Schooling and Youth Mortality: Learning from a Mass Military Exemption*

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Abstract

We examine the relationship between education and mortality in a young population of Italian males. In 1981 several cohorts of young men from specific southern towns were unexpectedly exempt from compulsory military service after a major quake hit the region. Comparisons of exempt cohorts from least damaged towns on the border of the quake region with similar ones from neighbouring non-exempt towns just outside the region show that cohorts exempt while still in high school increased their schooling while older ones, already beyond high school, did not. The probability of dying over the decade 1991-2001 is also significantly lower for the former group, while it is unchanged for the latter. Because all cohorts considered did not serve in the army, this finding plausibly reflects a causal effect of high school completion on mortality. Completing the degree reduces mortality between age 25 and 35 by about 0.15 percentage points, one tenth of the baseline probability of death.

Keywords: *education, mortality, health, human capital.*
JEL Codes: I20, I12.

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1 Introduction.

Education is the single most important correlate of own health (Kitagawa and Hauser (1973), Grossman and Kaestner (1997), Deaton and Paxson (1999), Cutler and Lleras-Muney (2006)). The mechanisms underlying such empirical regularity can be manifold. Education could affect health, because more educated people tend to smoke and drink less, do more physical exercise, monitor more closely their health (Cutler and Lleras-Muney (2006)), are more likely to use newly introduced drugs (Lleras-Muney and Lichtenberg (2005)), and can afford more expensive care[1]. Also, it may be that individuals in better health or with a longer life expectancy have stronger incentives (Ben-Porath (1967), Kalemli-Ozcan, Ryder and Weil (2000), Jayachandran and Lleras-Muney (2009)) or more chances to accumulate human capital (Case, Fertig and Paxson (2005)). Finally, it could simply follow from the presence of common determinants of both outcomes, such as an individual's rate of time preference shaping both the investment in schooling and that in health (Fuchs (1982)) or her family background (Currie and Moretti (2003)).

In this paper we explore whether schooling affects subsequent mortality rates in a population of young males in a quasi-experimental setting. In 1981, young men born before 1966 and living in specific towns of southern Italy were exempt from compulsory military service after a major quake hit the region few months earlier (Cipollone and Rosolia (2007)). Standard human capital theory (e.g. Ben-Porath (1967)) suggests that this exemption should increase the incentives to stay in school: being released from military obligations implies, all else equal, an increase in the proportion of one's lifetime over which returns to education accrue[2]. However, only younger exempt cohorts had the possibility to revise their human capital accumulation plans. Exemption was granted to them while

[1] More generally, education could increase the ability to assess the consequences of certain habits or exploit certain health inputs. Grossman (2006) distinguishes between allocative and productive efficiency. The former pertains to the greater awareness of the more educated of the health consequences of certain behaviors or choices; the latter regards the ability of the more educated to make more in terms of health out of given inputs.

[2] In a recent paper, Jayachandran and Lleras-Muney (2009) show that sharp declines in maternal mortality in Sri Lanka led to higher investment in human capital for young women.

still in high school and before becoming formally eligible for service. On the contrary, contiguous older cohorts were released from any obligation shortly after becoming eligible, at an age beyond high school.

The intervention provides a potentially exogenous source of variation for schooling. However, the exemption is clearly correlated with a major shock and with the ensuing compensatory interventions that may have also had direct effects on the outcomes of interest. To address this issue we develop a research design that replicates an experimental setting in which the only difference among individuals is exposure to the exemption and its timing over the lifecycle. More specifically, we compare schooling levels and subsequent mortality rates of younger and older exempt cohorts from towns on the inner border of the quake region with those of similar non-exempt cohorts from neighboring towns just outside the quake region. Inner treated towns are the least damaged by the quake, thus also unlikely to receive any compensation; neighbouring control towns are geographically close, thus largely sharing the same economic environment as treated towns but excluded by any quake related intervention[3].

We find that in 1981 high school drop-out rates of cohorts exempt while of high school age are significantly lower by 3.6 percentage points than those of similar cohorts from control towns, consistently with the predictions of a standard human capital model; a similar proportion of youths belonging to older exempt cohorts - released from military obligations after high school age - substituted instead army service with labor market participation. Ten years after the quake, in 1991, the former group displays a significantly larger share of high school graduates, by over 2 percentage points; all other circumstances of younger and older exempt cohorts are not statistically different from those of their non-exempt counterparts. The probability of dying over the next ten years, between 1991 and 2001, for the same younger exempt individuals is significantly lower by 0.35 percentage points, about one fourth less than the one recorded in comparable non-exempt cohorts. No difference can be detected

[3] Card and Krueger (1994) pionereed this kind of research design in their study on minimum wages and employment in New Jersey.

between older exempt cohorts and comparable non-exempt ones.

The findings imply that the exemption led over 3,000 youths from the quake region to complete high school instead of dropping out; over the subsequent decade 1991-2001, about 600 youths in their mid-20s also survived to their mid-30s. The effect on life expectancy at age 25 is also substantial. Under the extreme assumption that the mortality rate reduction induced by the exemption is limited to the ten years between age 25 and 35, life expectancy at age 25 increased by about one quarter of a year. To put this change in context, between 1980 and 1990 life expectancy at age 25 increased by about one year.

The implied 2SLS estimate of the effect of high school completion on an individual's probability of dying between his mid-20s and mid-30s points to a reduction of around 0.15 percentage points. The main assumption required to correctly interpret this figure as the causal effect of high school completion on subsequent mortality is that military service has no independent effect on health and life expectancy. This identifying restriction finds support in our data: the decline in conscription rates detected among older cohorts exempt when already beyond high school is not associated with higher schooling or with changes in subsequent mortality.

Only recently a variety of studies have addressed the nature of the relationship between schooling achievements and health outcomes adopting appropriate techniques to overcome endogeneity and reverse causality issues, finding support to the existence of an effect of human capital on health. Among others, Lleras-Muney (2005) finds that one additional year of schooling lowers the probability of death over the next ten years by about 3.5 percentage points, an increase in life expectancy at age 35 of about 1.7 years; Oreopoulos (2006) corroborates these results using self-reported health indicators and finds qualitatively similar effects for UK cohorts exposed to increases in minimum school-leaving age; Kenkel, Lillard and Mathios (2006) show that already by ages 35 to 42, successful high school completion improves body mass indexes and reduces the probability of smoking, two leading preventable causes of death.

Our paper contributes to this literature in several respects. First, we show that the relationship of interest is pervasive and emerges also at much younger ages than those typically investigated by the studies mentioned above. Second, the first stage results contribute to the related literature that studies the reverse causal link, namely from life expectancy to human capital investment, by showing that exogenous increases in the proportion of one's working life lead to more schooling. Third, the empirical approach allows for a clean identification of the effects of higher schooling on the population of those at risk of dropping out (Imbens and Angrist (1994)), presumably those most affected by interventions that aim to raise average schooling.

The paper proceeds as follows. We start with a description of the 1980 quake and related interventions. Next, we illustrate the research desing underlying our analysis. We discuss results and robustness checks in section 4. We then conclude.

2 Background.

2.1 Compulsory military service and schooling.

Military service has been compulsory for all Italian men until recently. Young men were subject to a thorough medical check-up in the year they turned 18. If minimal health conditions were met they were inducted as they reached the age of 19 and required to serve for at least 12 months. However, the total time absorbed by conscription could be much longer because of lenghty bureaucracy and because conscripts were inducted only few times a year. Employment before compulsory military service is also an unlikely event because employees have the legal right to be re-hired in the same job after military service, a costly prescription for employers. Finally, one-year deferments could be obtained provided the conscript was still participating in the education system and deemed to be reasonably on track.

Although military service was structured not to interfere with the natural completion of high school at 18-19 years old, being subject to such obligation could be a sufficient incentive for some marginal individuals to drop-out. If high-school completion turns out to be unlikely, staying in school only to defer service is suboptimal as it would only result in a delayed entry in the labor market and a loss of earnings[4].

2.2 The quake and the compensatory measures.

Figure (1) shows the area of southern Italy hit by a major earthquake in November 1980. The area was home to about 5 million persons (10 percent of the national population), and counted around 650 towns. The quake caused about 2,000 deaths and 10,000 casualties, mostly around the epicentre; about 300,000 persons were in need of temporary shelter; 60 percent of the residential estate stock was damaged in the epicentre and about 20 percent in the surrounding area (Ministero del Bilancio e della Programmazione Economica (1981)). Beyond its direct effects, the quake also represented a major labor market shock, with employment in the two most directly affected regions, Campania and Basilicata, dropping by 1.2 percent in a year.

Compensatory measures were explicitly targeted to the set of towns identified as damaged in the laws passed in the aftermath. Financial transfers were further linked to the extent of damages suffered as reported by several government agencies (Ministero del Bilancio e della Programmazione Economica (1981)). In the subsequent three years the funds budgeted for recovery by the Parliament amounted roughly to one fifth of the GDP of the area.

The relief package also included the cancelation of military obligations for all men born before 1966 living in the quake region, independently of the damages suffered by their hometown[5]. Figure (2)

[4]Staying longer in school to defer military service could be optimal even when degree completion is unlikely if an exemption is expected. For example, Imbens and van der Klaauw (1995) exploit the likelihood of such events for a study on the earnings consequences of military conscription in the Netherlands. However, such mass exemptions were extremely rare events in Italy. Maurin and Xenogiani (2007) put forth an opposite argument. They argue that compulsory military service provides incentives to stay in school just to defer it. However, the underlying theoretical explanation (Maurin and Xenogiani (2005)) crucially relies on the absence of a finite time horizon, whereby deferring service comes at no cost in terms of foregone earnings.

[5] While economic support was clearly linked to the magnitude of damages suffered by the town, the exemption was

shows the impact of such specific measure. It compares the share of men who had complied with their military obligations by 1998 in the quake region and in the centre-north of Italy, obviously unaffected by the quake and by its compensatory interventions, by cohort[6]. Unsurprisingly, the drop is larger for younger cohorts who learned about the change before actually becoming formally eligible for service. The cancelation of military obligations had plausibly different effects on the various exempt cohorts as they learned about it at different stages of their lives. Younger cohorts, born in 1963-65 were not yet eligible for service and, being still of high school age, had the time to revise their plans as concerns high-school completion. On the contrary, comparable older ones had already either completed or dropped out of high school. As shown by Keane and Wolpin (1997), schooling choices are highly persistent and fairly irreversible. Therefore, drop-outs belonging to older exempt cohorts were unlikely to change their plans as regards high school after learning about the exemption.

3 Research design

Our sample frame is constructed so as to replicate an experimental setting in which the only difference among individuals is exposure to the exemption and its timing over the lifecycle. We focus only on towns that lie on the border of the quake region (fig. 3). Towns along the border but *within* the quake region were exposed to the exemption as well as possibly to other compensatory measures (treated towns). Towns just outside the quake region, on the other hand, were untargeted by all quake-related measures (control towns). Overall, the sample consists of 117 towns (57 within the quake region and 60 just outside). The average town size is quite small, 7,700 people and the median is about 2,700. Treated towns are somewhat smaller: the median treated town is about 2,500 against a median control

eventually extended to all towns in the administrative regions mostly hit by the quake. Thus, in several cases also to youths from towns that were classified as undamaged.

[6]Data are drawn from the Indagine Longitudinale sulle Famiglie Italiane (ILFI), a representative longitudinal panel of about 5,000 italian households. It was started in 1997-98 with a full reconstruction of previous schooling, employment and mobility episodes. Individuals were also asked whether they were ever drafted for compulsory military service. See Pisati and Schizzerotto (2004) for details. A description of the data is available at http://www.soc.unitn.it/ilfi/eng.index.html.

town of 2,900[7]. The distance between neighbouring treated and control towns is also small, about 6 miles on average and always below 16 miles.

Within treated towns the exemption was granted almost simultaneously only to males born before 1966. Because they were at different points of their lifecycle, exemption status modified in different ways their incentives leading to different choices. In particular only those born in 1963-65 were still of high school age and presumably able to change their decisions concerning high school. As shown by Eckstein and Wolpin (1999), high school dropouts have significantly lower expected valuations of graduation. Therefore, an exogenous increase of the time span over which the benefits from a high school degree are reaped could increase the overall valuation of completing the degree, at least for some individuals.

This design has at least two appealing features. First, towns along the inner border of the quake region suffered minor damages or none at all, as reported by the government agencies assessing the consequences of the quake. Specifically, of the 57 towns, 18 did not record any 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[8]. This means that these towns were generally not entitled to significant compensatory financial transfers. However, all young men belonging to specific cohorts were still granted the exemption from military service[9]. Second, geographic proximity between neighbouring towns along the border of the quake region implies that they largely shared the same economic environment. Thus, for example, they were exposed in a similar fashion to the deep economic recession ensuing the quake. More broadly, any general equilibrium effect of the quake likely had the same impact on similar individuals across the border.

[7] Town sizes are computed as of 1979 from Istat' population records.

[8] Additional details are provided in Cipollone and Rosolia (2007).

[9] Since the quake mostly affected two regions, Campania and Basilicata, the government eventually decided to exempt all men in those regions irrespective of the actual damages suffered by their towns of residence, provided they belonged to one of the two regions.

We exploit three major data sources. The 1981 and 1991 full population censuses and a confidential dataset collecting the universe of death certificates for all deaths occurred in Italy between 1981 and 2001. This allows us to recover all the individual information available at the detailed geographic level required by the research design.

The 1981 population census allows to assess whether towns just outside the quake region represent the appropriate counterfactual for towns just inside the region[10]. Census data allow us to compare educational achievements and employment rates in the two groups of towns. We focus on the adult population, specifically people aged 30-50 at census date. Notice that the census was run the year after the quake. In principle, there are at least two reasons why this could impair the relevant comparisons. First, populations of treated towns might have migrated elsewere because of the quake so that those observed in 1981 might already be a selected subsample of those truly exposed to the quake and related interventions. Second, even if there is no migration the comparison might reflect already the presence of different effects of the quake. Geographic proximity should guarantee that both channels worked, if at all, in the same direction in both groups of towns. To empirically address the selection effect induced by differential outward migration we examine yearly gross outflow rates from the sampled towns between 1974 and 1987. These are on average around 2 percent and the difference between treated and control towns is an order of magnitude lower, around 0.2 percent. To establish whether the quake led to a larger outflow from treated towns we regressed yearly outflow rates between 1974-1987 on a set of year dummies and their interactions with town's treatment status. We found that only three interacted dummies (1977, 1978 and 1981) had a p-value below 0.15 but never below 0.05[11]. Moreover, an F-test of the joint significance of the interacted terms does not reject the null with a

[10]Specifically, for all our analyses we had access to the entire population census and extracted all records pertaining to the units of analysis. This allows to avoid sampling issues related to the small size of the relevant units (town-cohort) that would arise if we used the 5% public use census sample. Because of privacy legislation, access to the universe of census records was possible only through the National Statistical Institute (ISTAT) safecenter.

[11]We estimated a weighted regression with weights equal to average population size between t and $t+1$ clustering standard errors at the town level.

p-value of 0.3. Overall, it seems that outflows were very similar between the two sets of towns, and relatively low, both before and after the quake so that it is unlikely that the population living in the treated towns as of 1981 had changed significantly and in a different direction with respect to that of towns living just outside the border.

Figure (4) shows the differences by age between treated and control towns in the percentage of high school graduates among the adult resident population together with the associated 95 percent confidence interval. Differences are generally small and never statistically significant at customary confidence levels although among older cohorts there appears to be a generally lower level of education in treated towns.

The two groups of towns are similar also in terms of employment rates. In this respect, the census allows us to compare the populations also strictly before the quake, exploiting a recall question on employment status five years earlier, in 1976. Figure (5) displays the estimated differences by age and the corresponding 95 percent confidence interval, showing that before the quake employment rates were not significantly different. Importantly, by comparing the *change* in town-level cohort-specific employment rates between 1976 and 1981 we can assess whether the quake, either directly or indirectly, had a differential effect on the adult population. The comparisons, reported in figure (6), show that at all ages considered there are no statistically significant difference, supporting the assumption that the quake had similar, if any at all, effects on the two groups of towns.

4 The effects of the exemption.

The previous section has shown that the adult populations of treated and control towns not only shared the same environment but were also comparable, around census date, in terms of human capital and labor market performance. Therefore comparisons of exempt individuals to similar individuals from neighbouring non-exempt towns seem an appropriate strategy to estimate the effects of exemption status on a variety of outcomes of interest.

We start by providing some preliminary evidence based on unconditional comparisons between comparable subgroups from treated and control towns. We then move to the formal empirical analysis.

4.1 Preliminary evidence.

Table (1) reports for three subsequent cohorts the percentage of males from treated and control towns in a given condition and the corresponding differences at three points in time: at 1981 census date, in 1986 and at 1991 census date; data on 1986 are drawn from retrospective questions asked in the 1991 census. All cohorts from treated towns had been exempt from military service. The first three rows describe the shot-run effect of the exemption, as documented by the 1981 census. In 1981 youths born in 1963-65, who at that time were 16 to 18, were exempt although not yet eligible for military service. This nonetheless translated in an increase in the proportion of those enrolled in high school by 3.6 percentage points (p-value 0.027). Correspondingly, there was a decline in the percentage of those in the labor market, reflecting a choice towards schooling consistent with the mechanism that a longer expected working life increases the incentives to accumulate human capital (Ben-Porath (1967), Jayachandran and Lleras-Muney (2009)). At the same point in time, youths born in 1959-61 had been exempt from military obligations soon after becoming formally eligible to serve. The percentage of conscripts fell by 3.5 percentage points, a proportion similar to that choosing education in the younger cohorts. The lower conscription rates basically translated in a higher labor market participation rate rather than schooling. Older youths born in 1956-58 (age 23-25) were instead basically unaffected by exemption status, reflecting the fact that by that age only a tiny proportion have not yet complied with army service.

To track the long-run effects of exemption status we draw on the 1991 population census, which also collects retrospective information on 1986. Ideally, we would like to observe the same individuals in both data sources, that is those living in sampled towns as of 1981. Unfortunately, the 1991 census does not provide information about past place of residence. We thus proxy the town of residence as of 1981 with town of birth[12]. The evidence discussed in the previous section suggests that geographic mobility is not a relevant issue in our specific sample.

By 1986, all cohorts were equally likely to be in education, irrespective of exemption status. This is consistent with the idea that the exemption tilts the incentives in favor of more schooling only for a subset of the population that would have otherwise dropped out of high school, a set of youths unlikely to enroll in college. A slightly higher but still statistically insignificant proportion of exempt youths born in 1963-65 appears to be participating in the labor market. By 1991, the only relevant difference between exempt and non-exempt cohorts appears to be the higher proportion of high school graduates among youths exempt before becoming eligible and who, as of 1981, chose to stay more in school. The p-value of the unconditional difference in the percentage of high school graduates is 0.088 and compares with the much lower levels of significance and point estimates close to zero detected for the other cohorts and for all other outcomes recorded in 1986 and 1991. To put the difference in high school completion rates in context, it implies that in the quake region over 3,000 youths who would have otherwise dropped out completed high school because of the exemption.

Finally, the last three rows report the probability of dying between 1991 and 2001 for a given town- and year-of-birth cell. We recover this measure combining the information on the initial population at risk in cohort c born in town j ($N_{c,j}$) from the 1991 population census with that obtained from the individual death data. For each death, we observe the date of the event and a variety of individual characteristics including town and year of birth. We recover the total number of deaths occured over

[12]Proxying place of residence at young ages with place of birth is pretty customary, for example in the large literature started by Angrist and Krueger (1991) exploring the effects of changes in US compulsory schooling state laws. For the sake of comparability we should have performed also the analysis on 1981 data on the basis of town of birth. However, the 1981 provides information on the place of birth only at the larger province level. Thus by using place of birth instead of residence we would have included in the sample also people likely to live in towns more strongly exposed to the quake and to other compensatory interventions invalidating the spirit of the research design.

the relevant period in cell $\{c, j\}$ $(D_{y,c})$ and combine it with 1991 census data on the corresponding population at risk obtaining the measure of interest, $D_{c,j}/N_{c,j}$. The data also include detailed information on causes of death, according to the 9th release of the International Classification of Diseases[13].

The evidence reported in the table shows that the probability of dying over the decade 1991-2001 was lower among exempt 1963-65 cohorts by 0.34 percentage points (p-value: 0.034). At the same time, the estimated difference among older cohorts is basically zero, and is estiamted with a similar degree of precision as that between younger cohort. The difference among younger cohorts, as shown in the last two rows, is entirely due to a lower incidence of natural causes[14]. The drop in mortality rates is quite substantial: it amounts to about one fourth of the death rate from all causes estimated for non-exempt control cohorts and to about one half of deaths due to natural causes only. The difference implies that in the region targeted by the exemption about 600 males in their mid 20s who would have otherwise died earlier reached their mid 30s. The effect on life expectancy at age 25 is also substantial. Under the extreme assumption that the mortality rate reduction induced by the exemption is limited to ages 26 to 36, life expectancy at age 25 increased by about one quarter of a year. To put this in context, between 1980 and 1990 life expectancy at age 25 increased by about one year.

Summing up, the evidence shows that youths exempt from military service before becoming eligible substituted labor market participation with schooling, while those exempt immediately after becoming eligible chose to enter the labor market. No significant difference can be detected among those exempt several years after first becoming eligible for service. Five and ten years after exemption status was granted, the three groups turn out to be similar to their counterfactual counterparts along most dimensions except for the higher proportion of high school graduates among the youngest

[13]See http://www.cdc.gov/nchs/icd/icd9.html for more information.

[14] We code deaths on the basis of the *main* cause as reported in the death certificate according to the 9th revision of the International Classification of Diseases (ICD9). We define natural deaths those due to internal causes (codes: 0001-7999).

exempt cohorts[15]. Similarly, only for the latter group mortality rates over the subsequent decade are significantly lower than those of comparable non-exempt individuals. Also, mortality rates of the two oldest cohorts considered are not different both across exemption status and, by exemption status, across cohorts. This suggests that the early labor market start of the middle cohorts did not have effects on their subsequent mortality. Moreover, since their conscription rates are lower than those in the control group, this also suggests that serving in the army has no independent effect on subsequent mortality thus supporting the identifying exclusion restriction needed to use exemption status before eligibility as an instrument in a regression of mortality rates on high school completion.

In the following section we move to a formal regression analysis to show that such conclusions are robust to inclusion of a number of controls and robustness checks.

4.2 Regression analysis.

The evidence presented above, although suggestive, is based only on unconditional comparisons of oucomes across similar exempt and non-exempt youths. While the research design should guarantee that these represent unbiased estimates of the effects of exemption status on the various cohorts, a more efficient estimate can be obtained exploiting all the available information. In the following we present results obtained from estimation of linear models of the type:

$$y_{c,j} = \alpha + \beta E_{c,j} + \gamma X_{c,j} + \mu_c + \nu_j + e_{c,j} \tag{1}$$

where $y_{c,j}$ is the outcome of interest in cohort c from town t (for example, the share of high school graduates in 1991); $X_{c,j}$ are town-cohort specific characteristics; μ_c and ν_j are, respectively, cohort and town dummies[16]. $E_{c,j}$ is a dummy equal to 1 if cohort c from town j was exposed to exemption before becoming formally eligible. The exercise quantifies the effects of early exemption by comparing

[15] Incidentally, the high school completion rate of the population that changed schooling choices because of the exemption are close to 60 percent.

[16] Equation (1) can be thought of as the grouped version of an individual linear probability model $y_{i,c,j} = \alpha + \beta E_{c,j} + \gamma X_{i,c,j} + \mu_c + \nu_j + e_{i,c,j}$, where $y_{i,c,j}$ is an indicator for a given circumstance of individual i in 1991, for example high school graduation, or for whether she died between 1991 and 2001. Groups are defined by $\{c, j\}$.

outcomes early exemptees to those of similar non-exempt cohorts and to older exempt cohorts. In particular, $E_{c,j} = 1$ for cohorts born in 1963-65 in treated towns and zero otherwise. The relevant sample includes cohorts born between 1956 and 1965.

Table (2) displays estimates for β in equation (1). Row headings describe the dependent variables while columns correspond to different specifications of the control set or underlying samples. In column (1) we only include dummies for town of birth and year of birth. Town dummies capture all the unobserved heterogeneity across towns, including potentially different - direct and indirect - effects of the quake that are common across contiguous cohorts from the same town. In column (2) we add a quadratic in cohort size to capture within-town of birth heterogeneity due, for example, to local congestion effects; in column (3) we further add parental schooling and labor market characteristics[17]. In column (4) we allow for 5 groups of cohort dummies, defined on the basis of geographic location, to allow for possibly different secular patterns in the relevant outcomes. In column (5) we restrict the sample only to people from the 33 towns that either did not record any damage or were at the lowest of the official damage scale and to their 41 direct neighboring towns. Becasue this subset of treated towns towns was only exposed to the exemption, the exercise allows to rule out the possibility that previous estimates reflect the presence of cohort-specific effects of the quake unrelated to the exemption, for example due to the fact that only younger cohorts responded to the limited income transfers assigned to sampled treated towns. Finally, in column (6) we run a placebo experiment pretending it was cohorts born in 1963-65 in *control* towns to be exempt and comparing them with similar youths from towns further outside the quake region and neighboring on control towns. This provides an indirect test of the presence of direct effects of the quake that affect differently specific cohorts and change with distance from the epicenter. If younger cohorts were affected differently by

[17]Because data are obtained from the 1991 census, most of the youths in our sample have already left home (see Manacorda and Moretti (2006) for a discussion of the motivations of Italian youths to live in the parental household). We recover family background at the year of birth-town of birth level from the 1981 population census. Specifically, we compute average schooling and labor market conditions of parents of male children of the relevant age born in the relevant town. Unfortunately, the census does not collect any information on earnings, income and wealth.

the quake as compared to older cohorts from same towns and if, as it seems reasonable to assume, these effects decay with distance from the epicenter, we should expect to find some difference also when comparing younger males from control towns to similar ones from towns less exposed to the quake shock.

Columns (1) to (4) confirm the preliminary results of the previous section. In particular, only graduation rates from high school turn out to be significantly higher for early exempt cohorts. The estimated difference is above 2 percentage points, with p-values around or below 0.02 when controls are included and 0.057 percent in the barebone specification of column (1). Early exempt individuals display the same labor force participation rates as similar non-exempt cohorts, even accounting for unobservable heterogeneity across towns and for area-specific trends. Consistently with the idea that the population affected by the instrument is the one that would have otherwise dropped out of high school, school enrollment rates in 1986 and 1991 as well as the proportion of college graduates is unaffected by exemption status.

The last three rows of the table report results on the probability of dying between 1991 and 2001. As before, results in column (1) to (4) show a decline in total mortality of about 0.3 percentage points. The difference is now only marginally significant when we account for parental characteristics and cohort size, with p-values just above 0.1 . However, inclusion of these controls does not affect the evidence on deaths by natural causes. Point estimates are in line with those obtained as regards total deaths, suggesting that all the change is to be traced to a decline in deaths by natural causes, but precision levels are remarkably higher, with p-values always below 0.014. Importantly, all point estimates reported in columns (1) to (4) are of the same order of magnitude as the corresponding unconditional differences reported in table (1), suggesting that the research design does a good job in defining an approriate pseudo-experimental sample.

In column (5) we restrict the analysis to the group of treated towns that did not record any damage and to their direct neighbours, so as to exclude any potentially omitted effect of compensatory interventions. While the estimated increase in graduation rates is now higher, the differences detected on all other outcomes as of 1991 remain statistically insignificant and close to zero. However, consistently with the stronger increase the percentage of high school graduates, also the decline in mortality is stronger.

Finally, in column (6) the placebo experiment shows that youths in control towns and outer control towns, none exposed to the exemption, are equally likely to complete high school. Also, no difference can be detected as concerns subsequent mortality rates. This evidence allows to rule out that the change in schooling reflects stronger exposure to the direct effects of the quake rather than only to the exemption.

4.3 The effects of schooling on subsequent mortality.

The evidence discussed above provides elements to assess the effect of high school completion on subsequent adult mortality under the assumption that military service has no direct effect on mortality, so that early exemption can be taken as the excluded instrument. Under this assumption, the ratio of the estimated declines in mortality to that of the estimated increases in schooling reported in table (2) provide consistent estimates of the causal effect of interest[18]. This ranges between 0.15 and 0.10 percentage points lower mortality rate because of high school graduation and compares with a baseline probability of death of around 1.5 percent.

A first concern with this result is the validity of the exclusion restriction, namely the absence of an independent effect of army service on mortality. The conventional presumption is that army service increases unhealthy behavior and exposure to harmful environments. Indeed, several recent studies that use the Vietnam draft lottery do not find increased mortality, worse health conditions or more

[18]More specifically, without additional assumptions, the quantity identifies the effect of high school completion on subsequent mortality among those whose schooling increases because of the exemption, the so-called LATE (Imbens and Angrist (1994), Angrist, Imbens and Rubin (1996)

unhealthy behaviors among Vietnam veterans (among others, Angrist and Chen (2007), Dobkin and Shabani (2009), Conley and Heerwig (2009)) years after discharge. On the contrary, Bedard and Deschenes (2006) show that WWII and Korean War veteran status is associated to higher mortality and morbidity rates 20 to 50 years after discharge but relate this to increased smoking induced by freely available cigarettes. These settings however are not fully comparable with compulsory military service in peace time when most of the activity reduces to physical exercise, military training and guard duties. Indeed, because conscripts are subject to continuous monitoring and harsher punishment for misbehaviors, we conjecture that certain unhealthy habits (e.g. drugs and alcohol use) could even be lower than among comparable non-conscripts. Moreover, there are strong incentives to undertake medical checks when in the army because all duties are temporarily suspended thus leading to better health monitoring. Overall, from a theoretical point of view it is at least unclear whether such effect should exist in our setting.

Although the required assumption cannot be formally tested, the research design allows for some indirect inference. Table (1) provided some preliminary evidence that cohorts born in treated towns and exempt just after eligibility were significantly less likely to be in service in 1981 and yet did not record any significant change in subsequent outcomes in comparison with similar cohorts from non-exempt towns or with older cohorts from treated towns exempt later on in life. We pursue this argument in table (3). We estimate equation (1) on the sample of youths from treated and control towns born between 1953 and 1961 to check whether cohorts born in 1959-61 in treated towns, exposed to the exemption at age 20-22 thus shortly after becoming formally eligible, display any difference with similar non-exempt cohorts from control towns. Consistently, the dummy $E_{c,j} = 1$ for cohorts born in 1961-59 in treated towns and zero otherwise. While the precision of the estimates turns out to be higher than in the previous set of exercises, we are unable to detect any statistically significant difference between outcomes up to 1991. As concerns subsequent death rates, point estimates are basically zero in models (1) to (4) and still statistically not different from zero when restricting the sample to least damaged towns. Overall, the evidence is consistent with the assumption that exposure to exemption and absence of army service have no direct effects on subsequent outcomes, including mortality rates.

A second concern relates to the strength of the first stage regression, that is the change in schooling associated with exemption status before formal eligibility. The statistical significance of the results on high school completion based on the most comprehensive specification reported in column (4) of table (2) is around 2 percent. This corresponds to an F-statistic of around 5.5, below both the benchmark value of 10 conventionally suggested to claim robustness of the IV identification (Staiger and Stock (1997)) and the more appropriate critical values tabulated in Stock and Yogo (2005). This implies that standard tests have incorrect size, leading to wrong inference on the magnitude and significance of the structural parameters of interest. Recently, several test have been proposed that have correct rejection regions even under potentially weak instruments[19]. In particular, a convenient test for a just-identified model with potentially weak instruments as ours is the Anderson-Rubin (AR) test (Anderson and Rubin (1949), Moreira (2001)).

Table (4) reports the first stage results on the effects of the exemption on subsequent high school graduation rates along with standard errors. We then report the implied 2SLS estimate of the effect of completing high school on subsequent mortality rates as implied by the results in table (2) along with the p-values associated to the weak-instrument robust AR test. Columns (1) to (4) report results for the corresponding specifications of table (2) estimated on the sample of youths born between 1956 and 1963. Column (5) reports the results for the subset of quake towns that did not record any damage and their neighboring control towns. According to the appropriate test, the causal effect of schooling on total subsequent mortality is only weakly significant, with the AR test rejecting the null with p-values

[19] For a survey of the tests that are valid under weak instruments see Stock, Wright and Yogo (2002).

around 10 percent. A much higher estimation precision is achieved when focussing only on deaths due to natural causes, where the null of no effect is rejected with around 1 percent probability, and still below the conventional 5 percent level when we only focus on undamaged towns (col. 5).

Finally, in column (6) we restrict the attention to the subset of cohorts where the preliminary evidence showed a significant decline in conscription rates, namely those born between 1956 and 1965. Because in all these cohorts youths from treated towns display significantly lower conscription rates than control counterparts (contrary to youths exempt at older ages), IV estimates allowing for town fixed effects in practice account directly for potentially direct unobserved effects of military service on health and subsequent mortality. The results of the exercise confirm all previous findings and point to a statistically significant reduction of mortality of about 0.2 percentage points due to high school completion. As before, focussing only on natural deaths considerably increases the precision while leaving unchanged the point estimate of the effect of interest.

5 Discussion and conclusions

In this paper we have exploited the unexpected mass exemption from compulsory military service granted to several cohorts from specific Italian towns hit by a quake in 1980 to identify the effects of high school completion on subsequent mortality. Results are based on comparisons of youths living on the border of the quake region exempt from military service at different ages with comparable non-exempt youths from nearby neighbouring towns.

We find that cohorts exempt when still of high school age stayed longer in school and ten years later the proportion of high school graduates had increased by about 2 percentage points; cohorts exempt at a later age instead did not revise their schooling choices. Over the decade 1991-2001, overall mortality of early exempt youths was lower by about 0.3 percentage points; older exemptees display the same mortality rates of comparable non exempt cohorts. Our estimates suggest that high school completion lowers overall mortality rates between the mid-20s and mid-30s by about 0.1-0.2 percentage points. This amounts to about one tenth of the baseline probability of dying over this age range.

Perhaps the most surprising finding is that the mortality reduction associated with higher schooling is driven exclusively by a decline in deaths due to natural causes while deaths due to accidents or violent causes are largely unaffected. Unfortunately, the nature of the research design limits considerably the sample size thus preventing a further investigation into the incidence of specific natural causes. However, we conjecture that the observed effect might be driven by a decline in drugs- and alcohol-related deaths.

First, alcohol and drugs consumption are presumably the main causes of death that can be manipulated by individuals and whose consequences may manifest in a relatively short time span[20]. For example, Carpenter and Dobkin (2009) show that in the US the number of deaths increases by as much as 10 percent at age 21, the federal minimum drinking age. They show that this increase is only due to external causes, a definition that pools accidental and violent causes (motorvehicle accidents, homicides, suicides) and deaths with an explicit mention of alcohol or drugs. Noticeably, the latter group alone, that would be coded as natural causes in our framework, increases by as much as 30 percent at age 21 suggesting that increased alcohol consumption may have immediate and extreme consequences on a youth's health.

Second, epidemiologists have long recognised the difficulties in precisely relating a given death episode to substance or alcohol use (for a survey see Darke, Degenhardt and Mattick (2006)). While there are circumstances when this is an easy task (for example, clear episodes of overdose), in most cases death occurs for reasons that may or may not be determined by such behaviors, such as heart or kidney failure or hepatitis. In such cases only careful toxicological examination or deep knowledge of the deceased habits can inform the report on the specific causes of death. Such information is rare

[20]Other leading death causes can in principle also be manipulated but the effects are likely to be seen over a longer time period. For example, reduced smoking, increased physical activity and better food tend to reduce cardiovascular failures and cancer occurrences at older ages.

and typically collected only if the death is part of a criminal event. Therefore, even in largere samples than ours, a decline in drugs- or alcohol-induced deaths could easily reflect into a lower number of deaths by natural causes.

Third, we suspect that in addition to these objective difficulties, misclassification can arise because a physician reporting the cause of death may intentionally avoid to mention drugs or alcohol as concurring factors to avoid the family a negative stigma or social despise. This motive is likely to be relevant in the environment we study, characterised by small town size, a markedly traditional culture and strong social binds.

In conclusion, the evidence presented clearly suggests a significant and negative effect of schooling on youth mortality rates. This is per se an important ingredient in the assessment of schooling policies or in the evaluation of alternative interventions aimed at reducing youth mortality. A deeper investigation of which specific causes of death are mostly affected by higher schooling would help clarify the mechanisms at work. Richer data than ours is needed for such a task.

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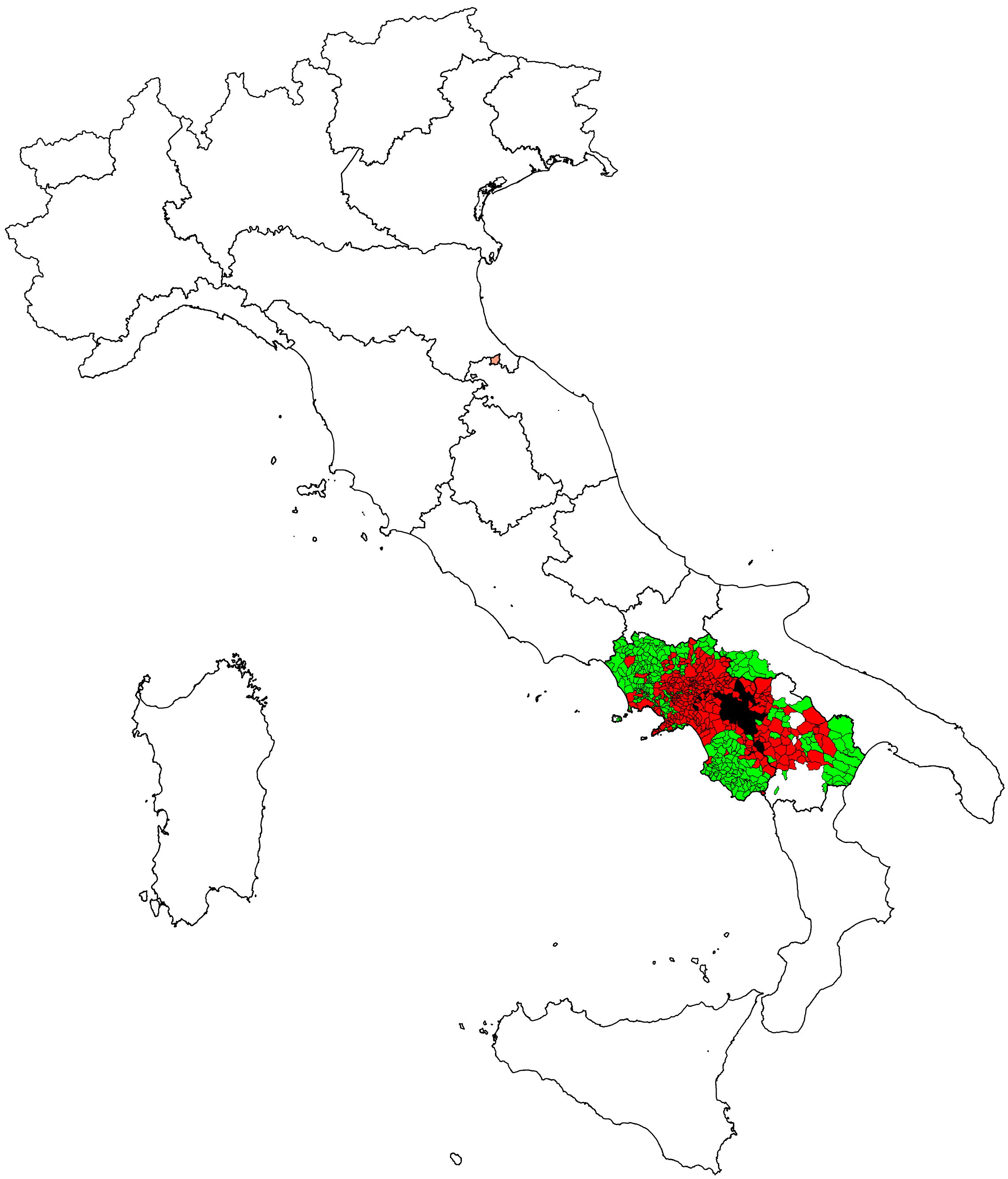
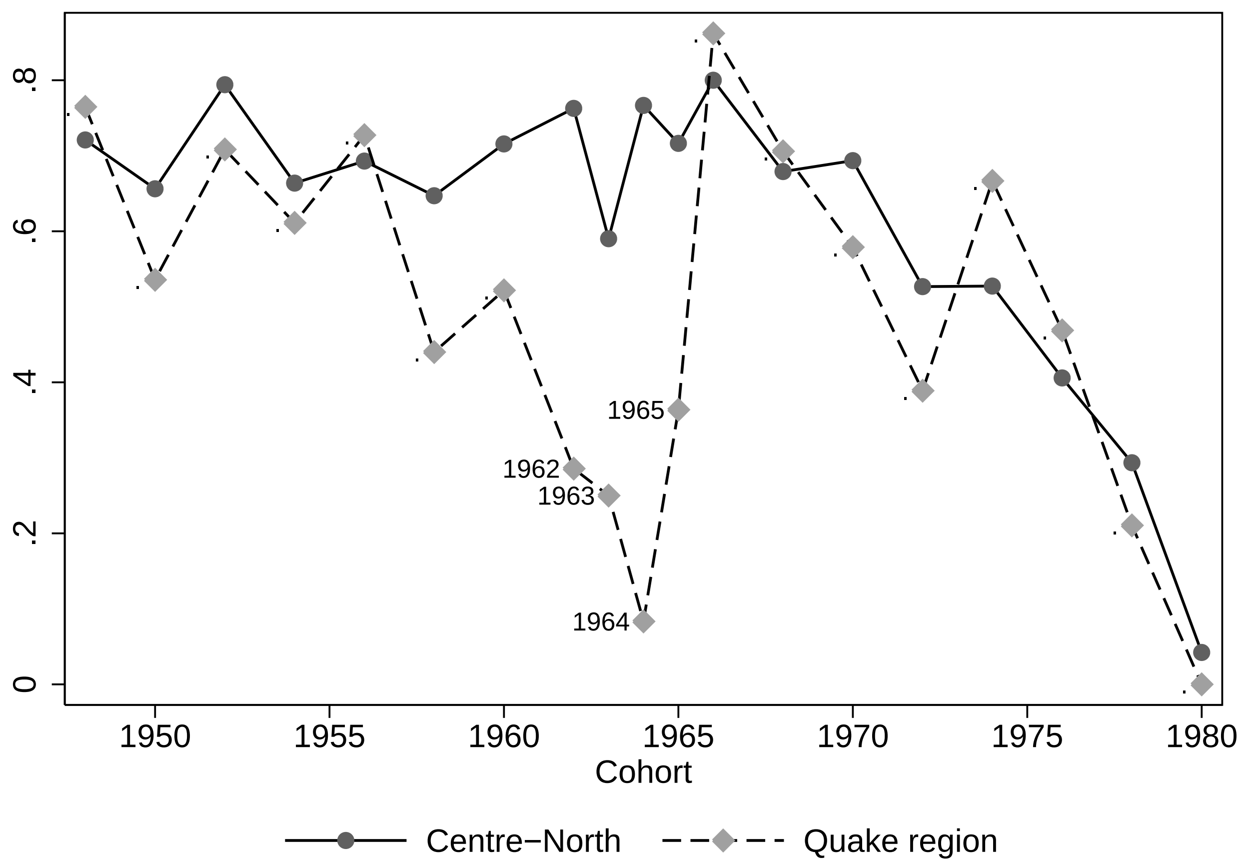
Figure 1: 1980 Earthquake.



Source: Official report on the 1980 earthquake, Ministero del Bilancio e della Programmazione Economica (1981).
The figure displays towns that were hit by the quake. Darker areas represent more damaged towns.

Figure 2: Compliance with military service.

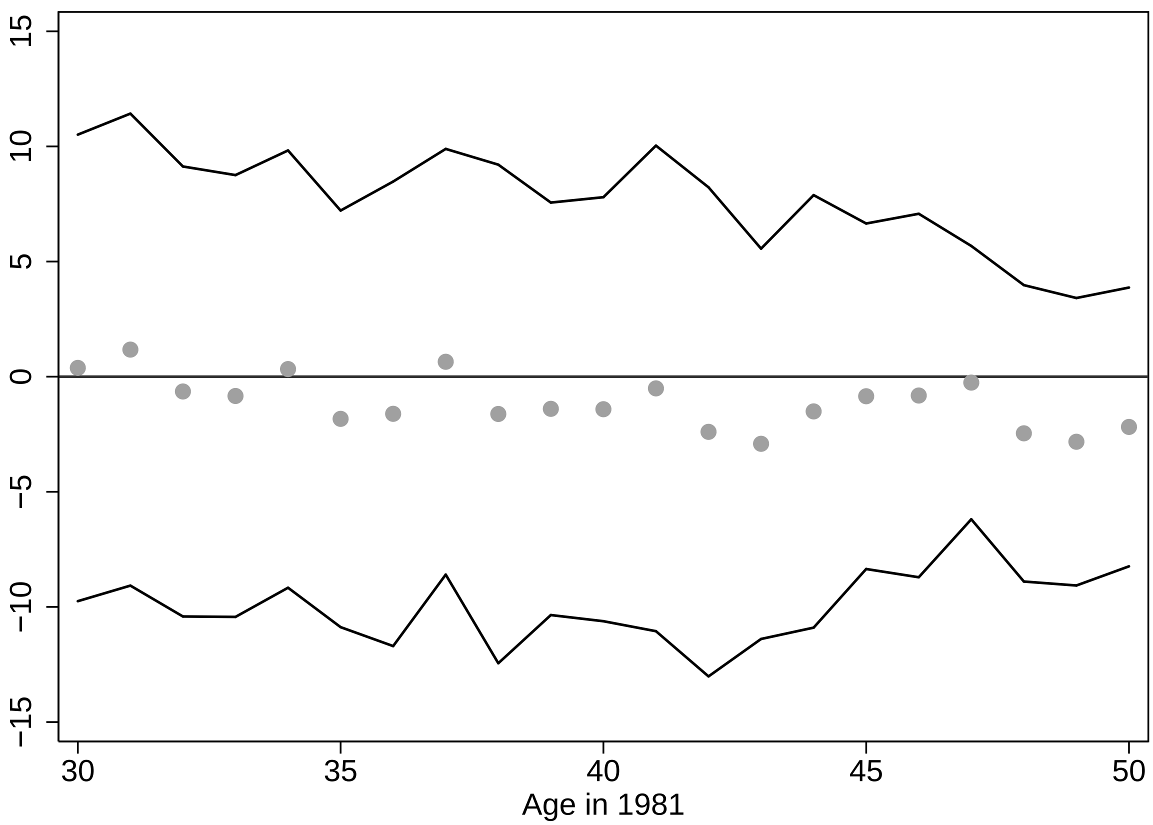


Source: ILFI, 1998-99.
The figure reports the share of men who report having complied with military obligations by 1998, by birth cohort. Older and younger cohorts correspond to pairs of consecutive birth years.

Figure 3: Treated and control towns

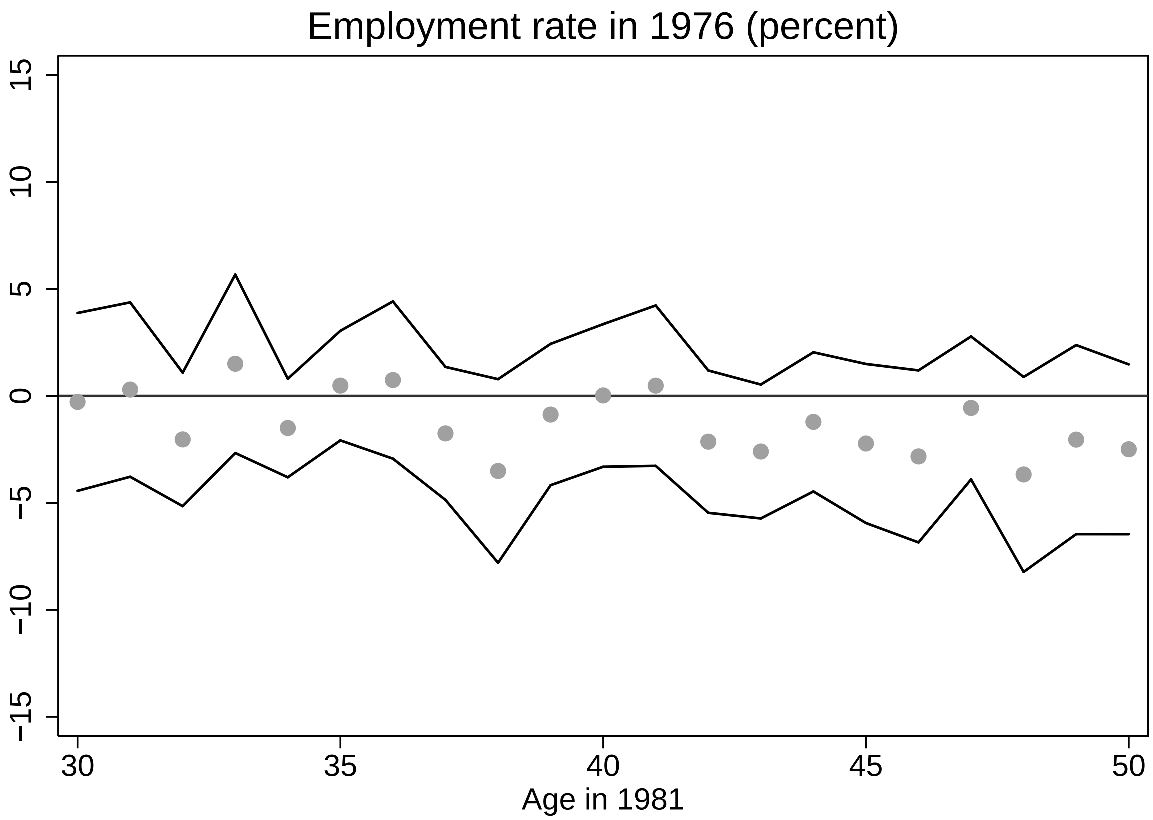
The figure displays inner treated towns (light) and outer control towns (dark). Southern treated towns were granted the exemption although not included in the final list of towns hit by the quake. See text for details.

Figure 4: High-school completion among adults 30-50 years old in 1981.



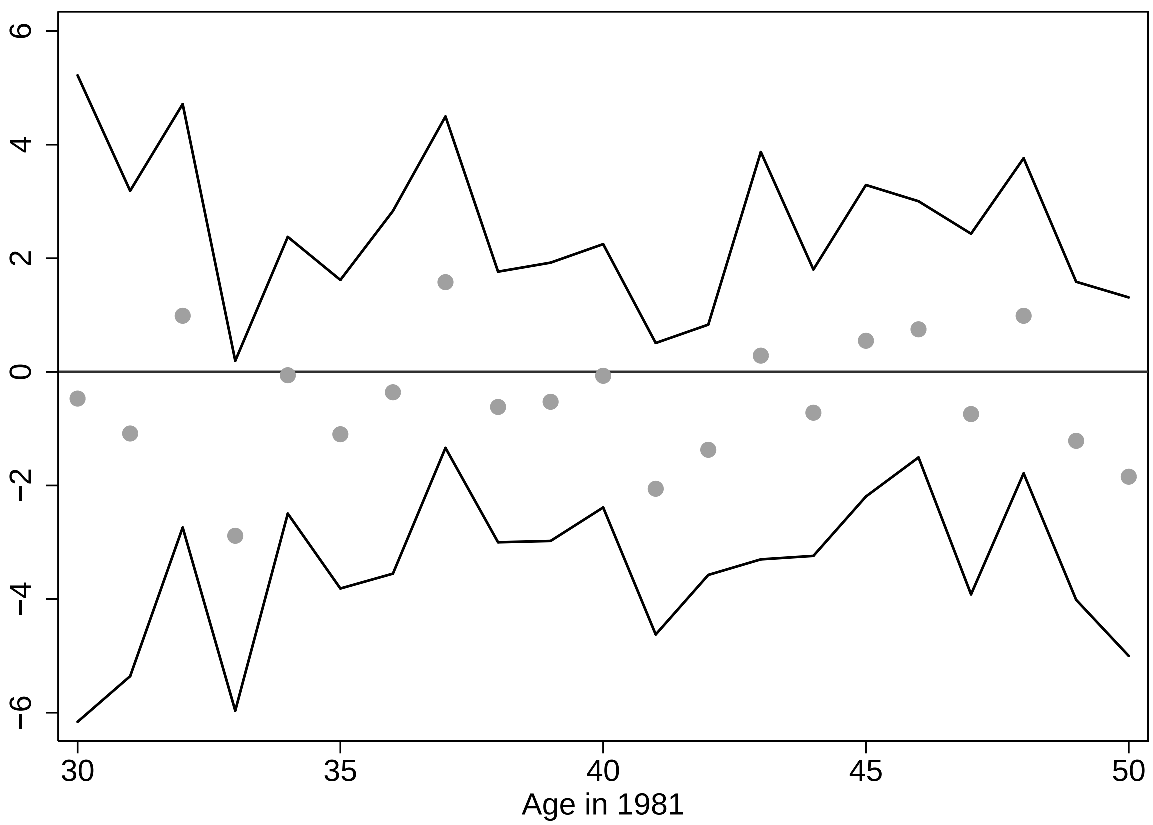
Source: 1981 population census.
The figure reports estimated differences in the share of male population with at least a high-school degree in 1981 and the corresponding 95% confidence intervals based on robust standard errors. Estimates are weighted by the number of observations in the town-age cell.

Figure 5: Employment rates before and after the quake.



Source: 1981 population census.
The figure reports estimated differences in the share of male population employed in 1976 and in 1981 the corresponding 95% confidence intervals based on robust standard errors. Estimates are weighted by the number of observations in the town-age cell.

Figure 6: Change in employment rates after the quake.



Source: 1981 population census.
The figure reports estimated differences in the employment rate change between 1976 and 1981 and the corresponding 95% confidence intervals based on robust standard errors. Estimates are weighted by the number of observations in the town-age cell.

Table 1: Treated and control: relevant outcomes by cohorts.

	1963-65			1959-61			1956-58		
	C	T	T-C	C	T	T-C	C	T	T-C
1981									
% Military service	0.4	0.5	0.11	10.5	7.0	-3.51	1.3	1.0	-0.28
	(1.0)	(1.3)	(0.13)	(7.9)	(7.5)	(0.90)	(1.4)	(1.7)	(0.18)
% Student	45.0	48.6	3.61	17.5	17.9	0.42	11.9	11.5	-0.40
	(14.2)	(13.6)	(1.63)	(7.8)	(8.8)	(0.94)	(5.6)	(6.3)	(0.67)
% in Labor market	53.0	49.3	-3.73	70.4	73.2	2.85	85.4	85.6	0.19
	(13.9)	(12.9)	(1.59)	(12.4)	(12.7)	(1.44)	(6.1)	(7.5)	(0.75)
Obs.	17529	7511		15237	6868		13180	5985	
1986									
% Student	15.5	15.9	0.47	7.6	7.8	0.14	4.0	3.7	-0.23
	(6.8)	(7.5)	(0.81)	(3.9)	(4.6)	(0.48)	(2.5)	(2.9)	(0.3)
% in Labor market	79.2	80.5	1.34	88.0	88.5	0.53	91.8	91.8	0.02
	(7.7)	(7.7)	(0.89)	(4.7)	(5.0)	(0.55)	(3.2)	(4.6)	(0.42)
1991									
% Student	5.5	6.2	0.70	1.6	1.7	0.10	0.7	0.7	0.02
	(3.5)	(4.6)	(0.45)	(1.8)	(2.1)	(0.21)	(0.8)	(1.4)	(0.12)
% at least High school	34.2	36.4	2.27	34.8	34.0	-0.79	34.9	34.4	-0.51
	(11.7)	(11.0)	(1.33)	(10.9)	(10.0)	(1.21)	(10.2)	(10.7)	(1.18)
% at least College	4.4	4.3	-0.12	7.0	6.6	-0.42	7.7	7.1	-0.55
	(2.9)	(3.2)	(0.34)	(4.3)	(4.1)	(0.48)	(4.3)	(4.4)	(0.50)
% in Labor market	91.3	91.4	0.10	95.6	95.5	-0.07	96.5	96.5	0.02
	(4.3)	(4.9)	(0.52)	(2.9)	(2.8)	(0.33)	(2.4)	(3.1)	(0.30)
Death rate 1991-2001 (%)									
All causes	1.4	1.1	-0.34	1.5	1.5	0.02	1.5	1.4	-0.07
	(1.3)	(1.6)	(0.16)	(1.0)	(1.9)	(0.16)	(1.2)	(1.5)	(0.14)
Natural causes	1.0	0.6	-0.39	1.1	1.1	-0.04	1.1	1.0	-0.08
	(0.9)	(1.2)	(0.12)	(0.9)	(1.5)	(0.13)	(1.0)	(1.2)	(0.12)
Accidental causes	0.4	0.5	0.05	0.4	0.4	0.06	0.4	0.4	0.00
	(0.7)	(1.0)	(0.1)	(0.50)	(1.1)	(0.09)	(0.7)	(0.8)	(0.09)
Obs.	18308	8264		18541	8763		1806	8731	

Source: 1981 and 1991 population census.
Table reports unconditional means of the row heading variable and corresponding differences between treated and control towns by cohort. Standard errors in parentheses.
Means and differences are weighted by the number of observations in the town and year of birth cell.
Death rates defined as the ratio of the total number of deaths between 1991 and 2001 in the relevant birth year-town cell over population at risk in 1991 (see text for more details).

Table 2: The effects of the exemption: cohorts 1956-1965.

	(1)	(2)	(3)	(4)	(5)	(6)
1986						
% Student	0.37	1.38	1.17	1.24	0.69	1.08
	(1.98)	(1.15)	(1.11)	(1.24)	(1.03)	(1.03)
% in Labor market	1.21	0.15	0.32	0.07	1.13	-1.12
	(1.99)	(1.28)	(1.24)	(1.30)	(1.12)	(1.17)
1991						
% Student	0.57	0.94	0.85	0.88	0.50	0.32
	(1.06)	(0.69)	(0.67)	(0.73)	(0.66)	(0.59)
% in Labor market	0.23	-0.23	-0.14	-0.26	0.36	-0.02
	(1.24)	(0.88)	(0.86)	(0.93)	(0.78)	(0.76)
% at least High school	2.04	2.58	2.36	2.13	4.35	-0.05
	(1.07)	(0.98)	(1.03)	(0.92)	(1.13)	(1.03)
% at least College	0.25	-0.03	-0.26	-0.13	0.17	-0.09
	(0.69)	(0.57)	(0.58)	(0.63)	(0.72)	(0.57)
Death rate 1991-2001 (%)						
All causes	-0.30	-0.29	-0.29	-0.29	-0.48	0.06
	(0.17)	(0.19)	(0.19)	(0.19)	(0.27)	(0.18)
Natural causes	-0.32	-0.32	-0.32	-0.32	-0.43	0.06
	(0.12)	(0.13)	(0.13)	(0.13)	(0.20)	(0.15)
Accidental causes	0.02	0.03	0.03	0.03	-0.05	0.01
	(0.09)	(0.10)	(0.10)	(0.11)	(0.14)	(0.08)
Town FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	N	N	N
Geo-Cohort FE	N	N	N	Y	Y	Y
Cohort Size	N	Y	Y	Y	Y	Y
Parental char.	N	N	Y	Y	Y	Y
Sample	B	B	B	B	ND	OC

Standard errors clustered at the town level.
Controls - col.(1): town and common cohort dummies; col.(2): town and common cohort dummies, quadratic in town-cohort size; col.(3): town and common cohort dummies, quadratic in town-cohort size, average parental education and 1981 employment rate in town-cohort cell; cols.(4)-(6): town and location-specific cohort dummies, quadratic in town-cohort size, average parental education and 1981 employment rate in town-cohort cell.
Sample: males born in 1956-65 in all 117 sampled tows (cols. (1)-(4)); only 33 no or least damaged towns and corresponding 41 neighbouring control towns (col. (5)); only in control and outer control towns (col. (6)). See text for mode details.

Table 3: The effects of the exemption: cohorts 1953-1961.

	(1)	(2)	(3)	(4)	(5)	(6)
1986						
% Student	0.70	1.30	1.15	1.0	0.33	-0.12
	(1.06)	(0.71)	(0.67)	(0.69)	(0.66)	(0.58)
% in Labor market	-0.35	-1.06	-0.86	-0.72	-0.09	0.34
	(1.26)	(0.81)	(0.76)	(0.77)	(0.68)	(0.67)
1991						
% Student	0.46	0.57	0.53	0.38	0.22	-0.32
	(0.49)	(0.42)	(0.40)	(0.33)	(0.27)	(0.37)
% in Labor market	-0.72	-0.94	-0.86	-0.67	-0.46	0.50
	(0.80)	(0.71)	(0.67)	(0.63)	(0.50)	(0.56)
% at least High school	-0.07	0.63	0.52	0.88	1.0	-0.14
	(1.09)	(0.94)	(0.94)	(0.92)	(1.08)	(0.87)
% at least College	-0.10	0.20	-0.03	0.10	-0.09	-0.05
	(0.56)	(0.62)	(0.57)	(0.56)	(0.65)	(0.43)
Death rate 1991-2001 (%)						
All causes	0.03	0.05	0.05	0.11	0.22	0.24
	(0.17)	(0.18)	(0.18)	(0.18)	(0.24)	(0.15)
Natural causes	-0.02	0.02	0.02	0.05	0.32	0.18
	(0.15)	(0.16)	(0.16)	(0.15)	(0.22)	(0.14)
Accidental causes	0.04	0.04	0.04	0.06	-0.10	0.06
	(0.10)	(0.11)	(0.11)	(0.11)	(0.16)	(0.10)
Town FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	N	N	N
Geo-Cohort FE	N	N	N	Y	Y	Y
Cohort Size	N	Y	Y	Y	Y	Y
Parental char.	N	N	Y	Y	Y	Y
Sample	B	B	B	B	ND	OC

Standard errors clustered at the town level.
Controls - col.(1): town and common cohort dummies; col.(2): town and common cohort dummies, quadratic in town-cohort size; col.(3): town and common cohort dummies, quadratic in town-cohort size, average parental education and 1981 employment rate in town-cohort cell; cols.(4)-(6): town and location-specific cohort dummies, quadratic in town-cohort size, average parental education and 1981 employment rate in town-cohort cell.
Sample: males born in 1953-61 in all 117 sampled towns (cols. (1)-(4)); only 33 no or least damaged towns and 41 corresponding neighbouring control towns (col. (5)); only in control and outer control towns (col. (6)). See text for mode details.

Table 4: The effects schooling on mortality.

	(1)	(2)	(3)	(4)	(5)	(6)
Completed high school (%)						
First stage						
Exempt	2.04	2.58	2.36	2.13	4.35	1.95
(SE)	(1.07)	(0.98)	(1.03)	(0.92)	(1.13)	(1.03)
Death rate by all causes (%)						
Reduced form						
Exempt	-0.30	-0.29	-0.29	-0.29	-0.48	-0.38
(SE)	(0.17)	(0.19)	(0.19)	(0.19)	(0.27)	(0.22)
2SLS						
High school	-0.15	-0.11	-0.12	-0.14	-0.11	-0.19
(p-val)	(0.078)	(0.127)	(0.127)	(0.127)	(0.075)	(0.087)
Death rate by natural causes						
Reduced form						
Exempt	-0.32	-0.32	-0.32	-0.32	-0.43	-0.40
(SE)	(0.12)	(0.13)	(0.13)	(0.13)	(0.20)	(0.19)
2SLS						
High school	-0.16	-0.12	-0.14	-0.15	-0.10	-0.21
(p-val)	(0.008)	(0.014)	(0.014)	(0.014)	(0.032)	(0.032)
Town FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	N	N	Y
Geo-Cohort FE	N	N	N	Y	Y	N
Cohort Size	N	Y	Y	Y	Y	Y
Parental char.	N	N	Y	Y	Y	Y
Sample	B	B	B	B	ND	B
Cohorts	56-65	56-65	56-65	56-65	56-65	58-65

Robust standard errors clustered at the town level.
Table reports first stage and reduced form estimates of the effect of exemption status for cohorts born in 1963-65 (Exempt) on high school completion rates and 1991-2001 cumulative mortality rates, and implied causal effect of high school completion on the relevant mortality measure (High school). P-values are those associated with the weak-instrument robust Anderson-Rubin statistics ($\chi^2(1)$).
Controls - col. (1): town and common cohort dummies; col. (2): town and common cohort dummies, quadratic in town-cohort size; cols. (3), (6): town and common cohort dummies, quadratic in town-cohort size, average parental education and 1981 employment rate in town-cohort cell; cols. (4)-(5): town and location-specific cohort dummies, quadratic in town-cohort size, average parental education and 1981 employment rate in town-cohort cell.
Sample - males born in 1956-65 in all 117 sampled towns (cols. (1)-(4)); males born in 1956-65 only in 33 no or least damaged towns and corresponding 41 neighbouring control towns (col. (5)); males born in 1958-65 in all 117 sampled towns (col. (6)). See text for mode details.