Neighborhood effects in schooling and in the labor market

PhD Thesis

by

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Barcelona

2004

Acknowledgments

I enrolled in the PhD program at UPF in 1996, about 8 years ago. It is a long time, and I want to take the chance to thank some of the people I have met meanwhile.

About four years ago Antonio Ciccone trusted that I would have eventually managed to write a thesis even if I was about to leave Barcelona and move to Rome to start working at the Bank of Italy with only few ideas to work on. For this and for his patience, encouragement and advice I am grateful.

That job at Bank of Italy was the result of an experiment suggested by Nicola Pavoni, whom I thank for having popped into my office that day. It gave me the chance of meeting stimulating people who gave a fundamental contribution to the development of the research lines collected here. Indeed, most of this work would not exist if Andrea Brandolini, Federico Cingano and Piero Cipollone had not been there.

I spent four years at UPF. These were fruitful years in many respect also because Matthias Messner and Alessandro Secchi, with whom I shared a lot and from whom I learned at least as much, were also there. Occasionally, things went on slowly for a while. There were good reasons, and I thank those that gave me the chance to think them over.

Unawarely, Arantxa Jarque was the final impulse to the completion of this thesis. Thanks.

Finally, I thank my family. About 15 years ago my mother Lina and father Gaetano strongly suggested I should take an exam, which I eventually did. At that time, neither they nor I suspected where following their advice would have brought me over these years. I thank them for having then believed that their sacrifices and effort would have been fruitful. My parents, my sister Antonella and my brother Sandro also deserve a special thank for having put up with a peculiar son and brother through these years.

Qui serait assez insensé pour mourir sans avoir fait au moins le tour de sa prison?

M. Yourcenar, L'Œuvre au Noir.

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Introduction

Over the past decade economists have increasingly recognized that in many situations individuals may affect each others' behaviors, choices, preferences, payoffs not only indirectly, through markets, but also directly, through imitation, learning, information sharing, conformation to social norms and the like. A growing body of studies has formally modeled and empirically investigated these forces in a variety of contexts: schooling, labor market, crime, location choices, health habits are just few examples. The existence of these *neighborhood effects* is relevant both from a positive and from a normative point of view. On the one hand, they help to account for the significant variability of certain phenomena across otherwise apparently similar groups of people. On the other, awareness of their existence and strength is a relevant ingredient for the design of efficient policies. The papers collected in this thesis are about the existence and some consequences of these effects in two specific environments: school and the labor market.

The paper *Social interactions in high school: lessons from an earthquake*, written with Piero Cipollone, asks whether peers' success in achieving a high school degree has a causal effect on a teenager's graduation probability. This is not a novel question but providing an answer is usually difficult since there are many reasons why people belonging to a defined group might behave similarly. To identify a peer effect a researcher ideally needs a source of exogenous variation in some individuals' behaviors which is also known not to affect directly that of other members of the group so that any change in the latter's behavior can be said to stem from peer effects. We look for this kind of variation in the earthquake that hit Southern Italy in November 1980. In the aftermath, young men from certain towns were exempted from compulsory military service. By appropriately selecting a suitable control group, we show that the exemption raised high school graduation rates of males by 3-4 percentage points. Similar comparisons show that graduation rates of young women, not subject to military draft, in the affected areas rose by about 2-3 percentage points. We argue that the impact on women is a consequence of peer effects working at the municipality level rather than to other earthquake-related factors.

The next two papers look at the labor market and investigate specific implications of neighborhood effects at play when an individual is searching for a job and, once employed, within workers employed at the same firm.

In the paper *People I know: social networks and job search outcomes*, written with Federico Cingano, we provide evidence in favor of a set of recent theoretical models that link social networks and job search outcomes. These works generally conclude that, all else equal, individuals belonging to larger and better networks find jobs more quickly and end up earning higher wages. Empirical work on this issue has been limited to comparing the role of networks to that of other methods of search. Our contribution complements the existing literature on the role of social connections for job search by tackling a so far empirically unexplored question: the role of network size, composition and quality on unemployment duration and entry wage. We address these relationships using a comprehensive data-set of matched worker-firm social security records. The data cover the universe of individuals employed in a very integrated local labor market in northern Italy over the period 1975-1997. We exploit the data to reconstruct individual working histories for over one million workers. By tracking who worked together when, where and for how long we can build several measures of networks workers have potential access to. We then relate these network indicators to unemployment duration and entry wages for a subset of over 13,000 exogenously displaced workers. Results point to statistically significant and robust network effects on unemployment duration, in line with several theoretical predictions. We use our data on individual working histories to argue that these effects are not likely to be driven by individual heterogeneity.

The paper *Unequal workers or unequal firms?*, written with Andrea Brandolini and Piero Cipollone, explores the consequences of within-firm neighborhood effects for earnings inequality. Recent theoretical contributions have shown how, in the presence of intra-firm spillovers, the labor market sorts workers across firms according to some individual characteristics. Moreover, the existence of these spillovers implies that a given characteristic is paid a different price in different firms, depending on the pool of workers employed. This is at odds with the standard approach to the analysis of earnings dispersion where, because of the lack of data, a researcher has to assume that all workers face, in a given period, the same wage schedule. We exploit a comprehensive matched employer-employee dataset covering the period 1980-1997, where for a panel of 1,500 firms we have detailed information on every individual working in a given year in the firm. This dataset allows us to estimate a year-firm specific wage equation and to explore the degree of heterogeneity in wage schedules across firms as well as its evolution over time. We can also assess the impact of each source of heterogeneity on the overall change in earnings dispersion. In our data, the decomposition based on a the standard approach attributes two thirds of the total change in wage dispersion between 1980 and 1997 to modifications in the characteristics of workers and only one third to variations in their reward. By contrast, characteristic and price effects contribute equally to the total change in wage inequality when we use the year-firm wage schedule approach. Most of the difference appears to depend on the bias that affects the average rewards estimated in the standard model.

Social Interactions in High School: Lessons from an Earthquake.

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Abstract

We provide new evidence on the impact of peer effects on the schooling decisions of teenagers. In November 1980 a major earthquake hit Southern Italy. In the aftermath, young men from certain towns were exempted from compulsory military service. Comparisons of high school graduation rates between men from the least damaged but exempted areas and men from nearby towns that were not exempt show that the exemption raised high school graduation rates by 3-4 percentage points. Similar comparisons show that graduation rates of young women in the affected areas rose by about 2-3 percentage points. We argue that the impact on women is a consequence of what Manski (1993) has termed an "endogenous" peer group effect working at the municipality level. Our estimates suggest that an increase of 1 percentage point of males graduation rates raises females probability of completing high school by 0.6 to 0.7 percentage points. A series of robustness checks, including comparisons across different age groups and with different definitions of the comparison areas, suggest that the rise was due to the earthquake-related exemption, rather than other factors.

Keywords: social interactions, peer effects, schooling.

JEL codes: I21, C90, C23.

[§]We thank Joshua Angrist, David Card, Antonio Ciccone, Armin Falk, Andrea Ichino, Brian Krauth, Jesse Rothstein, Emmanuel Saez as well as seminar participants at the Banff Workshop on Experimental and Non-Experimental Evaluations of Peer Group and Other Effects, Bank of Italy, European University Institute, IZA-Bonn, University of California at Berkeley, University of Padua, Universitat Pompeu Fabra, 2004 Winter Meeting of the Econometric Society, 2003 EEA-ESEM, 2003 EALE, 2003 AIEL for their useful comments. We also thank Manuela Brunori, Lt. C. Pietro Canale, Federico Giorgi, Giovanni Iuzzolino, Federica Lagna, Maurizio Lucarelli, Raffaella Nizzi, Simona Paci, Giovanni Seri, Carla Tolu for providing and helping us with the data and the legal aspects. We are the sole responsible for any mistake. The views expressed in this paper do not necessarily reflect those of the Bank of Italy. Correspondence: Research Department, Bank of Italy, Via Nazionale 91, 00184 - Roma - Italy. Email: piero.cipollone@bancaditalia.it, alfonso.rosolia@bancaditalia.it

1 Introduction

Many studies have investigated the role of peers' schooling outcomes in affecting individual ones (Hoxby (2000), Angrist and Lang (2002), Hanushek, Kain, Markman and Rivkin (2001), Sacerdote (2001), Zimmerman (2003)). Theoretical contributions have also pointed out how the efficient design of a school system hinges crucially both on the existence and on the specific determinants of these non-market interactions (Hsieh and Urquiola (2003), Winston and Zimmerman (2003), Epple and Romano (1998)). Yet an empirical assessment of this causal relationship is very difficult[1] (Manski (1993), Moffitt (2001)).

In this paper we provide robust evidence for the existence of a causal relationship going from the schooling achievements of young males to those of young females belonging to the same age cohort and living in the same town. Identification of this causal effect hinges on an exogenous shock to males' schooling, namely the exemption from compulsory military service granted to few specific cohorts of males in the aftermath of the earthquake that hit southern Italy in November 1980. We show that the exemption increased high school graduation rates by more than 3 percentage points for males and by 2-3 percentage points for females. The underlying research design is based on comparisons of individuals belonging to the least damaged but exempted towns and individuals from nearby towns that were not exempt. This ensures that these increases are due to the earthquake-related exemption rather than to other factors. Further comparisons of the exempted cohorts and the non-exempted within a given town controls for time-invariant town characteristics. Since in Italy females are not

[1]Individuals belonging to a given population may display similar schooling achievements for various reasons. It may be because they share the same environmental characteristics (same school infrastructure, teachers quality, etc.) or because they are sorted according to some individual, possibly unobservable, characteristic (parental education, ability, etc.). Finally, they may display similar achievements because of some externality at play within the population. These may be in turn grouped into two main categories, that Manski (1993) defined as *contextual* and *endogenous* effects. On the one hand, the external effects may stem from some exogenous characteristic of the population. For example, average parental education in a given class may affect individual outcomes because children do homework together and benefit from their classmates' parents' human capital through the help received when studying. Alternatively, external effects may arise from individual outcomes. For example, if some students do better they may help their classmates or they free up more teacher time to be devoted to more needy students. Another possibility is that doing well or poorly in school may become a social norm of a given population, to which members conform (Akerlof and Kranton (2002)).

subject to military draft, we argue that the change in their school achievements is the response to that of males: a one percentage point increase in males' high school graduation rates raises those of females by 0.6-0.7 percentage points.

We trust our results are important for several reasons. First, we believe that they constitute genuine evidence that peer performances, not only peer characteristics, affect individual outcomes. The difference between the two sources of peer effects is important since one (peer characteristics) is exogenously given whereas the other (their performance) can be manipulated by certain policies. Second, we contribute to the literature quantifying the effects of military conscription on schooling. Previous studies have mainly focused on the causal relationship between subsequent earnings and veteran status (Angrist and Krueger (1989), Angrist (1990), Imbens and van der Klaauw (1995)). To our knowledge, only Angrist and Krueger (1992) and Card and Lemieux (2001b) address a similar issue showing that draft avoidance behaviors increased college enrollment and graduation rates of potential draftees in the Vietnam-era. Their results are only apparently in contrast with ours since, differently from ours, in their setting serving in the army would generally imply being sent to war. Third, since our definition of group embeds the one usually underlying studies of peer effects in schooling, the classroom, the estimated peer effects are more general than those normally at work in school.

A simple evaluation of our results, based on recent OECD estimates of the elasticity of steady-state per capita GDP to average years of education (OECD (2003)), suggests that a permanent increase of 1 percentage point of male high-school graduation rates would permanently raise per capita GDP by about one fourth of a percentage point; neglecting the effect of males' schooling on females would underestimate the increase by about one tenth of a percentage point, putting the overall increase at 0.16 percentage points. Our results also magnify the social returns to education implied by recent estimates of the causal effects of schooling on various relevant outcomes identified in recent years (Acemoglu and Angrist (2000), Moretti (2004), Lochner and Moretti (2004)), Bresnahan, Brynjolfssonn and Hitt

(2002), Sacerdote (2002), Currie and Moretti (2003), Kenkel (1991)).

The paper is organised as follows. In the next section we briefly survey the literature and introduce the main features of our identification strategy. We then illustrate the institutional setting and the specific features of the natural experiment we exploit to recover our instrument. In Section (4) we describe our sample and discuss our definition of the reference group. Sections (6) and (7) present and discuss the results. We then conclude.

2 An outline of the empirical strategy.

Economists and sociologists have extensively documented the high correlation among behaviors, choices or performances of people in the same group or neighborhood.

As Manski (1993)) pointed out, an appraisal of the forces underlying this evidence has to take account of several facts. First, similar behavior may simply reflect the influences of a common environment. Second, groups are often endogenously formed so that the higher degree of homogeneity of behaviors within rather than between groups might reflect the tendency of people with similar tastes, attitudes, abilities, characteristics or possibilities to group together[2]. Third, individual behaviors or characteristics may directly affect those of other people by altering their choice sets, their preferences or the profitability of some actions[3].

Disentangling the latter effect is relevant for policy purposes. The existence of neighborhood effects is in fact one of the motivations underlying desegregation programs such as METCO, Moving to Opportunity, etc. (for example Katz et al. (2001), Ludwig, Duncan and Hirschfield (2001), Angrist

[2]Kremer (1997), Cutler, Glaeser and Vigdor (1999), Borjas (1995), Benabou (1993), Fernandez and Rogerson (2001). Evans, Oates and Schwab (1992) criticized early attempts to measure peer group effects pointing out that the choice of the group is itself endogenous. They show that when properly accounting for this fact all social effects vanish.

[3]The last decade has witnessed a growing body of empirical literature documenting the existence of such peer effects in many fields other than schooling. Among others, Case and Katz (1991), Glaeser, Sacerdote and Scheinkman (1996), Falk and Ichino (2003), Falk and Fischbacher (2002), Grinblatt, Keloharju and Ikaheimo (2004), Duflo and Saez (2003), Miguel and Kremer (2004), Bertrand, Luttmer and Mullainathan (2000), Katz, Kling and Liebman (2001), Lalive (2003), Ichino and Maggi (2000), Topa (2001), Gaviria and Raphael (2001).

and Lang (2002)). However, it is even more relevant to establish whether it is peer characteristics or performances that influence individual outcomes; that is, whether individual achievements respond to exogenously given features of the group of peers or to some modifiable outcome. For example, the fact that the better students exert a positive spillover on the less able is no guarantee that a reshuffling of students across groups will improve the average outcome[4]. On the other hand, if the channel is through peers' behavior, it could be exploited by a policy maker to design more efficient interventions. For example, it would be enough to transfer money only to some parents (perhaps the poorest) so that their children do better in school. Their improved performance will in turn affect those of their peers at zero cost for the policy maker and without worsening some other individual's outcome.

The dearth of suitable data is the major limitation preventing empirical analyzes from sorting out the two effects (Moffitt (2001)). They either rely on random assignment (Sacerdote (2001), Zimmerman (2003)) to break the potential correlation among individual outcomes due to sorting or they instrument peers' outcome with exogenous changes in group composition (among others, Hoxby (2000), Angrist and Lang (2002), Hanushek et al. (2001))[5]. The common feature of these studies is that the results do not allow to establish whether individuals are responding to peers' performance or to peers' exogenous unobservable characteristics. Recently, Boozer and Cacciola (2004) exploit experimentally induced variations in peer achievements at the class level to provide evidence on peer effects propagating through outcomes.

To address such an issue one would ideally need an exogenous variation of peers' performance holding their (un)observed characteristics constant and study the response of an individual to this change. In this paper we try to shed light on this issue by studying the reaction of girls' schooling to that of boys. The original feature of our exercise is that the change in males' schooling does not come

[4]See Hoxby (2000) for a thorough discussion of this issue.

[5]An alternative approach is developed in Glaeser, Sacerdote and Scheinkman (2002) and related works. It consists in comparing individual and aggregate estimated coefficients or in analyzing the spatial variance of the phenomena of interest (Glaeser et al. (1996)). See Manski (2000) and Moffitt (2001) for a thoughtful and critical review of the different approaches.

from an exogenous change in the underlying population of males but from an exogenous government intervention that is shown to have raised their high school graduation rates.

Formally, let y_{ik} equal 1 if i, who belongs to group k, has graduated from high school and zero otherwise. Let i be a female and her performance be determined according to:

$$y_{ik} = \alpha + \beta x_{ik} + \delta \bar{y}_k^M + u_{ik} \tag{1}$$

where $\bar{y}_k^M$ is the graduation rate of males in group k[6]. Our exercise aims at consistently estimating the parameter δ. Standard OLS may lead to biased estimates for several reasons. For example, if the error term includes some unobserved group feature that also affects males' graduation rates (e.g. school quality) then $E(\bar{y}_k^M u_{ik}) \neq 0$; the same spurious correlation arises if individuals are sorted according to some personal characteristic (say, more and less able students). Alternatively, if it is the case that males' performance also responds to that of females we are in the standard simultaneous equations framework and the usual identification problems arise[7]. Therefore, provided one has a clear concept of what a group is[8], what is needed in order to identify the parameter of interest is a reliable instrument for males' average performance $\bar{y}_k^M$. Moreover, to identify the effects of peer performance rather than those of (un)observed peers characteristics, the instrument must exogenously alter $\bar{y}_k^M$ leaving group composition unaltered.

We believe our instrument has those characteristics. We instrument males' high school graduation rates with the exemption from compulsory military service (henceforth, CMS) that was granted to some cohorts of males of high school age after the earthquake that hit some parts of southern Italy in November 1980. The exemption was granted only to males living in the area affected by the quake at the relevant date. There are sound reasons to believe that this intervention, while leaving the

[6]The theoretical foundations of such a specification might be recovered in models of social norms and group identity (for example, Akerlof and Kranton (2002)) or of standard human capital externalities (for example, Acemoglu (1996)).

[7]In Section (7) we discuss the implications of this and similar assumptions for the interpretation of our results.

[8]Manski (1993) shows how the definition of a group is a crucial ingredient for the identification of peer effects. "[...] Any specification of a functionally dependent pair (x, z) is consistent with observed behavior. The conclusion to be drawn is that informed specification of reference groups is a necessary prelude to analysis of social effects." We discuss this issue in Section (5).

composition of the group unaltered, modified males' high school graduation rates[9]. In Italy, because of the structure of the schooling system, the draft may actually result in the permanent termination of one's educational career. Therefore, it is reasonable to think that lifting this limitation will allow some marginal individuals who would have otherwise dropped out to advance somewhat more in the educational process[10].

Our empirical strategy is similar to that followed by Duflo and Saez (2003) and Miguel and Kremer (2004). In their work, identification of the crucial parameter is achieved by means of a partial-population intervention, that is by looking at how untreated individuals belonging to well-defined groups where some individuals have been treated perform afterwards. Duflo and Saez (2003) run an experiment where only some individuals randomly selected within certain university departments of a North-American university are informed about an advertisement fair concerning a retirement scheme. They find strong social effects as concerns fair attendance which in turn reflect on the decision to join the advertised savings program. Miguel and Kremer (2004) study externalities to medical treatments in subsaharan Africa. The intervention they exploit to achieve identification is a randomized school-based mass treatment with deworming drugs. They show that the deworming program substantially improved outcomes (school attendance and health status) of untreated children.

[9] The effects of military draft on earnings have been widely investigated (Angrist (1990), Imbens and van der Klaauw (1995)). Much less has been done as concerns the effects of the draft on schooling choices. Angrist and Krueger (1992) and Card and Lemieux (2001a) show that college enrollment as a draft avoidance device during the Vietnam era was effective in raising college graduation rates of potential draftees suggesting that in the absence of the draft these individuals would have not attended college. From a theoretical point of view, Lau, Poutvaara and Wagener (2002) develop a model of human capital accumulation in which the military draft, formalized as a period compulsorily spent working at a wage below market level, turns out to be detrimental to human capital accumulation.

[10] It could be argued that if marginal males stayed in school then group composition was altered if the group is, say, a class. Yet, in this case the change in the composition would be in the direction of lowering peer quality. Therefore, based on previous findings of positive peer effects, we should expect a drop in females' performance. We show below that this is not the case.

3 Background: institutional setting and the natural experiment.

In this section we describe the relevant institutional setting and the government interventions following the earthquake. We then explain why we think a military exemption may affect high school graduation rates of young males.

3.1 The compulsory military service.

Article 52 of the Italian Constitution states the general principle that defense of the homeland is a duty of each citizen and that military service is compulsory. For a long period after WWII the basic regulatory framework remained that designed in 1938 (Regio Decreto No. 329, February 24th 1938), which stated the obligation to serve for all males[11]. According to the rules prevailing in the early eighties, the period we are interested in, the length of the service was twelve months for those enrolled in the Army and Air Forces, and eighteen months for the Navy. The same regulation stated that the military authorities would assess the health conditions of all Italian males in the year they turned 18 to establish their physical and psychological conditions and, as a consequence, their suitability for military service. Upon fulfilling these requirements Italian males would be inducted as they turned 19[12]. Therefore, teenagers typically underwent the health assessment during the last year of high school and were drafted shortly after[13]. Military service could nonetheless be deferred under specific circumstances[14]. A request for deferment had to be filed each year and a one year delay was granted

[11]Females were not allowed to enter military corps, not even on a voluntary basis, until recently.

[12]The reasons for exoneration from service are strictly coded and quite restrictive. They typically require main physical disabilities or serious mental disorders. They must be ascertained by military qualified medical personnel in a thorough three-days visit.

[13]The Italian educational track is based on three different levels of education. A basic level, compulsory for everybody, that includes 8 grades and is usually completed by age 14. Upon completing the first level youths decide whether to enroll in high school, which consists of 5 grades and is usually completed by age 19. Throughout these grades students have to be admitted every year to the next grade. In case they fail, they have to repeat the grade failed and thus lag behind one year. Up to high school there are virtually no fees to be paid, thus the only costs involved are foregone earnings and expenditures such as books and other material. Graduation from high school requires passing a nationally administered exam. Upon passing this test students can freely enroll into college, the third level of education, that theoretically lasts anything between 4 (e.g. economics, law) and 6 years (e.g. medicine, engineering) depending on the subject. People that lag behind in college are penalized by higher yearly enrollment fees.

[14]Military service could be replaced by civil service under specific circumstances (conscientious objector status). In the eighties the length of this alternative service was longer than normal military service (24 months). The draftee would

provided certain requirements were fulfilled. In particular, people enrolled in high school could defer service until they were 22, provided this was enough time for them to be able to graduate by that age[15]; people in college could defer service until they turned 26 if enrolled in a four-year program, 27 if enrolled in a five-year program and 28 if enrolled in a longer program, provided they had passed at least two exams in the previous academic year[16].

3.2 The November 1980 earthquake.

In the evening of November 23rd 1980 an earthquake struck southern Italy. The epicenter was inland and it involved large parts of three regions (Campania, Basilicata and Puglia; figure (1)). Several legislative acts in 1981 precisely defined the area to be considered as damaged[17]. The final list included all the municipalities in the regions of Campania and Basilicata and a few neighbouring ones in the region of Puglia (figure (2)). In the aftermath of the earthquake the government undertook a series of measures to help the recovery.

A few months after the quake Parliament passed a law (No. 219, 14 May 1981) that defined the amount and the guidelines for the distribution of the financial resources targeted to the reconstruction of the damaged area. For the period 1981-1983 the total budget for recovery was equal to 17 percent of the 1980 GDP of Campania, Basilicata and Puglia (roughly 12 billion dollars at 2003 prices and exchange rate). About 80 percent of the sum was targeted to rebuilding private dwellings and public buildings. The remaining 20 percent was devoted to the reconstruction of factories, farms and basic infrastructures needed to help the recovery of economic activity. According to the official procedures, only people whose houses, farms or factories were damaged (as certified by the authorities) were

file a request for alternative service and after some months he would be called before a military commission to justify his request.

[15]For example, a 19-year-old individual enrolled in the 2nd year of high school will not be allowed to defer service since by age 22 he will at best reach the 4th year (of a five year program).

[16]Italian university degrees are obtained once all exams requested by the specific course have been passed and a graduation thesis is defended. Exams can be taken at any time and generally do not require a particular order.

[17]The actual laws (Decreti del Presidente del Consiglio dei Ministri) were issued on April 30th, May 22nd and November 13th.

entitled to this financial aid. In principle, no money accrued to municipalities where no family had suffered damage.

Among the several interventions deliberated by the government, the ones relevant to our purposes are a series of laws that modified or even canceled the obligation to serve in the military for some cohorts. A first law, passed on May 14th 1981 (law No. 219/81), gave to all males born between 1963 and 1965 and resident as of November 1980 in the area officially declared as hit by the quake the opportunity to comply with their military obligations by serving as civilians in alternative services active in the area hit by the earthquake. On April 29th 1982 another law (law No. 187/82) totally removed the obligation to serve in the army for all males born before 1964 and resident at the time of the earthquake in the towns included in the official list. Finally, on April 18th 1984, a new law (law No. 80/84) extended the military exemption to all those born before 1966 resident in those towns as of November 1980. Table (1) summarizes the situation of the exempted cohorts: it reports the age at which they obtained the exemption, the law that authorized it, the year the law was issued and the age at which we see these people in the 1991 Population Census, our main data source. Although the exemption was eventually granted to all males born before 1966, it probably had an uneven impact on the different cohorts because of the interplay between the dates at which the relevant laws were passed and the time when each cohort was supposed to serve. Males born before 1962 were largely out of high school by the time they received the exemption, either because they had completed it or because they had dropped out. Males born in 1962 were exempted at age 20. A non-trivial share of them were still in high school at that age (in 1979 at the national level almost 6 percent). Therefore the exemption could have had an effect on their high school graduation probability. Finally, the cohorts born between 1963 and 1965 learned about the possibility of alternative service as well as about the exemption when they were still in school. Therefore most of the effect of the exemption should be detected in the high school graduation rate of those students born between 1963 and 1965 and who are behind schedule.

In the next section we discuss the sample design and we argue it breaks any correlation between the exemption from CMS and other interventions that may have independently affected female schooling. Before turning to this, let us briefly explain why we think an exemption from CMS may induce some teenagers to stay longer in school.

3.3 High school and the military draft

Compulsory military service is structured in such a way as not to interfere with the normal progress of people along the educational track as far as graduation from high school is concerned. As a matter of fact, youths are required to serve at 19, an age by which they should have completed high school. CMS may nonetheless interfere with the advancement of those who are behind schedule, as they need to ask for deferment[18].

Eligibility for military service comes at a delicate moment in a teenager's life. Evidence collected by Istat (2000) suggests that more than 20 percent of teenagers who drop out of high school do so because they are discouraged. Our anecdotal experience as former high school students in southern Italy, in line with this finding, suggests that even unconstrained individuals (e.g. younger than 22) who are behind schedule, may decide to drop out because of a demotivation effect. For them, complying with their military obligations represents the first step towards an adult life; in Italy, firms would not hire anybody who had not previously fulfilled his military obligation so that serving is *de facto* a job requirement[19].

One may wonder whether deferring service has a value of its own. For example, in the Vietnam era, success in deferring the draft would sensibly increase the probability of not going to war. In Italy, as

[18]The link between CMS and dropping out from high school is hardly investigated in the Italian economic and sociological literatures.

[19]Hiring someone who has not accomplished those obligations compels the firm to give him a one-year leave when he is eventually inducted.

we have detailed above, military service is compulsory and must be complied with by the age of 22. Therefore, deferring the draft does not increase the probability of not serving[20]. Moreover, there is no incentive in deferring service to avoid a military conflict since Italy has not been in war over the past 60 years[21].

Let us sketch in a simple way the alternatives available to an individual who is eligible to serve (18 or older) but who can also defer service for one year if he stays in high school (younger than 22). Let us assume for simplicity that he will live T more periods and define w_t^0 the yearly wage he earns with t periods of labor market experience if he does not acquire any additional schooling; similarly, let w_t^1 be the wage earned after t years of labor market experience if an additional year of schooling has been acquired. His choice will be based on the comparison between the value of dropping out, serving and then entering the labor market (U_0) and the value of deferring service one year, serving and then entering the labor market (U_1):

$$\begin{aligned} U_0 &= M + \sum_0^{T-1} w_t^0 \\ U_1 &= S + M + \sum_0^{T-2} w_t^1 \end{aligned} \tag{2}$$

where S is a measure of the period cost/gain of spending one year in school and M is the period cost/gain of spending it in service[22]. He will drop out and serve in the military if $\sum_0^{T-1} w_t^0 > S + \sum_0^{T-2} w_t^1$. Assume now an exemption is unexpectedly granted to this individual. In this simple framework, this amounts to freeing a year of his life from military obligation: he can then spend it in school or in the labor market. To understand how his alternatives change, it is enough to substitute M with w_T^0 in the expression for U_0 and with w_{T-1}^1 in U_1; that is, the values of earnings in the last period of life that, as such, embed all the previous labor market experience. Therefore, provided the return to an additional year of education is higher than the return to an additional year of labor

[20]The probability of not serving could be expected to be higher because one expects a mass exemption, as seems to be the case in Holland (Imbens and van der Klaauw (1995)). However, this is a very unlikely event in Italy.

[21]Peace-keeping missions (Kosovo, Afghanistan, Somalia, Iraq, etc.) are generally run by volunteers and professionals.

[22]We neglect discounting to keep notation simple. The basic argument still holds.

market experience ($w^1_{T-1} > w^0_T$) the incentive to stay in school increases when he is exempted from service.

All in all, simple labor market considerations lead us to think that the exemption should have a non negative effect on the amount of schooling a given individual acquires.

4 The research design.

The exemption from CMS was eventually granted to certain cohorts of males living in Campania, Basilicata as well as in some town in Puglia at the time of the earthquake. This area, shown in figure (2), hosted more than 5 million people, about 10 percent of the Italian population, spread over 650 towns. Our strategy is to use the exemption as an instrument for males' education in an equation like (1). For the exemption to be a valid instrument it has to be uncorrelated with the residual of this equation[23]. Yet if the earthquake had a direct effect on females' level of schooling, say, because they did not attend school for some time or because of subsequent income transfers or other interventions, then the exemption would be correlated with an event that independently affects the level of education of both sexes. Therefore it could not be a valid instrument. This is a serious concern because in many municipalities the quake was quite disruptive: figure (1) plots the area officially considered as hit by the earthquake along with the intensity of the damage suffered as measured by the synthetic index elaborated by the experts of the Ministry of the Budget in the aftermath of the earthquake in order to define the monetary compensations needed to help the recovery (Ministero del Bilancio e della Programmazione Economica (1981)). The index combines six measures of disruption that take into account specific damage[24] and summarizes them into a scale that ranges from 6 to 30[25]. The black

[23]In particular, it has to be uncorrelated with any direct shock to females' schooling except those determined by males' performance or by changes in the pool of males.

[24]Specifically, it combines the number of deaths or people injured in the earthquake, the number of people who lost their house, an index of damages computed by the Ministry of Internal Affairs, the number of houses destroyed, the number of houses damaged, the number of temporary shelters needed

[25]A municipality would score 6 if there were no deaths or injured people, homeless were less than 2 per cent of the population, the index of the Ministry of Internal Affairs was less than 5 per cent, destroyed houses were less than 0.5 per

area represents the center of the quake and the municipalities that suffered the greatest damage; the dark gray area includes towns that had serious disruption but lighter than in the epicenter. The light gray area gathers municipalities that were only marginally affected by the earthquake. Finally, the white area recorded no damage.

Our research design breaks the potential correlation between our instrument (the exemption) and other policy interventions triggered by the earthquake that might directly affect females' schooling so as to satisfy our main identifying condition. Rather than considering all 650 municipalities included in the earthquake area and the individuals living therein as our treatment group, we selected only those that, albeit granted the military exemption, were the least affected by the earthquake according to the official evaluations performed by the government in the aftermath. This basically amounts to choosing those towns that were located farthest from the epicenter while still in the area exempted from military service. Therefore we retain only the municipalities at the boundary of the earthquake area as defined by the government, as shown in light gray in figure (3). This leaves us with 57 towns, with a population of about 300,000 people at the end of 1979. Table (2) reports the distribution of the synthetic indicator of the damages suffered by these municipalities along with the average percentage of damaged houses and the number of people left homeless per thousand inhabitants for each value of the index; 39 of our 57 treated municipalities recorded some damage; the other 18 towns, although included in the area officially involved in the quake, had none. As a whole, the table shows that the towns included in our sample were largely unaffected by the earthquake. This implies, according to the official procedures for the assignment of financial aid (law No. 219/81, May 14th 1981), that they received virtually no money.

To assess the effects of the exemption from military service we need to compare the school achieve-

cent, those damaged less than 1 per cent, and temporary shelter was needed for less than 5 per cent of the population. At the other extreme a municipality would score 30 if at least 10 percent of the population died or was injured, at least 20 percent were homeless, the index of the Ministry of Internal Affairs was at least 70 percent, more than 30 percent of the houses were destroyed or heavily damaged and temporary shelter was needed for more than 40 percent of the population.

ment of the exempted cohorts in the treated municipalities with that of some control group acting as counterfactual of the exempted population. We adopt a selection criterion inspired by the regression discontinuity approach and choose as a control group all the municipalities neighbouring on at least one treated town, as shown in dark gray in figure (3). With this selection rule we end up including 60 municipalities in the control group, with a total population of about 600,000 people at the end of 1979. Therefore the overall sample includes 117 municipalities belonging to 12 provinces of 5 regions[26] with a total population of about 900,000 people at the end of 1979.

This selection criterion has several advantages. First, since treated municipalities are rather peripheral to the epicenter they suffered little damage, very much like non-treated municipalities (see table (2)). Therefore, the earthquake probably had negligible direct effects. However, the choice of the control group allows us to further relax this assumption. Being very close it seems reasonable to assume that any direct effect of the earthquake was the same across the two groups of towns. For example, if after the earthquake people decided it was too dangerous to send their children to school for a few weeks, this would presumably have happened in both treated and control towns. Therefore, if we are confident that treated towns did not receive any financial help and were not targeted by any intervention other than the exemption from CMS, the only assumption needed in order to identify the effect of the exemption is that on average the direct effect of the earthquake was the same across the two groups of individuals[27].

Second, geographical proximity guarantees that treated and control towns are broadly similar. Table (3) describes and compares a set of structural characteristics. Population density is lower in treated municipalities; these are on average smaller and higher above sea level in comparison with control towns. The differences in birth and death rates, although statistically significant, seem on the whole

[26] In particular, the provinces of Frosinone and Latina in the Lazio region, Benevento and Caserta in Campania, Foggia, Bari and Taranto in Puglia, Campobasso and Isernia in Molise, Potenza and Matera in Basilicata and Cosenza in Calabria. The detailed list of the selected towns is available upon request.

[27] In Section (7) we explicitly address the issue of the robustness of our results to the possibility of other correlated interventions.

to be negligible. Mobility patterns as measured by population inflows and outflows at the town level are the same.

A description of school endowment as of 1979, reported in table (4), also reveals that the two groups are broadly similar. Up to 5th grade, treated municipalities, in comparison with control, have fewer students as a ratio of the population and smaller average class size. The difference in class size goes significantly down, to one student less per class, when we look at the junior high school (6th to 8th grade), when the ratio of students to total population also becomes the same across the two groups of towns. The differences disappear altogether when we move up the educational track and look at high school (9th to 13th grade): average class size and the ratio of students to population are now statistically indistinguishable.

A more detailed description of some other relevant population characteristics may be obtained from the 1981 Population Census[28]. Tables (5) and (6) report a set of characteristics for the control group and the estimated difference between treated and control by age class, respectively for males and females. Again, the basic message is that the two groups of towns are broadly similar. Educational achievements, reported in the first column, were, as of 1981, broadly the same. The same is true if we look at employment rates: they are the same for males and only slightly higher for females in core age groups in treated towns. Looking at the white-collar versus blue-collar composition of the pool of employees in the two groups we see that there are basically no differences. Family structure also appears to be the same: the average number of children per household is again statistically the same for the two groups. When it comes to the age structure, the treated towns seem to have a slightly older population, but again the differences appear negligible overall.

[28]The 1981 Population Census was run in October 1981, about one year after the earthquake and when the final official list of municipalities targeted by the interventions had not yet been released (November 1981). Although concerns about the representativeness of these figures may be raised on the grounds that they are obtained after the shock we are examining, we believe that they constitute reliable measures because of our main identifying assumption, namely that these towns were only marginally, if any, affected by the earthquake itself and interventions other than the exemption from military service.

A third advantage of our selection criterion is that, being so close, these towns are embedded in the same economic environment, so that any market interaction that could possibly bias our results is taken care of. For example, since these individuals work in broadly the same labor markets, if the earthquake brought about an increase in the high school degree wage premium in the local market (for example, because a more skilled labor force was needed for reconstruction) this would have affected also the choices of individuals in the control group. By the same token, if the net benefits of completing high school went down because of an increase in college attendance costs (say, the closest colleges were damaged), they decreased equally in both groups of towns since the set of colleges these individuals may access is the same.

Overall, the evidence presented in this section makes us confident that we have selected a group of broadly similar towns for our analysis: we have shown that they share the same structural characteristics and we have argued that geographical proximity guarantees that they were exposed to the same extent to any direct effect of the earthquake. Therefore the exemption from CMS can be seen as a rule that randomly assigns to treatment some males and a simple comparison of average high school graduation rates of males across the two populations provides a consistent estimate of the causal effect of the exemption. Moreover, the exemption can be used to instrument males' high school graduation rates in equation (1) to estimate the causal effect on females' graduation rates. In the next section we describe our main data source and introduce the definition of group within which males' education achievements affect those of females.

5 Data and definitions.

We take the education data from the 1991 Population Census, 11 years after the earthquake. Therefore the youngest cohort potentially affected by the exemption from CMS is aged 26 and far beyond high school. Ideally, our sample should include all individuals belonging to the relevant cohorts and living as of November 1980 in any of the 117 selected towns. Unfortunately, the census gives only place of birth and place of residence at census date. Therefore, we proxy the place of residence in November 1980 with place of birth and select from the universe of the Italian population all the individuals born in any of the 117 towns in our sample, regardless of where they live at the 1991 Census. In other words, we assume that the cohorts relevant to our analysis, aged 15 to 24 in 1980, were living in their hometowns at the time of the earthquake.

This assumption could pose a problem for our identification since mobility decisions taken both before and after the earthquake might have led to changes in schooling achievements not due to the earthquake. For example, if people were afraid a new earthquake would strike and therefore decided to move away, differences in high school graduation rates might reflect simply different school qualities. But the research design suggests that these mobility choices would have been the same in control and treated towns. Indeed, our evidence shows that inflow and outflow rates for both groups of towns were basically the same over a long period of time (figure (4)); moreover, there was no clear change in mobility flows after the earthquake, suggesting that it did not play a great role. Additionally, given the young age of the people under investigation, it is reasonable to assume that mobility decisions were not taken by them but by their parents[29]. Therefore, if the probability that a given youth moved away from the place of birth and went to school somewhere else is a function of parental characteristics, we should expect the resulting flows, their composition and their destinations to be the same in control and treated towns, since the distribution of parental characteristics appears to be the same (tables (5) and (6)). This implies that any effect on schooling due to migration is controlled for by the comparison group of individuals. Note that among these potential effects of migration we can include the fact that the group of peers is changing, since again it seems reasonable to assume that it changed equally for both sets of individuals. This feature further strengthens our interpretation of the causal effect of

[29]Italian youths are known to live with their parents much longer than those of any other country (Manacorda and Moretti (2003), Giuliano (2004)). In the eighties, even due to bad labor market conditions, this was even more common.

males' graduation rates on females' as peer effects that work through peer behaviors.

We now turn to a crucial question: what is the *group* over which interactions take place? This is a fundamental issue in studies addressing peer and social effects. Manski (1993) shows how a mistaken definition may lead to tautological models. Another source of concern is that focusing on a superset of the true *group* may underestimate the strength of local interactions if they become more diluted with some measure of distance. We believe that a reasonable definition of group, able to capture *productive* externalities at work in the classroom as well as role models and information transmitted by peers, is the set of individuals of the same age who live in the same town. Figure (5) shows the distribution of the size of these groups in our sample: the median size is 45, only twice the average high school class size (see table (4)). These are small groups, because towns are generally small[30]. This in turn implies that these people have most likely known each other since childhood. They went to the same school in the early stages of their education: in our sample there are on average 5 primary and 1.5 junior high schools per town and respectively 8 and 9 classes per school. The structure of the Italian education system is such that most of one's classmates in the first year of a given grade (primary, junior high, high school) will move together along the educational track up to the last year. This means they spend four to six hours a day, six days a week together for eight years. Moreover, the small size of the towns considerably raises the chances of getting to know peers who are not classmates.

Direct evidence on the importance of peer interactions can be obtained from the 1998 wave of the Istat's *Indagine multiscopo sulle famiglie, soggetti sociali e condizioni dell'infanzia* (Istat (1998)), a multi-purpose survey that periodically collects information on about 21,000 households focusing on various aspects of family life. The 1998 wave includes a special section on people younger than 18 where information on schooling, leisure time and social relationships are collected. As expected, teenagers[31]

[30]A municipality is the smallest official territorial unit in Italy. The country is divided into about 8,100 of them, with an average extension of 37 square kilometers. The median population is about 2,300 and the 75th centile is only 5,200; only 75 towns count a population larger than 70,000.

[31]The population underlying the figures reported is people aged 14 to 17 and living in southern Italy (about 1,500 observations). Unfortunately we do not know the size of the town where they live so that we cannot provide evidence as

go out very often (90 percent go out several times a week); virtually everybody meets friends several times a week; three quarters happen to go out with no particular purpose a few times a week (say, they take a walk down to the main square or to the courtyard); they also go out with definite purposes: eat out, have a drink, go dancing or see a football match (60 percent at least once a week); they also go very often to parties thrown by others (60 percent go to more than 3 parties in a month). Last, these interactions do not appear to be gender specific: about 40 percent of the southern Italian girls spend their time equally with boys and girls; males are more self-referential but 34 percent of them still meet boys and girls with the same intensity.

The 1991 census reports information about the highest educational degree attained. Our dependent variable at the individual level is an indicator of whether the person attained at least a high school degree. Taken as a proxy of high school attendance, this measure likely underestimates the effect of the exemption on attendance. For example, an individual who chose to spend an additional year in high school because of the exemption but did not manage to complete it is indistinguishable from those who did not modify an initial decision to drop out.

A problem with 1991 census is that it does not provide any individual control as of 1980, and it cannot be matched with previous census waves to infer information on background characteristics at the individual level (parental education, family size, etc.). We therefore focus on gender-group averages and look at the effects of males' high school graduation rate on that of females belonging to the same cohort and living in the same town. Formally, we express our basic equation (1) in terms of females' high school graduation rates at the group level:

$$\bar{y}_k^F = \alpha + \beta \bar{x}_k^F + \delta \bar{y}_k^M + u_k^F \tag{3}$$

Equation (3) thus becomes our baseline econometric model. From a theoretical point of view, lack of controls $\bar{x}_k^F$ is not a problem for the consistency of our estimates due to the orthogonality of

concerns this subpopulation which would be closer to the one in our sample.

our instrument with all other variables potentially determining girls' probability of completing high school ($\bar{y}_k^F$). Nevertheless, the precision of the estimates can be considerably increased widening the information set.

In the next section we present preliminary evidence on the effects of the exemption and on the existence of social interactions. Next, in section (7) we extend the empirical analysis in several respects.

6 Preliminary evidence.

We now go back to the original question: did males use the exemption from CMS to complete high school?

The upper panel of figure (6) provides a first answer and adds arguments to the reliability of the comparison between the two groups of municipalities. On the vertical axis we report, for each cohort, the share of males who completed high school in the two groups of municipalities; on the horizontal axis there is age in 1991. For cohorts not involved in the exemption (older than 29) our measure of schooling appears to be the same in both treated and control towns. The share of high school graduates rises steadily from about 20 percent up to 35 percent for those aged between 50 and 35 in 1991. The rise comes to a halt for people aged between 30 and 35. The two time series diverge clearly for the cohorts affected by the exemption (aged 26-28). While young males in the control municipalities display more or less the same achievement as older cohorts, those in the exempted towns show a strong increase in high school graduation rates, from about 34 to 38 percent. Turning to high school graduation rates of girls we find a pattern that closely tracks that for males. Apart from a stronger trend, this picture tells more or less the same story. In the non-treated cohorts, females in treated and control municipalities graduate from high school at the same rate. But a larger share of of girls aged 26 to 28 in 1991 graduated from high school in towns where males were exempted from military service.

A first simple quantitative assessment of the effects of the exemption on male and female graduation rates comes from comparing average educational attainments of the relevant cohorts across treated and control municipalities (table (7)).

Let T be the set of towns j where males were exempted from military service and C be the set of cohorts c (as of 1991) to which the exemption was targeted, $C = \{26, 27, 28\}$; y^s_{cj} with $s = \{m, f\}$ is the share of females ($s = f$) or males ($s = m$) in cohort c of town j with at least a high school degree. Under our identifying assumptions the quantities[32]:

$$\begin{aligned}\beta_M &= E(y^M_{cj}|j \in T, c \in C) - E(y^M_{cj}|j \notin T, c \in C)\\ \beta_F &= E(y^F_{cj}|j \in T, c \in C) - E(y^F_{cj}|j \notin T, c \in C)\end{aligned}$$

are a consistent estimate of the causal effect of the military exemption on males' and females' education.

Table (8) reports the results: boys' high school completion rates increased by over 2 percentage points and girls' slightly more, around 2.7 points. Even in this very essential version of the econometric model, both effects turn out to be statistically significant at customary confidence levels. In absolute terms, they imply an increase of about half a year in the average schooling of these groups.

The ratio of these two quantities $\hat{\delta} = \frac{\beta_F}{\beta_M}$ is the IV estimate of the causal effect of males' graduation rates on females'. An implicit assumption underlying the model laid out in equation (3) is that this causal effect is homogeneous across units. Therefore, if the assumption holds, $\hat{\delta}$ can be interpreted as the average treatment effect, meaning that increasing by 1 percentage point the graduation rates of males of a randomly selected group would increase those of females belonging to the same group by 1.2 percentage points. This causal interpretation still holds if the effects are heterogeneous provided they are uncorrelated with the share of males graduating from high school in each group (Card (2001)).

In contrast, if the homogeneity assumption does not hold, we can still interpret $\hat{\delta}$ as the local average treatment effect since we think the required set of assumptions is satisfied (Angrist et al.

[32]These are usually referred to as intention-to-treat effects (Angrist, Imbens and Rubin (1996)).

(1996)). First, we have shown that assignment to treatment is random: the exemption is granted because of the earthquake. Second, we believe it is reasonable to assume that the exemption does not directly affect females schooling choices (exclusion restriction); moreover, our sample design excludes the possibility of related shocks of which exemption might be a proxy. Third, we find a significant and positive effect of the exemption on males high school graduation rates (nonzero average causal effect of the instrument on the treatment). Fourth, we have argued that the exemption either increases or leaves unaffected males schooling choices (monotonicity). Angrist and Imbens (1995) show that for multivalued treatments the monotonicity assumption has the testable implication that the cumulative distribution functions (cdf) of the treatment intensities under the two assignments should not cross. Figure (8) displays the cdf of the graduation rates of males (our treatment intensities) belonging to cohorts exempted by CMS in the treatment and control towns along with those of older cohorts. The evidence is consistent with the required monotonicity of treatment. The last assumption to be made is that females education in a given group does not react to the treatment status of other groups, that is neighboring towns or cohorts (stable unit treatment value). While the evidence we have provided about interactions among teenagers suggests it is a valid assumption as far as other towns are concerned, it might be more questionable as concerns the treatment status of neighboring cohorts.

To sum up, the results shown in table (8) are suggestive of the presence of an interaction effect. However, they do not exploit all the information available and this can be detrimental to the precision of the estimates. In the next section we address this problem. Before turning to that, let us provide some additional elements in favor of our research design.

Our main identification assumption is that the observed change in females' high school graduation rates is the reaction to that recorded among males because of the exemption from military service. We have asserted that, by virtue of our sample design, any other common shock, such as income transfers to households in the earthquake area, is ruled out. To provide additional evidence on this crucial point, in figure (7) we show the university graduation rates in the two groups of towns for all core age cohorts. The figure tells us three things. First, if there were income transfer or other financial intervention targeted to the household level it would have provided additional resources also to people of college age at the time of the earthquake; therefore, we would expect to observe a significant change in their graduation rates from college. Yet for the older cohorts there is no such difference[33]. We take this as an additional argument in favor of the fact that the only shock affecting the cohorts targeted by the military exemption was the exemption itself. Second, the figure shows that, if anything, college graduation rates of older individuals living in treated towns are lower. Third, consistently with the fact that higher graduation rates from high school in the cohorts 26-28 in 1991 were due to marginal people obtaining a degree, we see no difference in their subsequent college graduation rates.

A way to lend further credibility to our research design is to compare the high school graduation rates in our treated towns with those in towns we left out; that is, towns closer to the epicenter. The latter enjoyed the exemption but also suffered the direct effects of the quake. To this purpose we selected a second inner ring of towns (those neighbouring with our treated towns) and compared their high school graduation rates with those recorded in treated towns (figure (9)). The upper panel of figure (10) shows these differences (circles) and their averages in older and younger cohorts (full lines) for males and females. The difference in graduation rates for cohorts aged 30-35 in 1991 is about 2 percentage points (1.9 for males, 2.3 for females). Consistent with our story, among younger cohorts graduation rates turn out to be higher in our treated towns than in those closer to the epicenter, the average difference increasing to more than 3 percentage points (3.3 for males, 3.6 for females). Additionally, to check that there was no substantial damage in the area of interest, we compared outcomes in our control towns with those in the outer ring of neighbouring towns. Differences across

[33] Regressions (not reported) controlling for town fixed effects and age patterns do not signal significant differences between the two groups.

these two groups of towns, reported in the lower panel of figure (10), are stable among younger and older cohorts as well as among males and females: for boys the difference is 4.5 percentage points in older cohorts and 4.4 in younger ones (for females 5.3 and 4.7 points, respectively).

7 Empirical analysis.

The previous sections have detailed the aims and the features of our econometric exercise and presented some preliminary evidence. Let us briefly summarize. We want to assess the existence and strength of social interactions among teenagers as concerns schooling outcomes. We do this in two steps. First, we show that the exemption from compulsory military service granted to young males living in the area hit by the 1980 earthquake in southern Italy determined an increase in their high school completion rates. Second, we explore the change in high school graduation rates of girls in the same cohort and born in the same town. The sample design guarantees that any direct effect of the earthquake that might alter both males' and females' schooling outcomes, thereby leading to biased results, is appropriately controlled for. A simple non parametric exercise has shown that males' and females' high school graduation rates did significantly increase because of the exemption from military service. The causal effect of males' education on females' turns out to be quite strong but imprecisely estimated.

To gain precision, we now exploit all the relevant information available. First, we want to account for systematic differences across towns (school endowments, income, structural differences, etc.). We do this by allowing for town-specific fixed effects, estimated by including in the sample also older cohorts, aged 19-24 in 1980 and therefore not affected by the exemption. Second, we include a set of time dummies to control for the secular increase in education displayed by our data. Therefore the basic specification of equation (3) is:

$$y_{cj}^{F} = \delta y_{cj}^{M} + \sum_{a=26}^{35} \nu_a^F A_a + \sum_{t=1}^{117} \eta_t^F G_t + \epsilon_{cj}^F \tag{4}$$

where c is an index for the cohort and j for the town, y^s_{cj} ($s = \{F, M\}$) is the share of females (males) in the group with at least high school education, A_a and G_t are, respectively, age and town dummies and ϵ^F_{cj} is the error term $E(\epsilon^F_{cj}) = 0$. We instrument males' schooling with a dummy R_{cj} equal to one when town j is in the earthquake area and cohort c has been granted the exemption and zero otherwise[34]. By virtue of our sample design, $E(R_{cj}\epsilon^F_{cj}) = 0$. Therefore the auxiliary regression is:

$$y^M_{cj} = \beta_M R_{cj} + \sum_{a=26}^{35} \nu^M_a A_a + \sum_{t=1}^{117} \eta^M_t G_t + \epsilon^M_{cj} \tag{5}$$

with $E(R_{cj}\epsilon^M_{cj}) = 0$. The next subsection presents the results on the effect of the exemption on males' schooling achievements. We then turn to the effect of this change on females.

7.1 The first stage: the effect of the exemption on males' schooling.

The first column of panel A in table (9) reports the results for the basic specification (5). The share of males achieving at least a high school degree in each town-cohort is projected on a dummy R_{cj} equal to one if the cohort has been exempted from military service, on a set of town dummies and on a set of age dummies. The sample includes cohorts aged 26 to 28 as of 1991, exempted from service in the towns hit by the earthquake, and cohorts aged 30 to 35 in 1991, not affected by the exemption in either group of towns[35]. The effect of the exemption was to increase high school graduation rates by 2 percentage points, very much the same effect we found in table (7) from the most basic econometric exercise but with a notable increase in the precision of the estimate.

The above specification only controls for systematic differences across towns and common to all cohorts within a town. This specification might leave unexplained part of the variance, if the time series patterns of schooling achievements were heterogeneous across towns. To tackle this problem we partition the sample into subsets of towns, each including both control and treated municipalities, and

[34]All regressions are weighted to account for the heteroskedasticity generated by the fact that the dependent variable is a mean.

[35]The estimation sample does not include the cohort aged 29 as of 1991 (18 in 1980). Their age when the relevant laws was issued is such that it is hard to make assumptions on whether and how they were affected by those interventions.

allow for subset-specific time trends[36]. Results for a partition into five groups defined on the basis of geographical proximity are reported in the second column of the upper panel of table (9). Allowing for this more flexible specification does not alter the result: we still find a highly statistically significant increase of about 2 percentage points in high school graduation rates.

In principle one would like to control also for those town-specific time-varying characteristics that are likely to determine changes over time in schooling achievements, such as changes in parental income, in education, in employment status, in school endowments and the like. Unfortunately, the structure of our data makes it impossible to recover individual controls as of the early eighties so as to build these measures at the group level. However, we believe that due to the limited age interval spanned by the sample, only 9 years, and the reasonably slow evolution of these characteristics most of the relevant variability is cross-sectional and is thus captured by town fixed effects.

One potential source of heterogeneity across cohorts in the same town that we can control for is cohort size. This is likely to play a relevant role in individual schooling achievement. Card and Lemieux (2000) find that larger cohorts have lower educational attainment, possibly because of supply effects: larger cohorts may generate congestion effects or changes in the resources available for any single individual. For example, a marginal increase in cohort size may lead a school to hire an additional teacher, which might lower class size and thus affect individual performances (Angrist and Lavy (1999), Krueger (1999), Krueger and Whitmore (2001)).

We therefore extend the basic specification to allow for a second order polynomial in cohort size. The results are presented in the third column of panel A in table (9). The previous findings turn out to be extremely stable to this extension[37]; we still find an increase of about 2 percentage points in males' graduation rates.

The sample design we described in section (4) was motivated by the fact that we needed to make

[36]Using time dummies to capture the evolution of the dependent variable, we cannot at the same time identify town-specific age patterns and the effect of the exemption.

[37]We also experimented with polynomials of higher order and the results were basically unaffected.

sure that any direct effect of the earthquake on individual high school graduation rates had been taken care of so that it would not distort our results. We selected as treated towns only those lying farther away from the center of the quake and as controls only the neighbouring ones not included in the official list of affected towns. We showed that treated towns were not substantially affected by the earthquake and that geographical proximity to control towns guarantees that any residual direct effect of the earthquake is accounted for. However, one could argue that although the direct effect of the earthquake may be reasonably assumed to be the same, only the officially listed municipalities qualified for government intervention. This intervention was linked to the official damage index, and we have shown that treated municipalities were generally at the low end of the damage scale, thus qualifying only for limited help. As a robustness check we further limited the sample only to those towns that qualified for the military exemption although they did not record any damage at all, and towns that were ranked at the lowest damage level by the Ministry of Internal Affairs (their damage score was 6). This leaves us with 31 treated towns; as a control group we retained only those towns neighbouring on one of the selected treated ones (41 towns). This selection shrinks the sample to 72 towns. Figure (11) shows their location. A comparison with figure (3) reveals that in this restricted sample southern and eastern towns are overrepresented with respect to the original sample while most northern ones and several ones in the southeastern part of the area drop out. Column (4) of table (9) reports the results for a specification that controls for town-specific fixed effects, three group-specific sets of age dummies, and a second order polynomial of cohort size estimated on this restricted sample. The effect of the military exemption on males' schooling is now stronger: the increase in graduation rates is now about 3.7 percentage points. This result suggests that the overall sample may still be affected by some residual direct effects of the earthquake that reduce school achievements. Note that this negative effect is consistent with the evidence shown in figure (10), where inner municipalities that suffered more serious damage recorded lower graduation rates for the cohorts still in school at the time of the quake.

So far, our results have borne on the effect of the military exemption on the share of people in the group having earned a high school degree. This measure does not take the sequential nature of schooling achievements fully into account. That is, a necessary condition for a high school degree is completion of all lower educational levels (primary and junior high school). Although in Italy there is compulsory schooling until age 15, so that everybody should get the junior high school degree, completion rates are not actually 100 percent. Figure (12) shows kernel density estimates of the distribution of the share of people in a group with a junior high school degree. The underlying groups are those aged 30 to 35 in 1991 thus unaffected by the military exemption. We learn two things. First, there is considerable heterogeneity in junior high school graduation rates. Second, these rates appear to be higher in treated towns. This means that the population that can potentially graduate from high school is relatively larger in treated towns. This might be due to many factors (regional school policies, heterogeneous school endowments, different social backgrounds, etc.). Most of them are not likely to vary much over time, so that controlling for town fixed effects takes care of them. Other factors may vary over time and differently affect subsequent cohorts within a given town in ways our set of age dummies cannot capture. To account for these sources of dynamic heterogeneity across municipalities and possibly correlated composition effects within each town, we have rerun all our regressions on a slightly different measure of outcome. We repeated the analyzes on the conditional high school graduation rate, namely the share of males holding at least a high school degree over the population of males who had at least completed junior high school. Results for all the specifications discussed above are reported in the lower panel of table (9). All the previous findings are confirmed with point estimates, which although slightly higher are statistically indistinguishable from the previous ones.

Overall, these results indicate that on average in the 117 treated towns about 400 male teenagers completed high school because of the exemption from CMS who would otherwise have dropped out.

We thus contribute to the literature by stressing that, apart from service and the implied loss of labor market experience (Angrist (1990), Imbens and van der Klaauw (1995), etc.), the existence of the compulsory military draft may have an effect on subsequent earnings because of lower educational attainments. To our knowledge this is a novel finding, and provides a new instrument for education.

7.2 Social interactions: the effect of the exemption on females' schooling.

We have shown that the exemption from CMS granted in the aftermath of the earthquake determined an exogenous increase in males' schooling. We have also provided evidence that this increase was due to the military exemption and not to the earthquake itself. We can then safely interpret any change in females' schooling as the causal effect of males' schooling achievements.

Table (10) reports the results of the IV estimates of equation (4). As previously, in the upper panel the dependent variable is the share of females in the group with at least a high school degree; in the lower panel we look at conditional graduation rates.

Overall, the results point to a significant causal effect of boys' schooling on girls'. As we move towards richer specifications, the precision of the estimate increases considerably while the point estimate is substantially stable. It is reassuring that the estimated effect reported in column (4), where the underlying sample is limited to towns that suffered no damage whatsoever, is virtually the same as those estimated on the full sample of municipalities. In this specification, robust to potential residual effects of other interventions correlated with the exemption from CMS, an increase of 1 percentage point in males' high school graduation rate raises that of females by about 0.7 points.

We can evaluate the impact of the social interaction effect in terms of per capita GDP. Recent OECD estimates find an elasticity of steady-state per capita GDP to average years of education of about 0.6 (OECD (2003)). On the basis of this figure, a permanent increase of 1 percentage point in male high-school graduation rates would permanently raise per capita GDP by 0.27 percentage points.

Overlooking the social interaction effect would lead to an underestimate of 0.11 percentage points for the overall increase in per capita GDP.

Strictly speaking, our data do not allow us to say much more. Moreover, one may question whether the meaning of the parameter we have identified hinges on the specific type of interaction we modeled. Our econometric model only allows for males to exert an effect on females, and is agnostic about the converse. A natural first extension of the model would be to allow for this reverse effect. As an example, consider the following empirical model, where for simplicity we have neglected covariates other than those of interest.

$$\begin{aligned} y_{ik}^F &= \alpha_F + \delta_F \bar{y}_k^M + u_{ik}^F \\ y_{ik}^M &= \alpha_M + \delta_M \bar{y}_k^F + \rho R_{ik} + u_{ik}^M \end{aligned} \tag{6}$$

where R_{ik} is the dummy for the military exemption and δ_F (δ_M) captures the causal effect of males' (females') graduation rates on those of females (males). The corresponding reduced forms for males' and females' high school graduation rates would then be:

$$\begin{aligned} \bar{y}_k^F &= a_F + \frac{\delta_F}{1-\delta_F\delta_M}\rho R_k + \xi_k^F \\ \bar{y}_k^M &= a_M + \frac{1}{1-\delta_F\delta_M}\rho R_k + \xi_k^M \end{aligned} \tag{7}$$

Thus, even in the presence of a feedback effect from females to males, our identification strategy consistently estimates the parameter δ_F, the causal effect of boys' graduation rates on girls'. It is clear that with our data we cannot say anything about δ_M. Yet if we are willing to assume that this peer effect is not gender-specific, meaning $\delta_F = \delta_M$, then the estimates presented in table (10) allow us to recover the social multiplier, namely the overall effect of an initial exogenous increase of 1 percentage point in males' graduation rates: male graduation rates will eventually increase by $\frac{1}{1-\delta^2}$ points and females rates by $\frac{\delta}{1-\delta^2}$ points. In our most preferred specification ($\hat{\delta} = 0.68$), this implies an increase, respectively, of 1.9 percentage points for males and 1.3 for females. If the initial increase in the male graduation rate were permanent, it would eventually determine a permanent increase of 0.5 percentage points in per capita GDP. We can move one step forward by noting that in model (6) no causal effect is allowed to work among males and among females. A more general formulation, then, would be one where, beyond not being gender-specific, this effect also influences people of the same sex with the same strength. This makes a total of three identifying assumptions. Formally:

$$y_{ik} = \alpha + \delta \bar{y}_k + \rho R_{ik} + u_{ik} \tag{8}$$

where now $R_{ik} = 1$ if i is male and exempted and $R_{ik} = 0$ otherwise. To map the above into our previous formulations, recall that the group mean $\bar{y}_k = m_k \bar{y}_k^M + (1 - m_k)\bar{y}_k^F$, where m_k is the share of males in group k[38]. Then the reduced-form equations become:

$$\begin{aligned} \bar{y}_k^F &= a + \frac{\delta}{1-\delta} m_k \rho R_k + \nu_k^F \\ \bar{y}_k^M &= a + \frac{1-\delta(1-m_k)}{1-\delta} \rho R_k + \nu_k^M \end{aligned} \tag{9}$$

Under these assumptions our estimated parameter identifies the overall effect of graduation rates on individual graduation probability. The implication is that an exogenous increase of 1 percentage point in the male graduation rate brings about, through the multiplier effect, an increase of 1.1 points in females' graduation probabilities and of 2.1 points in those of males. In terms of per capita GDP, this would yield a permanent overall increase of 0.5 percentage points.

8 Concluding remarks.

The existence of social effects in education, that is the causal effect exerted by the school achievement of some individuals on those of neighbouring people, is a widely investigated issue that has crucial implications. First, quantifying the implied social multiplier contributes to a correct assessment of

[38] Here we assume that $m_k = (1 - m_k) = 0.5$.

the true opportunity cost of resources devoted to schooling. Second, the shape, strength and source of these social effects are important elements in the design of efficient school systems.

The major challenge in measuring social interactions is to find an exogenous factor shifting the behavior of some individuals in a given group so that social effects can be identified by looking at how other members, unaffected by the shift-factor, behave.

In our exercise this exogenous shifter is the exemption from compulsory military service granted to all males born before 1966 in three regions of southern Italy (Campania, Basilicata and part of Puglia), after the 1980 earthquake. Since in Italy women are not required to do military service, we are sure that the exemption directly affects only males' outcomes, and we can use it to estimate the causal effect of male high school graduation rates on female graduation rates. A major concern is that the exemption is clearly correlated with the earthquake, a shock common to males and females. We control for this possibility through an appropriate research design. Our instrument, while changing males' behavior, does not alter the composition of underlying groups, so that we can confidently interpret our results as the response of girls to the behavior rather than the characteristics of boys. This is important since it is often easier for a policy maker to affect people's behaviors rather than their exogenous characteristics.

Our findings suggest a strong effect of compulsory military service on high school graduation rates. The first-stage results show that the exemption from CMS increased the share of males achieving a high school degree by 3.7 percentage points. We also detected a strong influence of males on females: an increase of 1 percentage point in the male graduation rate raises that of females by about 0.7 percentage points. The sum of those two effects, if permanent, would imply an increase in steady-state per capita GDP of 0.27 percentage points; neglecting the social multiplier of schooling, we would estimate an increase of only 0.16 percentage points. This evaluation overlooks other forms of social returns to education recently identified by the literature. In turn, these are magnified by our results:

a given exogenous increase in education not only improves, say, the health status of those directly treated but also the education of neighboring people who will, in turn, be healthier.

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Table 1: A summary of the timing of the exemption from CMS.

Year of birth	Exempted at age	Law	Age at 1991 Census
1961 and earlier	21 or more	April 1982, No. 187	30 or more
1962	20	April 1982, No. 187	29
1963	19	April 1982, No. 187	28
1964	19/20	April 1984, No. 80	27
1965	19	April 1984, No. 80	26

Source: Gazzetta Ufficiale - April 30th 1982 (No. 118), April 19th 1984 (No. 110).

Table 2: The official assessment of damage.

Damage index	Number of towns	% damaged houses	Homeless (1000 inh.)
0	18	0	0
6	15	0.07	1.1
7	5	0	21.6
8	4	1.7	24.7
9	4	0.3	39.5
10	2	1	45.5
11	2	2	13
12	2	6.5	58.5
13	2	6	73.5
14	3	9.7	35.7

Source: Ministero del Bilancio e della Programmazione Economica (1981).

Table 3: Structural characteristics, 1979.

	Control	Difference
Population density	1.84 (0.1136)	-0.90 (0.2025)
Extension (ha)	29055.4 (2052.3)	-12825.7 (3656.2)
Altitude	257.5 (22.7)	150.82 (40.4)
Births/population	1.63 (0.0327)	-0.17 (0.0583)
Birth rate	3.24 (0.07)	-0.37 (0.1158)
Death rate	0.80 (0.0242)	0.10 (0.0432)
Inflow rate	1.96 (0.1088)	-0.26 (0.1939)
Inflow rate from abroad	0.18 (0.0235)	0.00 (0.0419)
Outflow rate	2.10 (0.0872)	0.22 (0.1553)
Outflow rate abroad	0.15 (0.026)	0.01 (0.0463)
Population (6-14)/Population	15.05 (0.3436)	-1.01 (0.6122)

Source: Comuni, Comunità Montane, Regioni Agrarie al 31 Dicembre 1988 (Istat (1990)), Popolazione e Movimento Anagrafico dei Comuni al 31 Dicembre 1979 (Istat (1980b)).
Difference is the average difference of a characteristic betwen treated and control towns. Standard errors in parentheses.

Table 4: School endowments, 1979.

	Control	Difference
Primary school (1st-5th grade):		
Students/Population	9.45	-1.05
	(0.2012)	(0.3585)
Students/Rooms	25.39	-8.85
	(0.7873)	(1.4026)
Junior high school (6th-8th grade):		
Students/Population	5.60	0.04
	(0.1639)	(0.2921)
Students/Rooms	22.68	-1.29
	(0.2752)	(0.4905)
High school (9th-13th grade):		
Students/Population	4.93	-0.24
	(0.51)	(0.909)
Students/Rooms	23.66	-1.66
	(0.696)	(1.342)

Source: Annuario Statistico dell'Istruzione, 1979 (Istat (1980a)). Difference is the average difference of a given characteristic betwen treated and control towns. Standard errors in parentheses.

Table 5: Characteristics of the population by age class, males.

Age class	Years of schooling		Employment rate		White collar (share)		Number of children		Percentage of population	
	Control	Difference	Control	Difference	Control	Difference	Control	Difference	Control	Difference
15-19	7.4 (0.091)	0.3 (0.142)	0.20 (0.021)	-0.02 (0.034)	0.00 (0.004)	0.00 (0.006)	0.6 (0.113)	0.0 (0.213)	9.8 (0.127)	-0.5 (0.199)
20-24	8.9 (0.116)	0.4 (0.180)	0.42 (0.026)	0.02 (0.041)	0.05 (0.010)	0.01 (0.016)	0.7 (0.059)	-0.2 (0.098)	8.1 (0.077)	0.0 (0.138)
25-29	8.9 (0.124)	0.2 (0.198)	0.72 (0.024)	0.00 (0.039)	0.15 (0.015)	0.00 (0.023)	1.1 (0.057)	-0.2 (0.093)	7.1 (0.079)	-0.4 (0.129)
30-34	8.3 (0.131)	0.0 (0.212)	0.87 (0.020)	-0.01 (0.032)	0.23 (0.017)	-0.02 (0.026)	1.8 (0.060)	-0.2 (0.094)	7.1 (0.101)	-0.4 (0.160)
35-39	7.5 (0.146)	-0.2 (0.237)	0.89 (0.020)	-0.01 (0.032)	0.23 (0.018)	-0.02 (0.028)	2.4 (0.063)	-0.2 (0.099)	5.7 (0.075)	-0.5 (0.124)
40-44	6.7 (0.145)	-0.3 (0.230)	0.88 (0.020)	-0.02 (0.032)	0.2 (0.017)	-0.03 (0.026)	2.7 (0.068)	-0.2 (0.104)	5.7 (0.075)	0.2 (0.125)
45-49	5.8 (0.146)	-0.4 (0.227)	0.84 (0.023)	-0.02 (0.035)	0.15 (0.015)	-0.03 (0.023)	2.7 (0.073)	-0.3 (0.109)	5.6 (0.099)	0.6 (0.143)
50-54	5.6 (0.153)	-0.5 (0.233)	0.78 (0.027)	-0.03 (0.041)	0.14 (0.015)	-0.04 (0.022)	2.3 (0.076)	-0.2 (0.114)	5.6 (0.094)	0.7 (0.145)
55-59	5.5 (0.160)	-0.6 (0.244)	0.63 (0.031)	-0.06 (0.048)	0.13 (0.015)	-0.04 (0.022)	1.6 (0.074)	-0.1 (0.110)	5.2 (0.118)	0.8 (0.199)
60-64	5.2 (0.166)	-0.6 (0.260)	0.31 (0.032)	-0.03 (0.049)	0.06 (0.012)	-0.02 (0.019)	1.1 (0.070)	-0.1 (0.106)	3.6 (0.10)	0.3 (0.151)
65-69	4.4 (0.160)	-0.4 (0.242)	0.09 (0.023)	-0.02 (0.034)	0.01 (0.006)	0.00 (0.009)	0.6 (0.060)	0.0 (0.090)	3.9 (0.161)	0.7 (0.245)
70-74	3.7 (0.158)	-0.4 (0.238)	0.05 (0.018)	-0.01 (0.026)	0.00 (0.004)	0.00 (0.005)	0.4 (0.050)	0.0 (0.075)	3.3 (0.151)	0.7 (0.238)
75-	3.4 (0.160)	-0.4 (0.238)	0.02 (0.012)	-0.01 (0.017)	0.00 (0.003)	0.00 (0.005)	0.3 (0.043)	0.0 (0.065)	3.7 (0.194)	0.9 (0.295)

Source: Censimento (XII) Generale della Popolazione: 25 Ottobre 1981 (Istat (1984)).
Difference is the average difference of a given characteristic betwen treated and control towns. Standard errors in parentheses.

Table 6: Characteristics of the population by age class, females.

Age class	Years of schooling		Employment rate		White collar (share)		Number of children		Percentage of population	
	Control	Difference	Control	Difference	Control	Difference	Control	Difference	Control	Difference
15-19	7.5 (0.101)	0.4 (0.159)	0.07 (0.014)	0.02 (0.023)	0.01 (0.005)	0.00 (0.007)	0.3 (0.098)	-0.1 (0.142)	9.5 (0.115)	-0.7 (0.186)
20-24	8.7 (0.124)	0.5 (0.196)	0.18 (0.021)	0.04 (0.033)	0.07 (0.012)	0.01 (0.020)	0.5 (0.076)	-0.2 (0.114)	8.0 (0.077)	-0.2 (0.140)
25-29	8.3 (0.135)	0.3 (0.219)	0.28 (0.025)	0.08 (0.040)	0.14 (0.016)	0.03 (0.027)	0.7 (0.085)	-0.2 (0.119)	7.0 (0.084)	-0.4 (0.141)
30-34	7.6 (0.150)	0 (0.243)	0.34 (0.027)	0.08 (0.043)	0.17 (0.017)	0.02 (0.028)	1.1 (0.099)	-0.2 (0.152)	7.0 (0.110)	-0.3 (0.180)
35-39	6.5 (0.157)	-0.1 (0.256)	0.34 (0.028)	0.08 (0.044)	0.14 (0.017)	0.01 (0.027)	1.5 (0.115)	-0.2 (0.186)	5.5 (0.073)	-0.3 (0.136)
40-44	5.6 (0.154)	-0.2 (0.245)	0.33 (0.027)	0.08 (0.043)	0.1 (0.015)	0.00 (0.023)	1.7 (0.108)	-0.1 (0.172)	5.8 (0.065)	0.2 (0.119)
45-49	4.7 (0.150)	-0.4 (0.235)	0.32 (0.027)	0.06 (0.042)	0.07 (0.012)	-0.02 (0.018)	1.5 (0.102)	-0.1 (0.158)	5.8 (0.088)	0.6 (0.131)
50-54	4.6 (0.154)	-0.5 (0.236)	0.3 (0.028)	0.06 (0.042)	0.06 (0.012)	-0.01 (0.018)	1.2 (0.090)	-0.2 (0.137)	5.7 (0.101)	0.7 (0.163)
55-59	4.3 (0.155)	-0.6 (0.240)	0.2 (0.024)	0.03 (0.037)	0.04 (0.011)	-0.01 (0.016)	0.7 (0.073)	-0.1 (0.110)	5.5 (0.119)	0.8 (0.197)
60-64	4 (0.152)	-0.6 (0.235)	0.09 (0.019)	0.00 (0.029)	0.02 (0.009)	-0.01 (0.014)	0.5 (0.063)	0.0 (0.097)	3.9 (0.104)	0.4 (0.162)
65-69	3.4 (0.146)	-0.5 (0.226)	0.03 (0.012)	-0.01 (0.018)	0.00 (0.005)	0.00 (0.008)	0.3 (0.049)	0.0 (0.075)	4.2 (0.157)	0.6 (0.248)
70-74	3.1 (0.151)	-0.6 (0.228)	0.01 (0.008)	0.00 (0.012)	0.00 (0.003)	0.00 (0.004)	0.2 (0.041)	0.0 (0.062)	3.8 (0.159)	0.9 (0.244)
75-	2.9 (0.158)	-0.5 (0.238)	0.00 (0.005)	0.00 (0.007)	0.00 (0.001)	0.00 (0.002)	0.2 (0.037)	0.0 (0.057)	5.0 (0.254)	1.1 (0.386)

Source: Censimento (XII) Generale della Popolazione: 25 Ottobre 1981 (Istat (1984)).
DIFFERENCE is the average difference of a given characteristic betwen treated and control towns. Standard errors in parentheses.

Table 7: Average high school graduation rates.

	Males		Females	
	26-28	30-35	26-28	30-35
Treated	36.44	34.14	41.43	34.96
	(11.253)	(10.359)	(12.403)	(11.224)
Control	34.22	34.81	38.77	34.28
	(11.732)	(10.646)	(11.779)	(10.807)

Weighted means; weights are the number of individuals in each group. Standard errors in parentheses.

Table 8: The effects of the military relief on high school graduation.

	Coeff.	SE
Males (β_M)	2.23	1.33
Females (β_F)	2.66	1.39
Interaction effect ($\delta = \frac{\beta_F}{\beta_M}$)	1.19	0.951

Weighted regressions; weights are the number of individuals in each group.

Table 9: First stage: the effect of the military exemption on males' high school graduation rates.

	(1)	(2)	(3)	(4)
A. Dependent variable: high school graduation rate (%).				
Exemption	2.01	2.12	2.17	3.67
	(0.813)	(0.876)	(0.867)	(1.198)
	[0.014]	[0.016]	[0.012]	[0.002]
B. Dependent variable: conditional high school graduation rate (%).				
Exemption	2.17	2.48	2.51	3.97
	(0.926)	(0.996)	(0.989)	(1.423)
	[0.019]	[0.013]	[0.011]	[0.005]
Towns	117	117	117	72

Weighted regressions; weights are the number of males in each group. Standard errors in parentheses; p-values in brackets. All regressions include town specific fixed effects. Regression (1) includes common age dummies; regressions (2) to (4) include group specific age dummies. Regression (3) includes a second order polynomial in cohort size. Regression (4) only includes treated towns who scored 0 or 6 in the official damage assessment and their neighbouring control towns.

Table 10: Social interactions: the effect of the military exemption on females.

	(1)	(2)	(3)	(4)
A. Dependent variable: high school graduation rate (%).				
y_c^M	0.67	0.76	0.77	0.68
	(0.50)	(0.515)	(0.496)	(0.362)
	[0.179]	[0.138]	[0.121]	[0.060]
B. Dependent variable: conditional high school graduation rate (%).				
y_c^M	0.54	0.77	0.74	0.69
	(0.481)	(0.492)	(0.471)	(0.426)
	[0.262]	[0.118]	[0.116]	[0.105]
Towns	117	117	117	72

Weighted regressions; weights are the number of females in each group. Standard errors in parentheses; p-values in brackets. All regressions include town specific fixed effects. Regression (1) includes common age dummies; regressions (2) to (4) include group specific age dummies. Regression (3) includes a second order polynomial in cohort size. Regression (4) only includes treated towns who scored 0 or 6 in the official damage assessment and their neighbouring control towns.

Figure 1: The earthquake.

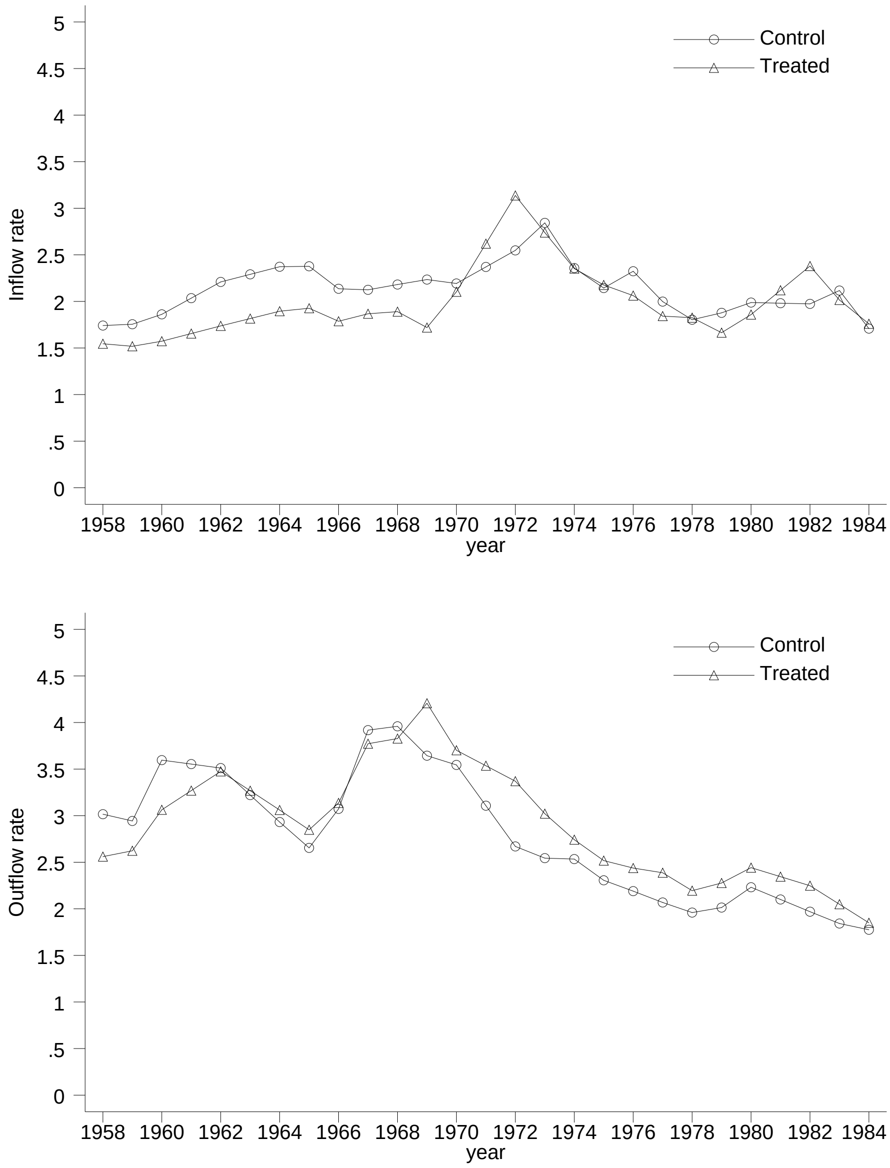
Source: Ministero del Bilancio e della Programmazione Economica (1981).

Figure 2: The area officially targeted by government interventions.

Source: Gazzetta Ufficiale - May 1981, law No. 219.

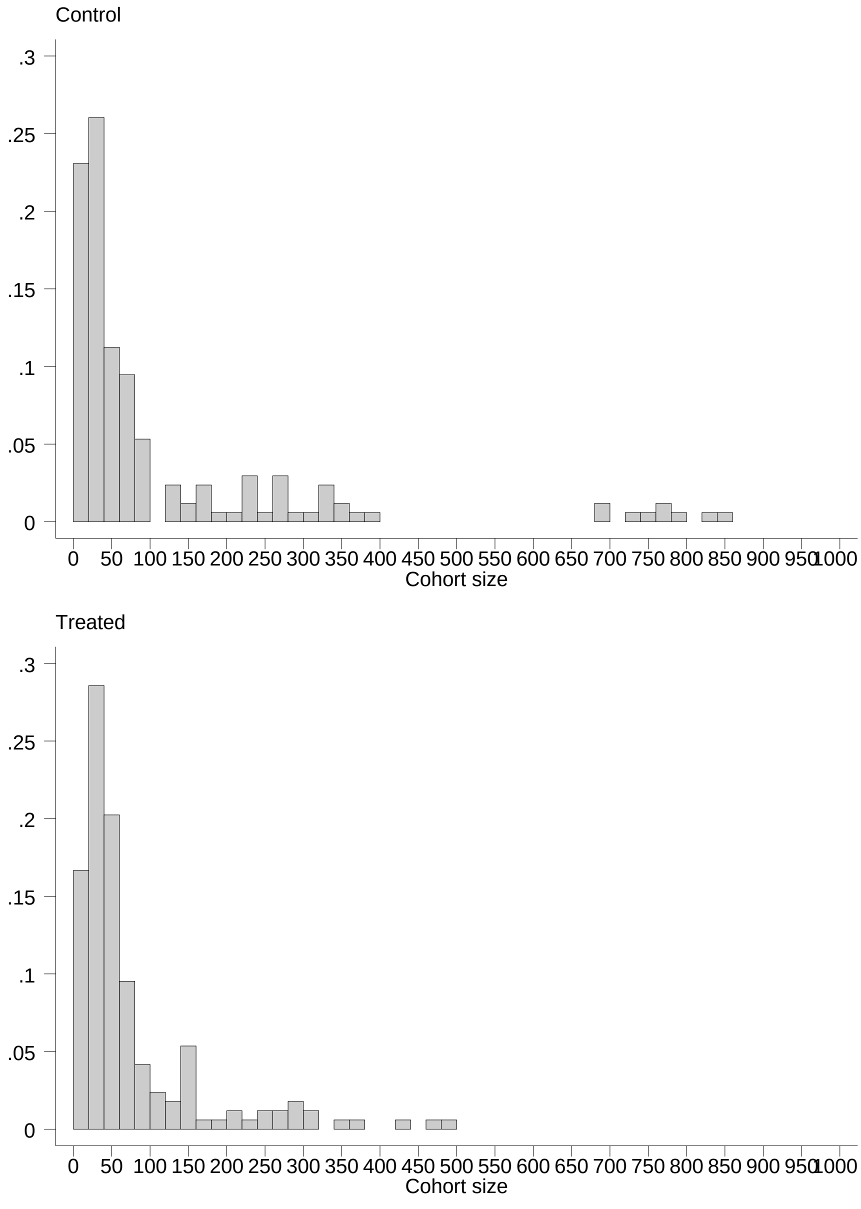
Figure 3: Treatment and control groups.

Figure 4: Mobility patterns.



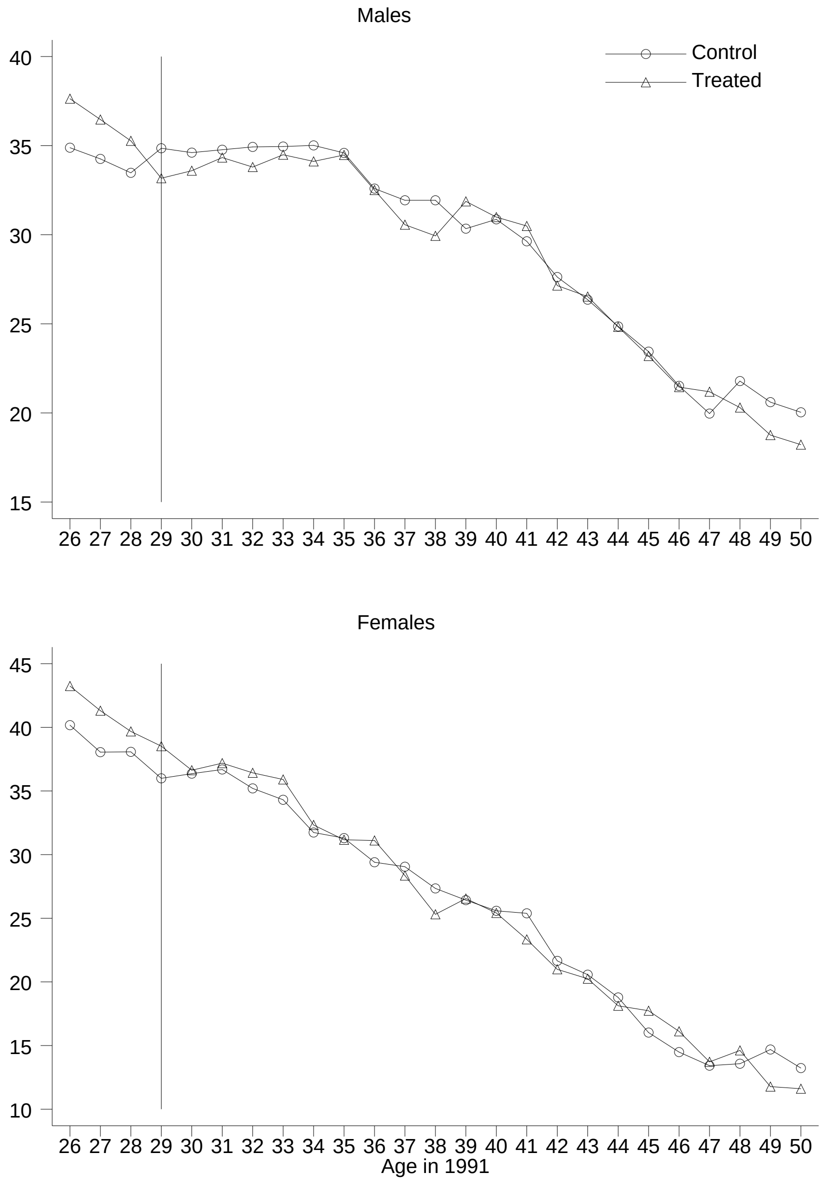
Source: Popolazione e Movimento Anagrafico dei Comuni (Istat (*various years*)).

Figure 5: Distribution of the size of the groups.



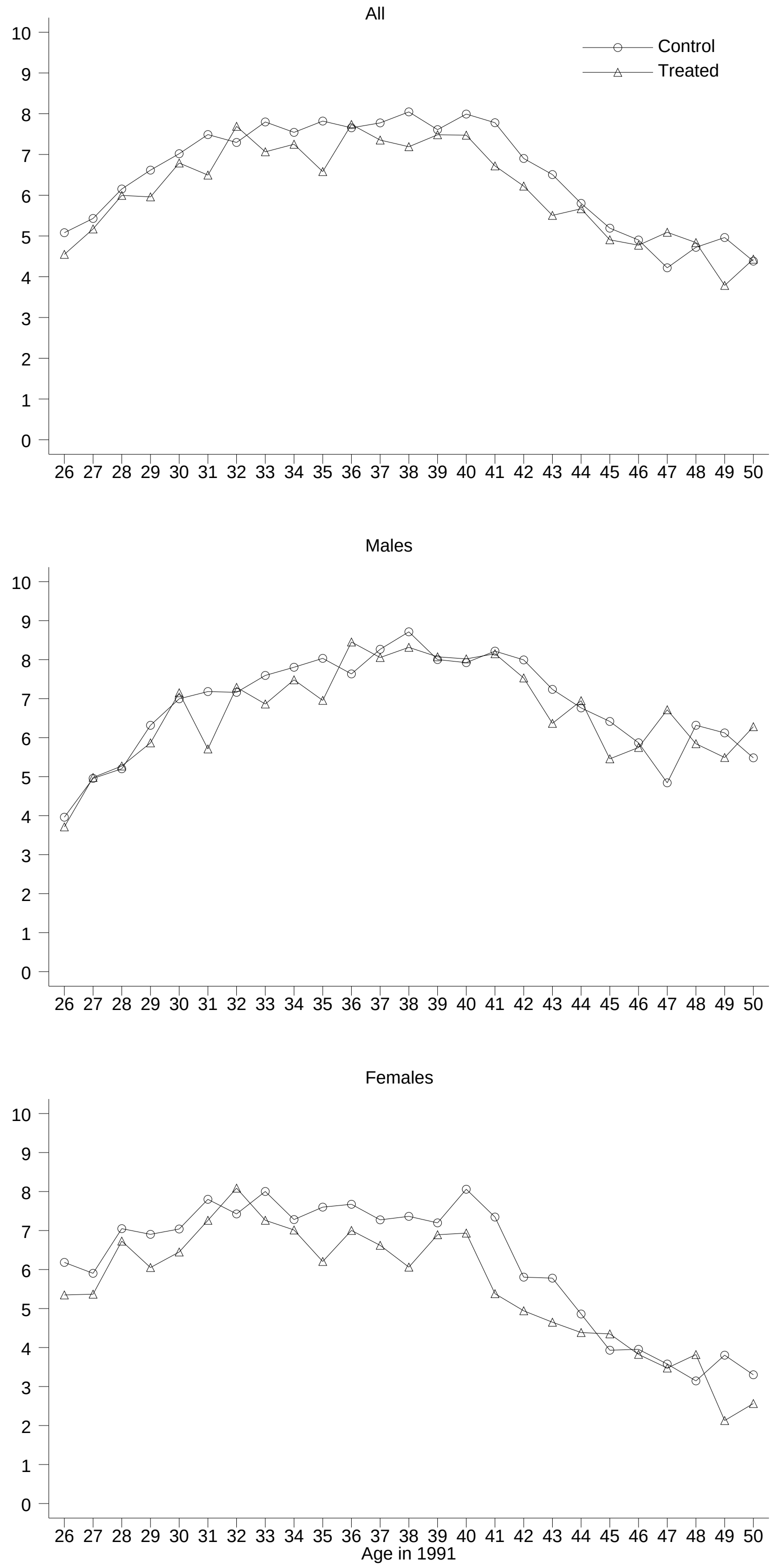
Source: Censimento (XIII) Generale della Popolazione: 26 Ottobre 1991 (Istat (1994)).

Figure 6: Share of cohort with a high school degree.



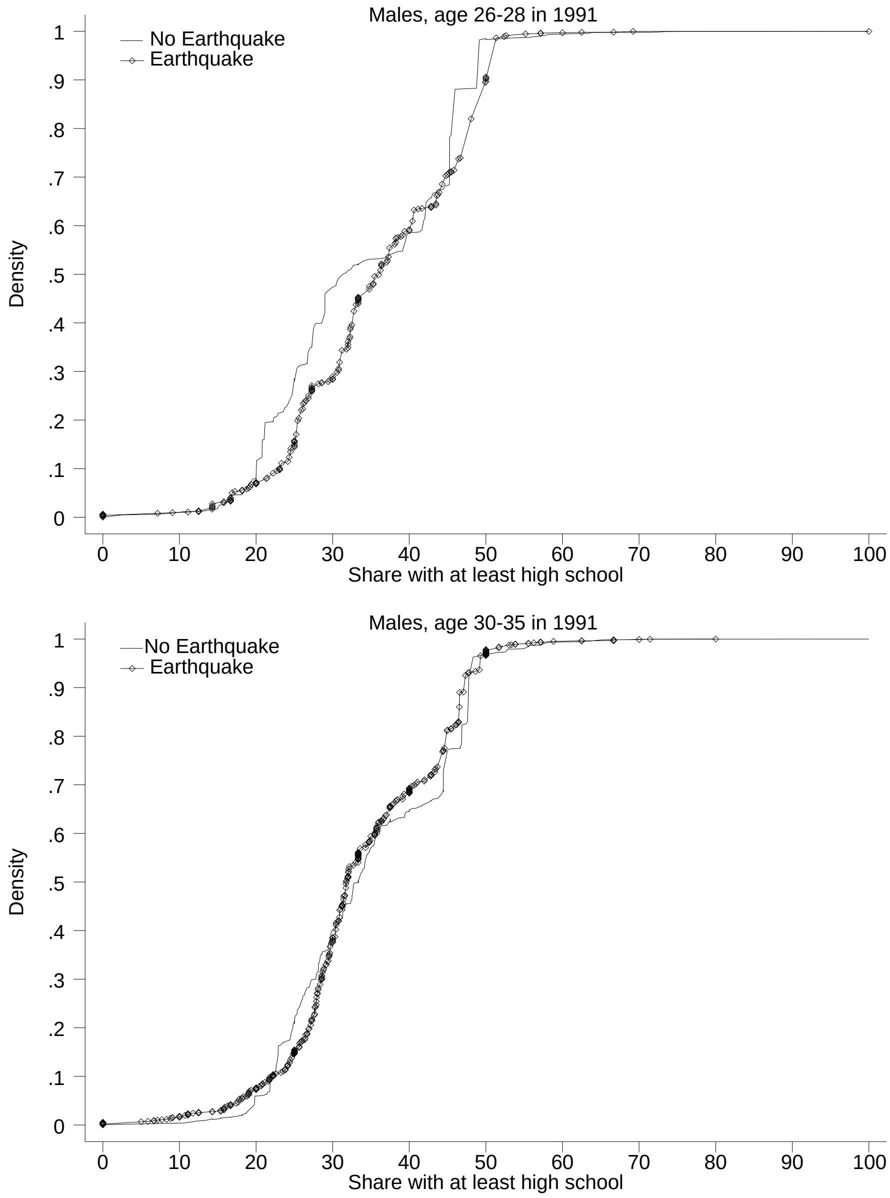
Source: Censimento (XIII) Generale della Popolazione: 26 Ottobre 1991 (Istat (1994)).

Figure 7: Share of cohort with a university degree.



Source: Censimento (XIII) Generale della Popolazione: 26 Ottobre 1991 (Istat (1994)).

Figure 8: Cumulative distribution of the share of cohort with a high school degree.



Source: Censimento (XIII) Generale della Popolazione: 26 Ottobre 1991 (Istat (1994)).

Figure 9: Outer and inner rings of towns.

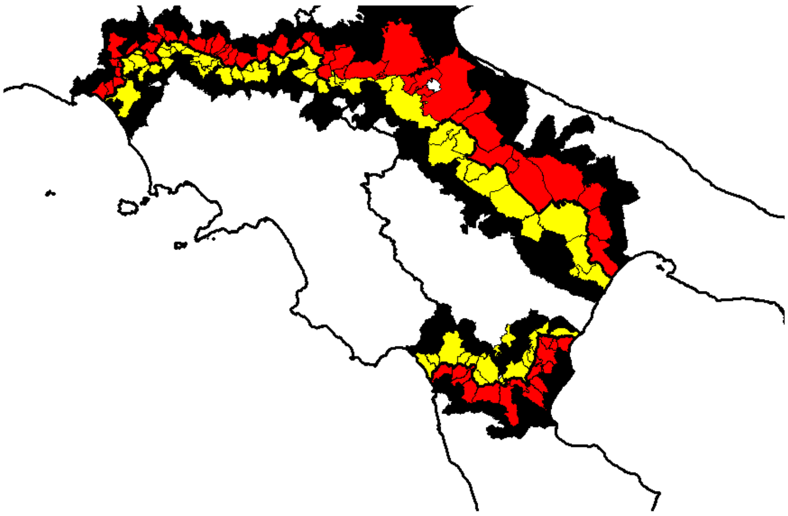


Figure 10: Comparisons with inner and outer rings.

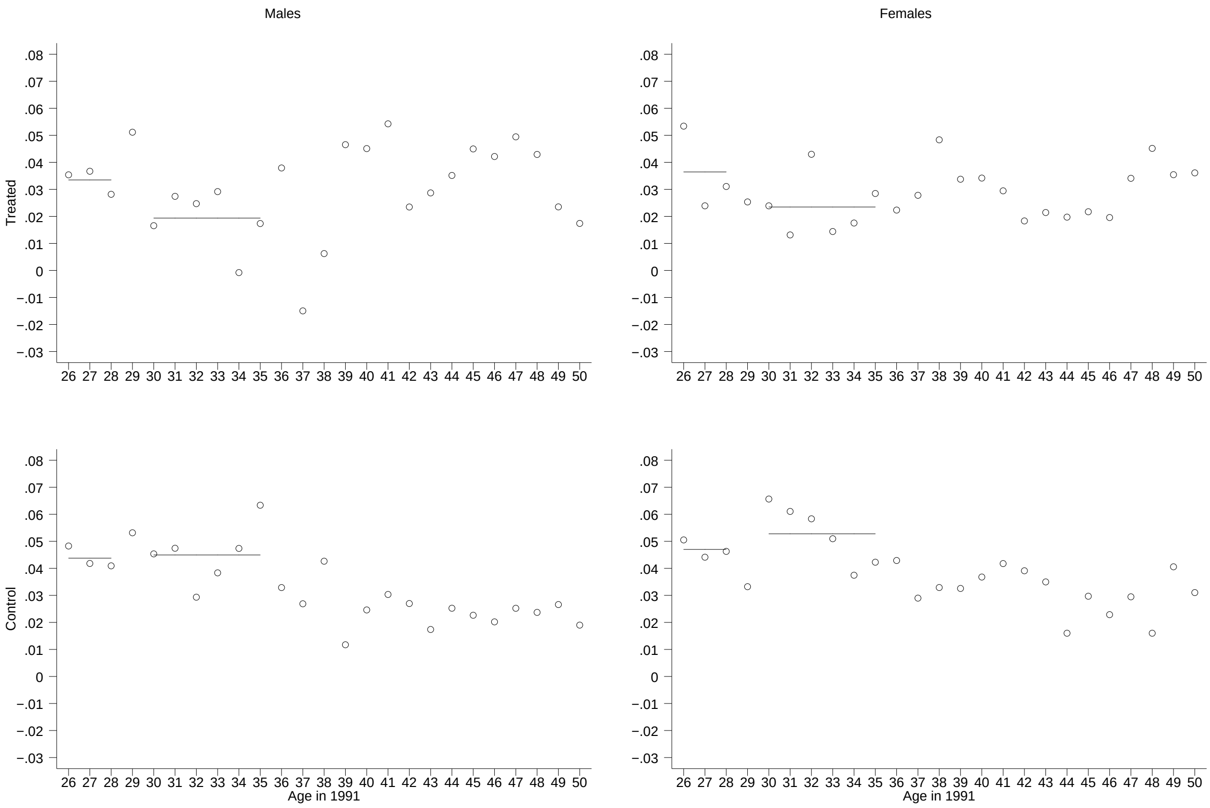


Figure 11: Restricted sample.

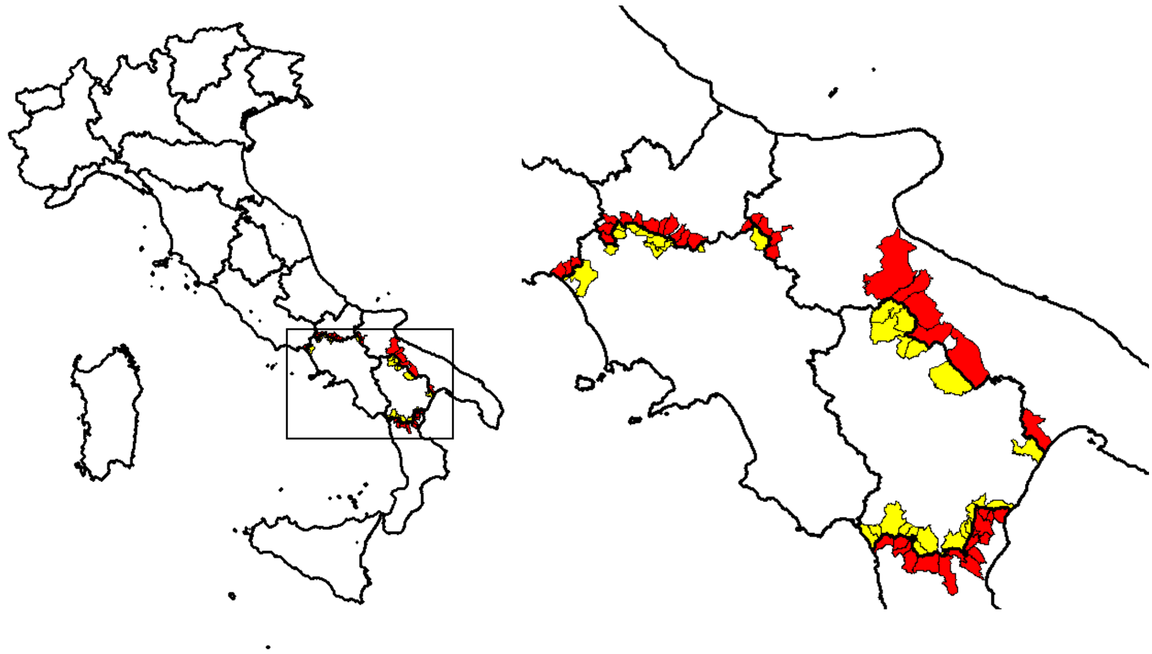
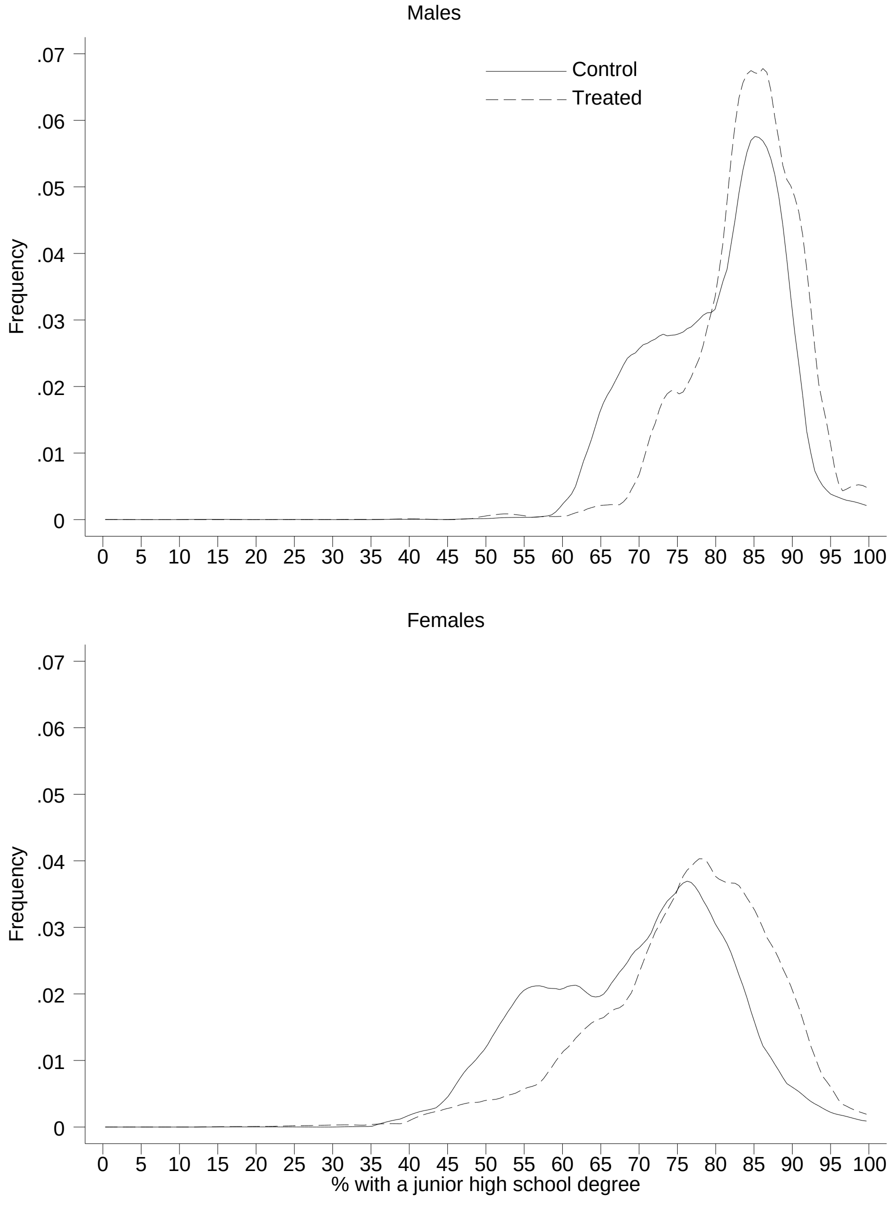


Figure 12: Distribution of junior high school achievements.



Source: Censimento (XIII) Generale della Popolazione: 26 Ottobre 1991 (Istat (1994)).

People I Know: Social Networks and Job Search Outcomes

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Abstract

Theoretical models linking social networks and job search outcomes generally conclude that, all else equal, individuals belonging to larger and *better* networks find jobs more quickly and end up earning higher wages. Empirical work on the role of networks for job search has so far been limited to comparing the role of networks to that of other methods of search. In this paper we tackle a so far empirically unexplored question: the impact of network size, composition and quality on unemployment duration and wages. We address these relationships using a comprehensive data-set of matched worker-firm social security records. The data cover the universe of individuals employed in a very integrated local labor market in northern Italy over the period 1975-1997, allowing us to reconstruct individual working histories for over one million workers. By tracking who worked together when, where and for how long we can build several measures of networks individual workers have potential access to. We then relate these network indicators to unemployment duration and entry wages for a subset of over 13,000 exogenously displaced workers. Results are in line with several theoretical predictions and point to statistically significant and robust network effects on unemployment duration. We use our data on individual working histories to argue that these effects are not likely to be driven by individual heterogeneity. The quantitative importance of networks on post-displacement wages seems to be rather limited.

[‡]We thank Antonio Ciccone. Many thanks to Giuseppe Tattara and Marco Valentini for supplying and helping us with the data. We are responsible for any mistakes. The views expressed here are our own and do not necessarily reflect those of the Bank of Italy. Correspondence: Economic Research Department, Bank of Italy, Via Nazionale 91, 00184 - Roma - Italy. Ph.: (++39)0647923077, Fax: (++39)0647923720, email: federico.cingano@upf.edu, alfonso.rosolia@bancaditalia.it

1 Introduction

Since the early studies by Granovetter (1973) a large body of empirical evidence has documented the widespread use of social contacts in the job search process and their effectiveness in shaping its outcomes. Jobs obtained through informal channels are in excess of 50 percent, independently of the country or the type of occupation under scrutiny. Moreover, social contacts are shown to be a relatively more effective search vehicle in terms of offer and acceptance rates, wages earned and subsequent tenure. Finally, it has been shown that even employers make large use of social contacts in the recruiting process and fill a relevant share of their vacancies with referred applicants (among others, Holzer (1987), Holzer (1988), Blau and Robins (1990)).

The theoretical underpinnings of this evidence are straightforward. In a world of imperfect information belonging to a network of contacts may improve the quantity and quality of the information one has access to: job seekers may be informed about potential employment opportunities by their contacts; employers may be informed about applicants' quality by their social ties. In the last decade theoretical contributions have explored several implications of these simple assumptions. Montgomery (1991) shows that wage dispersion arises if the hiring process is based on referrals and concludes it is positively related to the density and the degree of inbreeding of social networks; Calvo-Armengol and Jackson (2004) explicitly model the structure of social contacts and show that individual employment status is positively correlated across people in the same network and over time. Relaxing the assumption of fixed contacts by assuming that social ties are more likely to be established and maintained between people sharing the same labor market status, Bramoullè and Saint-Paul (2004) generate unemployment duration dependence. Fontaine (2004) embeds the workings of social networks in a formal model of a labor market with frictions to endogenize wages and job offers arrival rates and shows how equilibrium wage dispersion, employment rates and average wages are positively related to network size. In a similar framework, Bentolila, Michelacci and Suàrez (2004) show how the availability of more social contacts in a world with heterogeneous jobs and skills may lead to a mismatch of workers' comparative productive advantage and their occupational choices.

In this paper we provide a quantitative assessment of a common implication of most of these models, namely the fact that workers endowed with larger and *better* networks should exhibit a higher probability of leaving unemployment. We also explore the consequences of network characteristics for wages, although the theoretical predictions seem somewhat more ambiguous. The main contribution of the paper consists in exploiting measures of network extension and quality at the individual level: for a large sample of exogenously displaced Italian workers we obtain individual-specific measures of the number and quality of potential contacts they have access to when unemployed and investigate whether differences in size and quality of networks reflect in different unemployment durations and wages.

Our results show that larger social networks significantly reduce the time spent unemployed. Our most conservative estimate suggests that increasing network size from the first quartile to the median (23 additional contacts) reduces unemployment duration in our sample by 10-15 days. Moreover, we find that the labor market status of one's contacts matter: a 10 percentage points increase in the share of employed contacts reduces unemployment duration by more than one month; the effect nearly doubles if the higher employment share is due to contacts who recently experienced job-turnover. On the contrary, we only find limited effects as concerns entry wages. We exploit the availability of detailed information on the working histories of each individual in our data to argue that these results are not likely to be driven by individual heterogeneity.

These findings are relevant in several respects. For example, the existence of a causal effect of the number of contacts on one's employment probability may contribute to explain the so-called "labor market pooling" effects. Since Marshall (1890) argued that intense labor market interactions in more populated (urban) areas constitute one of the main sources of agglomeration economies a large literature has studied the mechanisms by which proximity improves the probability and quality of matching between firms and workers (Krugman (1991), Duranton and Puga (2004)); the reasonably higher number of ties one can establish in denser areas might constitute one such mechanisms. From the normative point of view, the most immediate policy implication is that interventions to sustain and improve the employment rate of a given population will affect also the employment outlooks of individuals connected to those directly targeted. Therefore concentrating interventions into tighter networks will magnify the initial effect, the more so the tighter the cluster. In turn, if clusters are not totally separated the effect will propagate to other, possibly indirectly, connected individuals (Calvo-Armengol and Jackson (2004)).

Despite their relevance the effects of network extension and quality on unemployment and wages have hardly been empirically investigated. Most studies have typically aimed at assessing the relevance of social contacts as a job search method. These works exploit information on how the current job has been found or on the search method adopted when unemployed to study whether and how relying on social ties rather than on alternative methods affects wages, observed tenure or unemployment duration[1].

Wahba and Zenou (2003) is the only work we are aware of to explore the effects of network characteristics on individual labor market outcomes. Using local population density as proxy for the number of potential contacts a given individual has access to, and local unemployment rate to capture the quality of such contacts, they estimate the effect of network characteristics on the probability that the current job was found through contacts as opposed to alternative methods. They find that employed individuals living in denser and low-unemployment cities are more likely to have found the

[1]Using different samples of US workers, Holzer (1987), Holzer (1988) and Blau and Robins (1990) showed that contacts (friends and relatives) are one of the most frequently used methods of search and it generates the highest rates of job offer and acceptance of job offer conditional on its use. Subsequent works extended those findings to European countries (e.g. Gregg and Wadsworth (1996) for the UK and Addison and Portugal (2002) for Portugal). Simon and Warner (1992) showed that consistently with standard matching models where contacts help reducing the uncertainty about productivity of the unemployed, jobs found through referrals yield higher entry wages (but lower subsequent growth) and longer tenure.

job through contacts, an effect they attribute to the size and quality of the network. Their approach differs from ours in many ways. First, their network variables have no individual variation since they assign the same network extent/quality to all workers in the same location. Second, we explicitly focus on unemployment spells caused by exogenous shocks (firm closures). Hence we can quantify (if any) the expected reduction in involuntary unemployment associated with changes in the extent and quality of networks for the representative displaced worker. Estimating the correlation between network characteristics and the probability of using contacts is uninformative in this sense. For example, in a world where the expected tenure of jobs found through contacts relative to other methods were larger in denser areas one would also find a larger share of jobs found through networks, even if the probability of exiting unemployment was independent of density and all workers chose randomly among the available search methods. Third, by relying on population density as a source of identification their estimates might be picking up other forces that determine choices of residence or place of work (city vs. country selection process) but are unrelated to network-effects. For example, it has been documented that better workers select in cities (Glaeser and Marè (2001)). If for some reason (as self-confidence, etc.) such workers rely more on contacts than, say, on employment offices with respect to other people, then even if there are no network effects, the estimated coefficient on density (capturing the city-country differences) would be positive. By relying on a measure of network that allows us to control for local characteristics we do not have similar problems.

The paper is organised as follows. The next section describes the dataset and illustrates how we recover our basic measure of individual's network extension. We then turn to the empirical analysis: in sections (3) and (4) we investigate the relationship between unemployment duration and network size and the employment rate of the network; in section (5) a similar analysis is developed for entry wages. We then conclude.

2 Data, definition and preliminary evidence.

Our data sources are three National Security Service (INPS) archives providing information on the worker, the firm and the characteristics of the match for any work episode occurred over the period 1975-1997 in two Italian provinces[2]; movers are tracked so that the same information is available even if they worked in other areas of the country. For each worker our data allow to recover several individual-specific as well as firm-specific characteristics. Most relevant to our purposes, for each work episode we know its starting and final dates (month-year) as well as the number of weeks paid during each year and yearly pay. We can therefore recover the precise length (in months) of any given employment or non-employment spell, as well as job tenure at any point in time and weekly wages.

Our goal is to relate network characteristics to the length of an unemployment spell and to the subsequent entry wages. Since we do not know the reasons why a given unemployment episode begun, we restrict our analysis to a sample of about 13,000 workers displaced by a manufacturing firm-closure[3]. The exogenous source of the job loss helps in limiting problems due to selection into unemployment. Figure (1) reports the empirical distribution of the length of the unemployment spell.

The main ingredient of our analysis is a measure of network extension and of its characteristics at the individual level. We exploit the fact that our data allow us to precisely identify all people working in any given month for any given firm to recover the number of potential contacts established on the job: we define one's social network as the pool of individuals she worked with in the five years prior to displacement, an arbitrary but convenient time interval we call the *network building* period (henceforth NB). Therefore, our basic measure of network size is simply the number of co-workers met during the five years prior to displacement. Figure (2) shows the distribution of this measure of social

[2]The two provinces (Treviso and Vicenza) are located in the north-eastern part of the country. The overall population was 1,6 million people as of the 2001 Population Census. As a whole, the north-eastern regions are characterized by small firm size, tight labor markets, high density of economic activity; in year 2000 per capita GDP in this part of the country was 22,400 euro, 20 percent higher than the national average. The two provinces we have data about account for 3,3 percent of the Italian GDP.

[3]In the appendix we provide a full description of the main data sources and we detail how we moved from the universe of workers to the final sample.

network. We exclude from the pool of potential contacts those being displaced by the same firm since they are more likely to be competitors than useful acquaintances. For any of the remaining useful contacts we can recover several individual characteristics that allow us to describe and explore other interesting features of this potential network.

Our basic measure of network extension is certainly a variable measured with error. For example, it may not include a relevant part of one's social network, namely acquaintances made off the job which may also play a role in the job search process. Alternatively, by including all co-workers at firms visited during the NB period we might be overestimating the extension of one's network for those individuals who were employed at larger firms. We will deal with these and other related issues and we will show that this broad measure of network extension conveys information on the underlying number of contacts. Moreover, we will argue that this information content can be improved in several ways.

3 Unemployment duration and network size

In this section we relate unemployment duration of exogenously displaced workers to a measure of the number of contacts they have potentially accumulated on the job. Individuals are therefore not associated to a specific search method. The underlying assumption is that using personal contacts is not an exclusive method and that it is virtually costless.

We start from the following simple linear specification of (the log of) the length of individual i's unemployment spell following her displacement from firm j at time t:

$$u_{ijt} = \alpha + \gamma \log(Size_{ijt}) + \beta_1 X_{ijt} + \epsilon_{ijt} \tag{1}$$

where $Size_{ijt}$ is our indicator of the extent of the network of potential social contacts as of time t, X_{ijt} is a quite exhaustive set of controls for individual and firm characteristics that will be detailed as we go on and ϵ_{ijt} is an error term. Our main concern in the estimation of the strength of network-size effects (γ) is spurious correlation between network size and unemployment duration induced by the omission of relevant variables. To this purpose we condition the estimation of γ in regression (1) to a large set of individual and firm-specific variables recovered from our worker-firm matched data. Such variables are included in X_{ijt} together with standard individual observable characteristics (age, gender, qualification and firm tenure). Let us briefly discuss them.

First, we account closing-firm fixed effects. This implies that the identification of parameter γ stems from comparing workers displaced by the same firm and endowed with different network sizes. Firm-specific fixed effects allow to control for many characteristics of the closing establishment (e.g. year of closure, sector, location, size or any combination of these) whose omission might induce spurious correlation between our measure of size and unemployment. As a simple example, suppose we were to compare two workers displaced in a period of expansion and in the trough of a recession, respectively. Other things equal the first worker is likely to have visited in the past larger expanding firms than the second, likely to have visited downsizing firms. Being in an expansion, it's also likely that the former will on average find a job earlier than the second, but this will be because of the business cycle and not thanks to the larger imputed network size[4]. A further important advantage of estimating (1) with closing-firm fixed effects is that they also allow to account for omitted sources of individual heterogeneity to the extent that these are correlated with firm characteristics. For example, it is often argued that good workers are employed by large firms[5]. In this case closing firm fixed effects would control for the fact that average workers quality is different across closing firms. Note, however, that only if good workers re-enter employment relatively more rapidly not controlling for sorting would

[4]Alternatively, a worker employed in a declining sector but populated by large firms (say, chemicals) will have a larger network than one working in a small expanding one. Yet, upon displacement, the former will reasonably take longer to get a job. Finally, if closures differ significantly as to the number of displaced workers, observed unemployment spells of workers displaced by larger firms might reflect the larger number of people searching for a job at the same time: this congestion effect might imply larger unemployment spells for workers displaced by larger firms.

[5]Abowd, Kramarz and Margolis (1999) for France, Casavola, Cipollone and Sestito (1999), for Italy. Among others, Kremer (1993), Kremer and Maskin (1996), Saint-Paul (2001) develop theoretical models where the technology is such that in equilibrium workers may end up being segregated according to their skills.

lead to biased results.

Second, we exploit the possibility of tracking the working history of any individual in the data to further control for other sources of heterogeneity which might confound our inference. For example, information on the displaced workers' wage profile during the NB period allows to control for the fact that past earnings might have a signaling role for prospective employers, higher wages signaling more productive workers. This source of individual heterogeneity might bias our results if, say, differences in workers' productivity depend on employers' provided training and this is more frequent in large firms[6]. Similarly, we will account for differences in the individual probability of re-entering employment or in the propensity to stay out of employment using the length of workers' past unemployment spells. Omitting this control could bias our results since, in our data, workers who are more often employed are potentially assigned, all else equal, more contacts[7].

Third, since we also observe the whole working history and several individual characteristics of each network member we can control for many features of the network likely to be informative about characteristics of firms visited prior to displacement. For example, it is known that the *quality* of firms one has visited in the past constitutes, everything else constant, valuable information to potential employers during the recruiting process. If better firms are also on average larger in size then network size would be spuriously correlated with observed unemployment duration.

A final concern arises from the fact we include in a given individual i's network of contacts all the persons she has worked with in the previous five years. Clearly, working for Microsoft does not imply that one has personally met all other Microsoft employees although it sounds reasonable that she knows more people than a comparable individual who has been employed in, say, a small car workshop.

[6]On the positive relationship between wages, tenure, training and firm size see Oi and Idson (1999).

[7]Note however that also past wages may constitute a control for heterogeneous turn-over rates. For example, in a frictional labor market firms post more vacancies for more productive workers. In equilibrium this leads to higher wages and exit rates from unemployment for these individuals. Alternatively, if workers are homogeneous as concerns productivity but different in terms of exit rates from unemployment (say, they bear different search costs), again workers with shorter unemployment spells earn more because of the higher outside option.

This feature of our measure would imply an attenuation bias under reasonable assumptions about the structure of the error term. For example, if we assume that the true underlying network (S^*) and our measure ($Size$) are related according to $S^* = min\left\{Size, \bar{S}\right\}$, meaning that an individual knows everybody up to $\bar{S}$ people so that working in larger firms at most allows to establish contacts with $\bar{S}$ co-workers, the measurement error will be positively correlated with the underlying true variable biasing an OLS estimate of γ towards zero.

3.1 Main results

The first column of table (1) reports the results of a baseline estimation of unemployment duration on a dummy for sex, a quadratic in age, a set of dummies for the qualification at displacement (blue-collar, white-collar and manager), a quadratic in job tenure at displacement and closing-firm fixed effects. In the second column this basic model is extended allowing two additional controls for individual heterogeneity: the (log of) average weekly wage at displacement and the share of time spent unemployed over the five years prior to displacement (our NB period). Following the previous discussion, we expect to estimate a negative coefficient on pre-displacement wage, a positive one on the time previously spent unemployed and a lower network-size elasticity with respect to the first column.

As can be seen comparing the results in columns (1) and (2) the effect of network size on unemployment duration is negative and significant in both specifications. Moreover, the coefficients on the two additional controls are significant and have the expected sign: more productive and more frequently employed workers experience shorter post-displacement unemployment spells[8]. Adding such controls reduces the estimated elasticity to network size from about 0.1 to nearly 0.07. The elasticity estimated

[8] We have also experimented with a more flexible specification of past wages and employment experience. In particular, wage at displacement and the measure of labor market participation have been replaced by the entire wage profile and employment history over the five years prior to displacement to proxy for potentially dynamic heterogeneity. We have included, for each of the 10 past semesters the weekly wage earned and the share of time spent employed. Results on the parameter of interest turned out to be robust to this more flexible specification of individual controls.

in columns (2) implies that increasing the number of contacts from the 25th to the 50th percentile of its distribution, that is 23 additional potential contacts, reduces unemployment duration by more than 4 percent (table (2)). The average unemployment duration in our sample is about 10 months; the 23 additional contacts thus reduce unemployment duration by about 15 days.

The specification in columns (2) only accounts for heterogeneity potentially correlated with network size that reflects into wages and labor market attachment. In column (3) we extend it and include controls for individual mobility costs and preferences. Since we know where the individual lives and where each firm she has visited in the past is located, we can recover the average distance she has commuted to go to work during the network building period. Given her wage and other characteristics, this should proxy for an individual's propensity to commute. We believe this source of heterogeneity might be relevant since, all else equal, an individual with a lower mobility cost has access to a larger labor market and might pick better opportunities: if these are correlated with firm size again our estimate of γ might be biased. In particular, results in column (3) include a quadratic of the average distance of one's residence from past workplaces and a set of dummies for the residence of the displaced individual (not reported) which control for any geographic difference among individuals (say, different economic density, better transportation infrastructures, etc.). As expected, there is a negative correlation between distance and unemployment duration. The coefficient of interest is basically unaltered; the point estimate is slightly larger in absolute value but still within the confidence interval of the one reported in column (2).

We have argued in the previous section that firm-specific FE may take partially care of individual heterogeneity if workers are sorted across firms according to these characteristics[9]. However, if sorting in the closing firm is not perfect (say, because of a negative shock to the workforce) this strategy is not enough. We try to account for this possibility by recovering from each individual network some

[9]Note however that if the labor market does not sort workers across firms according to some rule (say, better workers into larger firms), the parameter γ is consistently estimated.

measures that capture some relevant characteristics of past firms in a synthetic way. In particular, since we observe the whole working histories and wages of each network member, along with several characteristics (sex, age, qualification, residence) we estimated a wage equation with individual data where the log of weekly wages of each network member is projected on a set of dummies for sex, qualification, year and location along with a quadratic in age and gender- and qualification-specific returns to experience. We then computed for each individual network the average unexplained wage and included it in our main regression along with the age-sex composition of the individual network. The average wage premium in the network should therefore capture past firm as well as co-workers unobserved characteristics that might affect the length of one's unemployment spell. For example, if larger firms pay a premium and a given displaced individual has worked often in large firms because of some individual characteristic that also makes him re-enter more rapidly when displaced, the average wage premium in the network would account for this correlation. Moreover, we include in the control set a range of measures of geographic distance among network members. This is meant to capture the attractiveness or choosiness of a given firm. We expect better firms to be more choosy when hiring for a given position. This, in turn, should imply that its employees proceed from a wider area than those of a worse, less choosy firm. For example, a top department of economics is able to attract professors from all over the world. Results are again unaffected. As expected, the average wage premium in the network is negatively correlated with unemployment duration. This may capture either past firm characteristics that affect one's unemployment duration (say, having worked for a good firm is a good referral) or, for example, the quality of one's contacts, a higher wage premium signaling better contacts, more efficient in the refereeing process. As to the other controls, the age composition of the network seems to be positively correlated with one's unemployment duration, while the gender composition does not seem to play a role.

As a final robustness check, we estimated the model allowing jointly for the above mentioned sets of controls. Results, reported in column (4) are again unchanged with respect to column (2). Accordingly, the implied cuts in unemployment duration associated to raising the number of contacts from the 25th to the 50th percentile of its distribution are also very similar (see table (2)). In what follows we will refer to (2) as to our preferred specification.

All in all, the results are robust to several quite exhaustive specifications of the information set that control for individual heterogeneity stemming from worker's productivity, labor market attachment, mobility costs, local features as well as for past firms' characteristics as measured by firms' wage premia, gender-age composition of the labor force and firms' choosiness and/or attractiveness.

In table (3) we look at the same baseline regression for specific sub-groups. We see that the effect of a 25-to-50 shift is larger for males and is basically the same for people younger or older than 30; there seems to be no network effect for white-collars. Finally, focusing on a sub-sample of male blue-collars the implied effect of an increase in network size is in excess of 10 percent (and higher than 30 percent for a 25th-75th experiment). For them, the average unemployment spell would be shortened by 19 days.

3.2 Measurement and an estimation for true networks size

The evidence presented so far shows that the measure of network extension is negatively and significantly correlated with unemployment duration, a result robust to a number of extensions. Still, the variable we exploit is affected by measurement error since it implicitly assumes that an individual who has worked in a large firm knows all of its employees or, at the very least, that they all can contribute to shorten her unemployment spell[10].

We now try to assess whether the data point to a reasonable value for the *true* size of the network[11].

[10]Our measure may partly captures the idea of extended or indirect networks. Theoretical work has shown how also the structure of a network plays a role (e.g. Calvo-Armengol and Jackson (2004)). In general, the implication is that one need not to know directly her network members to access the information they collect.

[11]Modern anthropological and psychological studies have investigated whether there exists an optimal group size for human beings. Based on evidence drawn from modern hunter-gatherer societies as well as other productive organizations such as autarkic societies or professional military corps they generally conclude that it exists and ranges around a hundred

We do that assuming that social contacts are maintained in a simple but reasonable way. Formally, we hypothesize that an individual cannot maintain a useful relationship with more than $\bar{S}$ individuals at the same time. Therefore the measure of network size we use in our regressions is $S^* = min\left\{Size, \bar{S}\right\}$ where $Size$ is our original measure. The estimated equation then becomes:

$$u_{ijt} = \alpha + \beta X_{ijt} + \gamma \log\left(min\left\{Size_{ijt}, \bar{S}\right\}\right) + \epsilon_{ijt} \tag{2}$$

where we jointly estimate β, γ and the threshold $\bar{S}$. The strategy is to estimate equation (2) over a large grid of possible values for $\bar{S} \in [5, 250]$ and select the set of parameters that minimizes the overall sum of squared residuals. The non-differentiability of the model prevents from a formal calculation of the relative standard errors. We therefore bootstrapped 300 random samples from our data stratifying the sample at the closing-firm level and ran the grid search to estimate a bound for each sample. Figure (3) shows the values of the criterion function at the corresponding value of $\bar{S}$ in the baseline and extended specifications of the set of controls (respectively, columns (2) and (6) in table (1)) along with the empirical distribution deriving from the bootstrap exercise. In both cases the sum of squared residuals is minimized at around $\bar{S} = 40$. The empirical distributions show that this is indeed the value that is more frequently estimated. The corresponding point estimate for the elasticity to network size is $\hat{\gamma}_{40} = -0.197 \quad (SE = 0.0339)$, implying that increasing an individual's network size from 25th of the underlying distribution to $\bar{S}$ (15 additional contacts) reduces her unemployment duration by nearly 9 percent, about one month less out of employment. In about 85 percent of the simulations for the baseline model and 70 percent for the extended one the estimated network bound falls in the range $[10, 70]$.

A way to qualify the above result is to break up the set of potential contacts we recover from our data in order to try to isolate those that are more likely to be true contacts. The data provide individual characteristics of network members and allow us to focus in particular on three subsets of the gross

individuals (see Dunbar (1992) for a detailed survey).

network measure. First, we look at contacts living in the same town as the displaced worker. These are more likely to be truly known since, beyond work, there are other opportunities to meet (children in the same school, theater, etc.). Second, we focus on contacts having the same qualification or of the same gender on the grounds that these are the ones most likely to bear relevant information about job opportunities. For example, an accountant is more likely to collect information about suitable job openings from another clerk than from a janitor. The same thing will happen if occupation is gender segregated. Our estimating equation becomes:

$$u_{ijt} = \alpha + \beta X_{ijt} + \gamma \log(S(x)) + \gamma_C \log(S^C(x)) + \epsilon_{ijt} \tag{3}$$

where $S(x)$ is the number of contacts that comply with condition x (say, live in the same town) and $S^C(x)$ is the complementary set.

To gauge the effects of a plain recomposition of the network in favor of the core group holding network size constant one cannot simply compare the point estimates of the two elasticities: a recomposition of the network towards a specific subgroup may imply different percentage changes in each of the two subsets. Therefore a simple comparison of the two estimated elasticities, which implicitly amounts to assessing the net effect of two percentage variations equal in absolute value but opposite in sign, would also embed a change in network size. Letting $d = \frac{\Delta S(x)}{S(x)}$, the appropriate exercise consists in evaluating the quantity $\Delta u = d\left(\gamma - \gamma_C \frac{S(x)}{S^C(x)}\right)$ where it is clear that, unless $S(x) \approx S^C(x)$, comparing the point estimates may be highly misleading.

Table (4) summarizes the estimated coefficients γ and γ_C along with the effect (in percentage terms) on unemployment duration of a re-composition of the network towards the core group holding size constant[12]. In the first row, for example, we considered a shift from the 25th to the 50th percentile of the distribution of contacts living in the same municipality (calculated in correspondence of the median network size in the sample) and a corresponding reduction in the number of those living outside the

[12]Results are based on the elasticities estimated using the most exhaustive specification of the control set.

town. The implied effect of such re-shuffling (involving 7 contacts) is about 2.5 percent amounting, in correspondence to the length of the average unemployment spells, to a reduction of duration by nearly 7 days. Results in the second and third rows suggest that while redistributing the network towards contacts of the same qualification is not effective in reducing unemployment, increasing the number of contacts of the same gender as i is more effective, shortening the experienced unemployment spell by about 11 days.

4 Unemployment duration and network employment rate

A further implication of theoretical works on the role of social networks concerns the effects of one's contacts labor market status. Theory suggests at least two reasons why having more employed contacts shortens one's unemployment spell. On the one hand, employed contacts pass on more valuable information to unemployed ones since they turn down any job opportunity that pays a wage below the one currently earned. On the other hand, a higher employment rate (holding network size constant) implies a lower number of unemployed contacts so that there is less competition for this information. From an empirical point of view, however, such a correlation might reflect forces other than a genuine causal effect. For example, an individual and her contacts might be more likely to be employed simply because during the NB period they worked in a good firm and, as we mentioned above, this constitutes a positive signal to potential employers. As in the previous section, we control for this and similar issues using detailed information on the composition of each individual network and its average wage premium along with closing firm fixed effects and information on individual mobility choices and location.

Our estimating equation becomes:

$$u_{ijt} = \alpha + \beta X_{ijt} + \gamma \log(Size_{ijt}) + \delta \frac{E_{ijt}}{Size_{ijt}} + \epsilon_{ijt} \tag{4}$$

where E_{ijt} is the number of previously established contacts who are employed at the time i is displaced and δ captures the effect of a better network, that is one with a higher employment rate.

Table (5) reports results for the several specifications of the control set used in table (1). The estimated coefficients on the employment rate are extremely stable across specifications, always statistically very significant and imply sizable reductions in the length of an unemployment spell. Results reported in table (6) show that increasing the employment rate from the 25th percentile to the median reduces unemployment duration by between 8.2 and 9.5 percent. In terms of time, this means exiting from unemployment between 25 and 29 days earlier. As before, all additional controls are statistically significant. In particular, a higher network quality, as measured by the average wage premium, is associated with a higher probability of leaving unemployment. Note also that, consistently with the theoretical prediction that the number of contacts and their employment rate are positively correlated (Calvo-Armengol and Jackson (2002)), the estimated elasticities to network size turn out to be lower; the implied effects of increasing network extension are reduced by about 1 percentage point.

Another potential source of variability in the quality or quantity of information available in a given network is the job-mobility of one's contacts. For example, if some contacts have recently successfully changed job they may convey more useful information than contacts who still stick to their previous occupation: they have probably surveyed several opportunities, they have gathered information and so on. Once they find a job this information is no longer valuable to them and can be spread in the network. On the other hand, network members who meanwhile did not change job are likely to have less information on potential opportunities, either because they are not looking for a job or because they have not yet completed their quest for a new occupation To verify this hypothesis we estimate the following model:

$$u_{ijt} = \alpha + \beta X_{ijt} + \gamma \log(Size_{ijt}) + \delta \frac{E_{ijt}}{Size_{ijt}} + \delta_M \frac{M_{ijt}}{Size_{ijt}} + \epsilon_{ijt} \tag{5}$$

where, as before, E_{ijt} is the total number of employed contacts at the time of displacement and

$M_{ijt} \subseteq E_{ijt}$ is the subset of employed contacts who, by the time i is displaced, have successfully changed job since they first met i. Table (7) reports the results for the various specifications of the control set. Consistently with our story, successful job switchers are more effective in reducing one's unemployment duration: a one percentage point increase in the employment rate of the network, holding size constant, has about twice the effect on unemployment duration if it is due to network members that changed job as compared to the case where it is network members who did not leave their previous job. For the specification of column (2) shifting the employment rate from the 25th to the 50th percentile for a displaced worker with median network size would reduce unemployment duration by about 26 days in one case and by about 50 in the other.

5 Wages and network characteristics

Theory has extensively explored the consequences of the existence and use of social networks for wages and their dispersion. However, the theoretical predictions concerning wages appear to be somewhat ambiguous. On the one hand, a larger network produces more information so that a given unemployed individual may select the best offer out of a larger pool (Arrow and Borzekowski (2004), Calvo-Armengol and Jackson (2004)). Moreover, being endowed with a larger network increases the bargaining power of the employee when sharing the match rents with the employer (Fontaine (2004)). Alternatively, the screening role played by one's contacts leads to higher wages because of the better signal to the employer (Montgomery (1991)). On the other hand, if a larger network gives access to more jobs, but these are unsuited to a given individual, he may still decide to accept them trading off a lower wage with a shorter unemployment spell (Bentolila et al. (2004)).

Here we explore the sign of the relationship between network characteristics and entry wages of displaced individuals regressing the (log of) the entry weekly wage on the same control sets discussed in the previous sections. Results are reported in table (9). As shown in the first column network size appears to be positively correlated with entry wages. However, the relationship becomes insignificant as soon as we move to the next column where we add basic controls for individual heterogeneity (log of weekly wage at displacement and labor market participation during the NB period); the result is unchanged in column (a) where we use the extended set of controls. In columns (b) and (c) we include our measures of the network employment rate and of the job switching rate along with the extended control set. While the extension and the employment rate of the network do not turn out to be correlated with entry wages, the share of contacts who switched job since they where first met turns out to positively affect them in a statistically significant way.

All in all, we conclude that while we found quite convincing evidence that a larger and better pool of social contacts helps reducing unemployment duration, it does not seem to play a relevant role in determining the wage received upon re-employment.

6 Conclusions

Understanding the workings of the labor market contributes to shed light on several distributional and efficiency issues as well as to design policies.

In this paper we have investigated whether and how the network of social contacts one establishes on the job helps him in exiting unemployment and whether it affects his re-employment wage. Differently from previous studies, we focus on measures of social network defined at the individual level, which we recover from a comprehensive Italian matched employer-employee dataset for a sample of about 13,000 exogenously displaced individuals.

Consistently with theoretical predictions, our findings show that the number of contacts one has access to when unemployed and their current labor market status contribute in a statistically significant way to shorten his unemployment spell. In our sample, an increase of the number of contacts from the 25th percentile to the median shortens the average unemployment spell (about 10 months) by 10-15 days. A similar experiment conducted on the number of employed contacts, implying an increase of the employment rate of about 7.5 percentage points, reduces unemployment duration by between 25 and 30 days; we also show that contacts who recently changed job contribute significantly more than contacts who did not to shortening one's unemployment spell, the semi-elasticity of unemployment duration to the share of job switchers being almost twice as large. We do not find much evidence of an effect of the number of contacts or of their current labor market status on wages; this might reflect the fact that also theoretical predictions are ambiguous in this respect, since different and opposite effects may be at play. We extensively draw on our data to argue that these results are unlikely to be driven by omitted variables or other sources of individual heterogeneity. We perform several experiments to show that the set of potential contacts we identify is a reasonable proxy of the social ties one establishes on the workplace.

However, several questions remain open. First, one would like to exploit more comprehensive information concerning the recruiting process of firms to be able to disentangle the information sharing hypothesis from the referral one. Second, we have focused on on-the-job contacts. However, other social connections unrelated to the workplace (friends, relatives, etc.) might play a role.

Data appendix

We combine three National Security Service (INPS) archives providing, for any work episode occurred over the period 1975-1997 in two Italian provinces, information on the worker, the firm and the characteristics of the match, respectively; movers are tracked so that the same information is available even if they worked in other areas of the country over the same period. The workers file contains data on more than one million individuals, including gender, date of birth, place (or foreign country) of birth, place of residence and an identifier for the firm they are employed in. The information is anonymous and workers are identified by a progressive code. No information on education, marital status or family size is available. The contributive file contains information on the worker-firm match, including qualification (available with a breakdown in apprentices, blue-collar workers, white-collar workers and managerial workers) and type of contract (whether full or part-time). Gross nominal wages are recorded with a breakdown in periodic current earnings (*competenze correnti*) and other non-periodic payments (*altre competenze*) and include overtime payments. The wage information always relates to a single firm, and never spans more than one calendar year: it reports the total pay received for the year or fraction of year worked, together with the number of months/weeks paid. If the worker changes jobs, a new record is opened including the total pay for the period from the start of the new spell to the end of the calendar year (or the date of termination of employment at the new firm, whichever is more recent). The third archives (firms file) contains information concerning location (at municipality level), the industry affiliation with a three-digit level breakdown and the average employment size of all firms existing in the two provinces from 1975-1997, whether they are still trading or not. The exact dates at which they started and ceased trading (if occurred within the period spanned) are also provided.

Construction of the dataset

The period covered by our data contains about 20,000 firm closures. Most involve very small businesses: more than 50 percent of closing firms averaged only 1 or 2 workers in the 12 months previous to closure. In what follows we will focus on 2,136 firms that averaged more than 10 employees in the 12 months previous to closure (representing the upper 11 percent of the size distribution of the closing firms). At the date of closure these firms employed 15,683 workers, only 40 percent of the number of employees observed in the firms in the previous 12 months. In particular while the pattern of exit in the year preceding that date is quite regular (QUALIFY "REGULAR": with respect to what? Other firms that do not close?) they peak up in the last three months. Hence we decided to consider as *displaced* all workers who are observed in each firm up to 90 days previous to the date of closure. This rule is likely to leave in the sample only workers worse than average, that is those who either did not realize they were going to lose the job or who did not find any other job. Yet, since the cross-sectional nature of our data will not allow us to explicitly control for individual unobserved heterogeneity we believe this selection rule constitutes a first step in the direction of working with a relatively homogeneous sample of observations. This reduces the number of workers to 22,838. Moreover we only included displaced workers who completed a full NB period (five years) and only considered closures occurred over the period 1980-94 to limit right censoring of unemployment spells. Accordingly, we will restrict to 1,766 closures and 19,021 observations. The number firms in our working-sample is further reduced by dropping 209 *spurious* cases of establishments appearing as closing but whose workers are shortly after observed in a new firm whose name is just a re-labeling of the previous one. This reduces the number of workers by 2,754. Also, nearly 1,000 workers are dropped by the following reasons. First, we want to be sure our measure of unemployment spell is correct; by merging our data with the INPS archives of the self-employed we identified and dropped those workers (261) who moved to self employment during the period we attributed to unemployment. Moreover we dropped (178) workers who are observed having unreasonably long unemployment spell (i.e. larger that 100 months), suggesting they dropped out the workforce. Second, we excluded (92) workers experiencing multiple closures over the period spanned by our data, and limited our analysis to full time blue and white collar, dropping apprentices. Finally we had to drop workers employed in firms we could not localize geographically. The final sample amounts to 13,141 workers displaced by 1,372 firms.

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Figure 1: Distribution of unemployment spells.

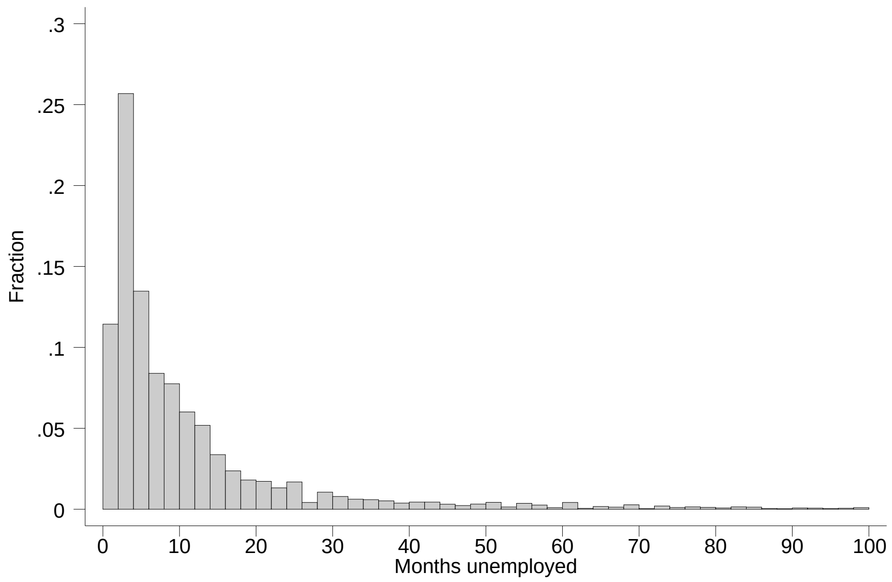

Figure 2: Cumulative distribution function of network size.

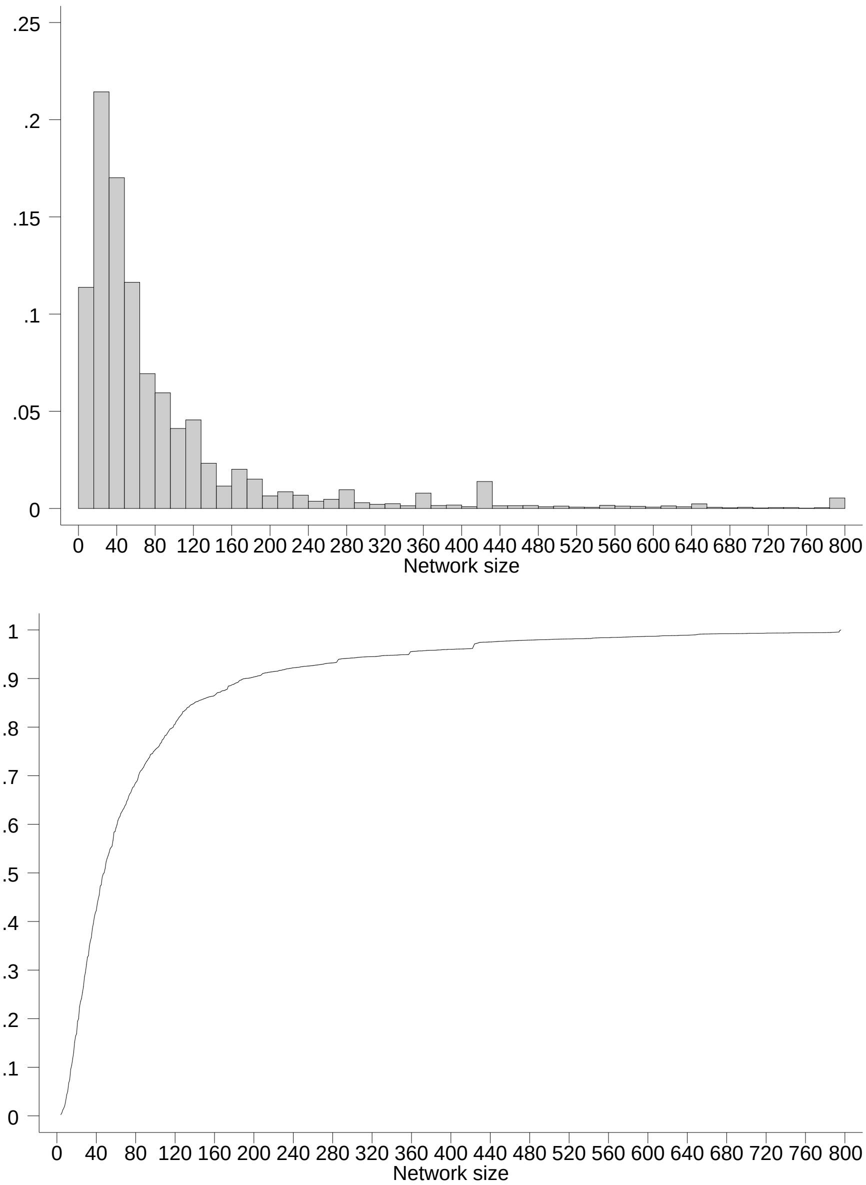

Figure 3: Network upper bounds and bootstrapped densites.

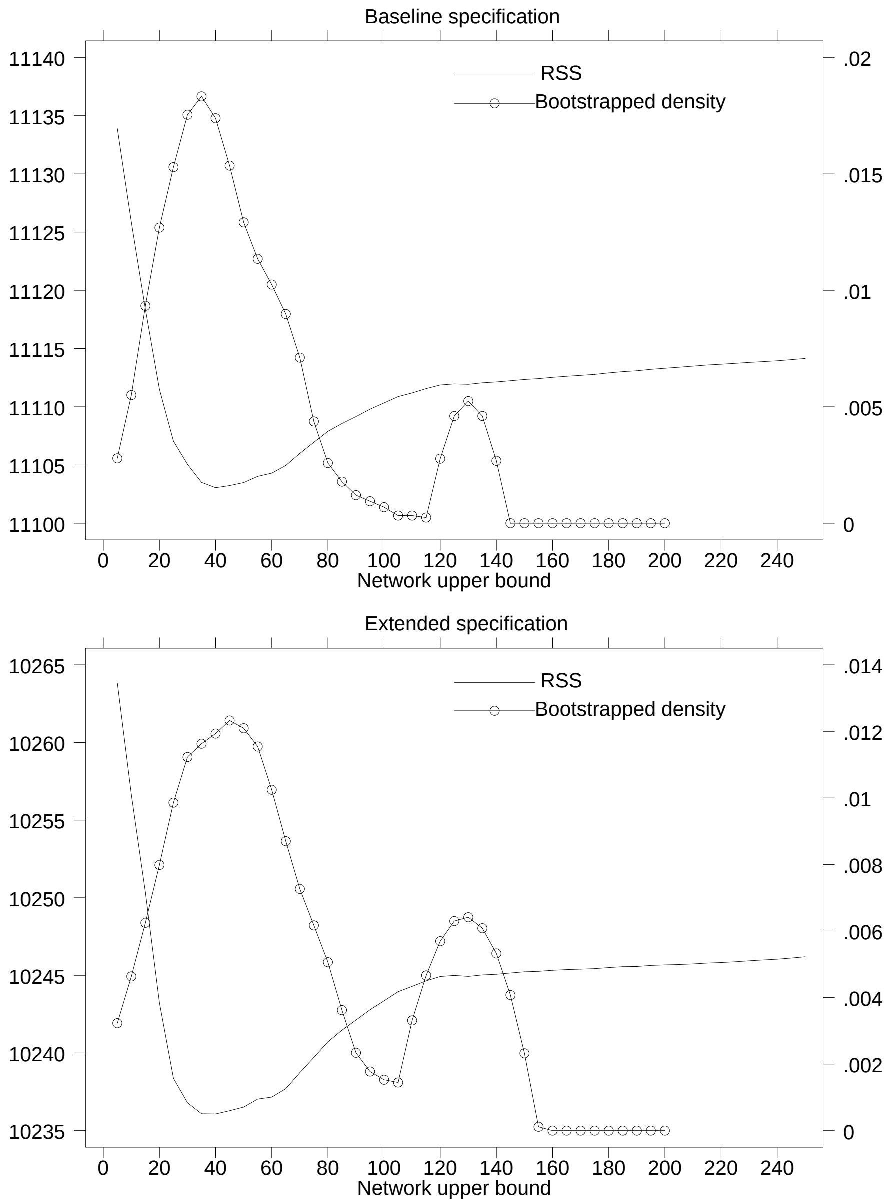

Table 1: Network size and unemployment duration.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
Female	0.3661	0.3577	0.3437	0.3790	0.3630
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age	0.0196	0.0292	0.0274	0.0283	0.0269
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age^2	-0.0002	-0.0003	-0.0003	-0.0003	-0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	-0.0008	0.0001	0.0000	0.0001	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure^2	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
White-collar	-0.1047	-0.0676	-0.0815	-0.0637	-0.0745
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Manager	0.1133	0.3158	0.2633	0.3369	0.2944
	(0.033)	(0.034)	(0.038)	(0.034)	(0.038)
Network size	-0.1111	-0.0684	-0.0723	-0.0703	-0.0720
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Wage at displ.		-0.1617	-0.1548	-0.1592	-0.1523
		(0.001)	(0.001)	(0.001)	(0.001)
Past Unemployment		0.3771	0.3726	0.3669	0.3657
		(0.003)	(0.003)	(0.003)	(0.004)
Distance:					
-from past firms (mean)			-0.2796		-0.2908
			(0.031)		(0.031)
-from past firms (mean)^2			0.1523		0.1558
			(0.029)		(0.029)
-among contacts (mean)				0.0164	0.1026
				(0.031)	(0.034)
-among contacts (median)				-0.1307	-0.2956
				(0.066)	(0.073)
Average wage premium				-0.5028	-0.4763
				(0.010)	(0.011)
Share of:					
-young females				-0.9735	-1.0348
				(0.411)	(0.124)
-young males				-1.1432	-1.1968
				(0.415)	(0.124)
-middle females				-1.1356	-1.2343
				(0.422)	(0.156)
-middle males				-0.8642	-0.9888
				(0.457)	(0.137)
-old females				0.0723	0.0000
				(0.550)	(0.000)
-old males				0.0000	0.2401
				(0.000)	(0.592)

Dependent variable: log of months spent out of employment. Obs: 13,195; firms: 1,367.

Table 2: Network size: implied percentage change in unemployment duration.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
Elasticity	-0.1111	-0.0684	-0.0723	-0.0703	-0.0720
25-50	-6.8	-4.2	-4.5	-4.4	-4.5
25-75	-13.8	-8.7	-9.2	-9.0	-9.2

25-50 corresponds to an increase of network size of 92% (23 additional contacts); 25-75 corresponds to an increase of network size of 292% (73 additional contacts).

Table 3: Unemployment duration and network extension in specific subsamples

	Subsample of:						
	Males	Females	Young	Old	Blue collar	White collar	Male-BC
Network size	-0.086 (0.023)	-0.066 (0.023)	-0.067 (0.029)	-0.069 (0.029)	-0.076 (0.017)	0.033 (0.057)	-0.105 (0.025)
25-50	-5.6	-4.2	-4.2	-4.3	-4.5	2.3	- 7.2
25-75	-11.2	-8.2	-8.9	-8.8	-9.7	4.7	-13.8

Dependent variable: log of months spent out of employment.

Table 4: Network composition

	γ	γ_C	25-50	25-75
Residence	-0.0229 (0.008)	-0.0526 (0.014)	-2.5	-2.6
Gender	-0.0431 (0.013)	-0.0218 (0.013)	-3.6	-5.6
Qualification	-0.03725 (0.017)	-0.03239 (0.014)	1.3	4.6

Dependent variable: log of months spent out of employment. Obs: 13,195; firms: 1,367.

Table 5: Network extension, employed contacts and unemployment duration.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
Female	0.3467	0.3369	0.3247	0.3553	0.3412
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age	0.0165	0.0278	0.0264	0.0274	0.0264
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age^2	-0.0002	-0.0003	-0.0003	-0.0003	-0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	-0.0013	-0.0003	-0.0003	-0.0003	-0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure^2	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
White-collar	-0.1014	-0.0668	-0.0812	-0.0649	-0.0771
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Manager	0.1193	0.3199	0.2753	0.3367	0.2973
	(0.033)	(0.034)	(0.038)	(0.034)	(0.038)
Network size	-0.1046	-0.0487	-0.0514	-0.0530	-0.0537
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Employment rate	-0.0115	-0.0134	-0.0129	-0.0122	-0.0116
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Wage at displ.		-0.1603	-0.1546	-0.1592	-0.1533
		(0.001)	(0.001)	(0.001)	(0.001)
Past Unemployment		0.4899	0.4841	0.4725	0.4699
		(0.003)	(0.003)	(0.003)	(0.004)
Distance:					
-from past firms (mean)			-0.2813		-0.2864
			(0.030)		(0.030)
-from past firms (mean)^2			0.1372		0.1409
			(0.028)		(0.028)
-among contacts (mean)				-0.0301	0.0562
				(0.031)	(0.034)
-among contacts (median)				-0.0695	-0.2268
				(0.065)	(0.072)
Average wage premium				-0.2848	-0.2620
				(0.011)	(0.012)
Share of:					
-young females				-0.7528	-0.6473
				(0.408)	(0.125)
-young males				-0.9088	-0.7977
				(0.412)	(0.125)
-middle females				-0.8208	-0.7570
				(0.419)	(0.158)
-middle males				-0.7836	-0.7259
				(0.453)	(0.136)
-old females				-0.0798	0.0000
				(0.545)	(0.000)
-old males				0.0000	0.3542
				(0.000)	(0.588)

Dependent variable: log of months spent out of employment. Obs: 13,195; firms: 1,367.

Table 6: Employed vs. unemployed: implied percentage change in unemployment duration.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
			Network extension		
Elasticity	-0.1046	-0.0487	-0.0514	-0.0530	-0.0537
25-50	-6.4	-3.0	-3.2	-3.3	-3.3
25-75	-13.1	-6.3	-6.6	-6.8	-6.9
			Employment rate		
Semi-elasticity	-0.0115	-0.0134	-0.0129	-0.0122	-0.0116
25-50	-8.3	-9.5	-9.2	-8.7	-8.4
25-75	-16.5	-18.9	-18.2	-17.4	-16.6

Network size: 25-50 corresponds to an increase of network size of 92% (23 additional contacts); 25-75 corresponds to an increase of network size of 292% (73 additional contacts). Employment rate: the implied effect is computed shifting the employment rate from the 25th to the 50th (or 75th) percentile of its distribution conditional on network size having the median size observed in the sample. 25-50 corresponds to an increase of the employment rate of 7.5 percentage points; 25-75 corresponds to an increase of the employment rate of 15.6 percentage points.

Table 7: Network size, employed contacts and job switchers.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
Female	0.3497	0.3388	0.3286	0.3607	0.3484
	(0.025)	(0.025)	(0.026)	(0.026)	(0.027)
Age	0.0169	0.0262	0.0241	0.0274	0.0258
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Age^2	-0.0002	-0.0003	-0.0003	-0.0003	-0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	-0.0008	0.0000	0.0000	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure^2	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
White-collar	-0.1122	-0.0743	-0.0857	-0.0733	-0.0832
	(0.031)	(0.032)	(0.033)	(0.032)	(0.033)
Manager	0.0900	0.2947	0.2618	0.3025	0.2701
	(0.180)	(0.182)	(0.193)	(0.182)	(0.193)
Network size	-0.1361	-0.0910	-0.0958	-0.0876	-0.0893
	(0.015)	(0.016)	(0.017)	(0.017)	(0.018)
Employmnent rate	-0.0108	-0.0123	-0.0117	-0.0120	-0.0115
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Job switching rate	-0.0121	-0.0115	-0.0116	-0.0117	-0.0120
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Wage at displ.		-0.1617	-0.1555	-0.1608	-0.1544
		(0.027)	(0.029)	(0.027)	(0.029)
Past Unemployment		0.3793	0.3621	0.3947	0.3835
		(0.056)	(0.059)	(0.057)	(0.060)
Distance					
-from past firms (mean)			-0.2962		-0.2954
			(0.172)		(0.173)
-from past firms (mean)^2			0.1256		0.1253
			(0.167)		(0.167)
-among contacts (mean)				0.0616	0.1489
				(0.174)	(0.182)
-among contacts (median)				-0.1607	-0.3093
				(0.253)	(0.266)
Average wage premium				-0.2107	-0.1847
				(0.103)	(0.107)
Share of:					
-young females				0.1929	0.3759
				(0.636)	(0.356)
-young males				-0.0785	0.1143
				(0.638)	(0.355)
-middle females				-0.0109	0.1280
				(0.643)	(0.397)
-middle males				-0.1788	-0.0516
				(0.668)	(0.368)
-old females				-0.1186	
				(0.732)	
-old males					0.3463
					(0.759)

Dependent variable: log of months spent out of employment. Obs: 13,195; firms: 1,367.

Table 8: Stayers vs. job switchers: implied percentage change in unemployment duration.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
			Network extension		
Elasticity	-0.1361	-0.0910	-0.0958	-0.0876	-0.0893
25-50	-8.3	-5.6	-5.9	-5.4	-5.5
25-75	-16.6	-11.5	-12.0	-11.1	-11.3
			Employment rate		
Semi-elasticity	-0.0108	-0.0123	-0.0117	-0.0120	-0.0115
25-50	-7.8	-8.8	-8.4	-8.6	-8.2
25-75	-15.6	-17.5	-16.7	-17.2	-16.4
			Job switching rate		
Semi-elasticity	-0.0121	-0.0115	-0.0116	-0.0117	-0.0120
25-50	-15.8	-16.4	-16.1	-16.3	-16.1
25-75	-30.2	-31.1	-30.6	-31.0	-30.7

Network size: 25-50 corresponds to an increase of network size of 92% (23 additional contacts); 25-75 corresponds to an increase of network size of 292% (73 additional contacts). Employment rate: the implied effect is computed shifting the employment rate from the 25th to the 50th (or 75th) percentile of its distribution conditional on network size having the median size observed in the sample. 25-50 corresponds to an increase of the employment rate of 7.5 percentage points; 25-75 corresponds to an increase of the employment rate of 15.6 percentage points. Job switching rate: the implied effect is computed assuming that the 25-50 (25.75) increase in overall employment rate is due to contacts who changed job meanwhile.

Table 9: Network size, employment and job switching rates and entry wages.

	Baseline	Basic Controls	Extended Controls (a)	(b)	(c)
Female	-0.2282 (0.009)	-0.2193 (0.009)	-0.2286 (0.010)	-0.2278 (0.010)	-0.2279 (0.010)
Age	0.0025 (0.003)	-0.0010 (0.003)	-0.0009 (0.003)	-0.0009 (0.003)	-0.0007 (0.003)
Age^2	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)
Tenure	0.0001 (0.000)	-0.0002 (0.000)	-0.0002 (0.000)	-0.0002 (0.000)	-0.0002 (0.000)
Tenure^2	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)
White-collar	0.2054 (0.011)	0.1766 (0.012)	0.1765 (0.012)	0.1766 (0.012)	0.1771 (0.012)
Manager	0.9428 (0.067)	0.8011 (0.067)	0.7448 (0.071)	0.7447 (0.071)	0.7479 (0.071)
Network size	0.0116 (0.005)	-0.0023 (0.006)	0.0005 (0.006)	-0.0002 (0.006)	0.0039 (0.007)
Employment rate				0.0004 (0.000)	0.0005 (0.000)
Job switching rate					0.0015 (0.000)
Wage at displ.		0.1124 (0.010)	0.1064 (0.011)	0.1064 (0.011)	0.1066 (0.011)
Past Unemployment		-0.1160 (0.020)	-0.1221 (0.022)	-0.1261 (0.022)	-0.1139 (0.022)
Distance:					
-from past firm (mean)			0.0980 (0.063)	0.0979 (0.063)	0.1008 (0.063)
-from past firm (mean)^2			0.0823 (0.061)	0.0829 (0.061)	0.0841 (0.061)
-among contacts (mean)			-0.0765 (0.067)	-0.0747 (0.067)	-0.0896 (0.067)
-among contacts (median)			0.1352 (0.098)	0.1326 (0.098)	0.1461 (0.098)
Average wage premium			0.2932 (0.038)	0.2851 (0.039)	0.2695 (0.039)
Share of:					
-young female			0.5226 (0.127)	0.5080 (0.128)	0.3797 (0.130)
-young male			0.4791 (0.127)	0.4640 (0.129)	0.3486 (0.130)
-middle female			0.5028 (0.143)	0.4847 (0.144)	0.3750 (0.146)
-middle male			0.4291 (0.134)	0.4191 (0.134)	0.3392 (0.135)
-old female					
-old male			0.2406 (0.278)	0.2363 (0.278)	0.2288 (0.278)

Dependent variable: log of weekly entry wage. Obs: 13,195; firms: 1,367.

Unequal Workers
or
Unequal Firms?

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Abstract

We investigate the importance of firm characteristics for the Italian earnings distribution by exploiting an extensive matched firm-employee dataset covering the period 1980-1997. The dataset includes detailed information on a representative sample of about 1,500 firms along with detailed information on wages, weeks worked and personal characteristics of individuals employed at any of the sampled firms. We estimate firm-year specific wage equations to assess the degree of heterogeneity in wage schedules across firms and over time. We then decompose the observed change in earnings dispersion in the component due to changes in the distribution of individual characteristics and that due to changes in the distribution of prices, namely the firm-year specific returns to observed individual characteristics. We further compare this decomposition with one based on estimation of a year-specific wage equation common to all firms, as is commonly done in most studies on inequality. Our exercise suggests that price heterogeneity matters in explaining the evolution of earnings inequality at least as much as the change in the distribution of individual characteristics.

Keywords: earnings dispersion, firm heterogeneity

JEL codes: J3, I3.

[§]We thank seminar participants at IZA Workshop on Wage Inequality, Technology and Institutions (2004), EALE 2004, AIEL 2004. We are grateful to Federico Giorgi for his excellent research assistance. The views expressed herein are those of the authors and do not necessarily reflect those of the Bank of Italy. Corresponding author: Alfonso Rosolia, e-mail: alfonso.rosolia@bancaditalia.it, ph. (+39)0647923077, Economic Research Department, Via Nazionale 91, 00184 Rome, Italy.

1 Introduction

Studies on the structure and evolution of inequality typically focus on changes in the distribution on workers characteristics and, possibly, changes over time in returns to those characteristics (e.g. Juhn et al. 1993, DiNardo et al. 1996) due to both market reaction to relative scarcity and changes in the labor market institution (Lee 1999, Manacorda 2000, Teulings 2002). Yet, another potential source of changes in inequality is the composition of the pool of firms in a country. Abowd, Kramarz and Margolis (1999) have extensively documented a high degree of heterogeneity as concerns systematic differences across observationally similar firms. In their analysis these differences are held fixed over time and affect all wages paid by the firm equally. We push the argument a step further and investigate the possibility that *marginal* returns to workers' skills may differ across firms and its consequences for the observed distribution of earnings. Recent theoretical contributions have pointed out how this may happen when there exist intra-firm externalities that lead workers to be sorted across firms along some individual characteristic (Kremer 1993, Kremer and Maskin 1996, Saint-Paul 2001).

We investigate the importance of firm characteristics for the Italian earnings distribution by exploiting an extensive matched employer-employee dataset covering the period 1980-1997. The dataset includes information on a representative sample of firms along with information on the whole working history of individuals who have worked for any of the sampled firms. We estimate firm-level wage equations in order to establish how much of the wage inequality can be attributed in each year to the heterogeneity across firms of the returns to standard worker characteristics (experience, tenure, etc.), along with the influence of other standard sources of inequality (e.g. distribution of workers' characteristics).

The paper is organised as follows. In section 2 we present some background evidence on the changes in the overall earnings distribution in Italy. In section 3 we describe our database. We then turn to an illustration of the methodology in Section 4. Section 5 introduces some evidence on the evolution of inequality on the side of firms. Estimates are presented in section 6. In Section 7 we perform several variance decompositions on the basis of two econometric specifications for wages. Section 8 concludes.

2 Background evidence

Between 1977 and 1989, both mean and median real monthly net earnings rose by 1.8 per cent per year[1]; from 1989 to 2002 the mean declined by around 0.5 per cent per year, and the median by 1 per cent (fig. 1, upper panel). Some of the reduction in the 1990s was due to the spread of part-time work, as is shown by the smaller drop in monthly earnings of full-time employees. Data on gross wages are not available in the SHIW, but a rough comparison with the national accounts suggests that some of the fall in net earnings in the 1990s may have been caused by the rising fiscal burden. The basic message is that the steady rise in the 1980s was replaced by an enduring fall of real after-tax incomes in the following decade.

The overall earnings dispersion, as measured by the Gini index, shows a narrowing during the 1980s, somewhat stronger at the beginning, a sharp widening in the early 1990s and substantial stability between 1993 and 2002. The decile ratio, i.e. the ratio of the 90th percentile to the 10th percentile, shares this same pattern, though its increase start in 1989. The intensity of changes and year-to-year variations may differ, but this pattern broadly describes the evolution of earnings inequality in the main sub-groups of the population: full-time employees, both male and female salaried workers, both residents in the North and in the South. This picture must be rectified for prime-age non-agricultural male workers employed throughout the whole year, for whom the tendency towards greater inequality emerged in the mid-1980s, although in a less extreme form. This asymmetry between core employment

[1]The evidence presented here is based on the micro-data of the Historical Archive (HA) of the Bank of Italy's Survey of Household Income and Wealth (SHIW). See Brandolini et al. (2002) for a thorough analysis and a detailed description of the data. Real monthly net earnings are calculated by dividing total earnings, net of taxes and social security contributions, by the number of months worked in the year in each job and deflating by the consumer price index for the population as a whole. Earnings refer to all primary job positions, excluding secondary job positions, i.e. the jobs that people may have in addition to their main occupation as employees or self-employed.

and the full sample indicates that the relevant changes were concentrated among workers at the margins of the market.

The long phase of diminishing earnings inequality that ended in the 1980s is largely confirmed by the other scattered evidence available, including the information on wage differentials provided in national accounts (see Sestito, 1992; Erickson and Ichino, 1995; Brandolini, 2000). There is also a fairly general consensus that this phase dates back to the late 1960s and early 1970s, the post-war period in which industrial conflict was at its highest. In those years, bargaining power shifted sharply in of workers and their strongly egalitarian demands, such as equal (lump-sum) pay raises for all workers regardless of grade (e.g. Regalia, Regini and Reyneri, 1978; Erickson and Ichino, 1995). Later on, these demands translated into the 1975 reform of the wage indexation mechanism, which granted a flat-sum wage increase for each percentage point rise in the cost-of-living index. Until early 1980s, the operation of this mechanism in the presence of double-digit inflation rates imparted a strong egalitarian push to the evolution of the earnings structure, which was only partially compensated by decentralized bargaining. On the basis of evidence up to 1991, Erickson and Ichino (1995, p. 298) concluded that "the overall picture of Italy is of a country with a compressed wage structure that is not yet undergoing the rapid decompression experienced elsewhere during the 1980s". The severe political and economic crisis of the early 1990s saw the number of resident employees, as measured in the national accounts, plummet by 670,000, or 4.0 per cent, in the fourth quarter of 1993 from the historical peak recorded in the second quarter of 1992. As is shown above, this drop in employment was accompanied by a substantial widening of wage spreads. In the rest of the 1990s, inequality did not revert to the low levels of the previous decade and, if anything, it showed a tendency to increase further.

The economic crisis as well as concomitant institutional changes may have unleashed a decompression of the wage structure, originating in factors already at work in other advanced countries. Manacorda (2000), for instance, argues that a tendency comparable in amplitude to that experienced in the United

States was latent since the early 1980s but failed to emerge because of the egalitarian wage indexation mechanism. Descriptive evidence hinting at a weakening of egalitarian demands during the 1980s is summarized by Regalia and Regini (1996, pp. 823-6), who report that, in the manufacturing sector, performance-related premia and individual bonuses gradually spread, with the support of unions, through bargaining agreements at company level. After 1994, the phasing-out of contribution relief for southern firms could partly account for the return to wider geographical differentials: some firms may have been able to transfer part of the higher cost burden onto the most vulnerable workers, reducing their net earnings. A further factor in the 1990s may have been the spread of part-time and fixed-term employment contracts. In any case, our evidence suggests that changes in the wage structure mostly affected marginal employees, or those at the bottom of the wage scale.

3 The INPS data

The administrative databases of the National Social Security Institute (INPS) provide precise figures on pre-tax earnings and a few individual characteristics since the mid-1970s for employees in the private sector who comply with the social security regulations (with the exclusion of certain employees at the managerial level); some characteristics of the firm where a worker is employed may be also available from the archive on employers. These data have been extensively used in recent years (e.g. Casavola, Cipollone and Sestito, 1999).

In our analysis, we use a special sample selected from the INPS archives. In particular, we have extracted from those archives the records concerning all workers who have been employed at any of about 1,500 manufacturing firms chosen among those surveyed every year by the Bank of Italy's Survey of Manufacturing Industry (SMI). This survey is very useful to our purposes since it collects detailed information on firm performance and decisions (sales, profits, liabilities, investment expenditure, number of plants, proprietary structure, etc.). Merging these two datasets provides us with the characteristics and individual weekly earnings of each worker employed at any of these firms over the period 1980-1997. The survey collects information on firms with at least 50 employees. Therefore, the final sample includes about 1,6 million observations for each year, roughly one third of total manufacturing employment.

Figures 2 and 3 report, respectively, the evolution of the variance of (log) earnings over the period 1980-1997 and the corresponding densities in the first and last year of the sample. They show an increasing pattern in inequality, the variance of earnings having increased by about 50 percent over the period. Recall, however, that we are focusing on a sample of long-lived firms (active throughout the period). This has some limitations. First, the corresponding sample of workers might not be representative of the total population of individuals. Second, some exercises cannot be conducted. For example, we cannot assess the effects of firm demography on earnings inequality. In the next section we illustrate how we use these data to decompose the variance of the earnings distribution.

4 Methodology

A decomposition of the variance of earnings often relies on modeling the wage with a standard mincerian equation:

$$w_{ilt} = X_{ilt}\beta_t + \epsilon_{ilt} \tag{1}$$

where i stands for individual, l for firm and t for time. Therefore, w_{ilt} is the wage of worker i in firm l at time t, X_{ilt} are her (possibly time-varying) characteristics and β_t is the (vector of) returns to those characteristics at time t. Exploiting the orthogonality of OLS residuals to the information set, the cross sectional variance (time t variance) can be decomposed into an explained and unexplained component:

$$V_t(w_{ilt}) = V_t(\hat{w}_{ilt}) + V(u_{ilt}) \tag{2}$$

$$= b_t' V_t(X_{ilt}) b_t + V(u_{ilt})$$

where b_t is the OLS estimates for β_t and u_{ilt} is the OLS residual. The first component represents the part of the cross sectional variance that is explained by the variability of the observed characteristics while the second term accounts for the unexplained variance.

The contribution of these components to the evolution over time of the overall cross sectional dispersion of wages can be evaluated by constructing appropriate counterfactual variances. For example, the effect of changes between two periods t and s in the prices β_t on the total variance can be appreciated by means of a counterfactual variance in which all components are held at their value at time t and prices are set at their s value:

$$V_t^s(w_{ilt}) = b_s' V_t(X_{ilt}) b_s + V(u_{ilt}) \tag{3}$$

These decompositions can be generalized to the standard within-between decomposition of variances. For example, assuming a group is a firm l, we can examine the effects of changes in the within firm distribution of characteristics while leaving unchanged their between firm variance. These decomposition techniques are largely used in the literature. Lemieux (2003) shows how they can be unified under an encompassing framework that relies on finding out the appropriate weighting scheme.

However a crucial building block of these techniques is the estimation of the wage equation. The statistical importance and the economic interpretation of the variance components impinges on the estimation of the prices β_t. Standard mincerian equations usually do not account for two important sources of variability in wages: individual effects and firms effects. Omitting these controls would have no consequences for the estimation of the price vector as long as they are orthogonal to observed characteristics. In this special case more variance would be loaded onto the unobserved component. However if firm or individual effects are correlated with observable characteristics the OLS estimates of the price vector are biased and the contribution of the change in the prices to the overall variance evolution might be unreliable. An additional source of variability comes from the fact that prices can vary across firms (e.g. Saint-Paul 2001). For example, a mean preserving shift in the distribution of prices might not be detected in a decomposition of the variance of wages that takes prices as homogeneous across firm. Most of these limitations come from the fact that the information available to researchers is limited to cross sections of workers. In this type of setting workers and most firms are observed only once. Only large firms have the chance of being sampled more than once limiting the scope for fixed effects. Our data set is a sample of firms and we have the whole history of all workers ever transited in one of them. Therefore at any point in time we have information on all workers in the sampled firms. This allows us to estimate a firm-specific time-varying price for the characteristics of workers[2]. Using this vector of prices we can perform a richer and more detailed variance decomposition.

To understand why our approach improves over previous research let us present the distortions introduced in the estimates of the price vector when unobserved firms and worker heterogeneity is ignored or price distribution is collapsed to one value. We start from a mincerian wage equation augmented with both firm and worker effects:

$$w_{ilt} = \alpha_i + X_{ilt}\beta_t + \delta_{lt} + \epsilon_{ilt} \tag{4}$$

where α_i is the individual fixed effect, δ_{lt} is the year t specific effect of firm l. This specification generalizes that presented by Abowd, Kramarz and Margolis (1999) in that we allow for time varying firm specific rewards of observable characteristics of single worker (β_{lt}) as well as unobservable firm wage components (δ_{lt}). OLS estimates of the price vector that ignore workers fixed effects (α_i), firms unobserved components (δ_{lt}) and heterogeneity of rewards across firms would have three sources of distortion. Assuming only one covariate the estimated coefficients would be:

$$b_t \quad = \quad \beta_t + \frac{cov(V_{lt}, \beta_{lt}) + cov(X_{lt}^2, \beta_{lt}) - X_t cov(X_{lt}, \beta_{lt})}{V(X_{ilt})} \tag{5}$$

[2] As it will be clear as we go on, we refer to prices in a broad sense including in their measurement also the reward for unobservable worker characteristics correlated to the specific observable one. For example, we would not estimate returns to education, meaning by that the additional earnings obtained by increasing education of the average individual, but the reward of education which will include also any worker characteristics correlated with it.

$$+ \quad \frac{cov(\alpha_i, X_{ilt})}{V(X_{ilt})} + \frac{cov(\delta_{lt}, X_{lt})}{V(X_{ilt})}$$

where β_t is the average price of characteristic X at time t, V_{lt} is the within firm l variance of workers' characteristics at time t, X_{lt} and X_t are, respectively, the within firm l and the grand mean of workers' characteristic X at time t. According to (5), the estimated coefficient (b_t) would differ from the mean of the true coefficients (β_t) because of *i*) the across firm co-variation between the prices and the within-firm dispersion as well as mean of the observed characteristics of the workers; *ii*), standard omitted controls for workers unobservable characteristics and *iii*) possible sorting of workers' characteristics into specific types of firms. The first type of distortion drops out even if rewards differ across firms as long as these differences are unrelated to those of the average workers' observed characteristics or their within firm dispersion.

The distortion due to sorting of workers across firms could be avoided by controlling for firms unobserved heterogeneity. OLS estimates that exploit differences among workers belonging to the same firm would deliver the following slope:

$$b_t = \beta_t + \frac{cov(V_{lt}, \beta_{lt})}{V(X_{ilt} - X_{lt})} + \frac{cov(\alpha_i - \bar{\alpha}_l, X_{ilt} - X_{lt})}{V(X_{ilt} - X_{lt})} \tag{6}$$

where $\bar{\alpha}_l$ is the average of the individual unobserved characteristic in firm l. If prices are uncorrelated with the within firm variance of observed characteristics the estimated coefficients are a mixture of the average true prices and the reward of unobserved workers characteristics.

Finally, estimating the augmented mincerian wage equation firm by firm delivers a coefficient that mixes the rewards of observed and unobserved characteristics:

$$b_t = \beta_{jt} + \frac{cov(\alpha_i, X_{ilt} | i \in lt)}{V_{lt}} \tag{7}$$

Let us make use of an example to explain how misleading a variance decomposition based on distorted slopes can be. Assume only one skill S is rewarded in the market (say, schooling) and that there are only two firms, rewarding schooling differently (for example, because one uses ICT more intensively). At time 0 workers with skill below S_0 work in firm A and those above it in firm B, where the marginal return to schooling is assumed to be higher (fig. 4). Suppose that at time 1 firm B becomes on average more productive (say, an increase in TFP or higher rents to be shared between employer and employees) while the marginal return to schooling stays the same (the wage schedule shifts up to B_1). This will imply that workers with schooling between S_1 and S_0 will move to firm B. Estimating a wage equation under the restriction that returns to schooling are constant across firms and can only vary over time would yield an increase in returns to S between time 0 and time 1 and, according to the above decomposition, a subsequent increase in inequality caused by this change. Yet, the example shows that this is not the case: what has increased is the overall return to production factors in firm B which has attracted workers with lower schooling. Notice that allowing for firm fixed effects in the wage equation would not solve the problem since marginal returns to skill S are still wrongly estimated.

Disentangling these two causes of inequality may turn out to be relevant in policy design. Our approach allows us achieve this goal because we can estimate the δ_{lt} components of the wage equation thereby purging the estimates of the prices by the sorting of workers into different firms.

5 Inequality across firms

In this section we exploit the available information to extract evidence on the evolution of inequality among firms along dimensions which are likely to be relevant for the distribution of individual earnings. The Survey of Manufacturing Industry provides, among other, information on total sales, investment expenditure, total hours worked and total employment. Figure 5 plots the evolution of the variance of (the log of) per capita sales, investment expenditure and hours worked. We investigate these variables on the grounds of their tight relationship with true but unobservable measures of firm productivity (e.g. Olley and Pakes (1996), Basu (1996)). Both per capita investments and sales seem to have been by a somewhat higher cross-sectional variability in the second half of the nineties; as concerns per capita hours a sharp declining trend emerges. The reported variances, though, do not control for structural features. Therefore we are not able to establish whether a substantially stable degree of heterogeneity along those dimensions indeed hides effects that in the aggregate cancel off. To gather some hint on the forces underlying these developments, and in particular on how much of this variance can be explained by a limited set of characteristics such as sector composition, size and geographical dimension just to mention a few, we have performed a simple exercise: we have regressed each variable for each year on a set of dummies capturing the interaction of 19 regions, 14 sector and 5 size classes[3]. The share of unexplained variance is plotted in figure 6. The common message is that along all three dimensions (hours, sales, investments) there has been a sharp increase in the share of unexplained variance, meaning that these selected observable characteristics are less and less able to explain the differences across firms. We expect these patterns to affect the distribution of earnings and the forces underlying its evolution.

We now turn to the estimation of the wage equations underlying the variance decomposition exercise. We document the heterogeneity across firms and time of these estimates.

6 Estimates

In this section we document the heterogeneity of firm level prices. We estimate year-firm specific wage equations (henceforth, fully unrestricted (FU) model) and compare the estimated coefficients equation (7)) with those obtained from two benchmark models. In the first one, coefficients are allowed to vary only over time (henceforth, fully restricted (FR) model; equation (5)). We therefore estimate a common wage equation for each year, an exercise comparable with what is usually done in the literature on inequality when the available data are from individual or family surveys. The

[3]Note also that these are generally the only controls for firm characteristics a researcher may include in a wage equation estimated on a representative cross-section of individual earnings.

second benchmark allows for time-varying firm effects (henceforth, partially restricted (PR) model; equation (6)), thus raising the data requirement since the number of observations per firm constrains the number of parameters that can be estimated. All regressions are run on the same control set: the log of weekly wage is projected on a set of dummies for gender, for qualification (blue-collar), for being a mover (i.e. for working in a province different from the one where the individual was born), for job interruption during the relevant year, a quadratic term in age and its interactions with sex and qualification, the number of weeks actually worked during the year and, as concerns the FR model, a set of sector dummies.

To give a of the amount of heterogeneity among coefficients and of its changes over time we report in figure 7 the cross-sectional distributions of some estimated coefficients in 1981, 1990 and 1997. We can see that there is indeed great heterogeneity across firms, especially for some coefficients. Moreover, it seems a common feature that this dispersion has steadily increased between 1981 and 1997. Yet, this heterogeneity could be totally unrelated to true differences across firms and simply be the of the usual randomness involved in OLS estimates. To establish whether this is the case note that both benchmark models are restricted versions of the FU specification which can in turn be tested with standard tools. Table 1 reports the likelihood ratios for the two tests for each year and as a whole. The restrictions involved implied by the FR and PR models are strongly rejected. Therefore allowing for firm specific coefficients significantly improves the explanatory power of the statistical model.

A second test relies on the 95 percent confidence intervals of each coefficient estimated at the firm level. If the true coefficients are the ones estimated by means of the restricted models we would expect the ones estimated at the firm level to be very close to the former. More formally, we would expect the FR or PR coefficients to fall very often in a confidence interval built around the corresponding coefficients estimated with the FU model. In particular, we expect to see them fall in the 95 percent confidence band in at least 95 per cent of the cases. Table 2 reports the share of employees for which this happens to be the case. Again, the share is far from being 95%. Only for the coefficients on the gender and mover dummies the share increases, although staying far below the expected level. This evidence corroborates the results of the LR tests and also shows that they are not driven by some specific coefficient but are rather general.

We conclude that the heterogeneity of estimated coefficients largely reflects structural heterogeneity rather than the usual variability of OLS estimates. Figure 8 displays the evolution over time of some selected coefficients. We show the average coefficient estimated in the FU model along with those estimated in the FR and PR models. The first thing to notice is that the time pattern is very much the same across the three specifications. Yet, in some cases the value of the estimates is very different: the average premium for males is around 5 percent lower in the FU specification when compared with the FR one; that for blue-collar is about 10 percent below the corresponding FR one; the estimated returns to experience move apart during the nineties and eventually become 0,5 per cent lower than the FR ones. Moreover, the change of the estimated coefficients between 1980 and 1997 turns out to be generally larger in the FU specification than in the benchmark models. This is a relevant feature since a counterfactual variance of earnings such as those introduced in the previous section would turn out very different in the three specifications. For example, neglecting the covariance between exogenous variables, the change of the premium to a blue-collar between 1980 and 1997 holding constant all other features of the data would imply an increase of the (log) earning variance of around 15 percent when coefficient estimates are obtained from the FR or PR specification; if, on the other hand, we had used the average return to blue-collar estimated in the FU model, the variance would have increased by 25 percent.

7 Changes in earnings inequality

In this section we explore whether and how the heterogeneity in coefficients documented in the previous section yields a different interpretation of the change in inequality observed in our data. We will focus on the comparison between the FR and the FU model on the grounds that the FR model is the only one a researcher can estimate when using cross sections of wages. In particular, since the FR model has clearly a lower explanatory potential due to the much lower number of degrees of freedom, we will focus on the change in the distribution of the explained wages, leaving aside the unexplained components. Therefore, our exercise will consist in decomposing the change in the variance of explained wages in the part due to the prices and in that due to individual characteristics; recall that among the prices we also have, in the FU model, a firm-year specific component. Formally:

$$V(\hat{w}_{M,97}) - V(\hat{w}_{M,80}) = V(\hat{w}_{M,97}) - V(X_{80}, \hat{\beta}_{M,97}) + V(X_{80}, \hat{\beta}_{M,97}) - V(\hat{w}_{M,80}) \tag{8}$$

where the two terms on the left had side are the variance of, respectively, the explained wage in 1997 and in 1980 using model $M = \{FR, FU\}$ and $V(X_{80}, \hat{\beta}_{M,97})$ is the variance of the counterfactual wage obtained using the distribution of individual characteristics as of 1980 and the prices estimated with model M in 1997. Therefore, the first difference on the right hand side tells us how much of the change in the variance is explained by the change in characteristics and the second one by the change in prices. Table 3 reports actual and explained variances for 1980 and 1997; clearly the FU model explains more of the data. We have already shown that this better fit is statistically significant. Still, for the subsequent analysis we have to keep this fact in mind.

Panel A in table 4 compares the absolute changes of the explained variance due to the two components for the FU and FR models. In the FU model the change in prices implies a change in the variance twice as large as the one obtained in the FR model; on the other hand, the absolute change due to characteristics is basically the same in the two specifications. Nonetheless, the above comparison does not take into account the fact that the explanatory power of the two models is different. A better appraisal of the magnitude of the two components requires controlling for this feature. To overcome the problem panel B compares the contributions of the two components to the total change in explained variance. The differences between the two models emerge strongly: in the FU model the relative contribution of the change in characteristics is only slightly higher than that of the change in prices. A completely different picture emerges when looking at the FR model: here the change in characteristics accounts for about three quarters of the total change in explained variance. This evidence shows how misleading not controlling for firm effects can be: a researcher using a cross section of wages that does not allow to control for firm specific prices would conclude that most of the explained change is due to changes in the distribution of the individual characteristics; prices would play only a minor role.

These comparisons show that allowing for firm specific prices changes the interpretation of the observed change in inequality. Yet, we have so far not disentangled the effect of the variability of these prices from the fact that they are estimated without some of the biases embodied in the estimates obtained from a FR model. To disentangle the two effects, we build an alternative counterfactual wage using the cross-sectional average estimated coefficients in the FU model, $w(X_Z, \bar{\beta}_{FU,Y}) = X_Z\bar{\beta}_{FU,Y}$ where $\bar{\beta}_{FU,Y}$ is the cross-sectional average of the coefficients estimated with model FU in year Y and X_Z is the matrix of characteristics as of year Z.

Table 5 reports explained and counterfactual variances of log weekly real earnings obtained from the FR and the FU models and those obtained using the average of the coefficients estimated with the FU model as described above. The latter values thus do not include the cross-sectional variability of the firm-level coefficients and allow us to assess how much this variability contributes to the overall explained dispersion. The first thing to notice is that, while in 1980 it is basically only the dispersion in estimated coefficients that explains the differences between explained FR and FU variances, it is no longer so by 1997, when most of the difference is due to the biases implied by the FR model which can be controlled for using the FU specification.

Second, when looking at counterfactual explained variances one again sees the consequences of the estimation biases on the interpretation. Using average coefficients, the increase in explained variance due to the change of average prices is above 60 percent (from 0.0602 to 0.0974), a value much above the 13.9 percent increase one would recover using a FR model.

Third, holding the distribution of characteristics fixed at 1980, the dispersion of 1997 prices implies an increase in the variance of counterfactual earnings of about 10 percent (from 0.0974 to 0.1073). Finally, since the explained variances obtained with the FR model are fully comparable with those obtained using average estimated coefficients, one establishes that unexplained inequality is now less of an issue.

8 Conclusions

In this paper we investigate the importance of firm characteristics in explaining changes over time in the variance of Italian wages. Two sources of firm heterogeneity are modeled: a time-varying unobserved characteristic and the specific reward to observed characteristics of workers. In this respect, this paper is an attempt to bridge the literature on wage determinants that exploits matched employers-employees data with the research on changes in wage inequality over time. We find two basic results. First, unsurprisingly, the more flexible model (with heterogeneous reward across firms) allows us to significantly improve the overall fit of the actual wage distribution and to achieve a better identification of wage determinants. Secondly, and less obviously, overlooking firm heterogeneity can distort our understanding of the causes of the evolution of the wage distribution. In the Italian case, the decomposition based on a standard Mincerian equation attributes two thirds of the total change in wage dispersion between 1980 and 1997 to modifications in the characteristics of workers and only one third to variations in their reward. By contrast, characteristic and price effects contribute equally to total change in wage inequality when we use the richer specification. Most of the difference depends on the bias that affects the average rewards estimated in the restricted model.

Further research should shed light on the causes of the observed price heterogeneity. We plan to pursue this task by exploiting the information on firm characteristics available in the Survey of Manufacturing Firms of the Bank of Italy to link these firm-level prices to firm features. We would therefore be able to further explain how much of the change in inequality is due to structural processes affecting the Italian economy such as the downsizing of manufacturing businesses, capital deepening, markets liberalization, etc. Moreover, one would like to assess how firm demographics reflect into the earnings distribution. For example, one might think that new-born firms enter at the highest of productivity or adopt most recent technologies. How this influences earnings might be relevant.

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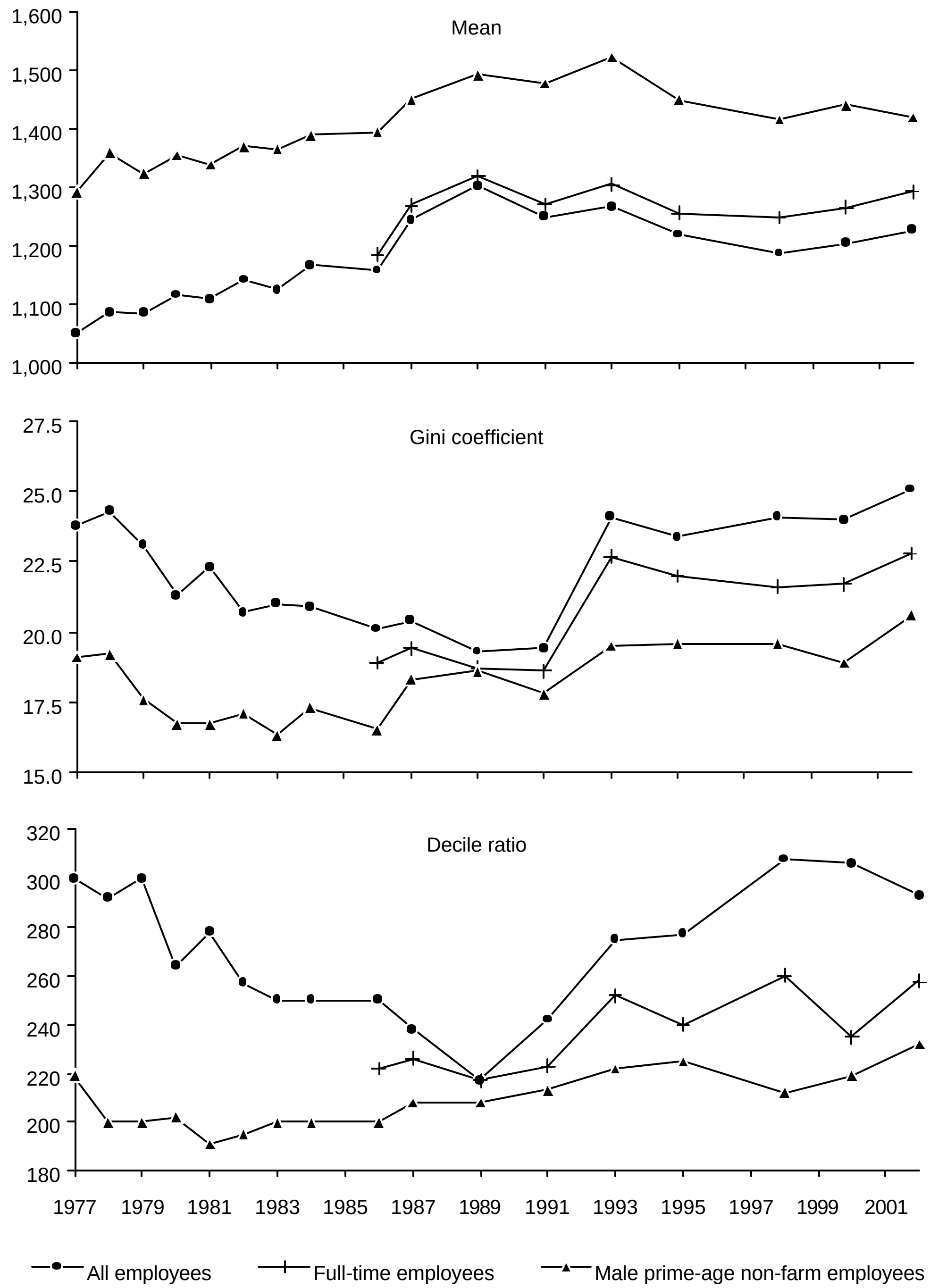
Figure 1

DISTRIBUTION OF REAL MONTHLY NET EARNINGS, 1977-2002

(euro at 2002 prices and per cent)



Source: authors' elaboration on data from SHIW-HA (Release 3.0, January 2004).

Fig. 2

VARIANCE OF LOG EARNINGS.

Fig. 3

ACTUAL DENSITIES OF LOG EARNINGS, 1980 AND 1997.

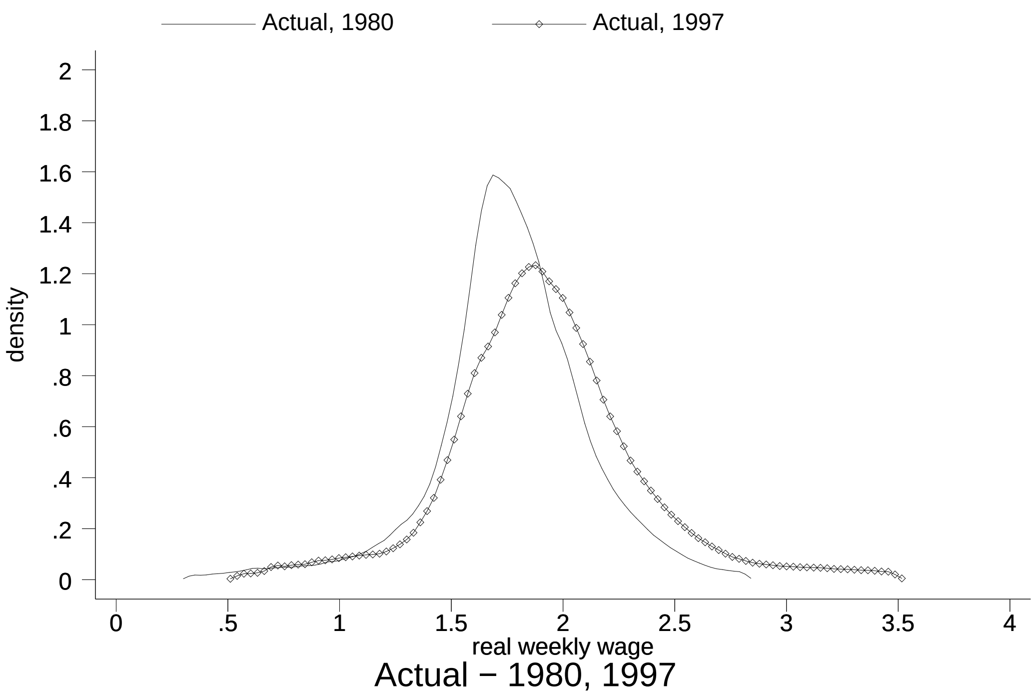


Figure 4

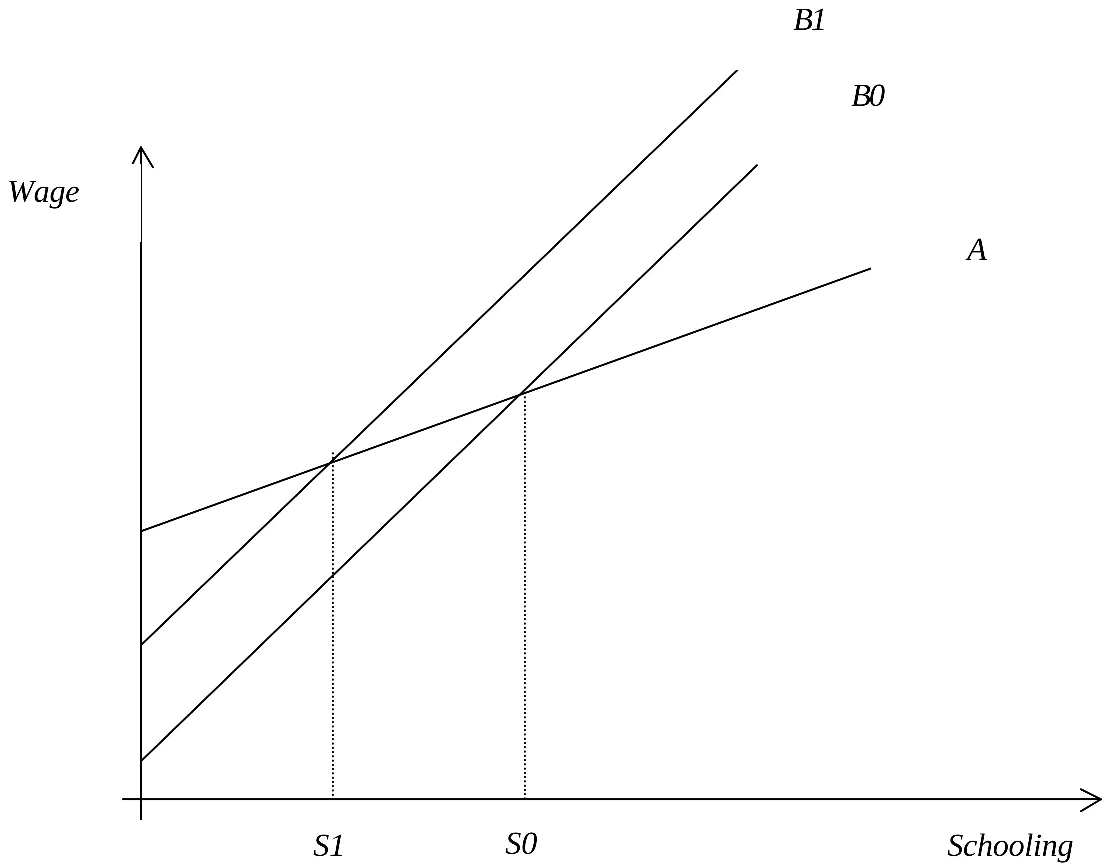


Figure 5: Overall variance.

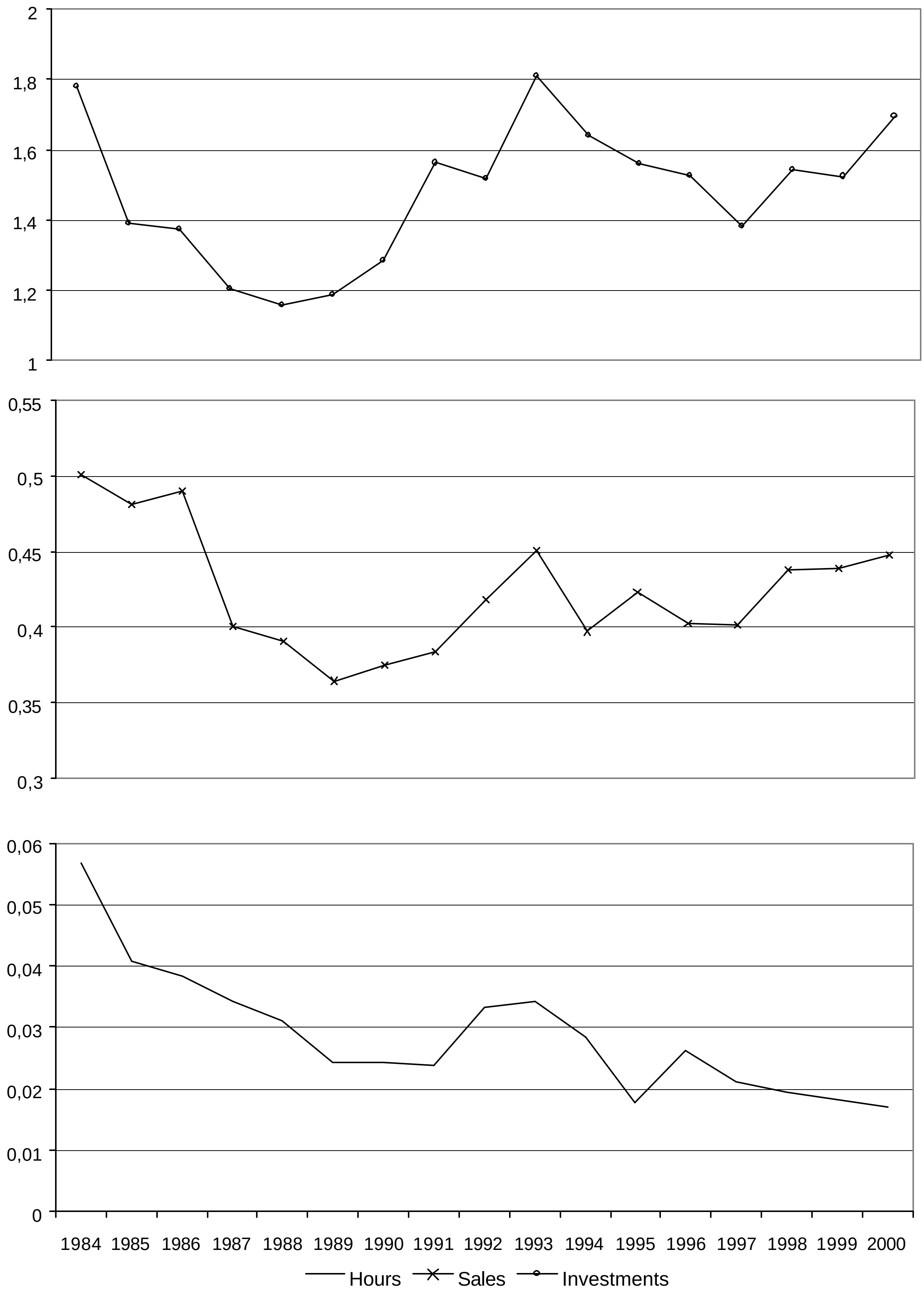


Figure 6: Share of unexplained variance.

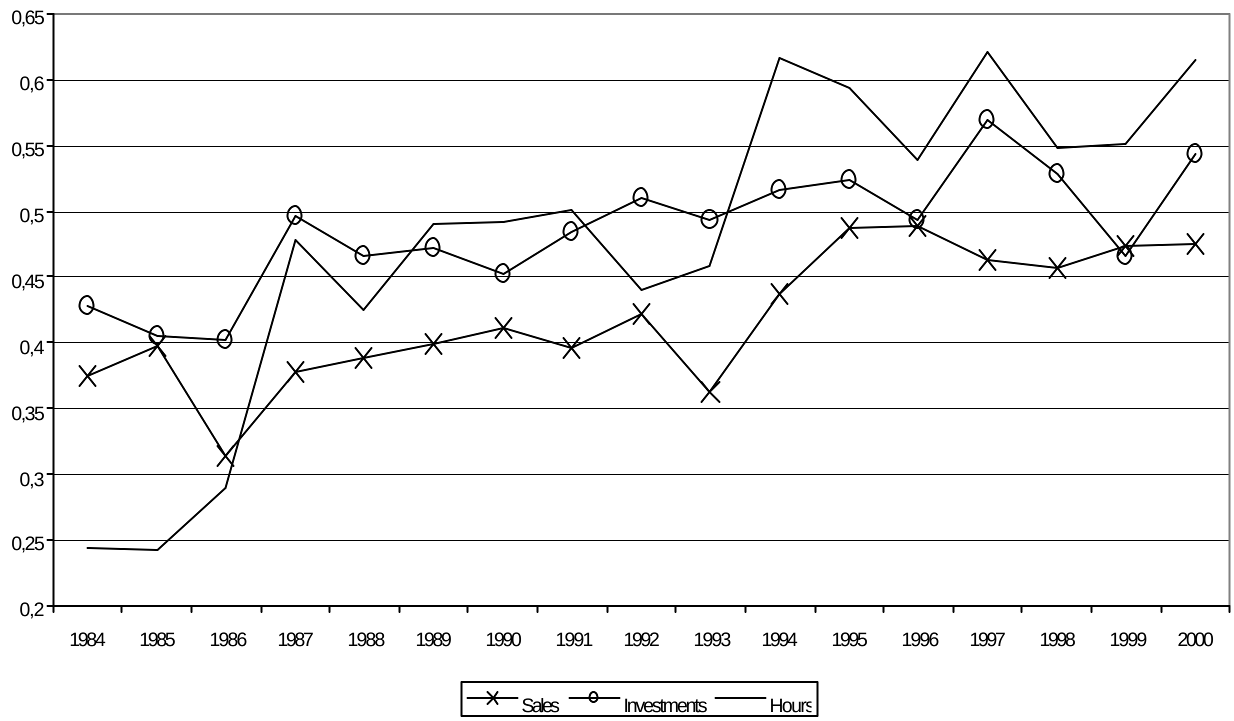


Fig. 7

CROSS-SECTIONAL DENSITIES OF ESTIMATED COEFFICENTS.

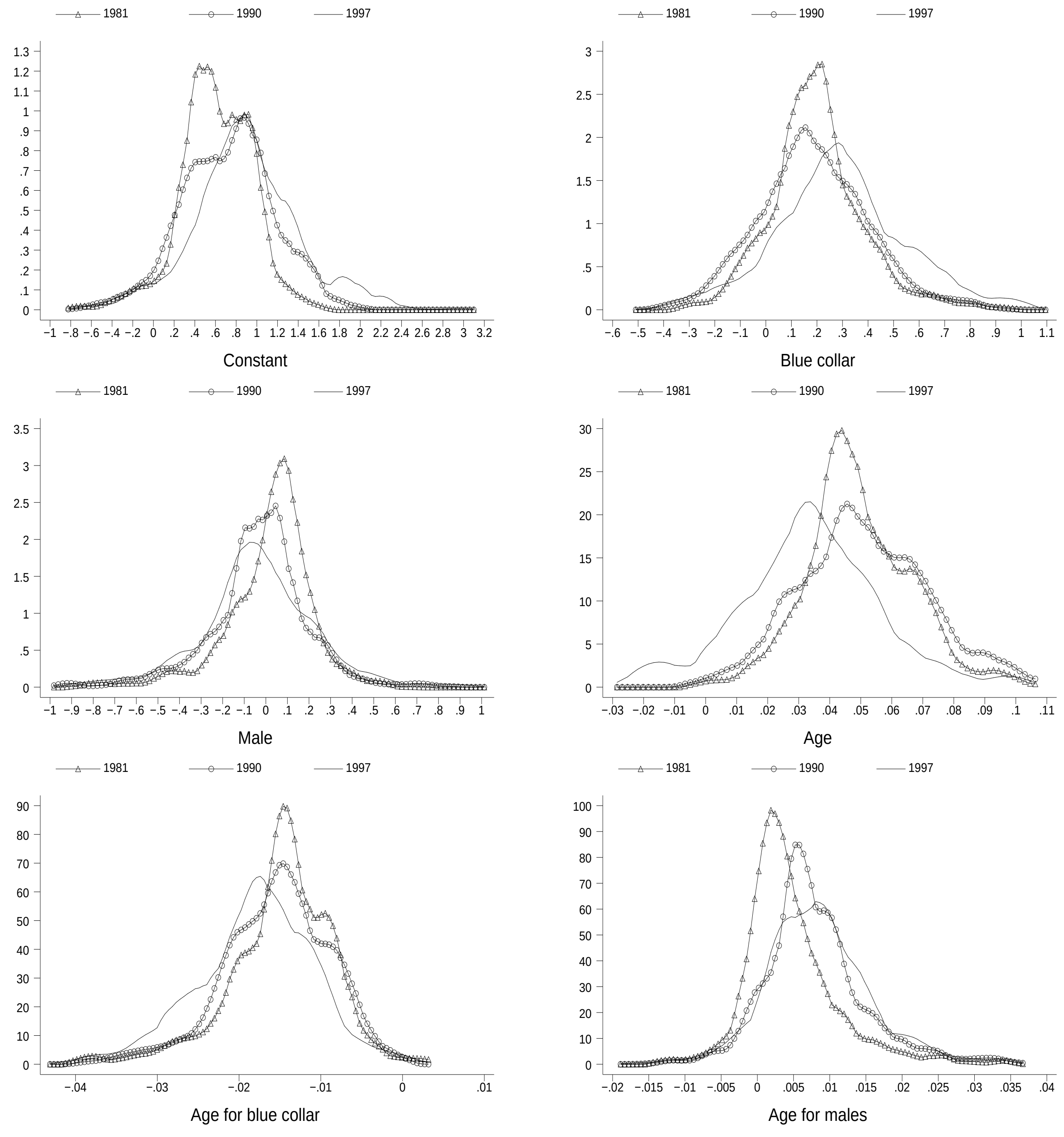
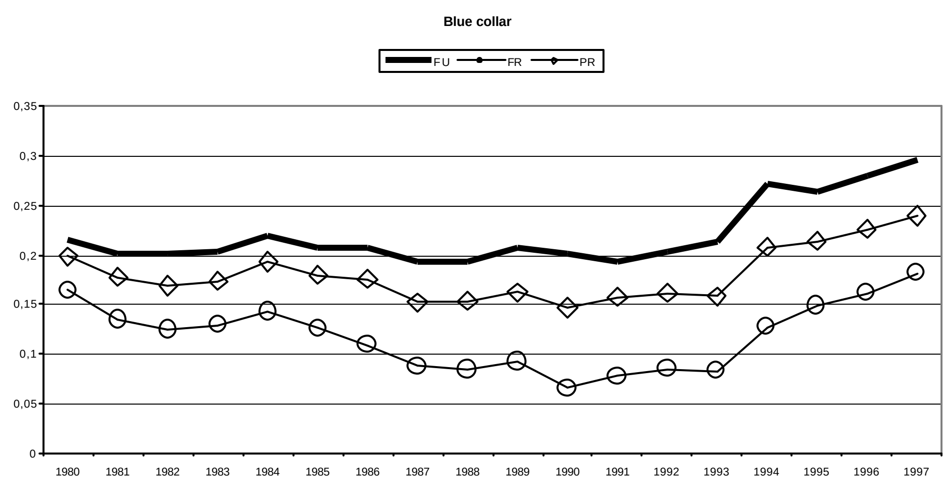
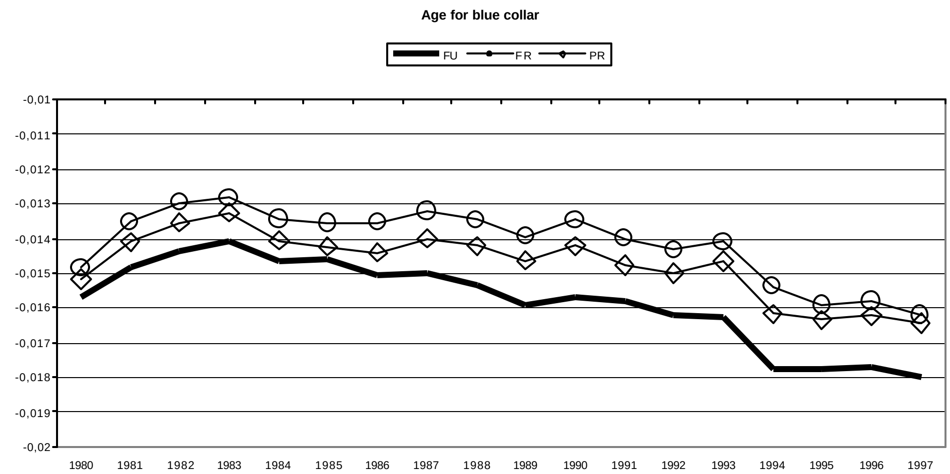


Fig. 8

EVOLUTION OF ESTIMATED COEFFICENTS




Age for blue collar
FU
FR
PR
-0,01
-0,011
-0,012
-0,013
-0,014
-0,015
-0,016
-0,017
-0,018
-0,019
-0,02
1980 1981 1982 1983 1984 1985 1986 1987 1988 1989 1990 1991 1992 1993 1994 1995 1996 1997

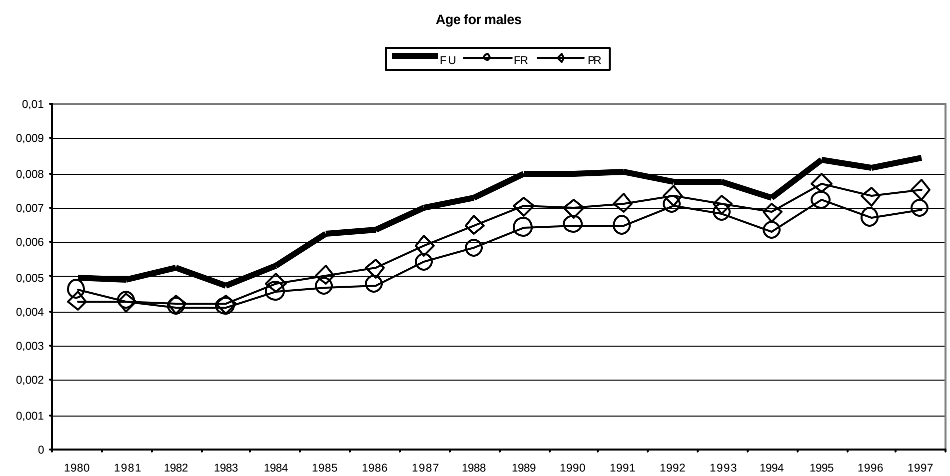

Age for males
FU
FR
PR
0,01
0,009
0,008
0,007
0,006
0,005
0,004
0,003
0,002
0,001
0
1980 1981 1982 1983 1984 1985 1986 1987 1988 1989 1990 1991 1992 1993 1994 1995 1996 1997

Table 1

LIKELIHOOD RATIOS AND P-VALUES

	FULLY RESTRICTED			PARTIALLY RESTRICTED		
	LR	*DOF*	*CHI2*	*LR*	*DOF*	*CHI2*
1980	29990,24	15280	0,0000	14333,62	13752	0,0003
1981	25759,93	15310	0,0000	13223,14	13779	0,9997
1982	28445,3	15300	0,0000	15321,49	13770	0,0000
1983	33833,73	15340	0,0000	20492,53	13806	0,0000
1984	35626,36	15400	0,0000	21239,63	13860	0,0000
1985	43826,88	15380	0,0000	28025,35	13842	0,0000
1986	37841,18	15400	0,0000	23090,87	13860	0,0000
1987	33671,65	15370	0,0000	20308,44	13833	0,0000
1988	29729,09	15370	0,0000	16375,42	13833	0,0000
1989	32980,7	15400	0,0000	18307,61	13860	0,0000
1990	31459,92	14760	0,0000	15721,86	13284	0,0000
1991	35560,13	15410	0,0000	18827,05	13869	0,0000
1992	37728,52	15390	0,0000	19304,8	13851	0,0000
1993	36227,8	15410	0,0000	17912,6	13869	0,0000
1994	40678,65	15380	0,0000	21523,57	13842	0,0000
1995	30155,22	15390	0,0000	13643,21	13851	0,8945
1996	32122,42	15390	0,0000	14827,46	13851	0,0000
1997	32567,99	15350	0,0000	15404,85	13815	0,0000
Total (*)	608205,8	276030	0,0000	327883,5	248427	0,0000

(*) Restricted model jointly estimated with all coefficients interacted with year dummies.

Table 2

CONFIDENCE INTERVALS:
SHARE OF FIRMS WHOSE 95% CONFIDENCE BANDS INCLUDE THE COEFFICENT ESTIMATED IN THE RESTRICTED MODEL.

	FULLY RESTRICTED									PARTIALLY RESTRICTED								
	Blue collar	Male * Age	Age	Age^2	Blue *Age	Week	Break-up	Mover	Male	Blue collar	Male *Age	Age	Age^2	Blue *Age	Week	Break-up	Mover	Male
1980	0,42	0,59	0,40	0,39	0,39	0,30	0,35	0,79	0,63	0,51	0,58	0,41	0,39	0,41	0,32	0,39	0,71	0,63
1981	0,48	0,52	0,42	0,40	0,35	0,31	0,36	0,75	0,58	0,46	0,52	0,41	0,40	0,45	0,30	0,35	0,77	0,56
1982	0,41	0,54	0,39	0,41	0,38	0,23	0,28	0,71	0,53	0,55	0,56	0,40	0,38	0,44	0,24	0,34	0,74	0,59
1983	0,49	0,63	0,38	0,38	0,46	0,24	0,38	0,79	0,60	0,49	0,67	0,42	0,40	0,44	0,24	0,34	0,75	0,64
1984	0,43	0,61	0,37	0,37	0,42	0,24	0,39	0,77	0,59	0,50	0,59	0,37	0,39	0,45	0,24	0,39	0,80	0,58
1985	0,43	0,59	0,39	0,40	0,40	0,19	0,33	0,76	0,60	0,51	0,59	0,42	0,40	0,45	0,13	0,30	0,73	0,61
1986	0,38	0,59	0,41	0,39	0,40	0,26	0,37	0,72	0,58	0,50	0,59	0,38	0,40	0,44	0,21	0,36	0,76	0,60
1987	0,44	0,61	0,38	0,38	0,38	0,27	0,41	0,79	0,61	0,50	0,57	0,37	0,40	0,47	0,25	0,40	0,77	0,57
1988	0,42	0,59	0,41	0,38	0,38	0,27	0,39	0,76	0,62	0,53	0,56	0,41	0,39	0,44	0,27	0,43	0,72	0,64
1989	0,43	0,58	0,44	0,39	0,39	0,30	0,43	0,76	0,64	0,46	0,61	0,45	0,40	0,45	0,30	0,45	0,77	0,66
1990	0,35	0,56	0,41	0,40	0,33	0,35	0,42	0,78	0,63	0,50	0,52	0,41	0,40	0,45	0,35	0,44	0,80	0,65
1991	0,37	0,58	0,48	0,43	0,38	0,34	0,41	0,71	0,65	0,48	0,55	0,48	0,43	0,44	0,34	0,42	0,74	0,65
1992	0,41	0,62	0,47	0,46	0,37	0,38	0,39	0,73	0,60	0,46	0,61	0,47	0,47	0,41	0,37	0,40	0,79	0,63
1993	0,42	0,60	0,47	0,51	0,41	0,37	0,41	0,75	0,57	0,46	0,57	0,51	0,50	0,40	0,34	0,45	0,72	0,60
1994	0,44	0,58	0,47	0,49	0,41	0,35	0,43	0,78	0,63	0,52	0,59	0,49	0,49	0,46	0,34	0,43	0,75	0,63
1995	0,44	0,61	0,54	0,53	0,48	0,37	0,51	0,72	0,60	0,56	0,64	0,55	0,55	0,50	0,36	0,51	0,78	0,65
1996	0,44	0,59	0,57	0,54	0,43	0,39	0,45	0,71	0,59	0,58	0,65	0,58	0,54	0,51	0,39	0,45	0,78	0,67
1997	0,44	0,63	0,60	0,56	0,48	0,35	0,44	0,78	0,61	0,57	0,64	0,59	0,56	0,52	0,36	0,46	0,79	0,65
Total	0,42	0,59	0,44	0,43	0,40	0,30	0,40	0,75	0,60	0,51	0,59	0,45	0,43	0,45	0,30	0,40	0,76	0,62

Table 3

ACTUAL AND EXPLAINED VARIANCES OF EARNINGS.

	1980	1997	% change
		Actual	
	0,174	0,252	44,8
		Explained	
FU	0,0883	0,1296	46,7
FR	0,0648	0,0972	50,0

Table 4

DECOMPOSITION OF THE CHANGE IN EXPLAINED VARIANCE.

	Change due to:	
	Characteristics	Prices
	A. Absolute change	
FU	0,022	0,019
FR	0,023	0,009
	B. Contribution to % change in explained variance	
FU	25,2	21,5
FR	36,1	13,9

Table 5

EXPLAINED AND COUNTERFACTUAL VARIANCES.

	1980	Counterfactual	1997
FR	0,0648	0,0738	0,0972
FU average coefficients	0,0602	0,0974	0,1237
FU	0,0883	0,1073	0,1296