satirized in Moliére’s play *Le malade imaginaire*. In Moliére’s play, one of the characters attempts to explain why opium causes sleep by invoking a certain *virtus dormitiva* of the drug. This amounts to explain why opium causes sleep by stating that it always does (under certain circumstances). But according to

Bunge assigning a name to a fact —or stating that it always happen under some precise circumstances— does not provide a genuine explanation. At least, it does not provide a deep scientific explanation. This is why Bunge submits that a scientific explanation proper has to be mechanismic, that is, a deductive explanation whose premises include a description of the mechanism responsible for the fact described in the explanandum.

In the case of strychnine poisoning, we need to describe the effect of that substance on the functioning of the organism's body. Research has shown that, once ingested, the drug is absorbed in the intestine and transported by the blood to different parts of the body, where it acts as an antagonist of neurotransmitters such as glycine and acetylcholine. Thus, strychnine molecules bind to certain receptors in spinal or brain neurons preventing molecules of glycine and acetylcholine to bind with such receptors. Since glycine, for example, is an inhibitor of motor-neuron impulses, the inhibition of glycine by its competitor (strychnine) results in constant muscle contractions, which affect the respiratory system leading eventually to the death of the individual.

In the subsumptive explanation we understand that the vulture died because strychnine is toxic to Turkey vultures and this particular individual ingested a high dose of the drug. In the mechanismic explanation we understand, in addition, that strychnine affected the vulture's respiratory system by competing with certain modulators of the motor nervous system. Furthermore, we also learn that the vulture ate the bowels of the dead fox, where there was a high concentration of strychnine, and that if the vulture had avoided the intestines of the carcass it had probably not end up poisoned.

This example illustrates an explanation of a biological fact (the death of a vulture) by a mechanism, a net of causal processes, that comprises several interconnected causal chains, at different levels of organization. At least two of these causal chains occur at the biochemical level. One involves the relative stability of the strychnine in the intestinal tissues of the dead fox. The other includes the competitive inhibition of the neurotransmitter glycine by the toxic strychnine once the vulture had ingested the tissues of the fox. Another causal chain, this one at the neurophysiological level, involves the effect of the uninhibited functioning of motor neurons on muscle contraction. Still another causal chain, this one at the level of organs, describes the effect of increased muscle contraction on the functioning of lungs. Still another causal chain consists in the vulture having eaten certain parts of the dead fox, a fact whose description belongs to a higher level than those already mentioned, namely to that of the organism and its relations with the environment, more precisely to the behavioral ecology of vultures.

In sum, while mechanismic explanation is richer and deeper than subsumptive explanation the former is not really an alternative to the latter, but a subtype of it. Here, the central concept is that of depth, which Bunge defines as follows:

> A theory T_1 is deeper than a theory T_2 if and only if (i) T_1 includes higher-level constructs (unobservables) than T_2 (epistemological aspect); (ii) these constructs occur in hypothetical mechanisms underlying the facts referred to by T_2 (ontological aspect); and (iii) T_1 logically explain T_2, i.e. $T_1 \vdash T_2$ (logical aspect).
>
> (Bunge 1998b: 577.)

Thus, according to Bunge, mechanismic explanation is superior to merely subsumptive explanation on several accounts. First of all, subsumptive models of explanation account only for the logic aspect of scientific explanation, but the mechanismic view is rooted on a systemic view of the world. Mechanismic explanations hypothesize the relevant components, environment, structure, and specific processes of the systems (subsystems and supersystems) under study. Secondly, the abundance of information in mechanismic explanations makes them empirically richer and more specific than merely subsumptive ones. Mechanismic explanations typically involve more levels of analysis than phenomenological theories and being more specific and having more empirical content, the former are more exposed to empirical tests than the latter.

In the poisoning example there is an important methodological difference between the procedure for providing a subsumptive explanation and that for supplying a mechanismic one. In the former, analysis ends up with the generalization that strychnine is lethal at certain doses, while the latter involves a deeper inquiry, one that points to further spheres of knowledge that are likely to require further research. Thus, a third reason for the superiority of mechanismic explanation is that its pursuit provides a much stronger stimulus for scientific research than that of the search for mere subsumptive explanations.

Bunge also claims that mechanismic explanations are superior to phenomenological ones also from a praxiological point of view. Being symmetrical with prediction subsumptive explanation can certainly offer some help in manipulating the system of interest. Black-box models, however, are of little help when the system behaves unexpectedly because of some factor hidden in the black box. The mere possession of a strong generalization or even a law will not suggest where or how to intervene for manipulating the system effectively, either for restoring its previous behavior or for further altering it. In contrast, representational models suggest some lines of thinking, as well as some courses of intervention, when the expected behavior does not show. Mechanismic explanation —the sort of explanation supplied by mechanismic or representational models and theories— describes the inner workings of the system whose behavior is to be modified. As a consequence of this, mechanismic explanations offer a preliminary blueprint for understanding what might have happened, as well as some guidance about what needs to be altered if the goal is to restore (or maintain or further modify) the behavior of the system of interest.

All said, in Bunge's view subsumptive explanations and mechanismic ones are not the horns of a dilemma, but they are frequently two stages in a scientific research. Besides, the representational character of mechanismic models and theories is a matter of degree. Indeed, between a black-box model and any translucent one, there may be a number of grey models with different degree of depth.

As for the relation between understanding and explanation, Bunge deems that the former is a psychological aspect of the later and that as such is relative to the subject who understands. He argues that the same proposition that can be transparent for one person can be utterly opaque for another, and that this relativity of understanding would preclude, in principle, its being used as a criterion for a good explanation. In the case of non scientists —or rather, non specialists— the role of (intuitive) understanding is clearly relative to the subject and consequently subject to unscientific biases. Indeed, since understanding depends on the subject's knowledge, which may be utterly unscientific in the worst of cases, its use as a criterion is still less recommendable. Moreover, because one usually understands by reducing unfamiliar ideas to familiar ones, common understanding goes in the opposite direction of scientific understanding, which more often than not requires the invention of novel hypotheses about unseen processes and/or entities. In other words, Bunge's view of scientific understanding goes hand in hand with his views on explanation, because the sort of understanding that scientific theories and models provide is quite different from common-sense understanding. The main difference between these two kinds of understanding is, in fact, a central difference between scientific and ordinary knowledge. While the former seeks to understand the familiar by means of the unfamiliar, or the unfamiliar by the more unfamiliar, that is, going deeper in the nature of things, the latter consists in providing familiar relations as analogies of the less familiar connections occurring among the things of interest.

However, we should take into account that here we are not discussing ordinary explanation, but scientific one. Perhaps understanding may be a useful criterion after all, but only if a certain cognitive homogeneity is assumed. This is one possibility, to assume that most researchers in one given field are more or less equally equipped to understand the models of their science. Yet, why use a subjective criterion when there is an objective one available? And such objective —or, at least, intersubjective— criterion for scientific understanding relates to the formal correction, the empirical corroboration and the depth of a given scientific explanation. A correct scientific explanation will supply a certain degree of intelligibility, which will be enhanced as the explanations grow deeper. Taking formal correction and empirical adjustment for granted, we may say that the degree of depth of a theory makes understanding superfluous as a criterion for a good explanation.

5.4 The contemporary mechanismic philosophy, a general assessment.

For all the differences in their views on the nature of mechanisms, the authors I have studied in this chapter share some central ideas. Furthermore, if we add a historical dimension to the development of the ideas of some authors, such as Craver, Glennan and Bunge, we find an interesting phenomenon of convergence. Those commonalities and convergence I deem to be the core of a promising philosophical movement that I have called the contemporary mechanismic philosophy. Other authors have previously labeled the movement centered on the study of mechanisms as the "new mechanistic philosophy". However, in the first place, I prefer not to use the word 'mechanistic' because outside the realm of analytic philosophy of science it has a strong reductionist, positivist connotation I would like to avoid. In the second place, the new mechanistic philosophy does not usually include philosophers such as Peter Railton and Mario Bunge, who have fiercely defended and still defend the centrality of mechanisms in scientific research, especially their importance for scientific explanation. I submit that there is a process of convergence taking place among all contemporary mechanismic philosophers, even when they use different vocabularies and emphasize different aspects of mechanisms. This convergence I take to be towards some form of scientific or naturalistic systemism, faraway from the strong reductionist tendencies of radical empiricism and even more faraway from the brands of holism that depart from naturalism, rigorous analysis, and moderate empiricism. The contemporary mechanismic philosophy also shows a convergence towards a certain form of scientific realism faraway from the logicism and from the rejection of metaphysics that characterized the philosophical tendencies heirs to logical empiricism.

In a certain way, much of the "new" philosophical talk about mechanisms is not to so new. Good old wine, however, may continue to make good wine when in new bottles and I take this to be the case of the philosophies of science that assign a central role to mechanisms and systems in scientific research. Thus, I submit the idea of a contemporary mechanismic philosophy in the hope of a future synthesis of the views I have discussed in this chapter.

In the following sections I will try to summarize the common features of those views and attempt to sketch the strengths of the contemporary mechanismic philosophy.

5.4.1 On the nature of mechanisms

The philosophers I include in the contemporary mechanismic movement are committed to a handful of ontological (Table 5.1) and epistemological (Table 5.2) theses that in my opinion justify treating their contributions as a philosophical movement.

One common feature of those authors is that they consider that mechanisms possess two main, related aspects, which are integral to their being a mechanism. On the one hand, there is a thing (or entity) aspect that involves the material objects that constitute the mechanism. On the other hand, there is the activity (operation, process or interaction) aspect, which consists in what the entities that make up the mechanism do. Indeed, while the emphasis in one of these aspects varies with the author, all of them deem that both aspects are essential to mechanisms. The reason is that they share the idea that it is always things that engage in activities (MDC) or perform operations (Bechtel) or enter certain interactions (Glennan) or undergo certain processes (Bunge; Glennan). This, in turn, implies two theses: first, that without things there cannot be activities; second, that, at least when it comes to mechanisms, things are always doing something. In other words, entities and activities (or things and interactions/processes) go hand in hand.

Table 5.1 Ontological aspects of mechanisms according to four views on the nature of scientific mechanisms, namely those of Glennan (2002), MDC (Machamer, Darden & Craver 2000), Glennan (2010a), and Bunge (2004). References are to works representative of the authors' relevant proposals on mechanisms.

Ontology	**Glennan (2002)**	**MDC (2000)**	**Glennan (2010a)**	**Bunge (2004)**
Nature of Mechanisms	complex systems	entities & activities	ephemeral "processes"	specific processes in systems
Composition	objects	entities & activities	objects?	network of processes
Relation to phenomena	produce phenomena	produce phenomena	produce phenomena	produce phenomena
Modes of causation	productivity	productive continuity	productivity	productivity by transferring (a) energy (b) signal
Productivity	from causal interactions	from causal activities	from causal interactions	from networks of processes, causal or random.
Regularity	repeatable event	regular "always or for the most part"[51]	unique	lawful, repeatable or not
Organization	determinant & robust configuration	determinant, robust temporal & spatial	determinant & not-robust configuration	determinant robust or not
Relation w/causation	causes are mechanistic outside fundamental physics	mechanisms are causal	causes are mechanistic outside fundamental physics	mechanisms may be causal, random or mixed

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[51] Machamer et al. (2000: 3).

Table 5.2 Epistemological aspects of explanations by mechanisms according to four views on the nature of scientific mechanisms, namely those of Glennan (2005), MDC (Machamer, Darden & Craver 2000), Glennan (2010a), and Bunge (2004). References are to works representative of the authors' relevant proposals on mechanisms.

Epistemology	**Glennan (2005)**	**MDC (2000)**	**Glennan (2010a)**	**Bunge (2004)**
Logical form of explanation	non-inferential	non-inferential	non-inferential	deductive
Explanandum	mechanically explicable invariant change-relating generalization	termination conditions	singular datum	scientific empirical generalization
Explanans	invariant change-relating generalizations	start-up and intermediate conditions	invariant change-relating generalizations	mechanismic laws & boundary conditions
Generality	type-mechanism	type-mechanism	unique event	type of mechanism
Multiple realizability	mechanical explanations admit of multiple realizability	_	does not admit multiple realizability	functional accounts, not mechanismic explanations admit of multiple realizability
Reductive	token reducibility, not necessarily	not necessarily	_	not necessarily
Intelligibility provided by	invariance	productive continuity	invariance	deductive form (superficial) & mechanism description (deep)
Counterfactual support	yes	yes	no	not relevant
Research strategy	mostly analysis	analysis & synthesis	mostly analysis	analysis & synthesis

Philosophers in the mechanismic movement also agree in that an important characteristic of mechanisms is their productivity. That is, in mechanisms the component entities *produce* the phenomenon of interest in virtue of the activities, interactions, operations or processes in which they take part. Productive activities, in turn, require that both components and activities be organized in certain ways. Thus we have two more important common notions in contemporary mechanismic ontology, that of productivity and that of an organized complex object (or system).

Production is the relevant result of activities, processes, operations or interactions that take place in a given complex object, whereby they bring about the changes that make up the complex-object's behavior we want to explain. More precisely, the component parts of the complex object engage in certain processes that are responsible for chains of changes that eventually make up the behavior —or fact— of interest, which is why the description of the former explains the description of the latter[52].

Production, then, is associated to sequences of changes or events in certain things that possess the capacity of making further changes in the same or other things. In a few words, production is the emergence of change (which includes stationary change or stability) from previous events. Here we find slight differences between MDC and Glennan on one side, and Bunge on the other. While MDC and Glennan consider causal chains or nets as the basis of production (e.g., Glennan 1996, Machamer 2004: 29), Bunge also makes room for productive non-causal, especially probabilistic, chains of events. Glennan does consider chance in his analysis of ephemeral mechanisms, but

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[52] This is not Craver's position, however. His claims that real mechanisms explain the facts of interest, and that this is the reason that models representing mechanism explain (Craver 2014).

only as a disruptive factor that alters the way in that the parts of the mechanism come together. In this sense, ephemeral mechanisms are mechanisms *in spite* of their parts being assembled in a chancy fashion. The productive work is still done by the causal robust interactions among the parts contingently configured. In contrast, Bungean mechanisms may include some portions whose determination is genuinely random, as in the case of the mechanism of chromosomes crossover or that of the radioactive decay of a piece of uranium (for the notion of probabilistic determination see Bunge 1959, 2003). Yet, this difference is of limited importance in the case of ecological events, where chance seems not to be of the irreducible kind admitted in quantum theories, but a product of the accidental intersection of causal trajectories.

In general, I consider that Bunge's views are more elaborated than those of the other philosophers of mechanismic leaning. The main reason is that Bunge provides precise elucidations of the notions involved —object, thing, system, cause, process— and a more general ontological and methodological framework in which mechanisms fit. For example, since Bunge's metaphysics is avowedly dynamicist —the main token of real (concrete) objects is changeability or, as one of his ontological mottos states, "to be is to become" (Bunge 1981: 30)— he considers that not only apparent changes are in need of explanation, but also (relative) stability needs explanation. Thus, for Bunge, descriptions of the relevant mechanisms explain not only the emergence (or coming to being) and the changes of organized, complex objects (i.e., systems) but also their (relative) stability, which is a form of change. A case in point is that of homeostasis or the maintenance of a relatively constant internal milieu in organisms. Biologists explain homeostasis as a result of a multiplicity of mechanisms operating in an organism. To indulge in metaphor, stability is not immobility but an analogue of the Red Queen's-race principle already proverbial in the realm of evolutionary studies, which states that organisms must “run” fast to keep in the same “place” relative to a changing environment.

Another common feature of the contemporary mechanistic philosphy is that mechanisms are associated to organized complex objects or systems. Bechtel and Craver (2006) call this the “componential aspect” of mechanisms, which is the property of being composed by “working parts”. In other words, simple objects cannot be mechanisms (MDC and Glennan), nor, by definition, can they have mechanisms (Bunge). Thus, the notion of a mechanism in these authors is associated to that of a system. Indeed, the entities that make up mechanisms are not just heaps of things but they are organized in a certain way. Organization includes some spatial and temporal order, both in the occurrence of things and in the activities in which they are involved. In consequence, descriptions of mechanisms should include descriptions of the things involved, the activities they perform —or processes they engage in— and their organization, that is, the relevant relations among the mechanism’s components themselves, as well as those with things in the near surroundings. Complex objects with a definite organization (structure or “architecture”) are usually called systems, so we may well state that a shared thesis of CMP philosophers is a minimal systemism, that is that there are systems. Mechanisms and systems go hand in hand.

It is true that Machamer and collaborators do not make explicit references to systems. However, their explication of mechanisms seems to presuppose some notion of a system because they emphasize the organized character of the entities and activities that make up mechanisms. The same presupposition seems to be in place when they state that “[t]he entities and activities in the mechanism must be understood in their important, vital, or otherwise significant context, and this requires an understanding of the working of the mechanism at multiple levels” (Machamer et al.: 23). In fact, one of the authors does acknowledge in one of his books (Craver 2007) the influence of the systemic tradition on his work. As for Glennan’s position, though he does not define the concept of a system, his view is obviously systemic in his mechanisms-as-*systems* model of explanation. In this case, mechanisms themselves are conceived as systems, that is, organized complex objects with a global behavior and properties that their component parts lack.

Yet, systems are conceptualized in different ways in the literature and thus systemism comes in a variety of forms, some of them close to holism, the thesis that wholes tower over their parts.

‘Holism’ and ‘systemism’ may have different ontological, epistemological, and methodological meanings depending on the author. However, there are certain common grounds. Ontological holists usually state that wholes are qualitatively different from their parts. In this, holism reminds of systemism and sometimes the two are taken to be the same (e.g., Reiners & Lockwood 2010). However, while ontologically similar in this aspect, holism and systemism differ in other ontological and methodological aspects. The holist’s whole is sometimes conceived as a solid block, while the systemist’s whole is always a composite, a complex object (Bunge 2003). Holism is methodologically associated with the study of the global properties of wholes and a tendency to reject analysis and reduction as methodological tools. Systemism, in contrast, is compatible with both types of cognitive operations and even with some forms of moderate (methodological) reductionism, especially in matters of explanation.

Systems are also often identified with mechanisms and even with structures (e.g., Mingers 2011). This, in my view, is the source of a variety of problems, such as that one may end up speaking about the structure of a structure. In this, Bunge's systemism is the more exact, elaborated, and demanding. For a start, the author hypothesizes that the whole world is a (super)system and that it is mainly populated by (sub)systems —i.e., complex objects with emergent properties (Bunge 1979). Secondly, Bunge offers a precise definition of a system (Bunge 1979, 2003, see also Section 5.3.1.3.2) and a whole research approach (Bunge 1998ab) based in that notion, while the other authors remain content with references to undefined notions of system. As I described in Section 5.3.1.3.2 above, Bunge distinguishes between a (real) system and its four different aspects, namely the system's components, environment, structure, and mechanism.

5.6.2 On the nature of explanation

The views on explanation held by the contemporary mechanismic philosophers have more similarities than differences, even though, contrary to Glennan and the MDC tandem, Bunge insists that explanations must be deductive arguments (Table 5.2).

Machamer et al. (2000) and Glennan (1996) admit of the possibility of different kinds of scientific explanation in fields other than neurobiology and molecular biology. On the other hand, Bunge (1964) and Glennan (2002) consider that explanation by mechanisms is more fundamental than other kinds of explanation, at least outside the realm of fundamental physics. In particular, Glennan deems mechanismic explanation to be more fundamental than explanation by unification as characterized by Philip Kitcher (1981; see Chapter 3, Section 3.3.3 of this dissertation). The justification of this claim is an ontological thesis: unification would be possible in virtue of the existence of a more or less reduced number of types of mechanisms common to the great majority of phenomena. For Bunge, on the other hand, unification is a result of the systematic power of theories and their deductive character. This is why he requires scientific explanations to be both deductive and mechanismic. In his view, mechanismic explanations are a subtype of deductive ones and good scientific models should provide both unification and explanation: unification via their deductive nature; explanation in virtue of their mechanismic nature. Thus, in Bunge's account, explanatory models provide both subsumptive and mechanismic explanation. In fact, this is also the case with Glennan's account of explanation by system-mechanisms. According to this view, mechanistic explanation is concerned with kinds of mechanisms, not with particular events (Glennan 2010a: 257). Thus, a system-mechanistic explanation of a given particular event should proceed in two steps. In the first place, one should provide a description of the stable, regular mechanism that produces the relevant kind of event. Then, one should show that the particular event in the explanandum is an instance of a type-level phenomenon, which in turn is a result of the operation of the described mechanism (Figure 5.1). This, according to Glennan (2010a: 257), is the most important difference with Salmon's causal nexus account, which is aimed at directly explaining particular events and ends up inspiring causal narratives.

Summing up, a common thesis in the contemporary mechanismic philosophy —one that is central to their motivations regarding inquiry on mechanisms— is the epistemological thesis that mechanisms descriptions are a source of explanatory power. In other words, according to these authors to explain a phenomenon consists in providing a certain kind of description. More precisely, to explain is to describe the mechanism that produces the phenomenon in the explanandum. Explanations are a source of intelligibility, which in turn results from the description of the sequence (or sequences) of events that *produce* the phenomenon of interest, but expressed through a mechanismic model, not through a causal narrative.

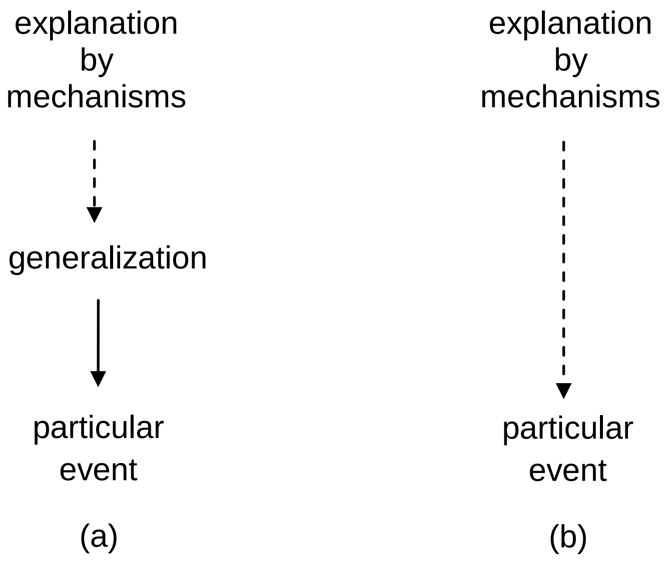

Figure 5.1 Two types of explanation by mechanisms. (a) Indirect explanation by mechanisms (Glennan's systems-mechanistic account and Bunge's mechanismic approach). (b) Direct explanation by mechanisms (Salmon's causal model and Glennan's ephemeral processes-mechanisms account). The dotted-line arrow stands for mechanistic explanation, while the full-line arrow represents subsumption.

These events are called 'activities' by MDC —maybe because they want to stress the productive aspect of the changes, maybe because it was inspired by their philosophical study of neurobiology, where neurons are, indeed, active. Glennan, calls them 'interactions', because he emphasizes the fact that mechanisms are objects made up of components whose exchanges produce the phenomenon to be explained. Finally, those events are called 'specific processes' by Bunge, who is interested in highlighting the dynamical aspect of the description of a mechanism as well as its specificity with regards to a particular kind of system. This implies the ontological irreducibility of the relevant system's properties to those of other kinds of systems, especially those of a lower level of organization, and is the basis for Bunge's pluralism of properties (see Bunge 1979).

5.4.3 On the precise form of our descriptions of mechanisms

Descriptions of mechanisms may have various forms, according to author and completeness, but all of them can be said to be models *lato sensu*, since they consist in idealized representations of the objects and processes associated to the mechanism that is described[53]. In other words, descriptions of mechanisms are scientific models that describe the relevant workings that bring about a certain phenomenon. Being models, such descriptions may have different forms. For example, one and the same mechanism can be described graphically, through either a qualitative (Figure 5.2) or a quantitative diagram (Figure 5.3). A mechanism may also be described verbally, just like Darwin did in his description of the mechanism of natural selection (Darwin 1859). Finally, a mechanism can be given a mathematical description, as in the case of competition in the view of the Lotka-Volterra equations (Chapter 2, Section 2.1.2.3.2).

Note the following points. In the first place, their precise form not withstanding, models of mechanisms are always idealizations, that is, descriptions of simplified, selected aspects of the objects and processes of interest. These idealized descriptions do not represent any concrete, particular instance of a mechanism, but a type of mechanism, and are called “schemata” by MDC when the degree of abstraction —lack of detail— is significant but do not affect productive continuity. In a sense, mechanisms are always described by schemata because there is always the possibility to enhance a given description by adding more detail about the relevant entities and activities (or interactions) they engage.

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[53] In her posthumous book on scientific modeling, Daniela Bailer-Jones defines a scientific model as “an interpretative description of a phenomenon that facilitates access to that phenomenon”, where ‘phenomenon’ refers to “things happening” (Bailer-Jones 2009: 1).

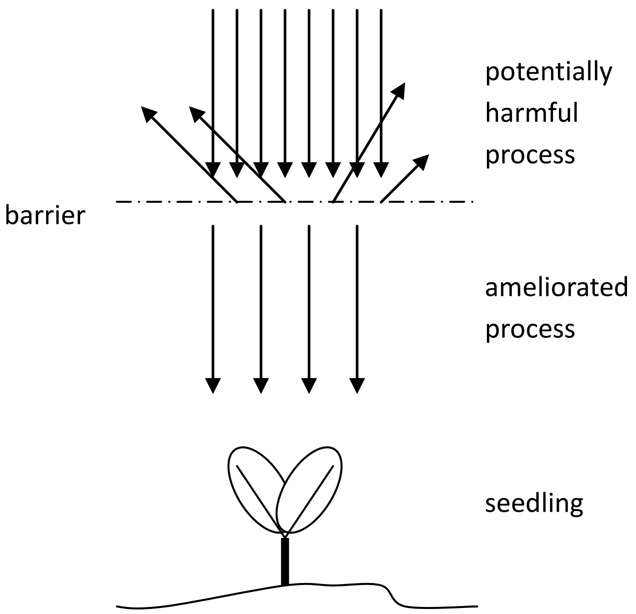

Figure 5.2 Graphical representation of the type-mechanism of facilitation by blocking (see Chapter 2, Section 2.1.1). The shading and the wind amelioration mechanisms can be both represented by the same graphic. The arrows represent any potentially harmful process (light waves in the case of shading, moving gases, vapor and soil particles in the case of wind amelioration. The horizontal dotted line stands for a barrier, that is, any object solid enough to interfere with the trajectories of the foregoing potential harmful processes so as to reduce its potential harmful capacity.

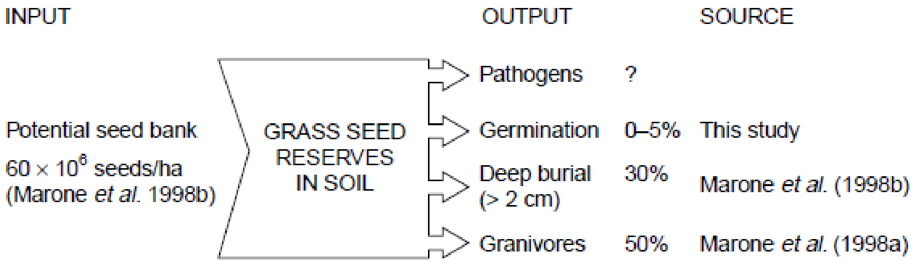

Figure 5.3 Quantitative graphic description (or model) of the different mechanisms of grass-seed losses from the soil bank in the Argentine Monte Desert. Destruction by pathogens, germination, deep burial and granivore consumption are the mechanisms proposed to explain seed losses. Taken from Marone et al. (2000: 948).

Deeper understanding of the mechanisms invoked to explain a certain event —for example, shading in Figure 5.1 or the reduction of the number of seeds in the soil bank in Figure 5.2— usually requires resorting to descriptions of further mechanisms belonging in the same or in lower levels of organization. For instance, once shading has been confirmed as a facilitation mechanism, the task to mechanismically explain shading remains. One may ask, for example whether shading prevents some light waves from damaging the photosynthetic tissues of seedlings, or whether it protects seedlings from hydric stress by preventing soil desiccation? Similarly, once predation by rodents is confirmed as one mechanism for seed loss from the soil seed-bank, it remains to be explored the precise sequence of events that leads to seed removal: Do rodents consume the seeds *in situ* or do they transport seeds to other places? If so, how do they do this?

Descriptions do represent particular mechanisms in the case of ephemeral mechanisms (in Glennan's sense). Yet, the interactions that bring about the fact to be explained are described as types of interactions. Besides, the difference between system-mechanisms and ephemeral-process-mechanisms may be construed as being a mainly epistemic difference, not an ontological one. Here is my reasoning. If two things (and their processes) are never identical, mechanisms are always particular and our ability to classify mechanisms into the type/token categories as well as the possibility to build mechanism general models and schemata rely on the fact that some things (and their processes) are equivalent regarding some aspects for the purposes of description. Equivalence of class is, then, the first epistemological brick of our conceptual buildings for representing types of mechanisms. Now, equivalence of class may be based on objective features of things, but the selection of those objective features is pragmatically laden. For instance, for some purposes it might suffice to say that the mechanism that explains a certain ecological pattern is facilitation. For other purposes, however, the term 'facilitation' might not be enough for other purposes and the researcher might be inclined to deepen her analysis to describe a more precise type of facilitation by shading. For other purposes, still, this might not suffice and a description of the shading mechanism will be sought out. This is because the sources of generality of mechanismic models are: repeatability of the same event in the same thing (ontic, though related to classification), spreading of equivalent events in a population of things (ontic, more dependent on classification), and level of abstraction (epistemic).

A third interesting aspect to be noted is that mathematical formulas only become a description of a mechanism (or of any given thing, for that matter) when they are assigned an interpretation. In fact, this is a general feature of mathematical models: they do not mean anything (factual) in particular unless they are interpreted on the base of some stipulations indicating what the numbers and their relations stand for.

Descriptions of mechanisms can also be classified according to their completeness. On the one hand, MDC propose to distinguish between complete descriptions of mechanisms —those that show the mechanism’s productive continuity— and mechanisms sketches, where start and termination conditions are not described.

MDC state that “mechanism schemata, as well as descriptions of particular mechanisms, play many of the roles attributed to theories” (Machamer et al. 2000: 16). Leaving aside the variety of meanings of the term ‘theory’, especially in fields such as ecology, what is true is that, being descriptions of more or less abstraction, completeness, and generality, schemata (as well as sketches) of mechanisms are just types of models, and models are the usual conceptual tools for describing, predicting, and explaining phenomena and their mechanisms. In fact, as Glennan (2005) has noted, mechanisms sketches and schemata are two stages of what is a continuum in the task of progressively filling in the black boxes in a system. This is also Bunge's view: black boxes, gray boxes, and translucent boxes are all stages in a continuum and are all integral to scientific research. But also integral to it is the gradual replacement of the former by the latter as research on a given field proceeds (Bunge 1964).

5.4.4. On productivity, intelligibility, and explanatory relevance

Another common feature of these philosophies is their emphasis on the centrality of the notion of productivity for mechanismic explanation. The reason is that productivity is both a source of intelligibility and a key to explanatory relevance.

The MDC team asserts that productive continuity from the field's bottom out entities and activities to termination conditions is what provides intelligibility to mechanism descriptions (Machamer et al. 2000: 21). Bunge, in turn, puts forth rationality —i.e. deductive derivation— as what we might call a basic condition of intelligibility, but he hastens to develop his view stating that scientific intelligibility is a matter of degree and, more importantly, that scientists usually strive for higher degrees of intelligibility than those offered by mere derivability. Indeed, according for Bunge, intelligibility is significantly enhanced as explanatory models become deeper. In other words, a given fact grows more intelligible as our descriptions of the specific processes from which that fact sprang involve more relevant detail and a higher number of relevant levels. An important feature of intelligibility as seen by all these authors is that it does not depend on the correctness of the mechanism description.

The processes described in a mechanismic explanation describe how the fact to be explained is produced. This is the solution for the problem of explanatory relevance that haunted the C-L (Chapter 3, Section 3.3.1.2) and the causal nexus (Chapter 4, Section 4.3.4.2) models of explanation. The problem evaporates the minute we accept the notion of productivity.

5.4.5 On the reality of mechanisms

All of the aforementioned authors seem to be realists of sorts in the sense that they assume that the mechanisms studied by scientists are not mere constructions of their minds but have an independent existence. This is especially true of Bunge (2003) and Craver (2014). In fact, without this realism, it would be difficult to see why mechanisms would have explanatory power at all, for according to the mechanisms view of explanation, facts are intelligible only as a result of the description of certain productive or generative activities, interactions, or processes that exist "out there". Consequently, the authors in the CMP distinguish between (real) mechanisms and the conceptual devices (models, schemata, sketches and so on) that researchers make up in order to describe them. This seems to be, in general, the case with MDC, who rightly, I think, state that "thinking of mechanisms gives a better way to think about one's ontic commitments" (Machamer et al. 2000: 23). It is the case even when they suggest that an algorithm that "represents" the central dogma of molecular biology may become "an actual mechanism of a very different kind when written in a programming language and instantiated in hardware that can run it as a simulation" (Machamer et al. 2000: 18). In any case, MDC distinguish between mechanisms and their descriptions, as is the case with Glennan (2005)[54], and indeed with Bunge (1979). The latter two authors note that the same mechanism may be variously described, a statement that assumes that mechanisms and

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[54] Glennan (2005) acknowledges in an explicit manner that he ascribes an ontic conception of explanation.

descriptions of mechanisms are different objects, and that the former are objective in the sense that their existence does not depend on the subject.

In sum, the mechanismic view of explanation presupposes some forms of philosophical realism. In the first place, there is ontological realism, for mechanisms must be independent of the subject's mind in order to be the source of the explanatory power of their descriptions, at least in the manner the contemporary mechanismic philosophers construe them.

In the second place, such descriptions must be aimed at representing mechanisms with some degree of faithfulness. For evaluating this "faithfulness" of mechanisms, Glennan (2005) relies on the concept of similarity in two respects: behavioral adequacy and mechanical adequacy. The behavioral adequacy of a mechanistic explanation relates to the predictive power of the model, more precisely to the scope of the model's predictions. The adequacy of the mechanical description relates to the correctness of the decomposition and localization of the mechanism as proposed by Bechtel and Richardson (1993). Bunge (1974, 2006a), in turn, uses his own notion of partial truth to evaluate mechanismic models, which is a variety of the correspondence concept of truth.

5.4.6 On the regularity of mechanisms

An important feature of the various contemporary approaches to mechanisms concerns the claim that some sort of regularity in the behavior that mechanisms helps explain. At the same time, all the contemporary mechanismic philosophers reject the received view of laws and some of them reject laws altogether. For instance, Salmon (1984), Bunge (2003), and Glennan (1996) require that mechanisms be described in terms of laws: causal laws for the former, mechanismic laws for the second, and "direct laws" for the third one. Yet, Bunge's notion of a law is quite different from the traditional logical empiricist one; and Glennan, in his later work, has changed the requirement of direct laws in favor of *invariant, direct change-relating generalizations* just in order to avoid the problems of the received notion of a law. As for Machamer et al. (2000), they explicitly argue against laws, but at the same time they require regularities that are valid "always or for the most part", a condition that with further qualifications might fit several conceptions of a law of nature, though not, of course that of the logical empiricists. Craver (2007) called these generalizations "mechanically fragile generalizations".

Since this debate on the role of regularities and scientific laws in research is of maximum importance for the philosophy of any science and, furthermore, the alleged lack of ecological laws is one the issues usually debated in the discipline as an obstacle for its due advancement, I will devote some space to it. However, I will not discuss the problem of scientific laws separately, but in the general framework of the subject matter of this chapter, that is, the problem of mechanisms and their different conceptualizations.

As we have already seen, Glennan (2010a) defends that ephemeral processes that bring about unique events should also be considered, their lack of stability notwithstanding, mechanisms. According to Glennan, descriptions of ephemeral processes explain unique events in a similar way that descriptions of system-mechanisms do. The difference is that while the latter explain regularities —"recurrent patterns of phenomena" (Glennan 2010a: 253)— the former explain unique (or "particular") facts. This is, following Glennan, the central difference between ahistorical and historical explanation. In the former, "the same explanation works" for different instances of a regularity, in spite of "subtle differences" between cases (Glennan 2010a: 253). In historical explanation, we need to describe the "particular" process responsible for the occurrence of the “particular” fact.

PART III

EXPLANATION IN ECOLOGY

6

ECOLOGICAL MECHANISMS IN THE LIGHT OF THE CONTEMPORARY MECHANISMIC PHILOSOPHY

In Chapter 2 I mentioned several mechanisms for ecological succession, and chose to analyze cases of explanation in which facilitation and competition are the mechanisms invoked to account for the phenomenon of interest. My decision was motivated by the general importance of these two mechanisms in explaining different aspects of ecological communities.

In the present chapter I analyze how each of the philosophical views on mechanisms and mechanismic explanation discussed in Chapter 4 fares when contrasted to the research cases reviewed in Chapter 2, starting with facilitation and then moving to interspecific competition. In the final section of the chapter I take stock of the strengths and weaknesses of the contemporary mechanismic philosophy in relation to explanatory practices in ecology.

6.1 Facilitation

Facilitation is a positive interaction between organisms, and it has been put forth as a mechanism for explaining certain aspects of ecological succession (see Chapter 2 of this dissertation).

Connell and Slatyer’s view on ecological succession identifies three possible avenues for successional processes, each of them characterized by its main mechanism (Connell & Slatyer 1977). The “facilitation model”, as they call the first avenue for succession, describes facilitation as a general type that comprises several concrete facilitation processes. These processes produce the particular changes that one organism (the benefactor) performs in the environment of another organism (the beneficiary), changes that are beneficial for the fitness of the latter.

Baumeister and Callaway's study on the facilitation effects of *Pinus flexilis* in the survival of seedlings of the conifer *Pseudotsuga menziesii* and the shrub *Ribes cereum* begins by asking the following questions: whether such facilitative effect does occur (a), what the particular facilitation mechanisms are (b), whether these facilitation mechanisms mutually interact (c), whether such interactions vary with climatic conditions (d), or with benefactor species (d) (Chapter 2, Section 2.1.1 of this dissertation).

More precisely, on the grounds of previous observations showing that numerous plant species occur under *P. flexilis* crowns but not in the open grassland, the authors propose facilitation as the general type of ecological interaction that explains the observed phenomenon (a tentative answer to question (a) above). Still more precisely, Baumeister and Callaway hypothesize that a combination of co-occurring concrete facilitation mechanisms —including shading, protection from wind, litter and snow-pack accumulation, and protection from herbivores— are responsible for the spatial distribution of *Pseudotsuga menziesii* and *R. cereum* individuals in the area (tentative answer to questions (b) and (c) above).

Baumeister and Callaway's observations and experiments confirm their hypotheses (a) and (b) and further suggest that facilitation mechanisms themselves interact in a nested hierarchical fashion, with shading as the mechanism determining the relative importance of the other mechanisms, especially that of wind amelioration. Snow accumulation was also important for *Pseudotsuga* sp. in the presence of shading. For individuals of this species, without shading none of the other mechanisms affected the survival of seedlings in a significant manner.

Schematically, the explanation provided by Baumeister and Callaway presents the following form:

Explanandum: Plants grow only under the crowns and at the leeward side of trees.

Explanans: Tree individuals act like barriers that filter potentially harmful luminous radiation and diminish wind speed reducing its kinetic energy and thus its ability to cause mechanical damages by collision and dehydration to seedlings. Thus, tree individuals facilitate (create environmental conditions that enhance) the survival of seedlings. Consequently, whenever seedlings happen to be located under the crowns and at the leeward side of trees, they have more chances to survive.

The previous explanation may be formulated in a different way, namely in terms of fundamental and realized niches because Baumeister and Callaway study is not intended to just confirm and explain the empirical pattern in the explanandum, but also to explore the possibility that facilitation be a key mechanism in expanding or producing the realized niche of the beneficiary species (Chapter 2, Figure 2.3).

In addition to the foregoing, the study shows that in the study area the *operation of the facilitation mechanisms is hierarchical* [“To our knowledge, the demonstration of hierarchically dependent facilitative mechanisms in interactions is unique in the literature”. (Baumeister & Callaway 2006: 1828)] and that facilitative strength depends not only on the species involved, but also on circumstances such as season of the year and biotic factors.

6.1.1 Facilitation as a system-mechanism

For Glennan, a mechanism is a system whose parts interact in a regular manner so that they jointly produce the regular behavior of the system (Chapter 5, Section 5.3.1.1). His model of explanation is about type-mechanisms and these are described by a certain kind of model. Glennan's system-mechanisms can be regular in two senses, namely that their behavior is stable through time (repeatability) and that different tokens of the type system behave in similar manner when in similar conditions.

In Glennan's view, building an explanatory model requires (a) identifying a general pattern of behavior, (b) describing it with an invariant change-relating generalization, (c) identifying and describing the component parts of the system of interest, and (d) identifying and describing with invariant change-relating generalizations the interactions among the system's components that bring about the behavior one wants to explain (see Chapter 5, Table 5.2).

In the case of interest, Baumeister and Callaway provide descriptions of a confirmed pattern of behavior (the spatial distribution of *Pseudotsuga* sp. and *Ribes* sp.) in need of explanation. The studied pattern is local because its main referent is a certain ecotonal community on the east front of the Rockies, but the system's *behavior is regular* in the sense that the spatial distribution of the vegetation they study is temporally stable: *Pseudotsuga* sp. and *Ribes* sp. mainly occur under the crowns of *P. flexilis* on a general basis. Previous work on the problem at hand suggests that in the area the pattern occurs year after year so that the pattern is a repeatable event. Moreover, other studies also suggest that analogous spatial patterns, involving plants species, have been found elsewhere, in areas with similarly harsh environmental conditions, leading the authors to claim that facilitation expands the realized niche of the beneficiary organisms. In other words, the study describes a *type of* *mechanism* —facilitation— that *brings about the pattern of interest* on a regular basis, as required by Glennan.

The study also describes the elements whose interactions are relevant for the production of the pattern and the interactions among those objects that produce the behavior of the system. From Baumeister and Callaway's paper it emerges that the *components of the system* of interest are populations of *P. flexilis*, *Pseudotsuga* and *Ribes*, among the biotic ones; and soil, light, wind, litter, and snow among the abiotic ones. If this is correct, then the composition of the system of interest poses a couple problems to Glennan's view on mechanisms. Glennan requires the parts of a system-mechanism to be objects, meaning highly integrated things, and biological populations are not objects in this sense. It is important to note that in their study Baumeister and Callaway did not manipulate populations, but individuals of each species. However, in order to obtain their explanatory description of the facilitation mechanism the authors generalize their experimental results to populations and to the entire community under study. This step of extrapolating the particular results obtained from measuring the response of individual plants in particular experimental plots accounts for the jump from individuals to populations as component parts in the description of the system of interest.

Likewise, Baumeister and Callaway's empirical investigation did not deal with facilitation interactions *in general*, but with particular cases of four kinds of concrete interactions —namely shading, wind amelioration, litter accumulation, and snowpack accumulation— in particular experimental plots. Their results, however, are meant to be valid for the entire community.

A different problem for Glennan's view is that, while soil, litter and snow are objects in that restricted sense, light and wind are not. Light is a kind of electromagnetic radiation and wind is air molecules moving in a particularly fast and directional fashion. In consequence, Glennan's thesis that components of system-mechanisms must be objects faces trouble when contrasted with ecological facilitation.

As expected according to Glennan's conception of mechanistic explanation, in order to account for the spatial distribution of seedlings of *Pseudotsuga* sp. and *Ribes* sp., Baumeister and Callaway do provide descriptions of the interactions that make up the facilitation mechanisms at the ecological level. Such interactions produce specific changes in the immediate environment of *P. flexilis* individuals and thus in the environment of the beneficiary seedlings. However, it is not clear that the way the facilitation mechanisms operate is consistent with Glennan's conditions for something to be an interaction and, furthermore, a causal interaction. Of the four facilitation mechanisms considered by Baumeister and Callaway, two of them, namely shading and wind amelioration, clearly are not continuous chains of causal interactions and the other two cases, litter and snow accumulation, are suspect.

As for the first two mechanisms, the facilitation effect is identical to diminishing the effects of potentially harmful factors. Such mitigation takes place by partially blocking those factors. In the case of shading, the crowns of *P. flexilis* filter light —i.e., block part of the electromagnetic radiation—, mitigating PAR intensities potentially dangerous to the photosynthetic tissues of the seedlings[55]. In the case of wind amelioration, the facilitation effect obtains by each individual of *P. flexilis* reducing wind speeds, and thus wind's

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[55] A different beneficial effect of shading could be moderating the temperature on leaves and soil, which would prevent the increase of evapotranspiration rates in the former and of evaporation rates in the latter. Yet, the experiments by Baumeister and Callaway (2006) did not detect the occurrence of this effect in soil moisture.

kinetic energy and its potentially harmful abrasive and desiccatory effects on the seedlings located on the leeward side of the tree. (Incidentally, while the effect was measured on an individual basis, it is likely that the grouping of *P. flexilis* stands enhanced the barrier effect of their crowns and trunks thus enhancing also the facilitation effect on seedlings).

The foregoing two facilitation mechanisms operate by blocking different harmful processes —light waves in one case, fast winds in the other— so that the beneficial effect of *P. flexilis* on *Pseudotsuga sp*. and *Ribes sp*. depends not on something that the trees make happen, but on something they prevent to happen. True, there is an interaction phase in each one of those two facilitation processes. In shading, the interaction consists in photons colliding with and being absorbed, reflected and diffracted by *P. flexilis* tissues. In wind amelioration the interaction involves air molecules in fast, directional motion and airborne snow and ice particles on the one hand, and the tissues of *P. flexilis* individuals on the other. However, these interactions do not produce changes but prevent change to occur. In this, the mechanisms of shading and wind amelioration are similar to the cases of so called “negative causation” analyzed in Chapter 4, Section 4.3.5, and provide a good example for investigating “negative causation” in the realm of ecology. Likewise, facilitation by blocking presents a problem for Glennan’s account of mechanisms where (positive) continuous causal interactions seem to be essential.

Baumeister and Callaway’s experiments allow them to explain the distribution of *Pseudotsuga* and *Ribes* in the study area by means of two principal mechanisms of facilitation described at the ecological level, namely shading and wind amelioration. However, they do not remain content with that and, in the Discussion of their report, they resort to an even finer grain of description for analyzing each of the four mechanisms they tested. Indeed, they go down to the physical and the molecular level to describe how each of the plausible explanatory mechanisms works.

Summing up, from a methodological point of view, Glennan’s proposal of scientific explanation fits some of the features of ecological explanation as it emerges from the study on facilitation. However, the adjustment is not perfect. As expected according to Glennan’s account of mechanistic explanation, Baumeister and Callaway’s explanation is quite general both regarding the explanandum and the explanans. Indeed, the phenomenon of interest is repeatable along the temporal and the spatial axes and also the interactions making up the explanans are rather regular and more or less portable depending on the species and the characteristics of the environments where one wants to transport the explanation. Some of those interactions are causal but at least two of the mechanisms (probably the four of them) include breeches in the continuity of those interactions and this is not consistent with Glennan’s characterization of mechanistic explanation. Another challenge for Glennan’s mechanistic account consists in the components of the system under study not being objects in his rather strong sense of an object, but aggregates.

A final issue for the adjustment of Glennan’s of scientific explanation to ecological explanation is that along their article, Baumeister and Callaway use the term “mechanism” to denote the collection of processes that bring about the facilitation effect that is responsible for the spatial distribution of *Pseudotsuga menziesii* and *Ribes cereum* in the eastern front of the Rocky mountains, not to denote the system, an ecotonal ecological community, of which those two plants are components. This suggests that ecologists do not view mechanisms as systems, but as processes in systems.

6.1.2 Facilitation as an entities-and-activities mechanism

Once the fact to be explained has been described, MDC's model of explanation requires that one pinpoints the entities and activities that bring about said fact, as well as its starting-, intermediate-, and termination conditions. Besides, MDC claim that mechanisms are highly organized along the temporal and the spatial axes, so that organization should be reflected in the descriptions of mechanisms.

Entities and activities: In our facilitation example, the relevant entities that make up the mechanism of interest are those already described in the previous section as candidate parts of the system-mechanism, namely populations of *P*. *flexilis*, *Pseudotsuga* sp. and *Ribes* sp., on the one hand, and soil, light, wind, litter, and snow on the other. The relevant activities, however, may be more difficult to identify, partly due to certain ambiguity of the word 'activity', defined by MDC as "the producers of change" (Machamer et al. 2000: 4). One way to understand the term is to distinguish it from the concepts of "interaction" and "process", as the authors seem to intend, by limiting the sense of "activity" to the *active* "doings" of an entity, a sense further connected to productive continuity. This *active* construal of the term activity is also suggested by MDC 's stating that activities "are constitutive of the transformations that yield new states of affairs or new products" (*ibid*., p. 4). If we accept this interpretation, then at least the two main facilitation mechanisms found in Baumeister and Callaway's investigation —i.e, shading and wind amelioration— are not constituted by activities and MDC's account of mechanisms faces a problem similar to that already described in the previous section. Indeed, *P. flexilis* crowns and trunks have an indirect influence on the survival of seedlings of *Pseudotsuga* sp. and *Ribes* sp. but they not "yield new states of affairs or new product". In fact, the crown and the trunk of each individual of *P. flexilis* prevent changes to occur in the environment of seedlings by partially blocking light and wind. Thus, in a sense, its not *P. flexilis* activities, but their “passivity” the factor that brings about the effect of interest. Just like in the case of Glennan’s system-mechanisms, facilitation by blocking seems to pose a problem to mechanisms seen as entities and activities.

Regularity: Machamer and collaborators state that activities possess some characteristic usually attributed to laws, among them regularity. Regularities of mechanisms are non-accidental and support counterfactual.

Facilitation as a mechanism satisfies the regularity condition, as I hope to have shown in the previous section. Furthermore, the regularities in the mechanism are non-accidental and support counterfactuals. Indeed, blocking may not be an activity, but it is a regular process and support the following counterfactual: should blocking not occur the facilitation effect would not take place and seedlings would die.

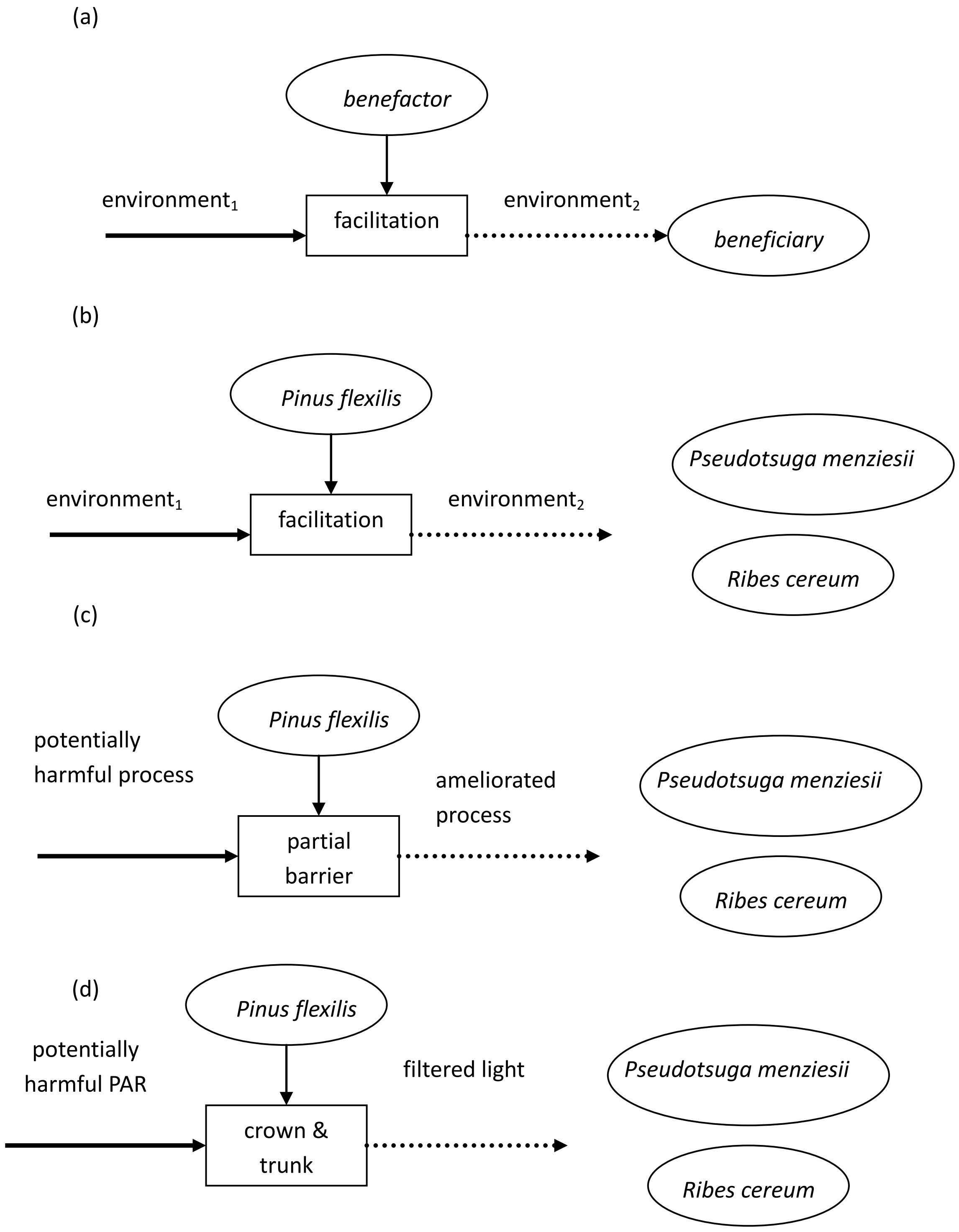
Temporal organization: Successional processes in general do have an important temporal dimension given by the ability of certain species to colonize areas that are not compatible with the survival of others. This is the case of the benefactor species. *P. flexilis*, in Baumeister and Callaway’s article, which is the only tree capable of colonizing prairie grasslands in the studied ecotone. The results of the study suggest that temporal order (perhaps better expressed as precedence) is an essential relation for facilitation to occur at least one aspect, namely that the benefactor organisms must be present in the area *before* the beneficiary ones arrive. At least in the example, individuals of *Pinus flexilis* must have been in the area before the arrival of *Pseudotsuga* sp. or *Ribes* sp. seeds, and not just before but with time enough to allow the individuals of *P. flexilis* to grow a crown. Otherwise, neither the shading nor the wind protection would have been effective. Moreover, facilitation mechanisms were effective in certain seasons and not in others, and seasons differ for each of the beneficiary plants. For example, in the experiments, mortality of planted *Ribes* sp. seedlings was lower under *P. flexilis* crowns and at leeward sites in summer, when the harmful power of the warm Chinook winds is maximal, than in other seasons. However, other temporal aspects of organization, such as duration, rate and rhythm do not seem to be important for ecological facilitation in the area, especially because, as showed above in the analysis of the entities and activities at play in the facilitation mechanisms found, those activities are not “active” but “passive”. In sum, for facilitation to occur minimal temporal conditions must be satisfied, but facilitation itself does not seem to be temporally organized in the way MDC suggest mechanisms should be.

Spatial organization: The location of the entities involved in the facilitation mechanism is important Baumeister and Callaway’s study. Indeed, they confirmed a “strong spatial association” between individuals of the benefactor and beneficiary species (Baumeister & Callaway 2006: 1817) and it is precisely this fact they set out to explain by means of mechanism description. In the study area the facilitation mechanisms operated only under the crowns of *P. flexilis* and on the leeward side of their trunks. Location and orientation are important because the main mechanisms at work in the study site operate by preventing excessive luminous radiation and strong winds —two important threats for seedlings— from reaching the beneficiary plants, and light and wind are strongly directional factors. In other words, the entities that make up the facilitation mechanisms studied by Baumeister and Callaway can only engage in facilitation “activities” if they are spatially organized. The importance of location and orientation for these facilitation mechanisms supports MDC’s condition for mechanisms to be organized.

Form of scientific explanation: MDC analyze the form that descriptions of mechanisms may assume and recognize three of them, namely explanations, mechanisms schemata and mechanisms sketches.

Since a schema is an abstract description of a mechanism and abstraction is a matter of degree, schemata may be more or less abstract. This is precisely the case with Baumeister and Callaway explanation by means of facilitation, which starts as an abstract schema that is modified through research until it becomes a mechanistic explanation (Figure 6.1). This is important because it shows that mechanistic research moves back and forward from different levels of abstraction just as MDC claim.

Like in Glennan's conception, the hierarchical nested organization of mechanisms fits MDC's view, for each of the explanatory mechanisms described by Baumeister and Callaway can be further decomposed into lower-level mechanisms. These lower-level mechanisms help understand the ecological-level effect of facilitation by allowing researchers to predict what would happen to the system should ecological circumstances change. In the case of shading, the lower-level mechanism belongs to the physical and the biomolecular levels. Baumeister and Callaway do not investigate this mechanism empirically, but rely in previous research that describes the effects of photoinhibition in cold temperatures on the tissues of seedlings' leaves.


(a)
benefactor
environment₁
facilitation
environment₂
beneficiary
(b)
Pinus flexilis
Pseudotsuga menziesii
environment₁
facilitation
environment₂
Ribes cereum
(c)
Pinus flexilis
potentially harmful process
ameliorated process
Pseudotsuga menziesii
partial barrier
Ribes cereum
(d)
Pinus flexilis
potentially harmful PAR
filtered light
Pseudotsuga menziesii
crown & trunk
Ribes cereum

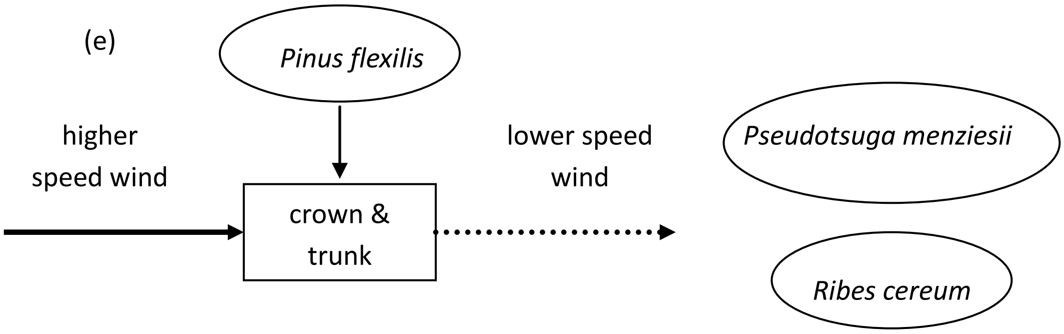


Figure 6.1 A series of explanation schemata for the spatial distribution of *Pseudotsuga menziesii* and *Ribes cereum* according to Baumeister and Callaway's paper (Baumeister & Callaway 2006) from less abstract (a)-(b) to more abstract (e). Graphics (a) and (b) represent mechanistic explanations at the ecological level for two facilitation mechanisms: wind amelioration (a) and shading (b). Graphics (c), (d), and (e) are mechanism schemata that differ one from each other in the degree of abstraction of the description.

In the case of wind amelioration, the authors relate the speed of wind to its power to produce mechanical stress in the seedlings and to physically reduce their boundary layer. Wind can carry snow and ice particles with the consequent abrasive and desiccating effects on seedlings' tissues as they collide with them, on the one hand, and increasing soil-water evaporation on the other. Again, the authors do not test wind amelioration at the molecular level, but rely on previous research to infer the molecular-level explanation from unquoted laws or generalizations of physics. In general, higher speeds increase harm to seedlings so by acting as a barrier and reducing wind speeds *P. flexilis* individuals protect seedlings.

The foregoing gives hints as to where ecological explanations bottom out. It would seem that when attempting to understand the phenomena they study ecologists do not neglect other-level mechanisms, even if they do not conduct research on them. Ecologists use mechanism sketches and schemata of such lower-level mechanisms to provide intelligibility to their own ecological explanations. This feature of ecological explanation supports two more theses of MDC's: that mechanisms are organized in nested hierarchies and that explanations bottom-out at a certain point.

Start-up- intermediate-, and termination conditions: The facilitation mechanisms described by Baumeister and Callaway differ as to their start-up- intermediate-, and termination conditions. In the first place, all of them are variable and vary in their own way. Shading, for example, is intermittent, for the mechanism operates only during the spans when light intensity is high enough to damage the seedlings. Shading does not operate during the night, for example. Besides, cold temperatures are an important condition for photoinhibition, so it is likely that shading mostly occur in winter. Likewise, snow accumulation operates mostly in winter. Wind amelioration also operates intermittently, mostly during the winter. Start-up conditions are specific of each facilitation mechanism. Shading operates when light is too intense for the seedlings to bear, a condition aggravated by cold temperatures. Wind amelioration begins to operate when wind-speed values are high enough to be a threat for seedlings. Again, it is important to note that, in both cases, changes occur in the environmental processes that pose a threat to seedlings while there are no related changes in *P. flexilis* individuals. Facilitation by blocking does not seem to have an intermediate stage at the ecological level. This type of facilitation is simultaneous with the emergence of the threat and finishes when the threat disappears, that is, when light intensity (in the case of shading) or wind speed (in the case of wind amelioration) values occur under a certain threshold.

Facilitation by accumulation on the other hand, does start at a certain point, that is, when the relevant material —litter and/or snow— begins to pile up under the crown of *P. flexilis* individuals simultaneously altering the environment of the seedlings in beneficial ways. Those environmental changes are complex and include a variety of exchanges between litter and the soil on the one hand, and between snow and the soil on the other, depending on the particular mechanism. Such exchanges make up the intermediate stages of the relevant facilitation mechanisms. A for the termination stages of the two accumulation mechanisms, litter accumulation does not seem to have one, because litter precedes seedlings and does not disappear from the environment even when beneficiary plants are full grown; snow accumulation, by contrast is not a continuous process, but an intermittent one. Snow accumulates mainly in winter and melts away when temperatures rise. The varied protective effects of snow on seedlings disappear when accumulated snow melts away.

In sum, of the four facilitation mechanisms studied by Baumeister and Callaway, only one —snow accumulation— satisfies the three stages required by Machamer and collaborators for entities and activities to make up a mechanism.

6.1.3 Facilitation as an ephemeral-processes mechanism

Glennan's view on mechanisms as ephemeral processes is rather similar to that of his mechanisms-as-systems conception. The difference between these two kinds of mechanisms lies in the unstable configuration of the parts of ephemeral-processes mechanisms. In Section 6.1.1 I analyzed the explanation offered by Baumeister and Callaway and deemed it to correspond to a rather robust system and, in consequence, not to an ephemeral-process mechanism. My criteria for doing so were that (a) the explanandum of the mechanical explanation offered by the authors is general and describes an empirical pattern, that is, a representation of a repeatable fact under certain conditions and (b) the interactions that bring about the pattern are also regular in that they regularly bring about the pattern any time circumstances are adequate.

However, for the reasons discussed in Section 6.1, facilitation does not seem to be a system-mechanism in the same sense of a vending machine or of a heart, mainly because the configuration of its parts is not as robust.

6.1.4 Facilitation as a specific-process-in-a-system mechanism

Bunge's view on explanation suggests that in order to scientifically explain the behavior of a given system one needs to describe the mechanism that makes the system "tick". Mechanismic explanation, however, is just one aspect of Bunge's systemism, mainly because mechanisms (M) are just one aspect of systems, the other being their composition (C), environment (E), and structure (S). Thus, in order to explain any aspect of a system one should set to know the other elements that make up the system. In other words, according to Bunge, to tackle a factual scientific problem one must apply the CESM approach (Chapter 5, Section 5.3.1.4).

For groupings of organisms or biopopulations such as the one in Baumeister and Callaway's study, which includes populations of organisms of several different species, Bunge offers the concept of an ecological community or biocoenosis:

> DEFINITION 5.2 A concrete system is a *biocoenosis* or *community* iff
> (i) it is composed of organisms belonging to (at least two) different biospecies (i.e., iff its composition is multispecific); or
> (ii) it is composed of (at least two) different biopopulations of unispecific organisms.
>
> (Mahner & Bunge 1997: 171.)

Thus, according to the foregoing definition, the group of organisms studied in our facilitation example is a community. This definition presupposes that communities are systems of sorts, that is, that communities possess composition, environment, structure and mechanism. If the ecotonal community of the study is a system, then we should apply the CESM approach and find each one of the elements in the quadruple.

The system in which facilitation takes place: Bunge (informally) characterizes a system as a complex object with properties that its component parts lack. Thus, to recognize a system, one needs to identify at least one global property of the system as well as its components. Besides, such global property must be a result of some processes (causal or random) connecting the relevant components of the system. In other words, global properties are not a result of aggregation processes, but a result of combination processes. For example, while the total biomass of the whole community studied by Baumeister and Callaway may be said to be a property of the whole community (our candidate to system status), it is not a global property in Bunge's sense. The reason is that total biomass is the result of adding the individual biomasses of the component parts of the putative system. In contrast, resilience with respect to disturbance —such as invasion by individuals of species not represented in the area— is a global property of a community because it is absent in the component parts of the community and emerges from processes that include interactions among the parts. Consequently, the community under study is a (real) system in Bunge's terms.

Being a real system it must have mechanisms, that is, processes connecting the system's parts, whose description explains the system's behavior. In Baumeister and Callaway's article the phenomenon to be confirmed and explained is the particular spatial distribution of surviving seedlings of *Pseudotsuga* sp. and *Ribes* sp. Once confirmed that seedlings occur mainly or only under *P. flexilis* crowns, the authors explain this by pinpointing and describing the facilitation mechanisms at work in the study area and the relevant components of the system.

Just as in our assessment of Glennan's system-mechanisms view, Bunge's conception does not encounter trouble regarding the systemicity of the objects studied in Baumeister and Callaway study. Now, however, the mechanism is not the system, but one of its aspects. The relevant elements of the system would be:

Component parts (system's composition)

- Individuals of the benefactor species (*P. flexilis*),
- individuals of the beneficiary species (*Pseudotsuga* sp. or *Ribes* sp. in our example),

Environment (environmental composition)

- environmental factors potentially harmful for seedlings, such as sunlight, moving air, ice and soil particles, and herbivorous animals (only mentioned in the study), and
- environmental factors potentially beneficial for the seedlings, such as snowdrifts, soil nutrients, litter and water in the soil.

Structure (relations)

- Perhaps the most important relations in the case of Baumeister and Callaway's study are spatial relations, since they determine the operation of the facilitation mechanisms. In the study area, facilitation between *P. flexilis* and seedlings of the beneficiary species can only occur if the seedlings are located under *P. flexilis*' crowns and, preferably, on the leeward side of them.
- Temporal relations are important at least in the minimal sense that, for the facilitation mechanisms to take place, the individuals of *P. flexilis* must be in the site before the arrival of the seedlings of the beneficiary species. This is because the two main facilitation

mechanisms responsible for the survival of the seedlings depend on the bulk of *P. flexilis* individuals.

Mechanism (specific processes): Baumeister and Callaway (2006) describe the four facilitation mechanisms they put to test (shading, wind amelioration, litter accumulation, and snow accumulation) in such a way it suggests that (i) mechanisms are processes and (ii) those processes are specific in relation to certain behavior (or function) of the system under the study, that is, of the particular ecological community made up of *P. flexilis*, *Pseudotsuga menziesii*, and *Ribes cereum*, among other organisms and abiotic elements on the one hand, and of early successional communities[56]. Two other characteristics of Bunge's mechanisms fit the example of facilitation. In the first place, there is the nested array of processes that make up the facilitation mechanisms. In the second place, the concurrence and interaction of mechanisms in a given system (recall Chapter 5, Section 4.4.3.2). I already dealt with the hierarchical organization of facilitation when discussing Glennan's view in Section 6.1.1. As for mechanisms interactions at the same level, Baumeister and Callaway explicitly test for the interactions among four different facilitation mechanisms and find that shading interacts with a number of them, especially wind amelioration.

Mechanismic explanation: As described in Chapter 5, Section 5.3.2.4, mechanismic explanation is about types of mechanisms. However, Bunge clearly classify those types as conceptual devices, even though the properties that allow classing them are expected to be objective. Seen under this light, there are no type-mechanisms because mechanisms are concrete (real) things and type-mechanisms are not concrete things but abstract descriptions of

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[56] 'Early' here does not mean necessarily the first stages of primary successions, but any early stage of a cycle of vegetation dynamics (see, e.g., Meiners et al. 2015).

equivalence classes of processes[57]. Yet, the ultimate referents of a mechanismic explanation are the concrete objects and processes mentioned or implied in the explanation.

As I suggested in Chapter 5, Bunge distinguishes different types of conceptual devices aimed at providing understanding, of which mechanismic explanation —the one he calls "scientific explanation proper"— is the more explanatory powerful.

As I described in Section 6.1.2, Baumeister and Callaway start with a rather sketchy and abstract hypothetical explanation and use the term 'facilitation' to encompass four different mechanisms. Viewed in this light, facilitation is not a mechanism strictly speaking, but a function that can be discharged by a variety of processes, the four tested for in Baumeister and Callaway's study among them. Thus, when ecologists account for a given pattern in terms of "facilitation", they provide a functional account of the fact of interest, but not a mechanismic explanation strictly. This is because (a) a functional account describes the role a given subsystem plays in a system and (b) mechanisms are "stuff specific" processes. In Baumeister and Callaway's study, the explanation only deals with mechanisms when such specific processes are described; that is, when instead of using the umbrella term 'facilitation', they describe the four facilitation mechanisms. Yet, the functional account is not useless because (a) it provides a certain (superficial) understanding of the phenomenon of interest by showing that systems of different kinds have dynamical analogies, and (b) it guides research towards the relevant mechanisms in each kind of system. In the case of facilitation studied by

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[57] The idea that "type-mechanisms" are conceptual devices is also valid for Glennan's mechanisms-as-systems view. In this case a type of mechanisms would be an equivalence class of systems.

Baumeister and Callaway (2006), advantage (a) is apparent in that the first approach to understand the spatial distribution of *Pseudotsuga* sp. and *Ribes* sp. in the area of interest is that *P. flexilis* somehow contribute to their survival. Once facilitation becomes a strong hypothesis it is possible to design experiments to test for the plausible mechanisms responsible for the facilitation effect (b).

Bunge defends a special form of D-N explanation that includes mechanismic laws among its premises (Chapter 5, Section 5.3.2.4). The explanation schemata pictured in Figure 6.1 do not have the form of a deductive argument nor explicitly include laws among their premises and this may be interpreted as problem for Bunge's explication of mechanismic explanation. However, like Hempel (1942) and Rosenberg (2001) among others, Bunge admits that scientific explanations are frequently offered as explanation sketches, that is, as narratives that *imply* the relevant laws and *could* be formulated in a deductive-nomological form. If one accepts this possibility, other possible objection might be against the possibility of ecological laws. Indeed, Baumeister and Callaway do not recur to laws explicitly to explain the spatial distribution of *Pseudotsuga menziesii* and *Ribes cereum* in the study area. However, in their mechanismic explanations the authors use and imply a number of generalizations, laws and even portions of models and theories from the field of ecology as well as from other disciplines that concern themselves with facts at lower levels of organization. For example, the explanatory narrative for the example I chose to illustrate facilitation goes more or less like this. In the ecotonal forest-prairie community of the study area, individuals of *Pseudotsuga* sp. and *Ribes* sp. occur mainly or exclusively under the crowns of *P. flexilis* individuals because the environment under *P. flexilis* is different from the environment in the open. Environmental differences consist in lower PAR, lower wind speed, more moderate temperatures, and more nutrients and water content in the soil. These differences may be more or less important all year round, but some of them are crucial for seedling survival under certain weather conditions (those prevalent in winter for *Pseudotsuga* sp. and those of late summer for *Ribes* sp.). The elements responsible for such environmental differences between the open grassland and the forest are *P*. *flexilis* individuals, which act as barriers that filter PAR, lower wind speeds, and favor snow accumulation. Photoinhibition, mechanical stress and desiccation are the main causes of seedling mortality faraway from the crowns of *P. flexilis* individuals. Thus, *P. flexilis* trees facilitate the survival of *Pseudotsuga* sp. and *Ribes* sp. seedlings through shading, wind amelioration and snow accumulation.

Now, the previous causal narrative takes a number of generalizations or laws for granted, most of them from levels of organization lower than that of ecological systems, but at least one from the latter: all organisms are coupled to their environment through energy flow and cycles of matter. This is important because all organisms require energy (which connects the narrative to the principles of thermodynamics), as well as certain kinds of matter —i.e., nutrients— (which connects the narrative to biochemical laws and physiological generalizations) for homeostasis and reproduction. Classical mechanics is needed to understand how the kinetic energy of the wind increases with its speed and a combination of classical mechanics and plant physiology explains why seedlings die when exposed to high wind speeds and why they keep living when wind speeds are not so high. Likewise, laws and generalizations from quantum physics and biochemistry help understand why seedlings suffer from photoinhibition when exposed to high PAR a low temperatures, and certain generalizations from plant physiology explain why sustained photoinhibition kills the seedlings.

In sum, laws and generalizations from different levels of organization are necessary to understand ecological explanation. This feature of ecological understanding supports Bunge's systemic view of explanation.

6.2 Interspecific exploitative competition (IEC)

Interspecific exploitative competition is a mechanism put forth for explaining a diversity of phenomena. One of them is the reduction or extinction of biological populations so, when populations of different species that use similar resources coexist, ecologists study the relevant competition mechanisms as well as those mechanisms that make such coexistence possible. The study case I described in Chapter 2, Section 2.1.2.3.1, invokes competition as one of the mechanisms for explaining the structuring of desert communities and investigates the competition between two groups of consumers connected through the use of the same food resource (Figure 6.2). The empirical investigation takes the form of a series of exploratory experiments oriented to reveal the effects of the absence of one of the putative competitors on the abundances of the resource and of the other putative competitor.

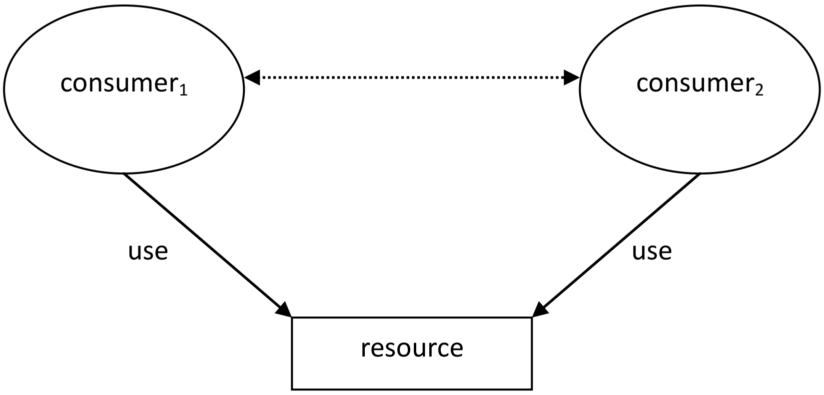

Figure 6.2 Schematic representation of the interspecific exploitative competition mechanism. Full-line arrows represent resource consumption, while the dotted-line, double-head arrow represents competition.

6.2.1 IEC as a system-mechanism

Systemic nature: For interspecific exploitative competition to be considered a system-mechanism, we need to first uncover its parts and interactions. Then, it is necessary to show that it has a stable configuration by pinpointing the invariant, change-relating generalization that describes the mechanism's behavior, as well as the invariant, change-relating generalizations that describe the interactions between the parts of the mechanism.

Component parts[58]: According to Brown and Davidson's description, the Glennan-like composition of the IEC mechanism in the Portal experiments would be:

- The two distantly-related taxonomic groups of competing populations belonging in the trophic guild[59] of granivores, namely (a) seed-eating ants (*Pogonomyrmex* spp., *Veromessor* spp., *Novomessor* spp., and *Solenopsis* spp.) and (b) seed-eating rodents (*Dipodomys* spp., *Perognathus* spp., *Peromyscus* spp., *Reithrodontomys* spp.).
- The food resource (seeds) the two groups of granivores compete over.

In Brown and Davidson's study the two groups of competitors are defined

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[58] Here I omit the components of the experimental set up as parts of the system because the ultimate idea is to evaluate whether the competition mechanism *in nature* is a system in Glennan's terms and the experiments intend to be extrapolable to the natural community of the Portal area, at the least. However, a description of the experimental set up is relevant to understand the results of the experiments, which is the first target of Brown and Davidson (1977) explanatory efforts.

[59] A trophic guild is a collection of populations of the same or different species that make use of the same trophic resource. The concept of "*trophic* guild" differs from that of "guild" in that the latter requires, in addition, that the resource be used in similar ways. Here I take both to be functional concepts. (For a review of the concept of "guild" see, e.g., Simberloff & Dayan 1991.)

according to functional and taxonomic criteria. The functional one consists in that all organisms considered belong in the same trophic guild, that of granivores (seed eaters). In other words, the groups are determined by their using the resource they compete over. The taxonomic aspect distinguishes between ants and rodents, the competitors. Interestingly, while the authors refer to groups of populations, they measure the abundance of each group differently. For ants, the relevant variable is the number of colonies in the site, while for rodents it is the number of individuals or their aggregated biomass that is used as a variable. The main reason for this methodological decision is that rodents predate seeds individually, but ants do so collectively, as colonies. This, again, seems to pose a problem to Glennans view on mechanisms that requires part of mechanisms to be objects and it is not clear in what sense ant colonies or rodent populations are objects *sensu* Glennan.

Causal interactions between parts: The mechanism explored in Portal by Brown and Davidson is described at the ecological level, then the interactions should be expressed in ecological language. Those interactions consist in each group of consumers predating upon seeds, an activity that has the effect of reducing food availability for the competitor. More precisely, the study suggests that the competing mechanism comprises interactions that consist in causal chains connecting seed predation by ants with seed predation by rodents through changes in seed availability. The connection between those two different groups of granivores (ants and rodents) occurs indirectly, through the effect that changes in the availability of seeds have in the fitness of individuals in each of the populations of granivores.

Applying Glennan’s model of explanation to the result of one of the exclusion experiments, the form of the explanation would be:

Behavioral description: In the absence of seed-eating rodents, seed-eating, colonies of ants increase in number.

Mechanical description: Rodents predate on seeds, which are main food source of ants, so when rodents are absent, there are more seeds for ants.

Configuration: Regarding the robustness of the configuration, consumer-resource ties seem to be robust enough to declare IEC a system in this case, though perhaps not in the precise sense of Glennan's examples of system-mechanisms for initial conditions, especially consumers and resource respective abundances may change the behavior of the mechanism.

6.2.2 IEC as an entities-and-activities mechanism

Entities and activities: The entities involved in the competition mechanism described by Brown and Davidson are those identified as component parts in the previous section, namely the two groups of consumers (populations of seed eater ants and rodents) and the resources (seeds). Activities, in turn, correspond to the interactions described in the previous section, that is, seed predation. Seed consumption is an activity as defined by MDC because the entities that make up the mechanisms —individual ants and rodents— bring about the mechanism's behavior

Temporal organization: The interspecific exploitative competitive mechanism does not show a temporal organization of importance. Seed predation may occur in any moment, in any order of precedence, and with any rate, frequency and duration. So in this respect, IEC does not support MDC's model of mechanisms.

Spatial organization: There are no hints of a spatial organization in the IEC mechanism as described in Brown and Davidson's investigation, save for the occurrence of some seeds in clumps. This grouping of seeds allows rodents to take large quantities of the resource once they have located it. Location, however, is not important in this case, for seeds may be eaten anywhere at the study site or transported far from the parental plants before being consumed. Neither orientation plays a role in seed consumption or in the relations between ants and rodents. Thus, this is another respect in which MDC picture of mechanisms does not fit well our IEC example.

6.2.3 IEC as an ephemeral-processes mechanism

The IEC mechanism shows some characteristics of system-mechanisms (Section 6.2.1), among them the composition and the regularity of seed consumption patterns, but does not fit the stability in configuration required by Glennan's model. Yet, the configuration of the system is not unique, so it cannot be considered an ephemeral-process mechanism.

6.2.4 IEC as a specific-process-in-a-system mechanism

Systemic nature: The community of granivores studied by Brown and Davidson in Portal is a system for the reasons already mentioned in Sections 6.1.1 and 6.2.1, so I will proceed to identify its different aspects according to the CESM approach.

Component parts

- Individuals of the trophic guild of granivores grouped in two collections of biopopulations, those of (a) seed-eating ants (*Pogonomyrmex* spp., *Veromessor* spp., *Novomessor* spp., and *Solenopsis* spp.) and (b) seed-eating rodents (*Dipodomys* spp., *Perognathus* spp., *Peromyscus* spp., *Reithrodontomys* spp.).
- Seeds of plants of different species that constitute the food resource over which member organisms of the trophic guild compete.

Environment

- The description of the study includes that of some general environmental factors and conditions, such as aridity. This is important because water is generally a limiting resource in arid zones and lack of water is a constraint for seed production.

Structure: The structure of the community studied by Brown and Davidson is made up by relations such as consumers being at a certain distance of the resource, precise location and distribution of the latter, etc. However, the study does not elaborate on those relations nor there are hints that structure is important for the description of the competition mechanism, save for the fact that some seeds occur in clumps, a feature that allow consumers to rapidly predate on large quantities of seeds once they have located to clump.

Mechanism (specific processes): According to Bunge's view, the IEC mechanism is the causal net of specific processes connecting the competitors through the negative effects they produce on each other. In this case, those processes are seed consumption by organisms of each one of the seed-eater taxonomic groups chosen by Brown and Davidson.

6.3 An assessment of the virtues and weaknesses of the contemporary mechanismic philosophy in the light of the ecological examples analyzed

The contemporary mechanismic philosophy captures many features of the ecological mechanisms studied in this dissertation. However, the fit between these philosophies and ecology is not perfect. The main ontological and epistemological theses of the mechanismic philosophers studied in this dissertation are summarized in Chapter 5, Table 5.1 and Table 5.2. In the following pages of this section I briefly discuss those theses, starting with the ontological ones (Table 5.1)

All contemporary mechanismic philosophers state that mechanisms produce "phenomena", that is facts, and that descriptions of the relevant mechanisms explain such facts. This is consistent with the practice of ecologists according to the examples I described in Chapter 2. Indeed, facilitation is offered as the mechanism that produces the survival of seedlings of under the crowns of *P. flexilis*. Likewise, the description of facilitation is proposed as an explanation for the spatial distribution of *Pseudotsuga sp.* and *Ribes sp.* and also as one of the mechanisms for understanding succession in the study area (Baumeister & Callaway 2006). Similarly, interspecific exploitative competition is invoked to explain a negative correlation between the abundance of populations of granivores in a desertic zone (Brown & Davidson 1977).

According to the contemporary mechanismic philosophy, mechanisms are not unanalyzable wholes[60] but composites, that is, complex objects with a composition (Glennan and MDC) or processes in complex things (systems) that undergo such processes (Bunge, Glennan). My ecological examples satisfy this condition of contemporary mechanismic philosophy because both ecological facilitation and interspecific exploitative competition involve individual organisms and biopopulations, as well as environmental items (harmful abiotic processes, resources) that are essential for the relevant mechanisms to exist. Facilitation and competition are unintelligible without the corresponding descriptions of the biotic and abiotic entities that engage in the activities or interactions that bring about the fact under study. Ecologists usually describe collections of organisms —biological populations, guilds or "species"— as components of the systems they study and this is certainly so in the cases of facilitation and IEC I have described in the present

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[60] A point of view usually associated to holism.

dissertation. However, the properties and the activities/interactions in which the components engage are not collective but individual. Indeed, it is each individual tree that operates as a shield against light or wind thus facilitating the survival of seedlings; and it is each individual rodent and ant the one that consumes seeds and thus reduces the availability of the resource for further individual see-eater organisms. This dualism of mechanism description is not a problem for Bunge, who distinguishes between real systems and their models. Real systems are composed of individuals even when their models refer to collections of them for the sake of generality, which is an ideal feature of scientific knowledge.

For the contemporary mechanismic philosophy, the parts of a system/mechanism must be connected through interactions, activities or processes. The ecological examples I discussed confirm this condition in different ways. Facilitation may be about causally connected parts of the system, as in snow accumulation, a facilitation mechanism that alters the environment of beneficiary seedlings by adding nutrients to the soil. This is unequivocally a causal activity (something the benefactor plant does to the soil), an interaction (between the benefactor plant and the soil, and between the soil and the beneficiary seedlings), and a process (a sequence of changes starting in the addition of litter to the soil, continuing with litter decomposition by weathering, invertebrates and microorganisms, and ending with the seedlings profiting from an enriched soil). The case is not so clear for activities when we look at the other two facilitation mechanisms, both of which rely on a barrier effect. Indeed, shading and wind amelioration are activities in the sense that it is the benefactor plant that mitigates the potentially harmful processes (light and wind) just by interposing[61] its tissues between such processes and the tissues of the beneficiary seedlings. However, the positive effect of filtering light or partially blocking the wind is not a product of a change in the environment, but a current condition of an environment long ago modeled by the benefactor. This time lag between the time of the benefactor's activity and the time of the facilitation effect may place doubts as to the causal connection between the blocking activity and the usual, "unchanged" activities of the beneficiary seedlings. If activities are types of causes (Machamer et al. 2000: 6) and the general term 'cause' must be specified or filled out by other verbs to become meaningful, then we need to search for those other verbs to discover where is the productive continuity in facilitation by blocking. But then, once the environment has been changed by benefactor plants, the relevant activities we need to describe with verbs are those of the beneficiary seedlings themselves and it is not clear that there is productive continuity in the example. A similar problem would affect interactions, since there is no real physical interaction between the benefactor plants and the beneficiary seedlings. For instance, ecologists do not describe the facilitation mechanism describing the non-burning of the photosynthetic pigments in the seedlings by excessive PAR or the non-breaking of the seedlings' tissues by abrasion by airborne snow and ice particles. Ecologists do take into account counterfactuals as heuristic tools to understand what would happen should the facilitation mechanism stop working and they use these counterfactual hypotheses to design their experiments.

The foregoing is an issue for the ontology of mechanisms-as-entities-and-

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[61] I use the verb interposing in its acceptation of *being* (rather than coming) between two things. (Merriam-Websters Online Dictionary, URL: <http://www.merriam-webster.com/dictionary/interpose>. Access: 11-04-2015.)

activities, mechanisms-as-systems and mechanisms-as-ephemeral-processes, but not for their epistemology. Glennan and MDC share the view that counterfactuals may —and usually— have a role in mechanistic explanation even when “they get at nothing ontological” (Machamer et al. 2000). Yet, how do these philosophies account for the lack of productive continuity in the blocking-type facilitation mechanisms? Machamer provides a hint: “An entity acts as a cause then it engages in a productive activity” (Machamer et al. 2000: 6) and, more to the point,

> Non-existent activities cannot cause anything. But they can, when other mechanisms are in place, be used to explain why a given mechanism did not work as it normally would, and why some other mechanism became active. Failures and absences can be used to explain why another mechanism, if it had been in operation, would have disrupted the mechanism that actually was operating. Maybe we should draw a distinction and say they are *causally relevant* rather than *causally efficacious*.
>
> (Machamer 2004: 35-6.)

So, according to Machamer, a possible solution for the problem of the lack of productive continuity between certain stages of putative mechanisms is resolved by (a) understanding that causes are not necessarily transitive, (b) distinguishing between causally relevant conditions and causally efficacious activities and, in consequence, (c) using the foregoing to distinguish more than one mechanism. This applied to the case of facilitation by blocking would render a first mechanism constituted by the blocking activity, and a second one consisting in the normal metabolic processes of seedlings. The ameliorated environmental conditions that are brought about by the first mechanism are not causally *efficacious* of seedling survival, but causally *relevant* for it. Glennan’s answer is similar to Machamer’s in the sense that he take counterfactuals to have explanatory import, but not any ontological substance.

Bunge, in turn, considers counterfactuals just a *façon de parler*, "rethorical tricks" in the best of cases, and he rejects all use of them, not only in ontological matters but also in epistemic ones, especially in building hypotheses, models and theories (and consequently explanations). His main reason is that counterfactuals are "logical outlaws because they are not propositions, and consequently they cannot be assigned truth values" (Bunge 2006a: 93)[62]. His solution for the ontological problem of negative "causation" is similar to that of Dowe's I reviewed in Chapter 4, Section 4.3.5 and to that of Machamer et al. (2000). The only causation is "genuine" causation, and while there can be causally relevant conditions, the causal relation is always positive for causation, just as randomness, is an objective "mode of becoming" (Bunge 1981). Returning to facilitation, since ours is "a world of systems" (Bunge 1979) mechanisms are everywhere, and the system in which facilitation occurs may be decomposed into a number of subsystems with their corresponding mechanisms. The blocking stage of facilitation by shading or wind amelioration is one mechanism whose operation allows other mechanisms (homeostatic, metabolic, etc.) in the beneficiary seedlings to keep operating.

Another thesis of the contemporary mechanismic philosophy is that mechanisms are composites. Ecologists describe composites when they explain ecological phenomena, but in their explanations they usually refer to populations or species, especially when those explanations have the form of formal models (as in the case of competition theory). However, the properties

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[62] For more on the unsuitability of counterfactuals in scientific and technological discourse see Bunge (1959, 1979, and 2003).

relevant for the causal interactions that make up the mechanismic basis for their explanations are not attributed to populations or species, but are properties of individual organisms. Likewise, in their interventions the ecologists of the cases I studied manipulated individuals and took measurements from individual organisms, not from populations. In consequence, one may think that this is a problem for the contemporary mechanismic philosophers. Indeed, what are the parts of a mechanism? Populations, guilds, individual organisms?

For Glennan and MDC parts must be objects and simple collections of organisms are no objects. Thus, neither functional groups or species are objects. Thus, the new mechanistic philosophers seem to be in trouble regarding their thesis of mechanisms as systems consisting of objects. However, if populations are a special kind of object, more precisely a system, then there would be no problem for Glennan. Indeed, biological populations are aggregates of organisms connected by certain bonds, such as reproduction. Yet, Glennan and MDC note that entities are able to engage in activities or interactions in virtue of their possessing certain properties and collections of organisms do not seem to possess the properties for entering the kind of relations I have discussed in the ecological cases. For example, in the case of facilitation, individual trees, but not populations thereof, are made of different types of tissues that may block light or wind. Similarly, in IEC, individual organisms —not populations— search for and predate on seeds when they find them reducing resource availability for other individuals in search of food. So, it is fair to apply the principle of charity and solve this apparent contradiction construing that it is organisms that, ultimately, compose mechanisms. Yet, while this may solve the componential aspect of mechanisms for the biotic parts of ecological mechanisms, there are still the abiotic ones. Indeed, shade, light, wind, temperature, humidity, etc. are not objects in any obvious sense and this time the principle of charity does not seem to be of help. Consequently, the thesis that mechanisms are made up of objects needs to be revised not because of populations, but because of abiotic factors.

The landscape looks slightly different for Bunge, who has developed an explicit sistemist philosophy where mechanisms are processes, not things. So, for Bunge the challenge is not the same. The referents of explanatory models are collections of organisms simply because models are abstract and general (Bunge 1998ab), but it is always individual organisms the ones that are causally efficient and capable to produce change. Bunge explicitly states that (a) scientific models are conceptual devices about types and that (b) biopopulations are real systems, but also that (c) individual organisms engage in causal processes, even in population processes (Mahner & Bunge 1997). As for abiotic factors, Bunge's view naturally makes room for them because those that are not objects, such as light, wind, temperature and humidity, are processes undergone by things, and mechanisms are processes undergone by composites of things.

Mechanisms are explanatory because they produce phenomena, another thesis of the contemporary mechanismic philosophy; and they do so by means of interactions (Glennan), activities (MDC) or processes (Bunge). Besides, Glennan and MDC commit to the idea of productive continuity, which allows for no gaps or lacunae in mechanisms. That productive continuity is apparent in IEC because resource consumption by organisms of one species affects resource availability for organisms of other species, thus affecting also the fitness of the latter. However, continuity is not so clear in the case of facilitation by blocking. In this case, the contemporary mechanismic philosophers need to rely on the idea of a diversity of mechanisms connected by causally relevant processes or conditions (as discussed in Bunge’s solution), but not by genuinely productive relations.

The regularity of mechanisms is an important thesis for the contemporary mechanismic philosophy. Glennan firstly described mechanistic explanation in terms of a special type of mechanical laws, but later decided to rely on Woodwardian invariant, change-relating generalizations, which are general but not exceptionless, both for describing the mechanism’s behavior (system-mechanisms) and for describing the interactions among parts (system-mechanisms and ephemeral-processes mechanisms). The main characteristic of this kind of generalization is that it remains invariant in case of ideal interventions, a feature ecological mechanisms usually possess. MDC invoke a rather uncertain “regularity” of mechanisms that is not difficult to comply with, so the issue of regularity is not an *issue* for them. Bunge, in turn, sticks to laws and claims that mechanismic laws are the core of mechanismic explanations. However, Bunge’s notion of a law is not the traditional one, but one whose applicability in ecology is much less unproblematic than that of traditionally understood laws. In any case, the most difficult problem seem to be for Glennan, who requires invariant generalizations to describe the phenomenon to be explained and forces him to distinguish two kind of mechanisms, those that satisfy that requirement (system-mechanisms) and those that fail to do so (ephemeral-processes mechanisms). Yet ecological mechanisms seem to be somewhere in between. The spatial distribution of the vegetation in the north-east front of the Rockies may be described by a generalization with limited scope, but there is no need for this kind of generalizations to have unlimited scope (Woodward 2003). The pattern of vegetation dynamics that the facilitation mechanism reviewed in Chapter 2 attempts to explain is not universal, but it is repeatable and one would expect it to hold for similar kinds of plants in any environment characterized by high

PAR and strong winds. The ecological literature indicates that this is the case (see Baumeister and Callaway 2006). Much less problematic is the case for mechanisms seen as ephemeral processes because even when the fact to be explained is described as a unique event, the interactions that bring it about are easily described by invariant change-relating generalizations. A similar reflection applies to Bunge's conception of lawful mechanisms because (a) his concept of a law of nature does not imply regularity but determination, and (b) Bunge emphasizes that facts are always concrete and singular, even though our models of them are abstract and general.

The thesis that mechanisms must be organized is a bit tricky for MDC, but not for Glennan or Bunge. The former authors assign a rather strong role to very specific forms of organization that do not occur in ecological mechanisms. While a modicum of spatial organization is necessary in the facilitation mechanisms, organization is not important for competition, so the organizational thesis does not fit my cases of ecological mechanisms. Now for the epistemological theses (Table 5.2).

Glennan and, especially, MDC choose a non-inferential conception of explanation, while Bunge sticks to the deductive-nomological format. Glennan and MDC do not require explanations to be D-N —nor forbid them to be so. This is important because invariant, change-relating generalizations may play a role similar to that of laws in D-N explanations and thus be part of explanatory devices similar to D-N explanation. Ecological explanations are not generally offered in a D-N format, unless those explanations come from the realm of theoretical ecology. For example, competition, predation and niche theory models inspired in the Lotka-Volterra equations may provide D-N explanations (Chase & Leibold 2003). However, those models do not include descriptions of the relevant mechanisms. Generally, when ecologists describe mechanisms, such as in the cases of facilitation and IEC I reviewed in Chapter 2, they use generalizations, but do not give explicit D-N form to their explanations. In consequence, it would appear that Bunge's requirement for explanations to be deductive arguments is too strong. Yet, he allows for more or less informal explanations that do not have explicit inferential form. Thus, writing about narrative explanation in biology, he uncovers the bridge between this and full-fledged mechanismic explanation:

> Viewed at a closer range, many narrative explanations are not purely descriptive, for they either tacitly imply or explicitly invoke laws, causes, and mechanisms, even though they are not stated in the form of proper deductive-nomological arguments. (See also Ruse 1973; M.B. Williams 1986.) For example, narrative explanations often conjecture some adaptive scenario. The use of the notion of adaptation, however, implies the application of the theory of natural selection, which, in turn, refers to a (general) mechanism of evolution. Moreover, reference to mechanisms and causes, in our strict as well as in the broad sense, presupposes the existence of laws, although they may not be explicitly referred to in the narrative.
>
> (Mahner & Bunge 1997: 111).

This argument is similar to that of Hempel's explanation sketch described in Chapter 3, Section 3.3.1.1.

In any case, all the contemporary mechanismic philosophers agree that the source of intelligibility in a mechanismic explanation is the description of a mechanism. Glennan's minimal requirement to call an explanation mechanistic is that the interactions that produce the phenomenon described in the explanandum are couched in general terms, more precisely in terms of invariant, change-relating generalizations. Bunge, in turn, requires those interactions to be lawful. However, his requirement should not be interpreted according to the traditional concept of a law but to a less strict notion of a law as in the following definition:

> DEFINITION 3.9. A factual statement is a *law statement* if, and only if,
> (i) it is general in some respect (e.g., it holds for a certain taxon);
> (ii) it is part of a (factual) theory (hypothetico-deductive system); and
> (iii) it has been satisfactorily confirmed (for the time being).
>
> (Mahner & Bunge 1997: 111, italics in the original.)

This definition may still seem too strict for a science like ecology where laws do not abound, unless we understand the explanatory practice as a progressive process of building different kinds of conceptual devices for understanding, in particular increasingly more powerful explanatory models.

In sum, contemporary mechanismic philosophy accounts for most of the traits of explanatory practices in ecology, but still needs refinement as to the role of generalizations and deductive inference in scientific explanation.

7

MECHANISMIC EXPLANATION IN ECOLOGY
CONCLUDING REMARKS

7.1 A pluralistic view of scientific understanding

Methodological naturalism in philosophy implies that, in order to explicate the philosophical aspects of science, the researcher must pay attention to real scientific practice. However, the rather variable practices among the different factual sciences, and even among different research groups, suggest that the philosopher needs to infuse his views with a modicum of normativity. I assumed this moderate form of philosophical naturalism for my inquiry on explanation in ecology.

In the life sciences and their philosophy there is ample agreement that there is a diversity and even "a profusion of explanatory patterns" (Braillard & Malaterre 2015: 1), all of them providing some sort of understanding. However, there are reasons to believe that those explanatory patterns are not equally powerful when it comes to understand why certain systems behave like they do, that is, when they are asked to perform the typical work a scientific explanation is expected to do: answer why-questions. For example, subsumptive explanation, as in the covering-law model formalized by Hempel and Oppenheim (1948), provides a certain understanding of *why we should expect* the system to behave as it does. The answer is that the behavior of interest is just an instance of some general law(s). We know this is the case because it is possible to formulate a deductive argument whose premises are the relevant general laws and plausible or confirmed descriptions of the relevant circumstances, and whose conclusion is the description of the fact we want to understand. However, as discussed in Chapter 3, the covering-law model of explanation faces a number of limitations suggesting that "Why should we expect X to occur?" is not the same question as "Why does X occur?" Besides, scientists do not always use laws or formulate their explanations as deductive arguments, especially in biology, where save for evolutionary ones, laws are scarce or even suspected to be absent (Beatty 2006).

A possible way to put some order into the problem of explanation in biology, especially in ecology, is to note that explanatory practices are an integral part of scientific research and that in general scientists do not come up with explanations in an instantaneous way. On the contrary, more often than not the practice of explaining scientific facts is a continuous process that consists in offering different representations of the facts of interest. These representations can be all valuable for different reasons. Some of them may be valuable as heuristic tools, while others may be valuable because of their predictive power or because of their explanatory power. They all may well provide some understanding and this is, in my view, one of the reasons for the view that "models" —hypotheses, models and theories— are useful tools (Cooper 2003, Chapter 8). However, understanding as to how to design the next experiment, or how a certain model behaves under given circumstances is not the same as understanding why a certain fact is the case or, more precisely, how a given fact came to occur. Consequently, there seem to be different forms of understanding and scientific representations can be ordered along different axes, each one of them representing a different cognitive value for the scientist. One of those values being explanatory power. Scientific understanding then is not exclusive of explanatory conceptual devices, be they hypotheses, narratives, models or theories. Indeed, other scientific practices such as description and prediction provide a certain understanding of the systems that scientists study. For example, put in context, a mere description of a system's composition may offer some understanding on the possible mechanisms of emergence and maintenance of the system; and prediction, as I said some lines above, help us understand that the case of interest is an instance of a type. Furthermore, largely false models may provide the kind of understanding that advances research without necessarily being explanatory of the type of facts they are related to. This, I believe, suggest the need for a theory of scientific understanding that relates it to the different conceptual devices —hypotheses, models, theories— that scientists use in scientific research, but that distinguishes the notion of understanding from that of explanatory power (Figure 7.1). My (undeveloped) view on the subject is that understanding relates to relations, something that all the foregoing conceptual devices describe to a certain extent. The type of relations that a certain conceptual device describes determines the kind of understanding it provides.

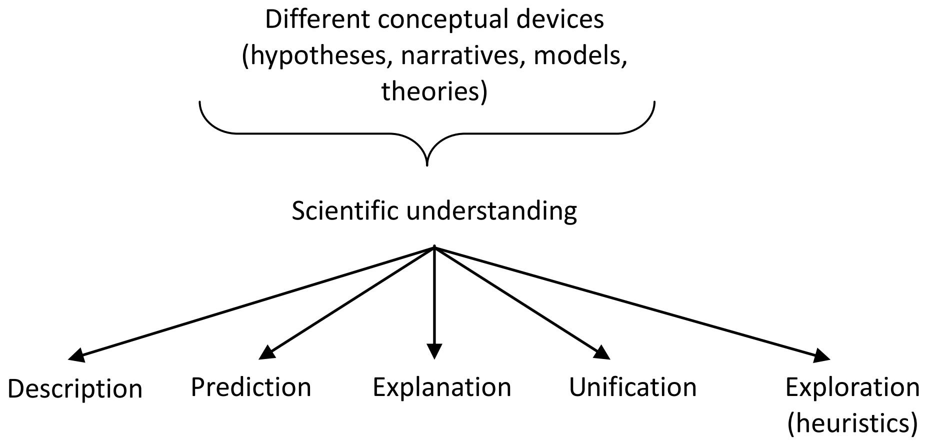

Figure 7.1 Scientific understanding may result from different conceptual devices and be of different kinds. We need a theory that clarifies the relations between kinds of understanding, kinds of conceptual devices, and epistemic values.

In the present dissertation I have been concerned with only one sort of conceptual devices that provides scientific understanding, namely scientific explanation, the one whose main characteristic is to maximize explanatory power, even against other desirable epistemic properties of scientific knowledge, such as unificatory or predictive power. In consequence, we need a notion of explanatory power in order to assess the different forms of scientific explanation. This is precisely one of the aims of contemporary mechanismic accounts of scientific explanation (Chapter 5), which identify explanatory power with the description of the mechanism that produces the fact in need of explanation.

7.2 Mechanismic explanation I: The ontological basis

The mechanismic approach to explanation implies a strong relation between the ontology of explanation and its epistemology, so much so that —following Salmon (1984)— one of the champions of the new mechanistic philosophy insists in considering the approach as an "*ontic* account of scientific explanation" (Craver 2014, my emphasis). Indeed, according to Craver, it is not the descriptions of mechanisms but mechanisms themselves the ones that hold explanatory power. Being explanation an epistemic operation and mechanisms, at least in the view of the contemporary mechanismic philosophy, concrete things and/or processes (or entities and activities), it is difficult to accept Craver's contention. However, in the light of what I have discussed in the present work, the idea that mechanism *description* is the main source of explanatory power is still ontologically committed and much less controversial.

7.2.1 The nature of ecological mechanisms.

My analysis of two ecological mechanisms, facilitation and interspecific exploitative competition, suggests that ecological mechanisms, as ecologists view them, are better understood as processes, not as complex objects or systems. In this, my investigation supports Bunge's view on the nature of mechanisms (Bunge 1997) in detriment of those of the new mechanistic philosophers.

Take interspecific exploitative competition (IEC), for example. It is true, IEC is something that occurs *in* system or *to* a system (an ecological community), but it is not a system itself. Instead, IEC is a family of concrete processes that have one characteristic in common, namely that resource consumption by each of the competitors exerts an antagonistic effect on all the individuals of the species involved. Thus, competition (without qualifiers) is a wider family that includes a more diverse collection of processes characterized by their negative effects in all the participants (Keddy 2006).

Likewise, facilitation occurs *in* a system or *to* a system (an ecological community), but it is not a system. More precisely, facilitation is an equivalence class of processes that somehow change the system or prevent it to change. The equivalence criterion, that is, the criterion for classing the processes denoted by the facilitation concept is functional: what makes a given concrete process an instance of ecological facilitation is that it benefits at least one of the organisms involved in the process without harming the others.

In sum, that facilitation and competition are types of mechanisms means that facilitation and competition are classes of functionally equivalent concrete processes. Therefore, the claim that some mechanism is a type-mechanism should be understood as an ellipsis stating that it is a type of mechanisms. In this, my conclusion also supports Bunge’s mechanismic perspective (Bunge 1997).

Although the ecological mechanisms studied in this dissertation are processes, they are not *ephemeral* processes *sensu* Glennan (2010a) because the behavior such mechanisms bring about is not unique. Indeed, one may claim that, in a way, each thing and each process in the world is unique and that strict identity does not exist, but this does not seem to be Glennan's approach. From examples such Barthes's demise, it seems that what Glennan deems unique facts depend on what scientists take o be unique facts, and this, in turn, seem to be conditioned by pragmatic reasons. One Jorge González's death by leukemia may seem unique for the people who loved him. However, for the physician and the biologist González's demise is just one instance of a class of cases of death by leukemia. Similarly, Barthes's death seems unique if one focus on Barthes's personality, but it is just an instance of a class of phenomena if one approaches the fact as a traffic casualty in a big city.

Another example. The extinction of dinosaurs may be seen as unique in that the complex processes leading to the death of the last member of the Superorder Dinosauria was triggered by a meteorite impact. But this is not the only possible approach to the problem of dinosaur extinction. In the first place, a fact such as a meteorite impacting the Earth may seem unique when one focus on the explanation of dinosaur extinction, but meteorites impact other astronomical objects on a more or less regular basis, so there is nothing intrinsically unique in the fact that one of them stroke the Earth. Besides, there is currently an ongoing debate related to the possibility that multiple impacts where responsible for the Earth's complex climate changes that led to dinosaur extinction. However, what in my view is important, is that the direct impact of the asteroid was not the cause that killed dinosaur and other varied organisms, but precisely a very complex network of ecological processes not very different to those that kill organisms nowadays: resource reduction and competition, predation, disease, etc. In other words, dinosaur extinction can be explained through a number of types of ecological mechanisms. The upshot is that the uniqueness of a fact heavily depends on the approach the researcher takes to study such fact. To paraphrase a cliché, uniqueness is in the eye of the beholder.

In a recent work, Pâslaru (2009) analyzed the mechanism of niche complementarity in the light of the new mechanistic philosophy[63] and concluded that ecological mechanisms are not systems, or entities and activities, but insensitive networks of causal processes. This ontological thesis is in agreement with my findings in this dissertation that ecological mechanisms are processes or networks of processes, not things. As for insensitivity, Pâslaru follows Woodward (2003) and takes insensitivity to mean that causal relationships "are not affected by modifications in the background conditions of variables *X* and *Y* or by changes in the actual circumstances of the relationship" (Pâslaru 2009: 834), where *X* and *Y* are the *relata* of a causal relationship. It would seem that insensitivity is not a property of the processes but a property of a collection thereof. Thus, a class of processes is said to be insensitive when different instances of the type maintain the functional equivalence in spite of changes in the background conditions. As a consequence, insensitivity is relative to those changes in background conditions and, in addition, should be a matter of degree (but Pâslaru does not develop this point), that is, some ecological (types of) mechanisms might be more insensitive than others. Interspecific exploitative competition in the Portal area, for example, seems to be more insensitive to temperature than facilitation by shading in Montana. Resource consumption by each organism involved may change with temperature, especially if they are of distantly related taxa, but the overall competitive relation between them will remain across a wide range of temperature values. On the other hand,

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[63] That is, the work developed by Stuart Glennan, Peter Machamer, Lindley Darden, Carl Craver and William Bechtel (see Chapter 5).

photoinhibition, the specific harmful process mitigated by shading, is heavily increased by low temperatures, which are usual the environment of interest. Facilitation is also more sensitive to spatial location than IEC, because the latter would continue to hold should the community be transported to a different place. However, facilitation by wind amelioration would drastically change or even not take place in a region where winds are not strong.

My analysis also provides support for the contemporary mechanismic thesis that mechanisms occur in nested hierarchies. However, the hierarchical arrange of mechanisms can be interpreted in at least two ways. One of them is the view, common to all contemporary mechanismic philosophers, that mechanisms at one level comprise different (sub)mechanisms that take place at a lower level of organization. This is the *nested* aspect of mechanisms. For example, facilitation by shading, an ecological-level mechanism, comprises physical-level mechanisms responsible for the behavior of light in different circumstances and biochemical-level mechanisms that produce the phenomenon of photosynthesis. This type of nested organization is a consequence of the systemic nature of things (at least to the atomic level of organization). Strictly speaking, it may be not necessarily a hierarchy, for it is not clear that lower levels dominate in any sense over higher levels of organization (although it is clear that things in lower levels *precede* higher levels from a genetic point of view: things in lower levels *constitute* things in higher levels). A second interpretation of the hierarchical arrangement of ecological mechanisms, the *hierarchy* aspect, is related to an altogether different phenomenon, namely that of the interactions among mechanisms. This aspect of mechanisms is nicely illustrated by Baumeister and Callaway's conclusion that facilitation by shading is a condition for other types of facilitation to be of significance (sees Chapter 2, Section 2.1.1). This kind of arrangement among mechanisms is hierarchical strictly speaking. More importantly, as the authors note, it suggests that ecological mechanisms should not be studied isolated from other mechanisms, but paying attention to their interactions. It also shows that a purely functional account of an ecological fact invoking "facilitation" would miss the possible interactions among mechanisms.

7.3 Mechanismic explanation II: Epistemological theses

Mechanisms descriptions are a part of the description of ecological systems. The usual motivation for such description of mechanisms is the search for an explanation of the behavior of the system of interest.

All contemporary mechanismic philosophers agree that a mechanismic explanation must include the description of the fact to be explained (explanandum) and a description of the processes (interactions and activities) and the things involved in the production of the fact described in the explanandum. My analysis of facilitation and IEC suggest that ecologists advance their understanding of ecological facts by means of conceptual devices of different explanatory power (see Figure 6.1). A first understanding of the spatial distribution of vegetation in the area studied by Baumeister and Callaway (2006) was approached firstly through a functional account: the type of mechanism that explains the spatial relations between *Pinus flexilis*, on the one hand, and *Pseudotsuga menziesii* and *Ribes cereum* on the other is facilitation. Yet, the concept of facilitation encompasses a variety of subtypes of concrete processes, among them shading, wind amelioration, litter accumulation and snow accumulation, the description of which enhances explanatory power. The more detailed the description, the more explanatorily powerful will be, and the less general. Ecologists have recognized this trade-off between detail and generality and they usually put it in terms of a gradient with "realism" in one extreme and "generality" in the other. Cooper (2003:

263) provides an adequate representation of the tension between these two epistemic values (representational realism vs. generality) relating three gradients: (a) “fidelity” (from high to low), (b) abstractness (from concrete to abstract), and (c) representational power (from causal/mechanical to phenomenological models) (Figure 7.2).

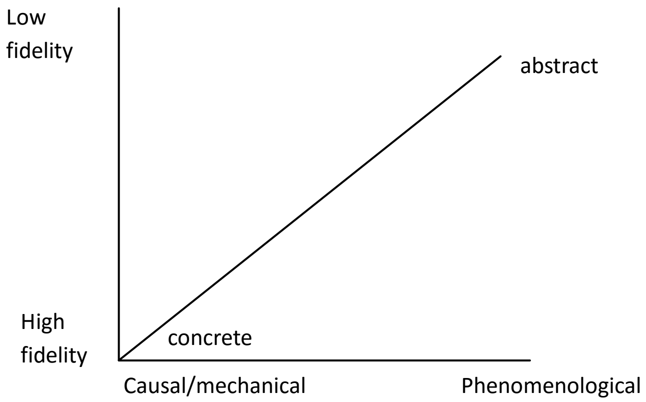

Figure 7.2 The tension between realism and generality represented in a conceptual space for models. (From Cooper 2003: 263.)

An important feature of ecological research is that while ecologists usually deal with individuals and concrete situations their explanations are given in more or less general terms. The step from individuals to types is made by abstraction and idealization in search of generality. Because of their generality, mechanismic generalizations may support counterfactuals. Counterfactual support is important for forecast and thus may help in experiment design.

Glennan requires that the mechanical description central to a mechanistic explanation be an invariant, change-relating generalization *à la* Woodward (2003). This kind of generalization —which also may be called a causal invariant— is characterized by being invariant under ideal interventions. This is also a counterfactual notion. Pâslaru (2009) claims that counterfactual support (an epistemic category) is a result of the operation of mechanisms (an ontological category), which is mostly correct. However, counterfactual conditionals cannot be assigned truth values and this is a problem for a generalization to be a part of a scientific explanation. This is one of the reasons to prefer an approach to explanation couched in terms of ecological laws. Yet, ecology does not seem to have general laws in the traditional sense of the word. Consequently, one has to choose between two possible strategies: either reject the possibility of general laws in ecology or redefine the concept of a law of nature. It is important to note that the first alternative does not amount to deem ecology unlawful. Ecology might lack ecological-level laws, but still be lawful because of all the laws at the physiological, biophysical and the biomolecular levels. Since ecological mechanisms are more often than not describable in terms of lower-level laws, ecological mechanismic explanation would still be using laws.

An alternative strategy would be to redefine the concept of a law of nature or replace it with a notion that, while offering some of the goods traditional laws provide, it is not as demanding as the latter. This is the avenue preferred by Woodward, Glennan, and Pâslaru, who rely on invariant, change-relating generalizations. This is also Bunge’s choice, though he keeps the name ‘law’ of nature and defines it as an objective pattern that relates properties such that the scope of one of them is included in the scope of the other (Chapter 5, Section 5.3.1.3.2). Thus, laws of nature are not generalizations and may have rather restricted scopes. This would be the case of ecology if non-controversial ecological laws were finally found. (For a recent attempt in that direction see Dodds 2009.)

For the time being, though, it should suffice to admit that ecological explanation implies regularities of diverse kinds and that the problem of how to precisely characterize them is still open. This does not mean that the problem of generality is not important, but it is a hint towards a distinction to be explored in the future: the relation between generality and explanatory power. At first blush, my intuition is that explanatory power comes from mechanism description. This is in consonance with the common sense idea of an explanation as a chronicle describing a succession of facts. However, science as a social cognitive enterprise goes beyond common sense and is more interested in regularities and commonalities than in uniqueness. Hence the construction of models, explanatory or other, that seek generality, systematization, heuristic power, and predictive power, among other epistemic values, in addition to explanatory power.

4. Final remarks

In light of the foregoing discussion, I submit that the contemporary mechanismic philosophy constitutes an adequate approach to the problem of explanation in ecology for it emphasizes what I take to be the main source of explanatory power, namely mechanism description. In addition, my analysis suggest that the philosophical movement usually called “new mechanistic philosophy” would benefit from taking into account the contributions made by Bunge to the ontology and the epistemology of scientific explanation, especially on the importance on the nature of mechanisms and their importance for scientific explanation.

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