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The Effect of Education on Youth Mortality*

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PRELIMINARY & INCOMPLETE

Abstract

We explore the relationship between education and mortality in a young population of males. We overcome omitted-variable bias by instrumenting education with the exemption from military service granted to certain cohorts of males in specific towns in the aftermath of a quake in Southern Italy in 1980. Results are based on comparisons of exempt and not exempt cohorts in towns least damaged by the quake with the same cohorts living in nearby towns outside the quake region. We find that the exemption increased high school completion rates by about 2.5 percentage points; mortality rates of the same cohorts also were significantly lower by about one fourth. Results are robust to a set of falsification exercises and consistency checks. Strikingly, we find the effect to be due to deaths for natural causes rather than accidental ones. We explore the channels through which education may affect mortality.

Keywords: *education, mortality, health, social returns.*
JEL Codes: I20, I12.

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1 Introduction.

Education is shown to be positively correlated with health status, negatively with the likelihood of risky behaviors and with mortality rates (Grossman and Kaestner (1997), Kitagawa and Hauser (1973), Deaton and Paxson (1999)). Understanding whether these empirical relationships are causal is of primary importance for the correct assessment of the social returns to the investment in education. Better health status or safer behaviors have relevant social returns: lower health care costs, less productivity losses from absences from work, less negative spillovers on the household and its members (hardship, need to reorganize household activity, etc.), likelihood of contagion fo rsome diseases, involuntary involvment of third parties in accidents, etc.

Previous research has mostly addressed the relationship between education and health status. However, lack of unquestionable instruments for schooling and the complexity of the setting does not allow to draw clear conclusions. On the other hand, the link between education and mortality, which is often seen as a sufficient statistic for health status or behavioral riskiness, is little explored. A notable exception is Lleras-Muney (2005)) who shows that individuals likely to have been affected by changes in US states' compulsory schooling laws in the early decades of the past century display a significantly lower probability of dying in adulthood. Her IV estimates are higher in absolute value than the LS ones, however she cannot reject the null that point estimates are equal. She focuses on mortality rates of adults: her youngest cohort is aged at least 35 and she follows it along two subsequent decades.

In this paper we explore the causal link between education and subsequent mortality rates in a population of young males in a quasi-experimental setting. The source of exogenous variation for schooling is provided by the exemption from compulsory military service (CMS) granted to few specific cohorts of males born in the towns hit by a major earthquake in 1980. We argue the exemtpion increased the incentives to stay in high school. Since the exemption from CMS is clearly correlated with a major shock that may have had a direct effect on subsequent mortality rates, we develop a research design that controls for this possibility. More specifically, we compare schooling levels and subsequent mortality rates of exempt and older not exempt cohorts from towns on the border of the quake region with those of comparable cohorts from neighboring towns just outside the quake region. Treated towns are therefore the least damaged ones, some of them recording no physical damage at all, and control towns are geographically very close supporting the assumption that cohorts share the same unobservable characteristics across towns. Systematic differences across towns are picked up by town fixed effects (Cipollone and Rosolia (2004)).

The nature of the data allows us to focus on mortality rates of a relatively young population, between the early 20s and mid 30s. This focus is important for two reasons. First, other potential determinants of health, behavioral riskiness and mortality caused by education itself (e.g. income, job safety, job quality, etc) are unlikely to have already kicked in at this stage of life. Therefore, our results can be thought of as mostly driven by the direct effect of education rather than by the overall direct and indirect effect. This has important implications in terms of policy making. Second, the population under investigation is relatively young and at the early stages of its working life. Death or bad health at this stage imply a considerable loss both from the private point of view and from the social one. Dying at this stage, implies that non only the individual but also society will not reap the benefits from the investment in education after having borne the costs.

Results show that 10 years after the quake exempt cohorts have a significanlty higher share of high school graduates. The most conservative estimates show an increase of 2-2.5 percentage points. In the subsequent 10 years, between 1991 and 2001, the same cohorts also display lower mortality rates by 0.3-0.5 percentage points, about one fourth less than not exempt comparable cohorts from control towns. Therefore, under the exclusion restriction that the exemption (or military service) is not a direct determinant of mortality, completing high school reduces an individual's probability of dying between his mid 20s and mid 30s by 0.1-0.2 percentage points. A set of robustness checks and placebo experiments confirms that we are picking up the effect of the exemption from CMS and not other correlated confounding factors. We find that 2SLS estiamtes are significantly different and larger in absolute value than corresponding LS estimates; LS point estimates are very small and not statistically different from zero.

Surprisingly, we find that even in this realtively young population all the effect is due to a reduction in mortality from natural causes rather than from violent or accidental ones. Unfortunaltely, the limited sample size does not allow us to explore more in detail the different natural causes in a reliable way.

The paper proceeds as follows. In the next section we describe the 1980 quake and related interventions and develop the research desing underlying our analysis. We then present the results and robustness checks. We then conclude.

2 The Quake, the Exemption, and the Sample.

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. As a whole, the earthquake was quite disruptive. 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. 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 and guidelines for the assignment of the funds allocated to this purpose[1]. Entitlement to financial aids depended on the magnitude of

[1] 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

the damages suffered as certified by the authorities.

Parliament also passed a set of laws that 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[2]. However, 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). Males born between 1963 and 1965 were likely to be the mostly affected by the normative change since they learned about the exemption while they were still in high school. In Cipollone and Rosolia (2004) we propose a rationale for a positive effect of the exemption on schooling: being exempt amounts to having an additional year that can be allocated to schooling or working. Schooling increases if returns to education are higher than those to experience.

Our goal here is to relate mortality rates to education and use the exemption as an instrument for schooling. Two concerns must be addressed. First, since the exemption was caused by the quake, the necessary exclusion restriction would not be satisfied if the quake or other quake-interventions had a direct impact on other determinants of health status and mortality of the affected individuals. For example, the quake may have damaged health infrastructures or determined hard living conditions for a certain period; alternatively, household targeted income transfers may have triggered higher health expenditures and healthier habits or reconstruction could have been the chance to improve on relevant infrastructure.

We address this first concern by retaining in the sample only the set of towns that, albeit hit by the quake, were the least affected according to the official evaluations performed in the aftermath (treated towns). These are 57 towns located right at the boundary of the quake region, the farthest away

to rebuilding private dwellings and public buildings. The remaining 20 percent was devoted to the reconstruction of factories, farms and basic infrastructures.

[2]In Italy, young males are called up for a medical check the year they turn 18 and, if suitable for service, inducted at age 18. Deferments can be obtained if still enrolled in formal schooling. Induction comes when still enrolled in high-school which, if the youth is on schedule, ends between age 18 and 19.

from the epicenter; 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[3]. We compare the outcomes of interest of cohorts in treated towns with those of comparable cohorts in towns just outside the earthquake region and neighbouring on at least one treated town (control towns). This rule selects 60 more towns. Figure (2) shows their location.

The geographic proximity guiding the sample design has several implications. First, treated an control towns are very close. Therefore, any quake-related intervention that was not directly targeted to household is taken care of[4]. For example, if a hospital was damaged or renovated, or a new one built, nearby towns would have had equal access to it. Second, general equilibrium effects are also accounted for. Nearby treated and control towns are embedded in the same economic environment so that, for example, local labor market conditions on subsequent mortality are accounted for[5].

A second concern is the possibility that exemption status itself affect subsequent mortality. However, we believe this is not the case. First, exempt individuals might not have gone through the medical check up so that, if anything, they missed the chance of early diagnoses of dangerous symptoms[6]. Second, serving in the Army does not imply being exposed to an unhealthy or dangerous environment; on the contrary, typical activities include work outs, shooting training, basic maintenance and guard duties. Additionally, there is a strong incentive while in the military to take medical checks since ordinary duties are suspended while undergoing the examinations. Overall we think it is hard to argue that

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

[4]We have argued that only the least damaged towns are retained in the sample and this limits significantly the amount and probability of tranfers to the households. However, we perform robustness checks on the subset of treated towns that recorded no damage at all and their direct neighboring towns so that transfers are totally ruled out.

[5]It could still be that mortality is affected by labor market conditions and that exempt and not exempt cohorts do not share the same labor market because one of the two moved to some other location. However, available evidence suggests this is not the case. We address this issue in detail later on.

[6]It must also be said that the medical check only involve a general medical visit, basic blood and urine analysis, lungs examination, and a psychological test.

serving in the military may involve cosinderable health risks that will manifest over the subsequent 10-15 years. Third, the year spent in the military hardly constitutes a reference for subsequent labor market participation. Fourth, a general exemption has no informative power as to the individual's ability or work motivation; even if only the more able or motivated individuals took the chance of not serving, it is hard to believe that this may reflect into a lower subsequent mortality since in Italy this information hardly reflects into determinants of health status such as better employer-provided health coverage[7].

The upper panel of figure (3) shows the evolution of the share of male high school graduates in treated and control towns across cohorts. Age is expressed as on 1991, when we observe school achievement from the population census. Since we do not know where individuals were living at the quake date, we proxy their being eligible for the exmeption with place of birth. Exempt cohorts are those aged 26-28 in 1991 and born in treated towns. The figure shows a clear increase in teh share of high schoolers among the exmept cohorts, while all older cohorts display the same school achievement. The lower panel draws on our second data source, individual mortality records, and reports the evolution of mortality rates over the period 1986-2001 across cohorts for treated and control towns. We chose to start in 1986, when the youngest cohort was aged 21 and out of high school to avoid the artifact that possibly less individuals die because, say, teh simply spend part of teh day in a supervised and safe environment like the classroom[8]. The 15-year death rataes of non exempt cohorts closely track each other, while they diverge among younger cohorts.

The evidence reported in the figures is consistent with the existence of a causal effect of schooling on subsequent mortality rates. The empirical analysis will be based on the sample of males aged 26-35 in 1991 born in any of the sampled towns. This allows us to control for town and cohort

[7]In Italy health insurance is publicly provided and subscription of private health insurance in only a very recent phenomenon

[8]For purpose of teh figure, teh initial population has been reconstructed summing to the populationn observed in 1991 teh numebr of deaths occurred between 1986 and census date

fixed effects. Before turning to the analysis, however, let us briefly describe some relevnat aspects of the sample to show that a difference-in-difference approach to the data accounts for most of the differences detected across towns. Table (1) reports some descriptive statistics. Panel A shows that treated towns are higher, less densely inhabited and smaller. This, if anything, is likely to determine a lower infrastructural endowment so that, as concerns health, these towns are probably worse off. Panel B reports a description of some demographic characteristics as of 1980. Towns are similar in terms of migration patterns, while treated towns display slightly lower birth rates and higher death rates, the net effect tilting upward the age distribution; family structure is basically similar. Tests that these differences are systematic over the pre-quake period never reject the null. Panel C describes the composition of the population older than 15 as of 1981, drawing on Census data. The only difference is that treated towns have higher self-employment rates.

Family background is known to be an important determinant of schooling and health. The difference-in-difference approach, however, does not allow to control for differential developments across cohorts within town. Therefore, one may be concerned that changes in schooling and mortality may actually be a consequence of changes parental background. We address the issue in table (2). The sample underlying the table is all individuals that in the 1981 population census were 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[9]. The table reports results of regressions of parents employment, unemployment and self-employemnt rates as well as their educational attainments on a set of dummies for the age of the child and their interaction with

[9]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.

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

We now turn to the empirical analysis and introduce the main results. We then offer some evidence in favor of our exclusion restriction.

3 Results.

We start out showing that the exemption from CMS had a statistically significant and positive effect on high school graduation rates of the exempt cohorts. We estimate by weighted least squares the following diferences-in-differences model:

$$hs_{aj} = \alpha + \beta EX_{aj} + X_{aj} + \mu_a + \nu_j + \epsilon_{aj} \tag{1}$$

where hs_{aj} is the share of cohort a born in town j that in the 1991 population census reported to have obtained at least a high school degree, $EX_{aj} = 1$ if cohort a from town j was exempt from compulsory military service, X_{aj} are a set of controls, μ_a is a common cohort effect and ν_j a town effect; ϵ_{aj} an i.i.d. residual.

The first column in table (3) reports estimates on cohorts 1951-1965 born in treated and control towns; therefore $EX_{aj} = 1$ if the cohort is born in 1963-1965 in a treated town and zero otherwise. We only control for a quadratic in cohort size, town and cohort fixed effects. Results show that the percentage of high school graduates was statistically higher than that in comparable not exempt cohorts by more than 2.5 percentage points. To make sure that the effect is due to the exemption itself, we have further reduced the number of cohorts retained in the sample to those born 1956-1965, aged 15-24 at quake date. This allows us to recover some controls for parental characteristics from the 1981 census (see table (2)). The oldest cohort was aged therefore 25 which, for Italian standard, is an age at which youths still live with their parents. Results in column (2) still show a significant increase of the graduation rate by more than 2 percentage points. Column (3) furhter limits the cohorts to those born in 1959-1965, finding pretty much the same effect on high school completion. In column (4) we address the issue that results may be driven by geographic differences in the secular trend of education; we have augmented the specification with 5 sets of cohort effects, according to the geographic location of the town. Results are unchanged and still significant. Last, in column (5) we restrict the sample of towns only to those that recorded no damage or were at the lowest of the damage scale (33 towns) and to their neighbouring conrol towns. There are 77 towns in this sample. This should control for potentially confounding factors that affect simultaneously high school graduation rates and subsequent mortality (e.g. income tranfers to more damaged towns, damages to health infrastructure, etc.). The effect of the exemption is now higher and always highly statistically significant.

In table (4) we report OLS, reduced form and 2SLS estimates of the effect of, respectively, the share of high school graduates, the exemption and, again, the share of high school graduates on mortality rates over the period 1991-2001. Data are drawn from individual death records. These records report cause and date of death, and some socio-demographic information. To our purpose, they report town and date of birth. We are therefore able to compute the number of deaths occurred at the town of birth-cohort level over a given horizon and match them to our schooling data from the 1991 census. The dependent variable in panel A of the table is the total share of deaths while in panel B we look at deaths by natural causes. Columns correspond to the specifications introduced in the previous table on the first stage analysis. The first thing to notice from panel A is that the OLS estimates are highly imprecise and point to a zero correlation between subsequent death rates and high school graduation rates. Looking at the reduced form estimates we find a negative effect of the exemption on overall mortality rates but not strongly significant except for the specification in column (5) where the sample is restricted to towns that either recorded no damage or were at the lowest damage level. Overall, the effect of the exemption is about 0.3 0.5 percentage points which corespond to about XXX of the baseline probability of dying in the next ten years for these specific cohorts. IV estimates are somewhat more precise, and point to a reduction of about 0.1-0.2 percentage points of the death probability if the individual obtains a high school degree. This is consistent with previous findings that find IV estimates to yield a larger effect than simple OLS. Things become more interesting in panel B where we look at mortality by natural causes only. Again OLS estimates point to no correlation. However, a look at reduced form estimates shows that the exemption had a significant effetc on mortality by natural causes. Additionally, comparing these estimates with the correspondin ones in panel A showns that while point estimates are basically the same what increases noticeably is the precision of the estimate, suggesting that the share of deaths by violent causes, the complement of natural deaths to total death rate, are a source of noise for our estimates[10]. IV estimates are now considerably more precise although point estimates are largely stable across panels and point to a reduction of the 10-year death probabiliy of about 0.15-0.2 percentage points for those who complete high school.

As additional robustness checks, we estimated the first stage and reduced form equations in samples where we expect not to find any effect since not affected by the exemption. The first row of table (5) reports results for th efirst stage and reduced form on the sample of control towns and neighbouring towns also outside the quake region adn thus unaffected by any intervention. We assing a fictitious exemption status to cohorts 1963-1965 born in control towns. As expected, we find no effect of the fictitious exemtpion. The next row looks at older cohorts in treated and control towns. Here, the fictitious exemption status is assigned to cohorts 1961-1956 born in treated towns and older cohorts

[10]Similar estimates on the share of deaths by violent causes unsurprisingly shows no significant effect of the exemption.

1951-1955 acto as control cohorts. None of these cohorts was in high school when the exemption was granted and as such we expect them to be unaffected by it. Results confirm the expectation: there is no difference in terms of schooling or mortality rates. In the third row we replicate the analysis controlling for parental characteristics (the control may be noisy since (some of the) older cohorts 1951-1955 may have been already living on its own in 1981 which is when we are able to compute parental controls. Again, no difference is to be found.

So far, we have exploited information on deaths occurring between 1991 and 2001 because we only observe education attainments in this year from the population census. While this allows us to provide 2SLS estimates for the causal effect of education on death rates we are focussing on death rates of a population 26 or older. To have sharper insights on whether we are isolating the direct effect of education on mortality or just the cumulated effect of all mortality determinants affected by education, such as income and job quality we exploit information on deaths occurring prior to 1991 and after the quake. This implies we are looking at the same population at an earlier stage of life. We can only provide reduced form estimates over this horizon since we are not able to recover the initial schooling endowments. Still, we think this has an informational content of its own. Table (6) therefore reports reduced form estimates of the overall mortality rate (upper panel) and mortality rate by natural causes (lower panel) over three different horizons: 1981-2001, 1986-2001 and, as a reminder, 1991-2001; columns correspond to alternative specifications as described in previous tables. The differences in mortality rates due to the exemption appear already during the 80s. Differences in the first half of the decade might be due to the fact that, by staying more in school, exempt individuals in treated towns experience different environments than the comparison groups. However, we see that there is a significant reduction in mortality also in the second half of the 80s when both groups are over with high school[11]. While not conclusive, these findings are consistent with the fact that education has

[11] In Cipollone and Rosolia (2004) we show that there are no differences in college graduation rates as of 1991, suggesting that the exempt individuals are in labor market rather than in college in the second half of the 80s very much like their not exempt counterpart.

an immediate effect on subsequent mortality, on top of possible other indirect effects through, say, income or job quality.

4 Conclusions.

It is widely recognised that education gives rise to relevant and valuable social returns (Acemoglu and Angrist (2000), Moretti (2004), Lochner and Moretti (2004), Bresnahan, Brynjolfssonn and Hitt (2002), Sacerdote (2002), Currie and Moretti (2003)). In this paper we have addressed the causal link between education achievements and subsequent mortality. While the positive correlation between education and health status and the negative ones between education and behavioral riskiness and mortality are widely documented, little evidence exists on the causality of such links. Unveiling such a causal relationship has important implications for policy design: while a better individual health status and safer behaviors have important spillovers on the society as a whole, both monetary and non monetary (lower health expenditure, slower diffusion of diseases, less accidents or risky behaviors that may involve third parties, etc.), these are not internalised in individual decisions on optimal education.

We explore this link in a quasi-experimental framework. The exogenous variation in schooling is determined by the exemption from CMS granted to few cohorts of males from specific towns of southern Italy after a major earhtquake hit them in 1980. A careful research design guarantees that our estimates isolate the effect of the exemption on schooling and on subsequent mortality rather than other determinants of both outcomes correlated with the exemption, such as other quake-related interventions or the direct effects of the quake itself. A set of robustness checks and placebo experiments confirm this fact.

We find that 10 years after the exemption the share of high school graduates in the exempt cohorts was higher by 2-2.5 percentage points. In the subsequent decade the share of deaths in the exempt cohorts turn out to be lower by 0.3-0.5 percentage points, roughly one fourht lower than mortality rates among not exempt cohorts of the same age. The implied 2SLS estimates suggest that obtaining a high school degree reduces the probability of death between mid 20s and mid 30s by 0.1-0.2 percentage points. We discuss the possibility that (not) serving in the military has a direct effect on subsequent mortality and conclude that it seems unlikely.

Our data allow us to focus on a very young population. This is a very relevant group since deaths (or bad health) at this stage of life imply a considerable loss for society as a whole since all human capital popssibly accumulated during formal schooling, for which society bears a cost, is lost before any benefit can be reaped. Additionally, focussing on this group has the major advantage that other education-realted determinants of mortality, such as income or job quality, are unlikely to have fully developped so that our estimates can be thought of as the direct effect of education on mortality rather than the cumulated direct and indirect (through other determinants) effect. We provide evidence that this may be the case by showing that differences in mortality rates between exempt and not exempt individuals are detected already from the early 20s and, also less clearcut, from teenagehood. Unexpectedly, we find that these differences are due exclusively to a lower number of deaths for natural causes, rather than to a reduction in violent and accidental deaths.

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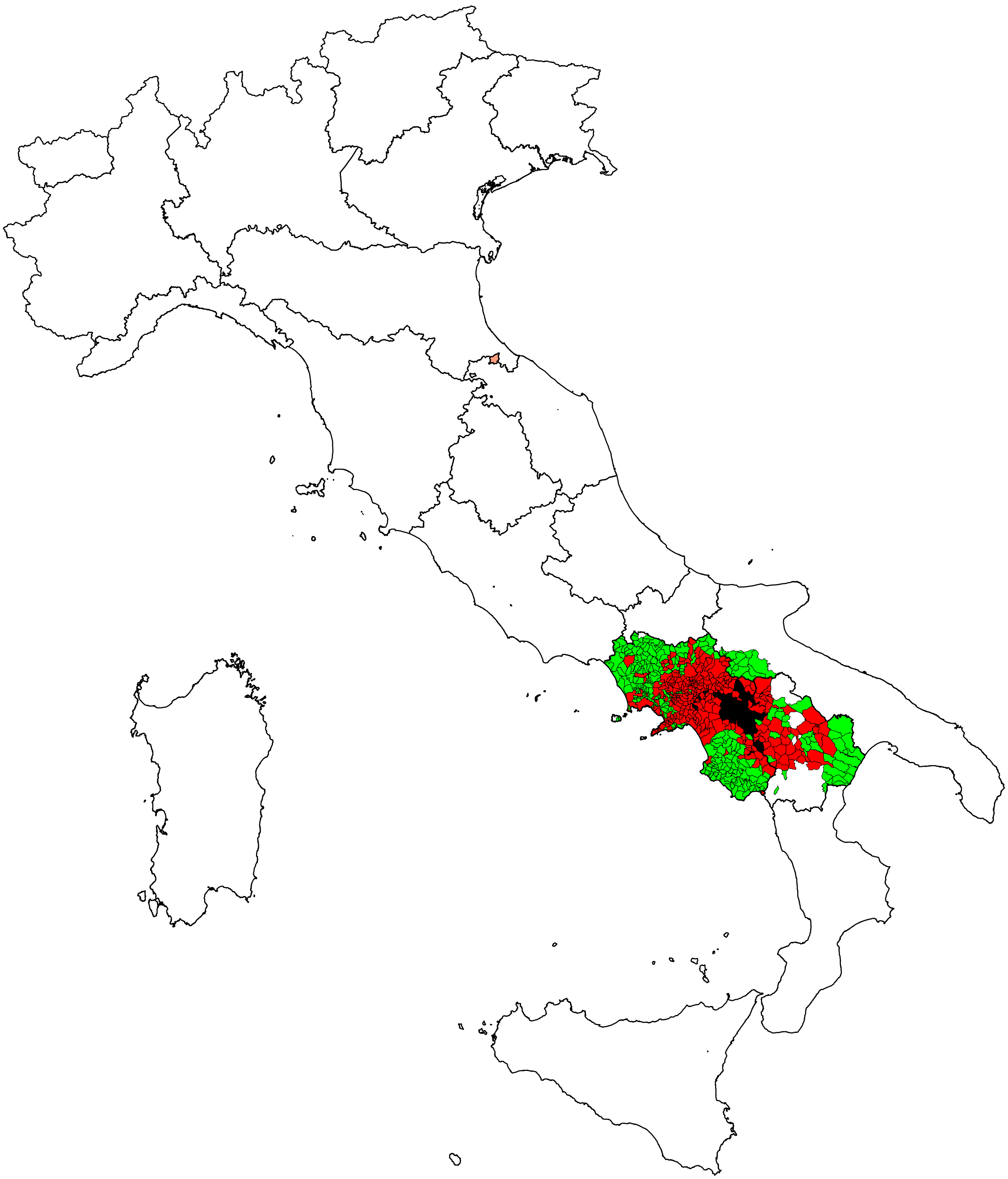
Figure 1: 1980 Earthquake.

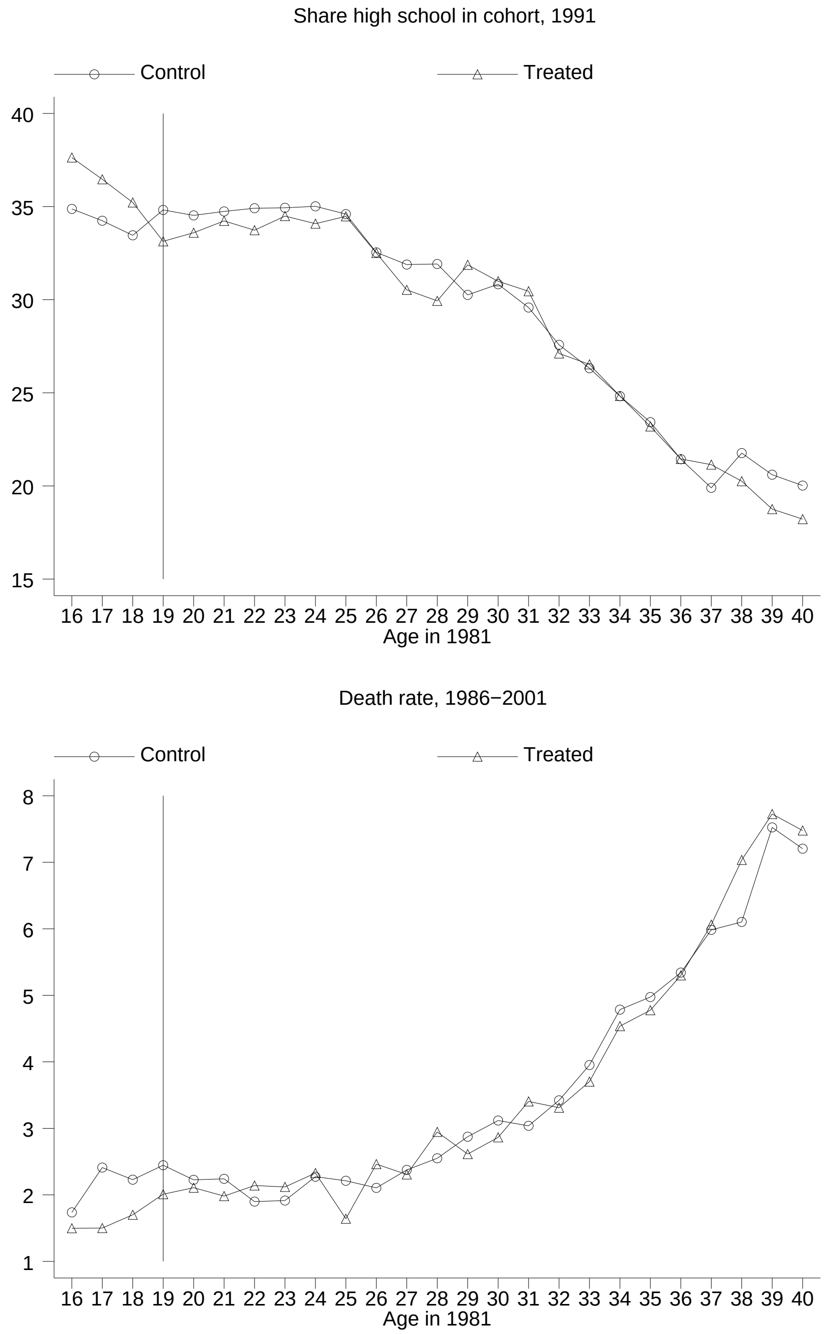


Source: Official report on the 1980 earthquake, Ministero del Bilancio e della Programmazione Economica (1981).

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 and mortality rates.



Source: Authors' calculations on 1991 census data, Istat (1994) and individual death records.

Table 1: Descriptive statistics: town and population characteristics.

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

Source: Authors' calculations on town structural characteristics (Istat (1990)), population flows data (Istat (1980)) 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. ([a]) significant at 5%; ([g]) 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
1965	53.831[g]	1.126	6.613[g]	2.111[g]	17.200[g]	3.145[g]	7.608[g]	-1.497[a]	1.926[g]	-0.314	46.670[g]	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)
1964	51.442[g]	3.446	7.050[g]	1.638[a]	16.106[g]	4.545[g]	7.491[g]	-1.211[a]	1.997[g]	-0.249	47.739[g]	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)
1963	51.103[g]	1.003	6.522[g]	2.103[a]	16.624[g]	3.152[g]	7.002[g]	-1.350[a]	1.853[g]	-0.248	48.760[g]	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)
1961	48.203[g]	1.203	5.936[g]	1.867[a]	16.580[g]	3.211[a]	6.632[g]	-1.456[a]	1.918[g]	-0.558[a]	50.862[g]	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)
1960	46.948[g]	1.285	5.533[g]	1.49	16.717[g]	2.968[a]	7.011[g]	-2.064[g]	2.110[g]	-0.579[a]	51.851[g]	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)
1959	44.819[g]	1.179	5.355[g]	0.627	16.133[g]	4.067[g]	6.759[g]	-1.729[a]	1.890[g]	-0.570[a]	52.890[g]	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)
1958	42.746[g]	0.669	5.009[g]	0.86	15.821[g]	3.157	6.994[g]	-2.208[g]	2.041[g]	-0.589	53.984[g]	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)
1957	41.296[g]	0.546	4.803[g]	0.691	16.690[g]	2.024	6.944[g]	-1.498	1.979[g]	-0.276	54.919[g]	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)
1956	39.531[g]	-0.405	3.820[g]	-0.113	17.091[g]	2.11	7.534[g]	-2.263[a]	2.493[g]	-1.074[g]	55.970[g]	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.
([a]) significant at 5%; ([g]) significant at 1%.

Table 3: First stage results: High school and Exemption.

(1)	(2)	(3)	(4)	(5)
2.65 [0.019]	2.28 [0.030]	1.98 [0.081]	1.97 [0.034]	3.80 [0.002]
Birth Year:				
51-65	56-65	59-65	56-65	56-65
Towns:				
117	117	117	117	74
Parental chars:				
N	Y	Y	Y	Y
Age dummies:				
C	C	C	S	C

P-values in brackets.
All regressions include also a quadratic in cohort size.

Table 4: OLS, Reduced form and IV estimates by cause.

	(1)	(2)	(3)	(4)	(5)
			All deaths		
RF	-0.29	-0.30	-0.36	-0.27	-0.52
	[0.116]	[0.137]	[0.141]	[0.159]	[0.055]
IV	-0.11	-0.13	-0.18	-0.14	-0.14
	[0.117]	[0.108]	[0.081]	[0.146]	[0.026]
OLS	-0.007	-0.008	-0.014	-0.007	-0.010
	[0.193]	[0.222]	[0.163]	[0.303]	[0.251]
			Natural deaths		
RF	-0.28	-0.31	-0.38	-0.30	-0.49
	[0.038]	[0.036]	[0.061]	[0.032]	[0.016]
IV	-0.11	-0.14	-0.19	-0.15	-0.13
	[0.073]	[0.035]	[0.022]	[0.043]	[0.008]
OLS	-0.007	-0.008	-0.012	-0.006	-0.001
	[0.167]	[0.139]	[0.129]	[0.280]	[0.092]
Birth Year:					
	51-65	56-65	59-65	56-65	56-65
Towns	117	117	117	117	74
Parental chars:					
	N	Y	Y	Y	Y
Age dummies:					
	C	C	C	S	C

P-values in brackets. P-values for IV estimates computed on the $\chi^2(1)$ distribution of the Anderson-Rubin statistic. All regressions include also a quadratic in cohort size.

Table 5: Placebo experiments.

	Dependent variable		
	% High School	% All deaths	% Natural deaths
A. Control vs. Outer Control-OC	-0.06	0.19	0.13
	[0.959]	[0.354]	[0.461]
B1. 30-40	0.38	-0.06	-0.06
	[0.642]	[0.692]	[0.672]
B2. 30-40	0.41	-0.05	-0.06
	[0.612]	[0.746]	[0.696]

P-values in brackets. P-values for IV estimates computed on the $\chi^2(1)$ distribution of the Anderson-Rubin statistic. All regressions include a quadratic in cohort size, town and cohort fixed effects. (A) and (B1) also include parental controls (share of parents employed, unemployed, self-employed, with a high school degree, with a college degree). Sample: (A) - cohorts 1956-1965 from Control and Outer Control towns, $EX = 1$ if born in Control town in 1963-1965; (B1) and (B2) cohorts 1961-1951 from Treated and Control towns, $EX = 1$ if born in Treated town in 1956-1961;

Table 6: Reduced form estimates over various horizons.

	(1)	(2)	(3)	(4)	(5)
			All deaths		
1981-2001	-0.45	-0.42	-0.46	-0.44	-0.45
	[0.047]	[0.107]	[0.091]	[0.087]	[0.235]
1986-2001	-0.47	-0.44	-0.52	-0.42	-0.62
	[0.031]	[0.072]	[0.054]	[0.071]	[0.061]
1991-2001	-0.29	-0.3	-0.36	-0.27	-0.52
	[0.116]	[0.137]	[0.141]	[0.159]	[0.055]
			Natural deaths		
1981-2001	-0.43	-0.47	-0.64	-0.47	-0.68
	[0.007]	[0.010]	[0.009]	[0.008]	[0.007]
1986-2001	-0.4	0.44	-0.59	-0.44	-0.63
	[0.010]	[0.018]	[0.013]	[0.013]	[0.019]
1991-2001	-0.28	-0.31	-0.38	-0.3	-0.49
	[0.038]	[0.036]	[0.061]	[0.032]	[0.016]
Birth Year:					
	51-65	56-65	59-65	56-65	56-65
Towns	117	117	117	117	74
Parental chars:					
	N	Y	Y	Y	Y
Age dummies:					
	C	C	C	S	C

P-values in brackets. P-values for IV estimates computed on the $\chi^2(1)$ distribution of the Anderson-Rubin statistic. (1) cohorts 1951-1965, includes quadratic in cohort size, town and cohort FE; (2) and (3) cohorts 1956-1965 and 1959-1965, includes quadratic in cohort size, parental characteristics, town and cohort FE; (4) cohorts 1956-1965, includes quadratic in cohort size, parental characteristics, town and geographic specific cohort FE; (5) cohorts 1956-1965, includes quadratic in cohort size, parental characteristics, town and cohort FE, only treated towns with 0 or 6 damage score and neighbouring controls.