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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. We show that the exemption raised high school graduation rates of boys by more than 2 percentage points by comparing high school graduation rates of young exempt men and older not exempt men from the least damaged areas and men of the same age groups from nearby towns that were not hit by the quake. Similar comparisons show that graduation rates of young women in the affected areas rose by about 2 percentage points. Since in Italy women are not subject to drafting, we interpret these findings as evidence of social effects of the decision of teenage boys of staying longer in school on that of teenage girls. Our estimates suggest that an increase of 1 percentage point of male graduation rates raises female probability of completing high school by about 0.7-0.8 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.

JEL classification: I21, C90, C23.
Keywords: social interactions, peer effects, schooling, compulsory military service.

Contents

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1 Introduction[1]

The role of peers for individual schooling performances is a widely investigated issue (Hoxby (2000), Angrist and Lang (2004), Hanushek, Kain, Markman and Rivkin (2001), Sacerdote (2001), Zimmerman (2003), Kremer, Miguel and Thornton (2004))[2]. The existence of peer effects has implications for the correct assessment of a number of issues: competition among private and public schools, school-finance and control systems[3], ability tracking[4], desegregation programs such as Metco, Moving to Opportunity, etc.[5].

We contribute to the literature on peer effects by providing evidence on the causal effect of the schooling achievement of young men on those of young women. Identification hinges on an exogenous shock to the probability of boys of graduating from high school. We exploit the exemption from compulsory military service (CMS) granted to few specific cohorts of males living in southern Italy as a result of an earthquake hitting this area in

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[2]A growing body of empirical literature has documented 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).

[3]Epple and Romano (1998), Angrist, Bettinger, Bloom, King and Kremer (2002), Hsieh and Urquiola (2003), Winston and Zimmerman (2003).

[4]Epple, Newlon and Romano (2002), Figlio and Page (2002), Hanushek and Woessmann (2005).

[5]Katz et al. (2001), Ludwig, Duncan and Hirschfield (2001), Angrist and Lang (2004).

1980. The exemption is shown to have increased boys' high school graduation rates by more than 2 percentage points. Graduation rates of girls belonging to the same cohorts as exempt boys increased by about 2 percentage points. Since in Italy women are not subject to military draft we argue that the change in their schooling achievements is the reaction to the schooling behavior of exempt males. Our estimates suggest these peer effects are rather strong: a one percentage point increase in males' graduation rates raises females' by 0.7-0.8 percentage points.

Peer effects may stem either from strategic complementarities in outcomes or from externalities based on individual exogenous characteristics[6]. The baseline empirical model of peer effects typically assumes individual outcomes are affected by average peer characteristics (e.g. ability) or outcomes in a linear fashion. In this class of models, the policy implications are different depending on the source of peer effects[7]. On the one hand, peer outcomes can be directly manipulated by a policy maker thereby improving aggregate performance and achieving overall efficiency gains. On the other, peer characteristics are given and can only be redistributed across groups. This implies any increase of someone's average peer quality comes at the cost of a reduction of the same amount of someone else's

[6]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 (Manski (1993) and Moffitt (2001) discuss the difficulties involved in identifying such external effects). These may be in turn grouped into two main categories, that Manski (1993) defined as *contextual* and *endogenous* effects. On the one hand, these 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)).

[7]See Hoxby (2000) for a discussion of the issue.

average peer quality thereby leaving unchanged their overall average outcome. However, the previous literature has been unable to disentangle these two sources because peer outcomes are generally instrumented with group composition (Sacerdote (2001), Zimmerman (2003), Hoxby (2000), Angrist and Lang (2004), Hanushek et al. (2001))[8]. In our setting the underlying exogenous characteristics of the group of peers (the boys) are unlikely to be modified by the instrument, thereby ensuring that we identify the response of girls' schooling outcomes to boys' schooling outcomes rather than to boys' characteristics.

Our results are based on comparisons of the schooling attainments of exempt and not exempt cohorts belonging to the least damaged towns and those of comparable cohorts in nearby towns not hit by the quake. The identification strategy is similar in spirit to those exploited in recent works by Duflo and Saez (2003), Miguel and Kremer (2004) and Kremer et al. (2004). In these works, identification of the strength of peer effects is achieved by means of a partial-population intervention, that is by studying how the treatment status of well-identified individuals in a given group affects outcomes of untreated individuals in the same group. 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. Identification is based on a randomized school-based mass treatment with deworming drugs. They show that the deworming program

[8] 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 review of the different approaches.

substantially improved outcomes (school attendance and health status) of untreated children. Kremer et al. (2004) is closest to our paper. They evaluate the effects of a merit scholarship program on adolescent girls' academic exam scores. They find girls' scores increased significantly in schools randomly selected for the program; positive effects of the program are also found on test scores of boys, ineligible for the awards, and of lower achieving girls, unlikely to win the award.

We also contribute to the literature that quantifies 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 (2001) address an issue similar to ours showing that draft avoidance behaviors increased college enrollment and graduation rates of potential draftees in the Vietnam-era suggesting that absent conscription college enrollment rates would be lower. This is only apparently in contrast with our findings since, differently from ours, in their setting serving in the military would generally imply being sent to war.

An approximate idea of the potential aggregate impact of such peer effects in terms of per capita GDP can be obtained from recent OECD estimates of the elasticity of steady-state per capita GDP to average years of education (OECD (2003)). Our results suggest 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 male schooling on girls' would underestimate the increase by about as much as one tenth of a percentage point. Our findings 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), Lleras-Muney (2005)).

The paper proceeds as follows. The next section illustrates the institutional setting: we briefly describe how CMS works in Italy, motivate why the exemption from CMS modifies male teenagers' schooling choices and review the interventions undertaken in the aftermath of the quake. Section (3) describes the research design and provides elements in favor of the main identifying assumptions. We then present the main results of the paper and provide additional robustness checks. Section (6) concludes.

2 The institutional setting

Compulsory military service and schooling choices

Until recently military service was an obligation for all Italian males[9]. In the early eighties, the period we are interested in, the length of the service was twelve months[10]. All boys would undergo a thorough medical assessment administered by military authorities in the year they turned 18 to establish their suitability for military service. Upon passing these medical exams boys would be inducted as they turned 19[11]. Therefore, the medical

[9] Women were not allowed to enter military corps, not even on a voluntary basis. In recent years the obligation has been canceled and military service has become a voluntary service open to both sexes.

[10] Service would last 18 months if drafted in the Navy. In the late '90s compulsory service was shortened to 10 months.

[11] 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. Military service could as well 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 file a request for alternative service and after some months he would be called before a military commission to motivate and defend his request.

examinations were typically taken during the last year of high school and teenagers were drafted shortly after[12]. However, one-year deferments could be obtained under specific circumstances by filing a motivated request. 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[13].

Although CMS is structured in such a way as not to interfere with graduation from high school it may nonetheless be an impediment and affect the choices of those who are behind schedule, as they need to ask for deferment. Therefore we expect that the exemption unexpectedly granted after the quake raised the probability of affected individuals of completing high school. Let us formally describe the alternatives available to someone 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). 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, $U_0 = M + \sum_0^{T-1} w_t^0$, and the value of deferring service

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

[13] 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).

one year, serving and then entering the labor market, $U_1 = S + M + \sum_0^{T-2} w_t^1$, 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[14]. 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 alternative setting where there is no obligation to serve. In this simple framework this additional year can be spent either in school or in the labor market. To see how 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, embeds all the previous labor market experience. Provided the returns to an additional year of education are higher than those to an additional year of labor market experience ($w_{T-1}^1 > w_T^0$), absent CMS the incentives to stay in school increase[15].

The earthquake-related interventions

Figure (1) shows the area of southern Italy hit by a major earthquake in November 1980; the area hosted more than 5 million people, about 10 percent of the Italian population, spread over 650 towns. Shortly after the quake Parliament precisely defined the area to be considered as damaged and to be targeted by relief measures; they also defined the amount

[14]We neglect discounting to keep notation simple. The basic argument still holds. In particular, under mild conditions it can be shown that the incentive to stay in school increases provided the difference between returns to schooling and those to experience is larger than the discount rate. We also assume for simplicity that there is no retirement decision.

[15]It could be argued that 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 Holland service might be deferred because one expects a mass exemption (Imbens and van der Klaauw (1995)). In Italy, as we have detailed above, military service was compulsory and high school students had to serve by the age of 22. Therefore, deferring the draft would not increase the probability of not serving or of being included in mass exemptions. 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 and peace-keeping missions (Kosovo, Afghanistan, Somalia, Iraq, etc.) are generally run by volunteers and professionals.

and guidelines for the assignment of the funds allocated to this purpose[16]. Entitlement to financial aids depended on the magnitude of the damages suffered as certified by the authorities; no money accrued to municipalities where no family had suffered damage.

Parliament also passed a set of laws that modified and eventually canceled the obligation to serve in the military for all males born before 1966 who were living in the relevant area as of November 1980[17]. Although the exemption was eventually granted to all males born before 1966, it had an uneven impact on the high school graduation rate of 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 exemption while they were still in school. Therefore most of the effect of the exemption should be detected in the high school graduation rate of students born between 1963 and 1965.

[16]An amount of about 12 billion dollars at 2003 prices and exchange rate (roughly 17 percent of the 1980 GDP of the area hit by the quake) was budgeted for recovery over the period 1981-1983. 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.

[17]Law No. 219/81 passed on May 14th 1981 gave to all males born between 1963 and 1965 the opportunity to comply with their military obligations by serving as civilians in alternative services active in the earthquake region; law No. 187/82 passed on April 29th 1982 completely removed the obligation to serve in the army for all males born before 1964; law No. 80/84 passed on April 18th 1984 extended the military exemption to males born before 1966 (Cipollone and Rosolia (2004)).

3 The research design

Identification of the causal effect of male high school graduation rates on the probability of girls completing high school requires that the exogenous shifter of boys' schooling attainments be not a direct determinant of women' success in graduating. Exemption from CMS is a candidate instrument: while affecting the incentives to stay in school for males, it does not directly enter females' choices since they are not subject to drafting.

However, two features of the exemption might hamper its validity as an instrument. First, it was caused by the 1980 earthquake, which generally led to disruptions of economic life and government interventions that potentially altered individual schooling attainments[18]. In this case the exemption would not be a valid instrument because of the correlation with a shock that directly affected the schooling of both males and females. This is a serious concern because in many towns the quake was quite disruptive. For example, according to the official damage assessment there were around 2,000 deaths, mostly in the epicentre, 10,000 injured and around 300,000 people in need of shelter; 60 percent of the houses in the epicentre were destroyed or severely damaged and 33 percent required structural intervention to restore habitability; outside the epicentre there were less damages but still 20 percent of the houses was inhabitable and structural intervention was required for another 30 percent. These direct shocks along with the subsequent income transfers and other quake-induced relief measures probably had a direct impact on the choice or the possibility of staying in school of the younger cohorts. Second, the exemption is age-specific so that any other cohort-specific shock to schooling outcomes

[18]For example, children may have not attended school for some time or households may have received income transfers.

may act as a confounding factor.

To break the correlation between the exemption from CMS and other quake-related shocks that might directly affect individual schooling we limit our attention to towns that, albeit targeted by quake-related interventions, were the least affected by the earthquake according to the official evaluations performed by the government in the aftermath (henceforth, treated towns). Treated towns in our sample therefore only include those located at the boundary of the earthquake area as defined by the government, the farthest away from the epicenter, but still in the area exempt from military service. There are 57 of these towns, with a population of about 300,000 people at the end of 1979; 18 out of 57 towns, although included in the area officially involved in the quake, recorded no damage; 15 towns ranked at the very lowest level of the damage scale, meaning only very mild and limited damage was suffered; the next 15 towns ranked below the median damage score and the remaining ones were slightly above[19]. As a whole, treated towns were largely unaffected by the earthquake and therefore by quake-related interventions other than the age-sex specific exemption.

To control for cohort specific shocks we compare the evolution of cohort schooling attainments in treated towns with that of schooling attainments of comparable cohorts in towns just outside the earthquake region and neighbouring on at least one treated town (henceforth, control towns). This rule selects 60 more towns, with a total population of about 600,000 people at the end of 1979. The final sample therefore includes 117 towns belonging to 12 provinces of 5 regions with a total population of about 900,000 people at the end of 1979 (Figure 2).

[19] Additional details are provided in Cipollone and Rosolia (2004).

The upper panel of figure (3) reports the share of males who completed high school in a given cohort in treated and control towns. Data are drawn from the 1991 population census, 11 years after the quake. Therefore the youngest cohort potentially affected by the exemption from CMS is aged 26 and far beyond high school. Cohorts not involved in the exemption (born earlier than 1962) display similar schooling levels in treated and control towns. The share of high school graduates rises steadily from about 20 percent up to 35 percent for those until the cohort born in 1955. The rise comes to a halt for people born between 1961 and 1955. The two time series diverge clearly for younger cohorts, born between 1963 and 1965. While young men in control towns display more or less the same achievement as older cohorts, those in treated towns, exempt from CMS, display a strong increase in high school graduation rates, from about 34 to 38 percent. Turning to women, except for a stronger trend, the pattern closely tracks that for males: older women graduate from high school at the same rate in treated and control towns. But a larger share of women born between 1963 and 1965 graduated from high school in towns where men were exempted from CMS.

Identification of the effects of the exemption requires that any direct effect of the earthquake on individual schooling was the same across the two groups of towns. Geographic proximity supports this assumption. 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. However, although the direct effects of the earthquake may be reasonably assumed to be the same on the grounds of geographic proximity, only the officially listed towns qualified for government intervention. Interventions were linked to the official damage index, and treated towns were generally at the low end of the damage scale, thus at best qualifying only for limited help. To allay concerns that the main findings are driven by some omitted intervention, robustness checks will also be performed on the subset of exempt towns that did not record any damage whatsoever or were at the very bottom of the official damage scale and their neighboring towns.

Geographical proximity also implies that treated and control towns are embedded in the same economic environment, so that any market interaction that could possibly bias the results is taken care of. For example, since these individuals work in broadly the same labor markets, any cohort-sex (possibly quake-related) specific labor market shock is controlled for. Thus, if the earthquake brought about an increase in the high school wage premium in the local market (for example, because a more skilled labor force was needed for reconstruction) this would have affected in the same way the choices of comparable individuals in the control towns. 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 individuals in neighboring towns may access the same set of colleges.

Table (1) compares several characteristics of treated and control towns. Treated towns turn out to be higher on the sea level, less densely inhabited and smaller. In the econometric specification these features are picked up by town fixed effects; however, robustness checks will also be conducted on samples ridden of the larger towns driving the above differences. Pre-treatment migration behaviour is not statistically different between treated and control towns, but treated towns display slightly lower birth and higher death rates, the net effect being a lower population growth rate in treated towns.[20]. This may partly explain the higher average age found in treated towns; the average family size also turns out to be very similar. As concerns the population older than 15, described in panel C of the table, educational attainments, employment and unemployment rates are the same; individuals in treated towns are however more likely to be self-employed[21]. While there seem to be no differences in terms of education and labor market status when considering town population as a whole, one would like to make sure that the same holds true for the parents of the cohorts underlying the analysis. The sample underlying table (2) is all individuals that, in the 1981 population census where living in any of the towns in the sample and had a child of age between 16 and 25, the cohorts we focus upon. While there is no guarantee that these are the specific parents of the individuals in our final sample, previous evidence on pre-treatment mobility flows mitigates concern that between quake and the subsequent census major and differential changes in the population of treated and control towns had taken place[22]. The table reports results of regressions of parents employment, unemployment and self-employeemnt rates as well as their educational attainments on a set of dummies for the age of the child and their interaction with the treatment dummy. While in some cases these differences are never significant, in others

[20]To test that these differences are systematic over time we regressed birth and death rates and population inflow and outflow rates for the available pre-treatment years (1972-1980) on a full set of time dummies and their interactions with the treatment dummy and tested that all interacted coefficients are equal. The tests never reject the null.

[21]Data are drawn from the 1981 census. This is the only data source close enough in time to treatment date with information on population characteristics. Although strictly speaking the census is run after treatment the research design makes us confident that no major changes were brought about by the quake. Moreover, while the census was run in October 1981, the bulk of the laws canceling the obligation to serve were passed after that date.

[22]We also verified mobility from the 1981 census; we regressed a dummy equal to one if child birthplace was different from residence in 1976 (as reported in a recall question in the census) on dummies for age and a dummy for the quake and found small and not statistically significant differences for the relevant cohorts. Results were basically the same when looking at 1981 residence.

they turn out to be. However, F-tests that these differences in parental characteristics are constant across cohorts never rejecet the null. Once again, town fixed effects should account for this heterogeneity. Still, our specifications will include these characteristics in the information set.

Several authors have shown the importance of class size, pupils-teacher ratios and in general per-capita resources devoted to schooling for individual school outcomes (Card and Krueger (1992), Angrist and Lavy (1999), Krueger (1999), Krueger and Whitmore (2001))[23]. Table (3) reports the average class size in control and treated towns in primary, secondary and high school for the available pre-treatment school years. Statistically significant differences in primary and secondary schools signal smaller classes in treated towns. However, in both cases the null that these differences are constant over time cannot be rejected. In the econometric specification, town fixed effects absorb systematic differences in endowments across towns while (a quadratic of) cohort size will account for changing per-capita resources within town. However, since differences are concentrated in lower grades (primary and secondary schools), to dissipate doubts that the main findings are driven by differences in pre-treatment schooling attainments determined by different school environments, we will perform robustness checks that explicitly control for schooling attainments in secondary school.

To further ensure that the differences in school outcomes seen in figure (3) are due to the exemption and not to confounding factors affecting both boys' and girls' schooling we perform two additional falsification exercises. First, we assign treatment status to control towns and exemption status to cohorts 26-28 from those towns and compare schooling

[23]See Hanushek (2003) for a critical review of the main results in this literature.

achievements with those of towns also outside the quake region (henceforth, outer control towns; see figure (2)) and neighboring on control towns. Second, we assign exemption status to cohorts aged 30-35 in 1991 in treated towns (indeed our control cohorts) and compare their high school graduation rates across treated and control towns with that of older cohorts aged 36-40 in 1991. Our results turn out to be robust to both exercises: we find no difference in the schooling achievements of the fake treatment groups.

4 Results

We are interested in assessing the effect of male high school graduation rates on the probability of a girl belonging to the same group graduating from high school:

$$y^F_{icj} = \gamma_F x_{icj} + \theta y^M_{cj} + \mu^F_j + \nu^F_c + \epsilon_{icj} \tag{1}$$

where $y^F_{icj} = 1$ if girl i of cohort c in town j graduated and zero otherwise; x_{icj} are individual controls; y^M_{cj} is the share of males with at least a high school degree in cohort c town j; μ^F_j and ν^F_c are, respectively a town and a cohort fixed effect and ϵ_{icj} an i.i.d. shock.

The model determining girls' school attainment laid out in equation (1) rests on the assumption that the relevant group for the kind of interaction effect we are after (θ) is the set of people belonging to the same cohort and born in the same town. In our sample these are small groups, because sampled towns are generally small[24]. The size of the median group in the sample is 45, only about twice the average high school class size (see table (3)). The small town size implies that these people have most likely known

[24] 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.

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 secondary 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, secondary, 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[25]. In this respect, this definition of group seems to be able to capture productive externalities at work in the classroom as well as role models and information transmitted by peers other than classmates[26].

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[27]. 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 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 (for example, they take a walk down to the main square); they also go out with definite purposes: eat

[25]Additionally, in Italy driver's licenses can be obtained only after turning 18 so that geographic mobilty of Italian teenagers is often very limited, more so in the area and years relevant for this paper, thus reinforcing the assumption that most of one's social life at this age takes place in one's hometown

[26]The definition of the relevant group 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.

[27]This survey is similar to the Canadian General Social Survey.

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 Southern Italian girls and 34 percent of boys spend their time equally with boys and girls[28].

We draw data on completed schooling from the 1991 population census. Ideally, the sample should include individuals living as of November 1980 in any of the 117 selected towns. However, the census only provides place of birth and place of residence at census date. We therefore proxy the place of residence in November 1980 with place of birth and select all the individuals born in any of the sampled towns. This amounts to assuming that the cohorts relevant to our analysis, aged 15 to 24 in 1980, were living in their towns of birth at the time of the earthquake[29]. Pre- and post-quake mobility could bias the results if the propensity to move were correlated with schooling attainment and differed between control and treated towns. For example, people from treated towns could have moved (possibly because of the quake) to towns with better schools and this would improve attainments of both boys and girls. However, aggregate inflows and outflows of treated and control towns are not significantly different over the 70s and 80s.

More in general, the 1991 census 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, labor market status, etc.). We

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

[29]Italian youths are known to live with their parents much longer than those of any other country (Manacorda and Moretti (forthcoming), Giuliano (2004), Becker, Bentolila, Fernandes and Ichino (2005)). In the eighties, also because of worse labor market conditions, this was even more common.

therefore look at the effect of boys' high school graduation rate on that of girls in the same group. Equation (1) then becomes:

$$y^F_{cj} = \gamma_F x^F_{cj} + \theta y^M_{cj} + \mu^F_j + \nu^F_c + \epsilon^F_{cj} \quad (2)$$

Equation (2) is estimated on cohorts born between 1956 and 1965 (age 26 to 35 in 1991) in one of the 117 sampled towns[30]. Exempt cohorts are those aged 26 to 28. Boys' high school graduation rate y^M_{cj} is instrumented with the exemption from CMS, a dummy equal to one if town j is in the quake region and cohort c is aged 26-28 in 1991.

Table (4) reports first stage and reduced form estimates of the effect of the exemption on boys' and girls' high school graduation rates and IV estimates of θ in equation (2) for several specifications of the information set. The first row corresponds to a baseline specification that only allows for cohort dummies and town fixed effects. First stage results show that the exemption raised boys' high school graduation rates by about 1.8 percentage points; even in this basic specification the effect is significant at the 5 percent level. Reduced form estimates show that girls of the same age living in the same towns as exempt boys also had a 1.25 percentage point higher probability of completing high school; the effect on girls is however less precisely estimated. The implied causal effect of boys' graduation rate on girls' is about 0.7, meaning that raising boys' completion rates by 1 percentage point increases girls' by around 0.7 percentage points. The Anderson-Rubin test rejects the null of a zero effect with a 9 percent significance[31].

The previous specification assumes all differences across towns are absorbed by town

[30] We exclude the cohort born in 1962 (age 29 in 1991). Their age when the relevant law was issued is such that it is hard to make assumptions on whether and how they were affected.

[31] The Anderson-Rubin test has the correct size when the model is just-identified independently of the strength of the first stage (Moreira (2001)).

fixed effects. The evidence discussed above shows that either differences across towns in parental characteristics and school endowments are not statistically significant or that they are systematic over time and cohorts. The next two rows of table (4) show that results are robust to the introduction of explicit controls for these sources of heterogeneity across towns and cohorts.

The second row of table (4) reports result for a specification that accounts for differences in school endowments across cohorts. While a large body of literature has shown class size is a determinant of individual school performance (Card and Krueger (1992), Angrist and Lavy (1999), Krueger (1999), Krueger and Whitmore (2001)) we cannot assign to each town-cohort a specific measure of class size since the available information is at town-grade level. Still, we can control for cohort size. Given the limited age interval spanned by the sample (9 subsequent cohorts) and the reasonably slow evolution of structural school endowments most of the relevant variability is cross-sectional and is thus captured by town fixed effects. Therefore, cohort size should proxy reasonably well for measures of school congestion or the availability of per-capita resources for a specific cohort over time. Indeed, Card and Lemieux (2000) find that larger cohorts have lower educational attainment, possibly because of supply effects. The information set is augmented with a second order polynomial in cohort size. First stage and reduced form results point to a stronger and more significant effect of the exemption: boys' graduation rates increased by 2.5 percentage points; girls' by 1.8 percentage point. The implied causal effect is basically unchanged but much more precisely estimated with a p-value of 1.4 percent.

The third row of table (4) further expands the information set by including controls for parental labor market status and education as described above in table (2). In particular, we include the share of parents employed, unemployed and self-employed and the shares of parents who completed high-school and university.

Inclusion of these controls does not modifiy the results. The exemption raised boys' high school completion rates by more than 2 percentage points; the share of girls completing high school increased by 1.8 points. Incidentally, tests of joint significance of the coefficents on the quadratic in cohort size and the set of parental controls in the first stage and reduced form equations strongly reject the null. The IV estimate now yields a slightly higher and still statistically significant causal effect of about 0.8; the null of no peer effect is rejected with 1.4 percent significance.

Taken together, these results imply that in treated towns about 180 adolescent boys and 150 girls 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 military service itself and the implied loss of labor market experience (Angrist (1990), Imbens and van der Klaauw (1995), etc.), the existence of compulsory drafting may affect subsequent male earnings also because of lower educational attainments. Also, we show that the negative effects of CMS on schooling extends to women, even if they are not subject to drafting, and potentially to their subsequent earnings as well.

5 Robustness checks

We now turn to a set of falsification exercises and robustness checks. We start by showing that the results introduced in the previous section are not just a chance occurrence by looking for similar effects in alternative samples. Next, we show they are robust to alternative sample specifications. Last, we provide additional elements showing that no other quake-related shock or pre-treatment differences in schooling drive the main results.

To allay concerns that the observed change in schooling among younger exempt cohorts is a spurious one we perform two falsification exercises. First, we compare schooling achievements of cohorts born in control towns and those of cohorts born in towns just outside the control ring (outer control towns, see figure (2)). Both groups are certainly unaffected by any quake-related intervention. We assign treatment status to control towns; therefore, the exemption dummy equals unity for cohorts aged 26-28 born in control towns and zero otherwise. Results reported in the first row of panel A in table (5) show no statistically significant difference suggesting that the results of table (4) are unlikely to be driven by sampling variation. Moreover, since control towns are closer to the quake region than outer control towns, the findings provide further support to the assumption that direct effects of the quake were absent: outcomes of younger cohorts not exempt are the same as those of comparable cohorts farther away from the quake region. Second, we assign exemption status to cohorts aged 30-35 in 1991 in treated towns and compare high school graduation rates of cohorts aged 30-40 in 1991 in treated towns to the same cohorts of control towns; the exemption dummy now equals unity for cohorts aged 30-35 and born in treated towns and zero otherwise. Results reported in the second row of panel A again show no statistically significant difference. Third, we replicate the above exercise only on cohorts aged 30-35 and assigning exemption status to those aged 30-32 and born in treated towns. Results, reported in the third row of panel A, again do not show significant differences in schooling attainemnts. We conclude that the findings of the previous section are unlikely to be driven by specific schooling trends in treated towns.

Interpretation of an IV estimate of θ as a causal effect requires a minimal set of assumptions to be satisfied (Angrist, Imbens and Rubin (1996)). In particular, that female 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)[32]. 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. Panel B1 of table (5) reports a set of robustness checks where alternative definitions of the older cohorts have been experimented. The previous findings are confirmed. The exemption increased boys' high school graduation rates by about 2 percentage points and those of girls by around 1.8 percentage points although results seem to be more sensitive to the specification of the age control group. The implied causal effect, always statistically significant, ranges between 07 and 0.9[33].

While assignment to treatment can be thought of as random since it depends on being affected by the quake, tables (1) showed some differences across treated and control towns: control towns are on average larger and more densely populated than treated towns. In panel B2 we show the main findings are robust to the exclusion of larger towns according to various definitions. The effect on boys' and girls' graduation rates are slightly larger and still statistically significant. The estimated causal effect seems to increase as average town

[32]Angrist et al. (1996) show that along with the stable unit treatment value (SUTVA) assumption, a causal interpretation of the estimates requires that (a) assignment to treatment is random, (b) the instrument does not directly affect the outcome variable (exclusion restriction), (c) nonzero average causal effect of the instrument on the treatment, (d) the instrument either increases or leaves unaffected the treatment (monotonicity). In our framework, (a) is naturally satisfied since assignment to treatment depends on the quake, (b) is fulfilled since women are not subject to drafting and the research design excludes or controls for the possibility of other quake-related shocks, (c) will be shown to hold in the next section, (d) has theoretical foundations and empirical support (Angrist and Imbens (1995), Cipollone and Rosolia (2004)).

[33]A referee also raised concerns that our main results might be sensitive to the inclusion of some specific older cohorts. These results show this is not the case.

size shrinks, which is consistent with the idea that the underlying mechanism becomes stronger the smaller the group[34].

To lend further credibility to the main identifying assumption that no other quake-related shock beyond the exemption affected younger cohorts in treated towns we perform two exercises. First, we limit 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 33 treated towns; as a control group we retained only those towns neighbouring on one of the selected treated ones (41 towns); this selection rule shrinks the sample to 74 towns, mostly southern and eastern ones with respect to the original sample. Results reported in the first row of panel C1 of table (5) show a stronger effect of the exemption on both boys' and girls' graduation rates in this specific subsample: boys' completion rates increase by 3.8 percentage points, those of girls go up by 3.3 percentage points; the implied causal effect is in line with previous results and highly significant. Results are confirmed if we further limit the sample to towns that, although exempt, were not included in the list of damaged towns (18 towns) and their neighbors (19 towns). Boys' graduation rates in this sample increased by 3.9 percentage points while girls' did by 3.2 points; the estimated causal effect is again unaltered and although less precisely estimated, still different from zero at 3 percent significance. Second, we look at university graduation rates on the full sample of 117 towns. If any income transfer was responsible for the increase of high school graduation rates in younger cohorts we would expect some effect in graduation rates from university of older cohorts as well. We

[34]In a different setting, Duflo and Saez (2003) find that the effect of treatment on the treated is no larger than that on untreated individuals in the same small group.

compare men and women university graduation rates for the cohorts aged 30 to 40 in 1991 and allow for a differential effect for cohorts 30-35, of college age when the quake hit, in treated towns. Results in panel C2 show these coefficients are small, negative and not statistically significant. We conclude that our main findings are unlikely to be driven by quake-related shocks other than the exemption.

Next, we focus on the effects of schooling attainments in lower grades. Table (3) showed differences in pre-treatment schooling endowments between treated and control towns. Our preferred specification controls for cohort size, reasonably the main determinant of per-capita resources given the limited age span considered in the analysis. However, to verify that the main findings are not driven by the potentially larger population at risk of completing high school in treated towns we perform two additional exercises. First, in the first row of panel D in table (5) we augment the preferred specification with the share of people in the cohort who had achieved a secondary school degree by 1981 (as reported by the 1981 census); the effect of the exemption on boys and girls' schooling is unchanged and so is the IV estimate. Second, we regress the conditional high school graduation rate on the preferred information set. The conditional graduation rate is defined as the share of high school graduates out of secondary school graduates in the cohort. Again, as showns in row D2, results turn out to be stable to this extension. We conclude that pre-treatment school endowments and the higher attainments in lower grades cannot explain the higher graduation rates from high school observed for boys and girls in the town-cohort cells affected by the exemption.

Finally, we verify that the main results are not driven by geographic heterogeneity in the developments of schooling levels. We divide the sample in 5 equally sized groups of towns on the basis of their geographic proximity and allow for group specific sets of age dummies. Point estimates reported in panel E of table (5) are in line with previous results with p-values below 3 percent; the IV estimate shows a somewhat higher peer effect, significant at 2 percent. We conclude that the main results are not driven by geographic heterogeneity in the age pattern of schooling.

6 Concluding remarks.

The existence and the nature of peer effects have been recognized as an important element to assess the efficiency of a given school system, its funding arrangements, the existence of tracking, etc.

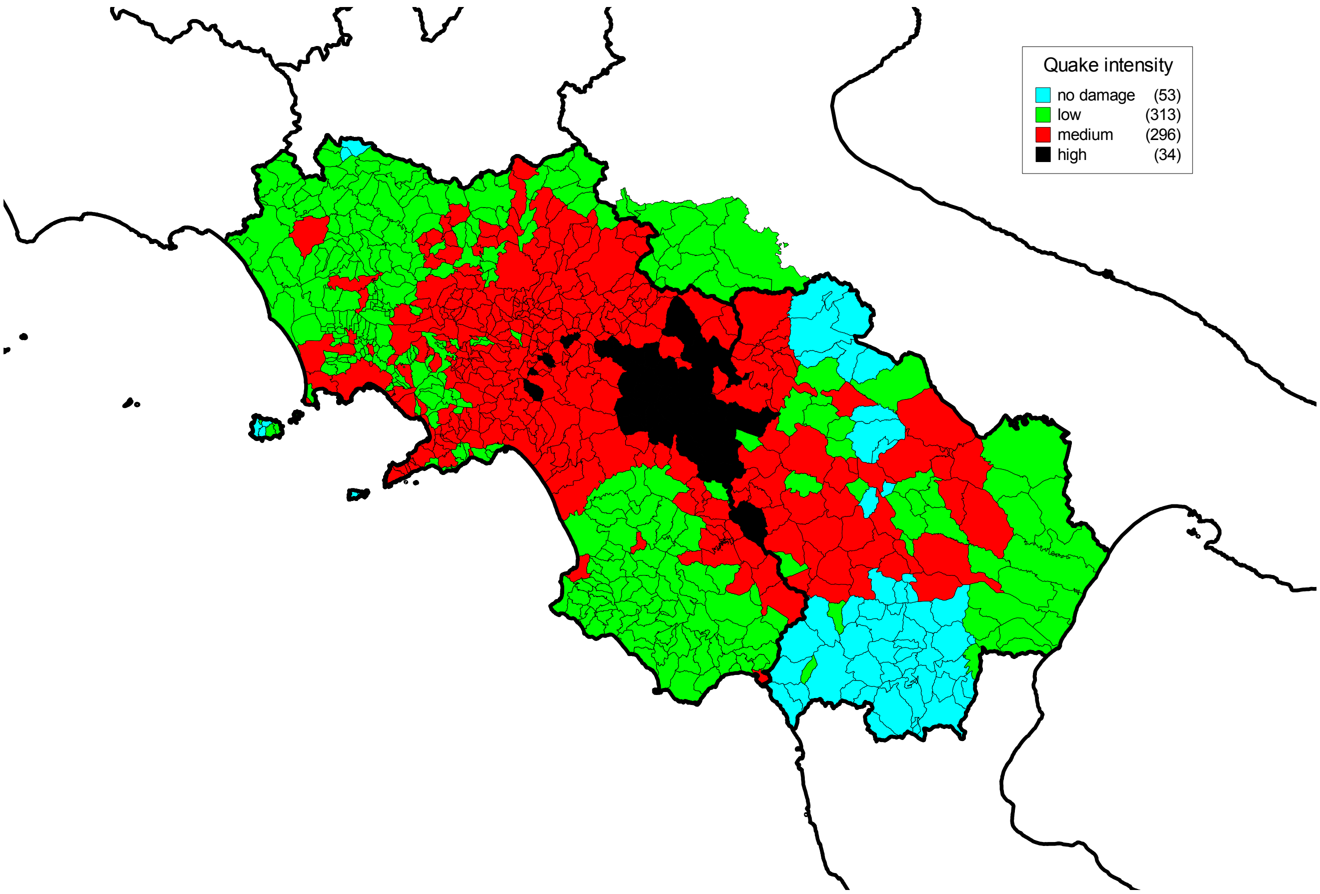
This paper provides evidence of a causal relationship of the high school graduation rate of boys on the probability of girls of the same age and living in the same town completing high school. Results are based on difference-in-difference estimates of the effect of the exemption from compulsory military service granted to specific cohorts of young men as a result of a quake hitting Southern Italy in 1980 on male and female high school graduation rates.

The exemption is shown to have increased the share of male high school graduates in the exempt cohorts by more than 2 percentage points. Women of the same age and born in the same towns as exempt males also display higher high school completion rates by about 2 percentage points. The estimated effects nearly double when we limit the analysis to the subset of towns that recorded the mildest damage or no damage at all. The research design and a set of robustness checks guarantee these results are the consequence of the exemption from CMS and not of the quake itself or other quake-related interventions that alter schooling decisions of both boys and girls.

Since in Italy women are not subject to military service, we interpret this finding as evidence of peer effects at the cohort-town level. Moreover, in our setting the exemption does not change the members of the peer group, the boys of a given cohort in a given town. Therefore, higher peer average outcome is the result of boys staying longer in school or of them putting more effort into the accumulation of human capital. Hence, we identify the effect on teenage girls' schooling outcomes of a behavior of peers.

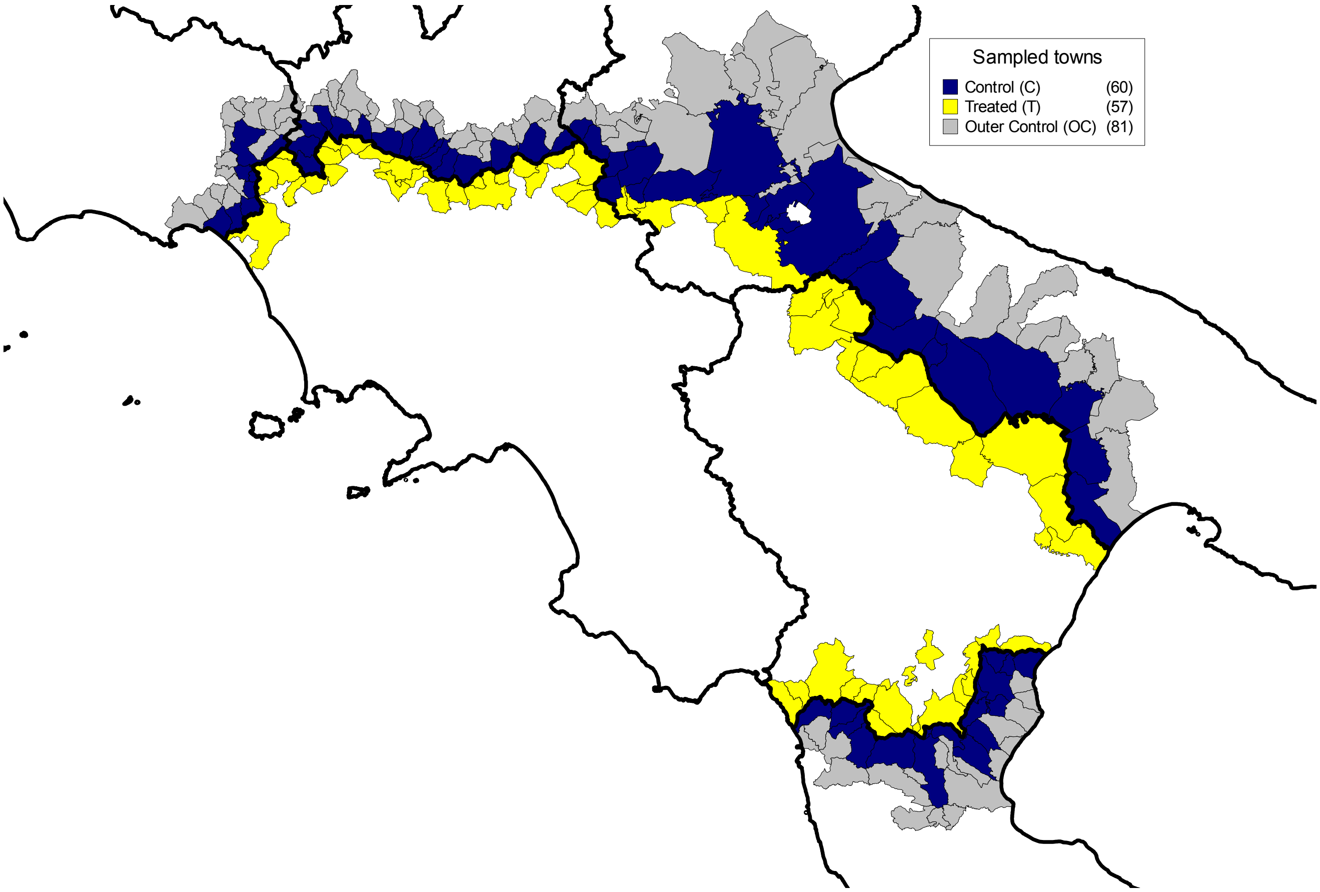
This kind of peer effects have crucially different policy implications than those of peer effects working through the exogenous characteristics of peers. For example, in the latter case an increase in the number of compulsory years of schooling would only raise school attainments of low achievers since the exogenous characteristics of the cohort are not changed by the intervention. Alternatively, if peer effects work through behaviors the same policy would also increase attainment of those not directly affected by the reform since the average outcome in the cohort increases because of the better performance of low achievers.

Figure 1: Exemption from CMS and quake intensity.



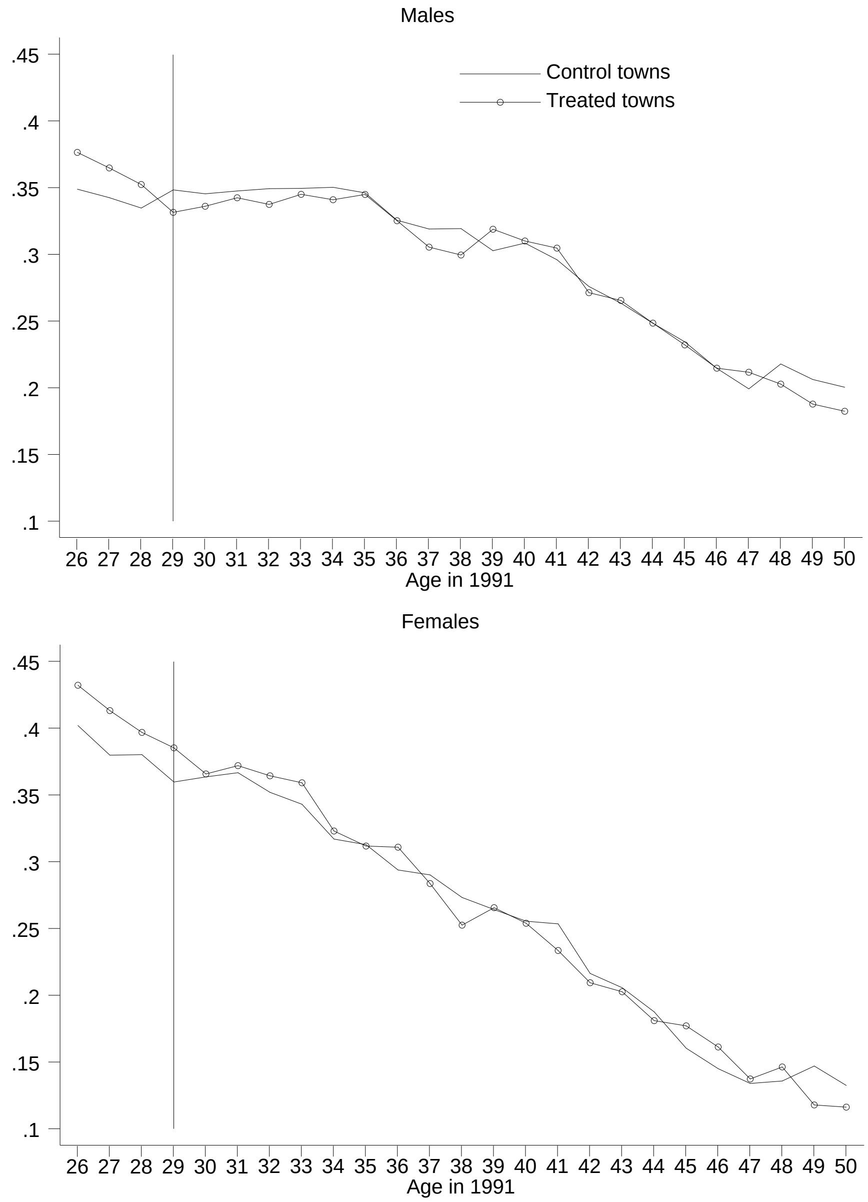
Source: Official report on the 1980 earthquake, Ministero del Bilancio e della Programmazione Economica (1981).

Figure 2: Sampled towns.



Source: Official report on the 1980 earthquake, Ministero del Bilancio e della Programmazione Economica (1981).

Figure 3: High school graduation rates.



Source: Authors' calculations on 1991 census data, Istat (1994).

Table 1: Descriptive statistics: town and population characteristics.

	Control	T-C
A: Structural characteristics.		
Altitude	257.428**	152.108**
	(22.716)	(40.418)
Inhabitants per square km (1979)	184.293**	-90.413**
	(11.363)	(20.218)
Extension (km^2)	290.584**	-128.708**
	(20.527)	(36.524)
B: Demography.		
Inflow rate	1.960**	-0.268
	(0.109)	(0.194)
-from abroad	0.178**	-0.004
	(0.024)	(0.042)
Outflow rate	2.104**	0.214
	(0.085)	(0.151)
-abroad	0.146**	0.005
	(0.026)	(0.046)
Birth rate	1.635**	-0.175**
	(0.032)	(0.056)
Death rate	0.802**	0.098*
	(0.024)	(0.042)
Family size	4.231**	-0.182**
	(0.029)	(0.053)
Age	32.565**	2.301**
	(0.315)	(0.562)
C: Education and labor market.		
%≥ High school	15.478**	-1.236
	(0.722)	(1.273)
%≥ University	2.709**	-0.479
	(0.155)	(0.274)
% Employed	37.423**	0.769
	(0.452)	(0.797)
% Unemployed	11.564**	0.958
	(0.400)	(0.704)
% Self-Employed	10.302**	2.470*
	(0.582)	(1.027)

Source: Authors' calculations on town structural characteristics (Istat (1990)), population flows data (Istat (1980b)) and 1981 population census (Istat (1984)).
Weighted means; weights are town population. Panel C refers to population 15 year old and above. Standard errors in parentheses. (*) significant at 5%; (**) significant at 1%.

Table 2: Parental characteristics, 1981.

Cohort	Employment		Unemployment		Self-Employment		High school		University		Age	
	Control	T-C	Control	T-C	Control	T-C	Control	T-C	Control	T-C	Control	T-C
26	53.831**	1.126	6.613**	2.111**	17.200**	3.145**	7.608**	-1.497*	1.926**	-0.314	46.670**	0.362
	(1.475)	(2.688)	(0.436)	(0.795)	(0.627)	(1.143)	(0.333)	(0.607)	(0.119)	(0.218)	(0.138)	(0.252)
27	51.442**	3.446	7.050**	1.638*	16.106**	4.545**	7.491**	-1.211*	1.997**	-0.249	47.739**	0.302
	(1.476)	(2.682)	(0.437)	(0.793)	(0.628)	(1.141)	(0.334)	(0.606)	(0.119)	(0.217)	(0.138)	(0.251)
28	51.103**	1.003	6.522**	2.103*	16.624**	3.152**	7.002**	-1.350*	1.853**	-0.248	48.760**	0.369
	(1.555)	(2.841)	(0.460)	(0.841)	(0.661)	(1.208)	(0.351)	(0.642)	(0.126)	(0.230)	(0.146)	(0.266)
30	48.203**	1.203	5.936**	1.867*	16.580**	3.211*	6.632**	-1.456*	1.918**	-0.558*	50.862**	0.215
	(1.703)	(3.037)	(0.504)	(0.899)	(0.724)	(1.292)	(0.385)	(0.686)	(0.138)	(0.246)	(0.159)	(0.284)
31	46.948**	1.285	5.533**	1.49	16.717**	2.968*	7.011**	-2.064**	2.110**	-0.579*	51.851**	0.001
	(1.815)	(3.216)	(0.537)	(0.952)	(0.772)	(1.368)	(0.410)	(0.727)	(0.147)	(0.260)	(0.170)	(0.301)
32	44.819**	1.179	5.355**	0.627	16.133**	4.067**	6.759**	-1.729*	1.890**	-0.570*	52.890**	0.209
	(1.922)	(3.421)	(0.569)	(1.012)	(0.818)	(1.455)	(0.435)	(0.773)	(0.156)	(0.277)	(0.180)	(0.320)
33	42.746**	0.669	5.009**	0.86	15.821**	3.157	6.994**	-2.208**	2.041**	-0.589	53.984**	0.129
	(2.128)	(3.785)	(0.630)	(1.120)	(0.905)	(1.610)	(0.481)	(0.855)	(0.172)	(0.306)	(0.199)	(0.354)
34	41.296**	0.546	4.803**	0.691	16.690**	2.024	6.944**	-1.498	1.979**	-0.276	54.919**	0.188
	(2.355)	(4.128)	(0.697)	(1.221)	(1.002)	(1.756)	(0.532)	(0.933)	(0.191)	(0.334)	(0.221)	(0.387)
35	39.531**	-0.405	3.820**	-0.113	17.091**	2.11	7.534**	-2.263*	2.493**	-1.074**	55.970**	0.158
	(2.534)	(4.505)	(0.750)	(1.333)	(1.078)	(1.916)	(0.573)	(1.018)	(0.205)	(0.365)	(0.237)	(0.422)
F-test		0.999		0.832		0.962		0.981		0.641		0.995

Source: Authors' calculations on 1981 population census (Istat (1984)).
Weighted means; weights are number of parents in relevant cell. Standard errors in parentheses.
(*) significant at 5%; (**) significant at 1%.

Table 3: School endowments: class size, 1972-79.

School-year	Primary school		Secondary school		High school	
	Control	T-C	Control	T-C	Control	T-C
1972	29.781**	-10.858**	23.450**	-1.859**	21.513**	1.242
	(0.944)	(1.663)	(0.336)	(0.592)	(0.812)	(1.528)
1973	30.331**	-11.817**	23.009**	-1.881**	22.492**	-0.464
	(0.937)	(1.636)	(0.333)	(0.582)	(0.811)	(1.501)
1974	28.313**	-10.062**	22.833**	-1.637**	23.598**	-1.465
	(0.932)	(1.630)	(0.331)	(0.580)	(0.800)	(1.491)
1975	26.985**	-9.555**	22.879**	-1.323*	23.477**	-1.359
	(0.928)	(1.635)	(0.330)	(0.582)	(0.796)	(1.510)
1976	26.132**	-8.714**	23.737**	-1.953**	23.591**	-1.702
	(0.923)	(1.631)	(0.328)	(0.580)	(0.801)	(1.497)
1977	25.924**	-8.779**	23.847**	-1.853**	23.634**	-1.551
	(0.919)	(1.627)	(0.327)	(0.579)	(0.802)	(1.535)
1978	25.635**	-8.624**	23.686**	-1.794**	23.678**	-1.648
	(0.915)	(1.624)	(0.325)	(0.577)	(0.788)	(1.490)
1979	25.396**	-8.862**	22.678**	-1.296*	23.660**	-1.657
	(0.912)	(1.622)	(0.324)	(0.577)	(0.809)	(1.561)
F-stat		0.51		0.20		0.45
P-value		0.82		0.99		0.87

Source: Authors' calculations on Istat's education yearbooks (Istat (1980a)). Weighted means; weights are town population in relevant year. Standard errors in parentheses. (*) significant at 5%; (**) significant at 1%.

Table 4: First stage, reduced form and IV estimates.

Dependent variable:	Males' HS graduation rate		Females' HS graduation rate			
	OLS				IV	
	First stage		Reduced form			
	Exemption	p-value	Exemption	p-value	Males' HS %	p-value
Age & Town dummies	1.82	0.031	1.24	0.107	0.68	0.086
+ cohort size	2.50	0.004	1.82	0.022	0.73	0.014
+ parental characteristics	2.15	0.014	1.84	0.022	0.85	0.014

Weighted regressions; weights are the number of females in each group. Sample: 117 towns, 1050 town-cohort cells. OLS are estimated coefficients on exemption dummy and IV are IV estimates of θ in equation (2) in the paper; instrumented variable is males' high school graduation rate; instrument is exemption dummy. Exemption dummy equals 1 if cohort aged 26-28 and born in treated town. P-values of IV estimates are based on the Anderson-Rubin statistic ($\chi^2(1)$). Parental characteristics are: share of parents employed, unemployed, self-employed, with a high school degree and with a university degree.

Table 5: Robustness checks.

	Males		Females			
	OLS				IV	
	First stage		Reduced form			
	Exemption	p-value	Exemption	p-value	θ	p-value
A: Falsification on alternative samples						
A1. Control vs Outer control	0.16	0.821	0.12	0.867	-	-
A2. 30-35 vs 36-40	0.43	0.552	0.64	0.339	-	-
A3. 30-32 vs 33-35	0.60	0.557	0.24	0.793	-	-
B: Sample specifications						
B1: Older cohorts						
–30-32	1.88	0.055	1.68	0.067	0.89	0.042
–33-35	1.99	0.052	1.88	0.053	0.94	0.032
–no 30	2.12	0.021	1.75	0.036	0.83	0.023
–no 31	2.08	0.023	1.85	0.029	0.89	0.018
–no 32	2.14	0.016	1.96	0.019	0.91	0.011
–no 33	2.22	0.013	1.94	0.019	0.87	0.012
–no 34	2.03	0.025	1.99	0.016	0.98	0.010
–no 35	2.11	0.019	1.51	0.070	0.71	0.051
B2: Town size						
– $\leq$50,000 inh.	2.45	0.015	2.19	0.018	0.89	0.011
– $\leq$40,000 inh.	2.55	0.013	2.67	0.004	1.04	0.002
– $\leq$20,000 inh.	2.72	0.022	3.02	0.005	1.11	0.003
C: Correlated shocks						
C1. No Damage						
– score: 0 & 6	3.76	0.002	3.27	0.002	0.87	0.001
– score: 0	3.87	0.017	3.16	0.044	0.82	0.028
C2. University graduation rates	-0.28	0.512	-0.34	0.428	-	-
D: Pre-treatment schooling						
D1. + secondary education	2.15	0.014	1.84	0.023	0.85	0.015
D2. conditional HS %	2.35	0.022	1.76	0.066	0.75	0.050
E: Area specific age dummies	1.97	0.028	1.83	0.027	0.93	0.016

Weighted regressions; weights are the number of females in each group.
Dependent variable: (males' or females') high school graduation rate except C2 (university graduation rates) and D2 (conditional high school graduation rates). Control set: cohort and town dummies, quadratic in cohort size, share of parents employed, self-employed, unemployed, with high school and university degree; D1 also includes share of cohort with secondary school degree; E allows for specific age dummies for 5 groups of towns identified by geographic proximity. Sample towns: Treated and control except A1 (control and outer control), B2 (treated and control towns below threshold), C1 (only treated town with damage score 0-6 or 0 and neighboring control towns). Sample cohorts: 26-28 and 30-35 except A2 and C2 (30-40), A3 (30-35) and B1 (26-28 and as reported in the table). OLS are estimated coefficients on exemption dummy and IV are IV estimates of θ in equation (2) in the paper; instrumented variable is same as dependent but defined on males; instrument is exemption dummy. Exemption dummy defined according to specification in corresponding row (see text). P-values of IV estimates are based on the Anderson-Rubin statistic ($\chi^2(1)$).

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