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ABSTRACT

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

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 exempted 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, by 1991, the cohorts exempted while still in high school display significantly higher graduation rates. The probability of dying over the decade 1991-2001 was also significantly lower. Several robustness checks confirm that the findings do not reflect omitted quake-related confounding factors, such as the ensuing compensatory interventions. Moreover, cohorts exempted soon after high school age do not display higher schooling or lower mortality rates, thus excluding that the main findings reflect direct effects of military service on subsequent mortality rather than a causal effect of schooling. We conclude that increasing the proportion of high school graduates by 1 percentage point leads to 0.1-0.2 percentage points lower mortality rates between the ages of 25 and 35.

JEL Classification: I12 and I20
Keywords: education, health, human capital, mortality and natural experiment

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1 Introduction

Education is strongly correlated with own health and life expectancy (Kitagawa and Hauser (1973), Grossman and Kaestner (1997), Deaton and Paxson (1999), Cutler and Lleras-Muney (2006)). Policy makers are paying growing attention to this link as it suggests the possibility of efficiently designing interventions that improve both quality of life and human capital. For example, the European Commission recently restated its health strategy (European Commission (2007)) emphasizing the need to integrate health in all policies and to recognize the links between health policy and other policy areas. Improving the years of healthy life and reducing health disparities are major goals of the US Healthy People 2010 ten-year health strategy (US Department of Health and Human Services (2000)).

The mechanisms underlying this empirical regularity can nonetheless be manifold. Education may affect health because more educated people tend to smoke and drink less, do more physical exercise, monitor their health more closely (Cutler and Lleras-Muney (2006)), are more likely to use newly introduced drugs (Lleras-Muney and Lichtenberg (2005)), are less exposed to job-related stress factors and less at risk of unemployment (Marmot (2006), Sullivan and von Wachter (2009)), and can afford more expensive care[1]. However, these behavioral, environmental and endowment differences do not fully account for the educational gradient in mortality and for its substantial strengthening over recent decades (Cutler, Lange, Meara, Richards and Ruhm (2010)). A complementary explanation for the empirical regularity 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 better chances (Case, Fertig and Paxson (2005)) to accumulate human capital. Finally, the relationship 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

[1] More generally, education may 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 on the part 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.

and that in health (Fuchs (1982)) or her family background (Currie and Moretti (2003)).

In this paper we investigate whether schooling affects the mortality of young men in a quasi-experimental setting. In 1981, young men born before 1966 and living in specific towns of southern Italy were exempted from compulsory military service after a major quake hit the region a 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 of revising their human capital accumulation plans. The exemption was granted to them while 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 life cycle. More specifically, we compare schooling levels and subsequent mortality rates of cohorts from towns on the inner border of the quake region exempt before and after reaching the eligibility age with those of similar non-exempt cohorts from neighboring towns just outside the quake region. Inner treated towns were the least damaged by the quake, thus also unlikely to receive any compensation; neighboring control towns are geographically close, thus largely sharing the same economic environment as treated towns but were excluded from any quake related intervention[3].

[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.

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

We find that in 1981 high school drop-out rates of cohorts exempt before formal eligibility and 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 first group displays a significantly larger share of high school graduates, by over 2 percentage points; all other observed circumstances of younger and older exempt cohorts five and ten years after the exemption are not statistically different from those of their non-exempt counterparts. In particular, we do not detect differences in their employment rates. 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 figure recorded in comparable non-exempt cohorts. No difference can be detected between older exempt cohorts and comparable non-exempt ones.

The findings imply that the exemption led over 3,000 youths from the whole quake region to complete high school instead of dropping out; over the subsequent decade 1991-2001, about 600 additional youths in their mid-20s also survived into 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 into context, between 1980 and 1990 life expectancy at age 25 increased by about one year.

The implied 2SLS estimate of the effect of a one percentage point increase in the proportion of high school graduates on mortality rates between the mid-20s and mid-30s suggests a statistically significant reduction of 0.1-0.2 percentage points, about one tenth of the baseline probability of death. The main assumption required to interpret this figure correctly 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 exempted when already beyond high school age is not associated with either higher schooling or 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, generally finding support for 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 between 3.5 and 6 percentage points, an increase in life expectancy at age 35 of about 1.7 years; Oreopoulos (2006) corroborates these results by 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; Deschenes (2010) shows that exceptionally larger cohorts at birth display both higher average education levels and lower adult mortality. Clark and Royer (2010) instead find little evidence that changes to British schooling laws in 1947 and 1972 affected subsequent mortality.

Our paper complements the above evidence in several respects. First, we explore the relationship between schooling and mortality in a younger population than the ones usually investigated. This is likely to dampen the role played by factors that may induce a correlation between schooling and health due to the different jobs, careers and wealth accumulation profiles higher schooling gives access to. For example, Marmot (2006) discusses how job-related stress factors may partly account for differences in mortality across socio-economic backgrounds; Sullivan and von Wachter (2009) document that job displacement has significant long-run effects on individual mortality. Moreover, the presence of a publicly provided universal health care system is further likely to weaken the effects of income and wealth differences.

Second, we study the mortality returns to high school graduation within a pseudo-experimental framework that allows us to clearly identify the population exposed to treatment, that is potential high school dropouts. The studies most similar to ours have instead focused on the effects of additional education exogenously achieved at either younger or older ages. For example, Lleras-Muney (2005), Clark and Royer (2010) and Oreopoulos (2006) study the mortality and health consequences of increased education induced by changes in compulsory schooling age, typically much lower than high school graduation age; Deschenes (2010) implicitly looks instead at returns to college graduation.

Third, contrary to those studies, our identification strategy rests primarily on comparisons of mortality of similar cohorts over the same time span thus further limiting the possibility of confounding factors related to secular developments in schooling and mortality.

Finally, our results also contribute to the study of the determinants of human capital accumulation, in particular by quantifying the relationship between schooling choices at young age and the length of one's potential working life. This finding has potentially interesting policy implications, for example, for the assessment of reforms of retirement regimes and labor markets[4]. It is also at the heart of recent efforts to develop a unified theory of the demographic transition (Galor and Weil (2000), Galor and Moav (2002), Cervellati and Sunde (2005)).

The paper proceeds as follows. We start with a description of the quake and related interventions. Next, we illustrate and motivate the research design underlying our analysis. We then quantify short- and long-run effects of exemption status and of military service avoidance on a set of relevant outcomes. Finally, we present and discuss least squares and 2SLS estimates of the effects of high school completion on subsequent mortality. We then conclude.

[4]See, for example, the discussion in Heckman and Jacobs (2009)

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 12-18 months depending on the specific military corp assigned. However, the total time absorbed by conscription could be much longer because of lengthy bureaucracy and because conscripts were inducted only a few times a year. 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 this 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 this would only result in a delayed entry in the labor market and a loss of earnings[5]. On the other hand, compulsory military service is unlikely to be an alternative for marginal youths. Military service is unpaid, nor does it provide any kind of formal qualification or training with subsequent market value, given that it involves mostly guard duties, physical and basic military training.

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

[5] 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.

650 towns. The quake caused about 2,000 deaths and 10,000 casualties, mostly around the epicenter; about 300,000 persons were in need of temporary shelter; 60 percent of the residential estate stock was damaged in the epicenter 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 to roughly one fifth of the GDP of the area.

2.3 The exemption from military service.

The relief package included the cancellation of military obligations for all men born before 1966 living in the quake region who had not yet served, independently of the damages suffered by their hometown[6]. Specifically, the exemption was first introduced in March 1981 as the possibility of complying with military duties by doing social work in one's hometown for all those due to be drafted in 1981, 1982 and 1983 and living in the relevant region when the quake struck. This was a major concession since serving in the army involved moving in other areas of the country. Moreover, doing social work is not a full-time commitment, does not require leaving one's household and is generally compatible with school enrollment. However, before the quake-related exemption social work was not a viable alternative: service would last 24 months and the request had to be defended before a military commission in lengthy selection procedure with a high chance of a negative outcome. Shortly afterwards laws were

[6]While economic support was clearly linked to the magnitude of damages suffered by the town, the exemption was eventually extended to all towns in the administrative regions most hit by the quake. Thus, in several cases also to youths from towns that were classified as undamaged.

proposed to cancel all military obligations for the same groups and the law was definitively introduced in February 1982. The population targeted by the laws included all males eligible for draft in 1981, 1982 and 1983 and living in the quake region at the relevant date. This included youths turning 18 in those years (i.e. those born in 1963, 1964 and 1965) who thus were exempted before becoming formally eligible and still in high school and their older peers who had deferred service.

3 Research design

Our sample frame is designed so as to replicate an experimental setting in which the exemption is as good as randomly assigned. This allows us to exclude that variation in the outcomes of interest associated with exemption status is due to confounding factors, such as complementary measures undertaken in the aftermath of the quake or unobserved characteristics.

For this purpose, we focus on individuals from towns that lie on the border of the quake region (fig. 2). Towns along the border but *within* the quake region were exposed to the exemption as well as to the quake itself and possibly to other compensatory measures. Towns just outside the quake region, on the other hand, were excluded from all quake-related measures. 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. Inner towns are somewhat smaller: the median size is about 2,500 against a median size of outer towns of 2,900[7]. The distance between neighboring towns lying across the border is also small, about 6 miles on average and always below 16 miles.

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

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

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 neighboring towns across the border of the quake region implies that they largely shared the same economic environment. Thus, they were exposed in a similar fashion to the deep economic recession ensuing the quake. More broadly, any general equilibrium effect of the quake probably had the same impact on similar individuals across the border. This implies, in particular, that changes in schooling choices induced by changes in wages and employment opportunities are of the same magnitude and accounted for in comparisons of similar individuals across the border of the quake region.

The 1981 population census allows us to assess whether towns just outside the quake region represent the appropriate counterfactual for towns just inside the region[10]. In particular, census data allow us to compare educational achievements and employment rates in the two groups of towns. Unfortunately, the Italian census does not collect earnings and income data. We focus on the adult population, specifically people aged 30-50 at the census date.

Figure (3) shows the age-specific differences in the percentage of high school graduates among the adults living in inner and outer towns as of 1981 together with the associated 95 percent confidence interval. Differences are generally small and never statistically significant at customary confidence

[8]Specifically, the lowest level of the damage scale corresponds to situations in which no one was injured or died because of the quake, less than 2 percent of the population lost their home, temporary shelter was needed for no more than 5 percent of the population, less than 1 percent of residential estate was damaged and less than 0.5 percent was declared inhabitable. Analogously, the highest damage level assigned to towns outside of the epicenter corresponds to situations in which more than 10 percent of the population was injured or died, more than 20 percent lost their home, temporary shelter was required for more than 40 percent of the population and more than 30 percent of residential estate was declared damaged or inhabitable.

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

[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 us 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, located in Rome, Italy.

levels although older cohorts in inner towns appear to be slightly less educated. 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 (4) displays the estimated differences by age and the corresponding 95 percent confidence interval, showing that before the quake employment rates were not significantly different.

One concern with this evidence is that the census was run one year after the quake. In principle, there are at least two reasons why this could impair the relevant comparisons. First, populations from inner towns might have migrated elsewhere 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 already reflect 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 address empirically 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 towns just inside and just outside the quake region is an order of magnitude smaller, around 0.2 percent. To establish whether the quake led to a larger outflow from inner towns we regressed yearly outflow rates between 1974-1987 on a set of year dummies and their interactions with an indicator for towns inside the quake region. None of the interaction terms was statistically significant at customary levels: p-values are always well above 0.15 except for three years (1977, 1978 and 1981) for which they are still larger than 0.05[11]. Moreover, an F-test of the joint significance of the interacted terms does not reject the null with a p-value of 0.3. Overall, it seems that outflows were very similar between the two sets of towns, and relatively low both before

[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.

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 compared to towns lying just outside the border.

Finally, we explore whether the quake had different effects on labor market outcomes of adults in treated and control towns. We exploit a retrospective question on employment status 5 years before the census, as of 1976, to compute town-cohort specific changes in employment rates between 1976 and 1981. Figure (5) reports the cohort-specific estimated difference between treated and control towns along with the 95 percent confidence bands. None of the difference turns out to be statistically significant at conventional levels and the null that they are jointly zero cannot be rejected. Failure to detect statistically significant differences further supports the assumption that the population from sampled towns belonging to the quake region was not affected by quake-related interventions and that the direct effects of the quake, if any, were similar in the two groups of towns.

4 Short and long-run effects of avoiding military service.

In this section we explore the short- and long-run effects of avoiding military service. We start by documenting how exemption status granted while in high school or shortly after affected a set of relevant current and subsequent outcomes. We then turn to difference-in-difference estimates of the effects of exemption status before formal eligibility for service.

4.1 Preliminary evidence

Table (1) reports unconditional comparisons of schooling and other labor market outcomes of youths from towns on the inner border of the quake region to those of similar youths from towns just outside the region in the aftermath of the quake. Throughout the paper the observational unit is the cell town and year of birth and all estimates are weighted by the number of individuals in the cell. We focus separately on two groups of cohorts, youths born between 1963 and 1965 and between 1959 and

1961[12]. When the exemption was passed the first group was 16 to 18 years old, while the second was 20 to 22. Individuals in the first group were not yet eligible for service and thus could change their schooling decisions; those in the second group were instead already beyond high school age. The evidence discussed in the previous section supports the view that differences between exempt and non exempt cohorts reflect the effects of exemption status rather than those of confounding factors correlated with exemption.

In rows A to C we look at the short run effects of exemption status. We draw on the 1981 population census which, importantly, was run in October, at the beginning of the school year. Therefore the evidence describes quite precisely the enrollment decisions of the cohorts affected by the exemption. Youths born in 1963-65 were not yet eligible for service (row A). However, the proportion that was still in high school at the 1981 census date was higher by 3.6 percentage points than that of non-exempt counterparts (p-value 0.027; row B). Correspondingly, a lower percentage entered the labor market (row C), reflecting a choice toward 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)). Youths born in 1959-61 had been exempt from military obligations soon after becoming formally eligible to serve. In 1981 the percentage of conscripts was lower by 3.5 percentage points (row A), a proportion similar to that choosing education in younger exempt cohorts. The lower conscription rates basically translated into a higher labor market participation rate rather than school enrollment (row C).

The lower part of table (1) draws on the 1991 population census to describe the long run effects of the exemption by comparing outcomes of exempt and non exempt youths belonging to the two cohorts under analysis ten years after the exemption. The census also includes some retrospective information as of 1986. Ideally, we would like to observe the same individuals in both data sources,

[12]We exclude the 1962 cohort from the comparisons as its members were 19 when the exemption was passed, an age when it is hard to make assumptions about whether they were still in high school or already beyond it.

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[13]. The evidence discussed in the previous section suggests, however, that geographic mobility is not a relevant issue in our specific sample.

The proportion of youths exempt before eligibility who completed high school by 1991 is higher by 2.3 percentage points than among similar non exempt ones (row D). The p-value of the unconditional difference is 0.088 and compares with the much lower levels of significance and point estimates close to zero detected for the other long run outcomes and for all observable circumstances of the older cohort. In particular, there is no statistically relevant difference in the proportion of males enrolled in formal education or participating in the labor market as of 1986 (rows E-F) and as of 1991 (rows G-H). Also, as of 1991 there is no difference in the proportion of those who had completed college between exempt and non exempt youths in both cohorts (row I), consistently with the idea that individuals whose behavior was changed by the exemption are those at risk of high school dropout, thus unlikely to pursue a college education. To put the difference in high school completion rates in context, it implies that in the entire quake region over 3,000 youths who would have otherwise dropped out managed to complete high school and around 200 in the sample of towns under investigation.

Finally, the last three rows of table (1) compare the probability of dying between 1991 and 2001. We recover this measure by 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 occurred over the relevant period in cell

[13]Proxying the place of residence at a young age with the place of birth is fairly 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 census provides information on the place of birth only at the larger province level. Thus by using the 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.

$\{c, j\}$ $(D_{c,j})$ and combine it with 1991 census data on the corresponding population at risk obtaining $D_{c,j}/N_{c,j}$, the measure of interest. The data also include detailed information on causes of death, according to the 9th release of the International Classification of Diseases[14]. The evidence 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 difference among the two older cohorts is basically zero, and is estimated with a similar degree of precision as that between the younger cohorts. The difference among younger cohorts, as shown in the last two rows, is entirely due to a lower incidence of natural causes[15]. 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 entire 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.

4.2 Simple estimates of the long-run effects of avoiding military service

The results presented above represent the change in outcome associated with a randomly offered exemption from compulsory military service, the so-called intention-to-treat (ITT) effect. To infer the effects of not serving in the army on the outcomes of interest from the ITT it is necessary to quantify the proportion of the cohort that did not serve because of the exemption. In principle, if retrospective information on military status was available for the cohorts of interest, we should simply compare

[14]See http://www.cdc.gov/nchs/icd/icd9.html.

[15] 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 as those due to internal causes (codes: 0001-7999).

the proportions of exempt and non exempt males that served in the army and use the difference as denominator of a Wald estimate of the effects of skipping military service. Unfortunately this is not the case in our data. We observe individuals in 1981, only learning whether they are in service at census date, and in 1991 and beyond, without any retrospective information on their military service status. As the discussion below will make clear, the fact that military service was compulsory for all males is not sufficient to conclude that the proportion of interest is close to unity.

To illustrate the point, we borrow from the average causal response approach notation (Imbens and Angrist (1994)). Let $\{y_{i0}, y_{i1}\}$ be, respectively, i's outcomes when serving and not serving in the army and Z_i an index for the randomly assigned exemption status in a given cohort. Similarly, let $\{D_{i0}, D_{i1}\}$ be indexes of whether i does not serve in the army when exempt ($D_{i1} = 1$) and not exempt ($D_{i0} = 1$). In principle the quantity of interest, that is the effect of not serving in the army ($E(y_{i1} - y_{i0})$), cannot be estimated because only one of the two potential outcomes is observed. The ITT effects reported in table (1) represent the difference in average outcomes by exemption status, $E(y_i|Z_i = 1) - E(y_i|Z_i = 0)$. Imbens and Angrist (1994) show that under specific assumptions the following holds[16]:

$$E(y_i|Z_i = 1) - E(y_i|Z_i = 0) = E(y_{i1} - y_{i0}|D_{i1} > D_{i0})P(D_{i1} > D_{i0}) \tag{1}$$

Therefore, the effect of not serving on a given outcome can be estimated at least for the population that does not serve because of the exemption (so-called compliers) provided the quantity $P(D_{i1} > D_{i0})$ can be measured. Even if military service is compulsory, this quantity is likely to be different from unity. On the one hand, there can be individuals who never serve ($D_{i1} = D_{i0} = 1$, the so-called always-takers) because, say, of health conditions[17]; on the other, some individuals may choose to serve independently of exemption status ($D_{i1} = D_{i0} = 0$, the so-called never-takers). More importantly, in

[16] Required assumptions are that Z_i be independent of the potential outcomes and treatment assignments (i.e. as good as randomly assigned) and that $D_{i1} \geq D_{i0} \quad \forall i$, i.e. the instrument shifts everybody's behavior in the same direction.

[17] Alternatively, there may be institutional reasons. For example, the Italian law gave the chance to third-born and beyond male children not to serve if their older brothers had already complied with military obligations.

our setting this second group includes those that had already served when the exemption was passed so that their behavior could not be affected.

A tentative estimate of the quantity $P(D_{i1} > D_{i0})$ can be obtained from the available data as follows. Let $s(a)$ be the proportion of conscripts at age a, so that $s(a) = 0$ if $a \leq 18$, and $C(a) = 1 - \sum_{j=18}^{a} s(j)$, the corresponding proportion who has not served by age a. The function $C(a)$ can be approximated by the cross-sectional relationship between age and the proportion in service in non-exempt towns at the census date. The function, reported in the top panel of figure (6), shows that about 30 percent of eligible males serve when 19 years old, an additional 20 percent at 20 years old, and so on. Importantly, about 30 percent of eligible males has not served by age 35 and presumably will not serve thereafter. Therefore, the proportion of always-takers ($P(D_{i1} = D_{i0} = 1)$, i.e. those that always skip service) is about 0.3[18].

Consequently, the proportion of males of a given age who did not serve because of the exemption can be estimated as follows. Let $\tilde{s}(a)$ be the proportion of youths of age a from exempt towns who report being in service at census date. Then the proportion of those who did not serve because of the exemption can be proxied by $\tilde{C}(a) = 1 - \sum_{j=18}^{a-1} s(j) - \tilde{s}(a)$ under the assumption that all those who have not yet served when exempted will not do so in the future. That is, we assume that until age $a - 1$ (or, alternatively, 1980) youths from exempt towns served in the army as youths from non exempt towns and then, because of the exemption, all were freed except those who were already in service in 1981. The lower panel of figure (6) reports for each cohort the estimated proportion of men from exempt towns who did not serve in the army. All youths exempt before formal eligibility were freed from service, while only about a half of the cohorts born between 1959 and 1961, exempt after

[18]This proportion is remarkably consistent with what can be estimated from the Indagine Longitudinale sulle Famiglie Italiane (ILFI; http://www.soc.unitn.it/ilfi/eng.index.html). The survey was started in 1998 with a full set of retrospective information on a representative sample of about 5,000 Italian households. Individuals were also asked whether they were ever drafted for compulsory military service (see Pisati and Schizzerotto (2004) for details). The nationwide proportion of adult males reporting never to have served in compulsory military service is between 0.25 and 0.3 and fairly stable across cohorts (data available upon request).

formal eligibility, had not yet served when exempt. For all groups, it is likely that one fourth of the initial population would not have served anyways. Therefore, among the 1963-65 born the quantity $P(D_{i1} > D_{i0}) \approx 0.7$, and about 0.2 for the 1961-59 born.

Combining the results of table (1) with this information as suggested by equation (1) yields a Wald estimate for the increase in the probability of completing high school induced by not serving in the army among males younger than 19 of approximately (2.3/0.7)≈3.3 percentage points with a standard error of (1.33/0.7)≈1.9[19]. Similarly, among the same population, the absence of military service reduces the probability of dying between the mid-20s and mid-30s by (0.34/0.7)≈ 0.5 percentage points (s.e. (0.16/0.7)≈0.23) and by about 0.56 percentage points (s.e. (0.12/0.70)≈0.17) the incidence of deaths by natural causes. Other outcome differences among youths born in 1963-65 remain statistically not significant.

4.3 DD estimates of the effects of exemption status

The results reported in table (1) are obtained by simple comparisons of similar cohorts from exempt and non exempt towns. We now turn to a difference-in-difference framework to quantify the effects of exemption status at a young age. This allows us to control for potentially omitted determinants of the outcomes of interest. Specifically, we exploit the fact that older cohorts were either unaffected or exposed to a much more limited extent to the exemption to control for unobservable town effects and for secular trends in the outcomes of interest. In table (2) we report estimates for β from models of the type:

$$y_{cj} = \alpha + \beta E_{cj} + \gamma X_{cj} + \mu_c + \delta_j + e_{cj} \tag{2}$$

[19] As discussed in Angrist (1990), asymptotic standard errors of this Wald estimate are given by the limiting distribution of $(\sqrt{N}(\bar{y}^E - \bar{y}^N))/\bar{dp}$ where $\bar{dp} = \bar{p}^E - \bar{p}^N$, the difference in compliance rates among exempt and non exempt youths, converges to a constant. Therefore, the standard error is given by that of the numerator, reported in table (1) for each outcome, multiplied by a factor $1/\bar{dp}$.

in which y_{cj} is the average outcome of interest in cohort c from town j, X_{cj} are observable characteristics of cohort c from town j, μ_c and δ_j are, respectively, cohort and town fixed effects and e_{cj} a residual. $E_{cj} = 1$ for cohorts from exempt towns born in 1963-65, exempt before formal eligibility, and zero otherwise[20]. Row headings describe the dependent variables while columns correspond to different specifications of the control set or underlying samples.

In columns (1) to (3) we focus on males born between 1956 and 1965 in treated and control towns. We start by including town and year of birth dummies (col. 1). Town dummies capture all unobserved heterogeneity across towns, including potentially different - direct and indirect - effects of the quake that are common across contiguous cohorts from the same town. The proportion of youths exempt while in high school who by 1991 had completed the degree is about 2 percentage points higher (p-value: 0.059); no difference exists as concerns school enrollment or college graduation or the degree of participation in the labor market as of 1991. Differences are not statistically significant also as of 1986, suggesting that exemption status did not alter the progression into the labor market or the choice between school and employment in a long-lasting fashion. This is consistent with the argument that the intervention affected the choices of youths at risk of dropping out, thus unlikely - upon high school completion - to go further ahead in the educational track. Differences in mortality rates remain of the same order of magnitude and significance level than those estimated by unconditional comparisons of similar youths in table (1). Inclusion of a quadratic in cohort size (col. 2) to absorb within-town of birth heterogeneity due, for example, to local congestion effects and of heterogeneous cohort effects (col. 3) does not substantially change the picture[21].

In columns (4) and (5) we replicate the exercise of column (3), restricting the sample to cohorts born

[20] Estimation at the cohort-town level eliminates the problem of within cohort-town correlation of residuals (Moulton (1986)). Inference based on DD estimates may still suffer from neglected serial correlation of residuals (Bertrand, Duflo and Mullainathan (2004)). We address this possibility by clustering robust standard errors at the town level.

[21] Heterogeneous cohort effects are captured by a set of interactions of cohort dummies with dummies for 5 geographic regions obtained dividing the sample, from north to south, into 5 non-overlapping groups of towns containing treated and control towns.

between 1959 and 1965. This implies that, as shown before, all cohorts were exposed to a significant, although different, extent to the exemption. Moreover, because in 1981 individuals belonging to these cohorts were between 16 and 22 years old they were still cohabiting with their parents. This allows us to include controls for a number of parental characteristics at the town-cohort level[22]. Even in this limited sample and with parental controls, we still detect a rather significant increase in high school completion rates of about 2 percentage points and a decline in death rates of about 0.3 percentage points among younger exempt cohorts.

In column (6) we perform a complementary exercise and estimate equation (2) on the sample of males born in 1963-65 and in 1956-58. With respect to the previous exercise we thus exclude from the sample youths born in 1959-61 a quarter of whom was likely affected by the exemption. On the contrary, in the sample underlying column (6) older cohorts are completely unaffected by the exemption as all of them were at least 23 when exempt. However, this does not allow us to control for parental characteristics since as of 1981 many of the older cohorts were no longer living with their parents. Results confirm the increase in high school completion rates in young exempt cohorts and the decline in mortality between 1991 and 2001.

In column (7) we restrict the sample to youths from the 33 exempt 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. Because this subset of treated towns was only exposed to the exemption, the exercise allows us 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. The increase in the high school

[22]The 1991 census does not provide information on family background. We thus recover the information from the 1981 population census as follows. For each town and cohort, we select all households living in the town in 1981 with a male belonging to the relevant cohort and compute the employment rates and educational achievement of parents. In 1981 the males in the relevant sample were 16 to 23 years old, thus largely still living in their parents' household (see Manacorda and Moretti (2006) for a discussion of the motivations of Italian youths to live in the parental household). Unfortunately, the 1981 census does not collect information on income or wealth.

graduation rate and the decline in mortality rates are now larger and more precisely estimated, while all other outcomes as of 1986 and 1991 remain not statistically significant from zero.

Finally, in columns (8) and (9) we perform two robustness checks. In column (8) we compare youths born in 1959-1965 in control towns to similar ones from towns further outside the quake region imputing the exemption status to youths born in 1963-65 to those born in 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 the quake as compared to older cohorts from the same towns and if, as it seems reasonable to assume, these effects decay continuously 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, such as those further out of the quake region. Results show no difference along any dimension, further supporting the interpretation of previous results as determined by exemption status rather than by other quake-related unobserved effects.

In column (9) we estimate equation (2) on youths born between 1953 and 1961 in treated and control towns and impute exemption status to youths born in 1959-61 in treated towns. As discussed above, a significant proportion of youths in these cohorts had not yet served when exempt. Yet they were beyond high school so that we do not expect to detect any difference with respect to comparable non exempt youths as concerns schooling. Importantly, because a large proportion did not serve in the army, evidence that their mortality rate changed would suggest that military service has an independent effect on health. Again, outcomes of youths born in 1961-59 in exempt towns are not statistically different from those of the same cohorts from non exempt ones. In particular, estimated differences in mortality rates have p-values in the order of 0.5 thus supporting the assumption that military service has no direct effect on adult health.

To sum up, the evidence reported in table (2) confirms that exemption status granted while still in high school led to a statistically significant increase in graduation rates from high school and to lower adult mortality rates between the mid-20s and mid-30s while not affecting other relevant outcomes. Additional evidence (not reported) suggests that the findings are not due to the linear nature of the empirical models. Estimation of probit models on the same samples and definitions of the control set yield remarkably similar results. Furthermore, somewhat surprisingly the decline in mortality appears to be due to a dramatic reduction of deaths by natural causes rather than by accidents. To this we turn in the concluding discussion. In the next section we discuss the implied 2SLS estimates of the effects of high school completion on subsequent mortality.

5 The effect of schooling on mortality

Our purpose is to establish whether education has a causal effect on subsequent mortality. Letting m_i be an indicator of whether individual i died over a given time spell, h_i her school achievement at the beginning of the spell and X_i an individual's observable characteristics, the relationship of interest is:

$$m_i = \theta h_i + \beta X_i + \epsilon_i \tag{3}$$

with ϵ_i an i.i.d. residual. Reliable causal inference requires that the variation in schooling identifying θ is independent of other determinants of mortality. As discussed above, this is likely not to be the case.

Exemption from compulsory military service provides an ideal source of exogenous variation in schooling achievements because it extends the length of one's potential working life, a major determinant of human capital accumulation decisions (Ben-Porath (1967)). 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. Alternatively, the simple lack of the external constraint imposed by military service could have induced some of the less motivated individuals to stay on and complete the degree.

In our setting, youths from the quake region born before 1966 were simultaneously exempt from compulsory military service. Because they were at different points of their life cycle, however, exemption status modified in different ways their incentives leading to different choices. Those born in 1963-65 were exempt before becoming formally eligible, when still of high school age and able to change their human capital accumulation strategy. On the contrary, older individuals were exempt after high school age and when already formally eligible for service. Keane and Wolpin (1997) show that drop out decisions are fairly irreversible so that youths who had not completed high school were unlikely to go back once exempt.

5.1 Instrument validity

The randomly assigned exemption from military service can serve as instrument for schooling in estimating the effects of the latter on subsequent mortality provided it does not affect directly or through channels other than schooling the outcome of interest.

As concerns the possibility that exemption enters subsequent mortality through other channels, for example because the exemptions correlated with compensatory interventions, the previous discussion has shown that exemption status is only associated with significantly higher schooling achievements and lower mortality rates among young exempt cohorts, while leaving unaffected all other observable circumstances. However, the following empirical analysis will address this possibility in more detail.

As concerns the direct effects of military service on health, the conventional presumption is that army service increases unhealthy behavior and exposure to harmful environments. Yet, several recent studies that use the Vietnam draft lottery do not find increased mortality, worse health conditions or more unhealthy behavior among Vietnam veterans years after discharge (among others, Angrist and

Chen (2007), Dobkin and Shabani (2009), Conley and Heerwig (2009)). On the contrary, Bedard and Deschenes (2006) show that WWII and Korean War veteran status is associated with 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 misbehavior, 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 a direct effect of military service should exist in our setting.

Although the required exclusion restriction cannot be formally tested, the research design allows for some indirect inference based on the presence of reduced form effects in samples with no first stage[23]. As shown above, about one fourth of the cohorts born in 1959-61 did not serve in the army because of the exemption. Therefore, the absence of statistically significant differences in subsequent mortality rates between exempt and non exempt youths belonging to these cohorts suggests it is unlikely that military service has independent effects on mortality. In fact, we can predict what should have been the differential mortality if the effect detected among younger 1963-65 cohorts was due only to absence of military service. Since among younger exempt cohorts about three quarters did not serve in the army because of the exemption we should expect a difference in mortality between older (1959-61) exempt and non exempt cohorts of about -0.34(0.20/0.70)$\approx$-0.1 percentage points, with an associated standard error of 0.16(0.2/0.7)$\approx$0.046 and 95 percent confidence interval approximately equal to $[-0.19, -0.006]$, against an actual estimate of $+0.02$ (table (1) that lies instead outside the 95 percent confidence bands.

[23] Angrist, Lavy and Schlosser (2009) adopt a similar strategy in a study on the effects of family size on offspring's adult outcomes.

Similar results are obtained if the same computation is based on the estimated differences reported in columns (5) and (6) of table (2). In particular, results in columns (6) and (9) are based on comparisons of exempt and non exempt cohorts born, respectively, in 1963-65 and in 1959-61 with those born in 1956-58. In this case, if the effect on mortality detected in column (6) was due to absence of service, in column (9) we should have found a point estimate of about -0.066 with 95 percent confidence interval of $[-0.21, 0.075]$ instead of a non statistically significant +0.16. Taken together, this evidence supports the assumption that exemption from military service before formal eligibility is a valid instrument for completed schooling in a setting where the variable of interest is subsequent mortality.

Under this assumption the ratio of the mortality rate difference between exempt and non exempt younger cohorts to the high school graduation rate differential reported in table (1) provides a Wald estimate of the effect of increasing high school completion by one percentage point on subsequent mortality rate of the cohort of about 0.34/2.23≈0.15 percentage points. The statistical significance of this Wald estimate is rather low. While this might reflect the lack of a significant effect of schooling on mortality, it must be noted that it is based only on unconditional differences by exemption status of similar cohorts. In the following section we thus move to a proper IV setting and show that these preliminary indications resist the inclusion of a number of controls and several robustness checks.

5.2 LS and 2SLS estimates

Table (3) reports OLS and 2SLS estimates of the effect of completing high school on mortality rates between the mid-20s and mid-30s. The excluded instrument is a dummy for exemption status from military service at age 16-18 (those born in 1963-65).

One concern with our strategy relates to the strength of the first stage regression, that is the change in schooling associated with exemption status before formal eligibility. While the statistical significance of the estimated increase in high school completion induced by exemption status based on the most comprehensive specification reported in column (6) of table (2) is acceptable at conventional levels (p-value≈ 0.02), it might signal that the instrument is not sufficiently robust: the associated F-statistic is about 5.5, below both the conventionally suggested benchmark value of 10 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 tests have been proposed that have correct rejection regions even under potentially weak instruments[24]. 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)). For this reason, in table (3) we report both the robust standard errors clustered at town level associated with the relevant estimate (LS and 2SLS) and, for the 2SLS, the p-values associated with the weak-instrument robust AR test.

In column (1) we focus on youths born between 1956 and 1965 in treated and control towns and only include town and cohort fixed effects and a quadratic in cohort size. Least squares estimates show that a 1 point increase in the percentage of high school graduates is associated with a statistically significant decline in 10-year adult mortality rates of about 0.015 percentage points. The corresponding 2SLS estimate suggests a one order of magnitude larger decline which turns out to be statistically significant at conventional levels only for deaths by natural causes. Similar results are obtained in columns (2) and (3) in which we restrict attention to younger cohorts born in 1959-65 and include controls for parental background and, in column (3) allow for area-specific cohort effects. In both cases, a 1 point increase in the proportion of high school graduates significantly lowers mortality rates by about 0.2 percentage points both for all deaths and for natural ones only. In column (4) the sample is further restricted to towns with the least recorded damages still yielding a significant and similar effect.

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

In column (5) we check our results by including retrospective information on the proportion participating in the labor market and in education as of 1986. In particular, if the lower mortality detected among cohorts exposed to the exemption is a mechanical artifact of a longer stay in school and consequent lower exposure to labor related risk factors we should expect the estimated causal effects to weaken. However, the results of column (5) are remarkably in line with those of the basic specification of column (1), also based obtained on the sample of 1956-65 born in treated and control towns. In column (6) we further augment the control set with the proportion in the labor market in 1991. Similarly to the one above, this exercise accounts for the possibility that education affects mortality rates indirectly, through the better working opportunities it commands, rather than directly, for example through cognitive or information processing abilities. Again, we find that point estimates are unchanged although estimated somewhat less precisely.

Finally, in columns (7) and (8) we replicate two placebo experiments. In column (7) we impute exemption status to youths born in 1963-65 in control towns and verify whether 2SLS estimates are significant in a sample of 1956-65 born in control and outer control towns, none affected by the exemption. Consistently with our expectations, the estimated effect is highly non significant. In column (8) we perform a similar experiment on cohorts born in 1953-61 from treated and control towns imputing exemption status to youths born in 1959-61 in control towns, exempt when beyond high school. Again, the 2SLS turns out to be non significant even if, as shown above, about one fourth of the 1959-61 cohort from exempt towns did not serve in the army because of the intervention.

5.3 Discussion

The results suggest that high school completion lowers subsequent mortality rates by a significant and quantitatively relevant amount: increasing by 1 percentage point the proportion of graduates reduces subsequent 10-year mortality rates by 0.1-0.2 percentage points. Three facts stand out from our results. First, LS estimates are in absolute value much lower than 2SLS ones in all specifications, a finding common to studies similar to ours (e.g. Lleras-Muney (2005)), against the a priori that the sources of bias in LS estimates would imply lower absolute values of 2SLS estimates. Second, the 2SLS suggest very large effects of education on subsequent mortality, especially through a reduction of the incidence of natural causes. Specifically, completing high school would lower the *individual* 10-year probability of death between the mid-20s and mid-30s by 10-20 percentage points. While the range of variation of these estimates also reflect the sample choice, we note that in most cases they are not too far away from those reported by Lleras-Muney (2005) and Deschenes (2010) based on older populations than ours. Third, the estimated effect stems almost exclusively from a decline in deaths by natural causes.

Several non exclusive arguments can account for the first two features. If the bias induced by measurement error in self-reported schooling dominates that due to omitted variables correlated with education, 2SLS estimates would be larger than LS (e.g. Card (2001)). Another explanation relates instead to the specific instrument used in the analysis: if treatment effects vary across individuals conventional IV estimators yield the effect of treatment on the population whose behavior is changed by the instrument (Imbens and Angrist (1994), Angrist, Imbens and Rubin (1996)). Therefore, if education affects subsequent mortality differently the 2SLS reflects the average effect among those affected by the exemption, that is youths at risk of dropping out who choose to stay longer in school if exempt from military service, the LATE. Moreover, Angrist and Imbens (1995) show that coding a multivalued treatment variable in a binary fashion generally leads to estimates of the causal effect of interest that are larger than the true causal response[25]. Therefore, if mortality rates respond to the actual number of years of schooling rather than to high school completion the resulting estimate

[25] Specifically, they show that the 2SLS estimate of a model with variable treatment intensity can be interpreted as the weighted average of the treatment effects of a unit increase in the treatment at each treatment level affected by the instrument; the weights associated to these unit treatment effects are the (normalized to 1) proportion of the population whose treatment at the specific juncture is modified by the instrument.

will generally be larger than the causal effect of one additional year of schooling[26]. Indeed, a simple Wald estimate of staying at least one additional year in school obtained as the ratio of the mortality differential between exempt and non exempt cohorts to the high school enrollment differential as of 1981 (3.6 percentage points, row B in table (1)) suggests a weaker mortality decline of 0.1 percentage points. The magnitude of the effect is more in line with that estimated by Lleras-Muney (2005) on older individuals exposed in their teens to changes in compulsory schooling and child labor laws in the US; she estimates that one additional year of schooling lowers the 10-year mortality rate of individuals in their 40s to 60s by 3.7 to 6.1 percentage points. Finally, the estimated effect may reflect the presence of spillover effects of individual education on group members behavior. For example, better individual habits determined by higher schooling may positively influence the habits, behavior and preferences of peers independently of their educational achievements[27]. These effects have been documented extensively, especially with reference to deviant behavior: Case and Katz (1991), Gaviria and Raphael (2001), Kawaguchi (2004), Bayer, Pintoff and Pozen (2009) find evidence that criminal behavior, drugs and alcohol use and smoking are heavily influenced by one's peers; evidence in Kremer and Levy (2008) and De Giorgi, Pellizzari and Redaelli (2010) suggests that peer effects may reflect changes in individual preferences rather than in choice sets.

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

[26] Intuitively, by coding schooling in a binary fashion, the standard interpretation of the 2SLS traces all the change in mortality in a group to the change in the proportion of high school graduates, while mortality can be lower also because some of those who did not complete high school still acquired more schooling because of the exemption.

[27] Formally, if mortality of person i in group c (m_{ic}) is determined by her own education h_{ic} and average education in the group $H_c = E(h_{ic}|c)$ by $m_{ic} = \beta h_{ic} + \gamma H_c + e_{ic}$, then the relationship between the proportion of deaths in the group $M_c = E(m_{ic}|c)$ and average education H_c is $M_c = (\beta + \gamma)H_c + e_c$. To disentangle the two effects in our setting we would need to have an identifiable subset of group c not be affected by the exemption so as to study their mortality in relation to the exemption status of the targeted population ((e.g. Cipollone and Rosolia (2007), Laschever (2009), De Giorgi and Angelucci (2009)). A candidate subpopulation in our case would be females, who are not subject to military service but presumably interact with young men of the same cohort in the same town. However, in a companion paper (Cipollone and Rosolia (2007)) we document the presence of peer effects in education that led also young women from exempt towns to increase their education achievements in response to the higher achievement of young men. Moreover, females might not be a suitable candidate because they are exposed to quite different causes of death. Deschenes (2010) shows that female adult mortality rates are not affected by cohort size at birth, contrary to male ones.

violent causes are largely unaffected. Unfortunately, the nature of the research design limits the sample size considerably, 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. On the one hand, alcohol and drugs consumption are presumably the main causes of death that can be manipulated by individuals and whose consequences may emerge in a relatively short space of time[28]. 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 (motor vehicle accidents, homicides, suicides) together with deaths with an explicit mention of alcohol or drugs. Noticeably, these deaths alone, which 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. On the other hand, they represent activities for which strong peer effects have been documented, thus potentially leading to a considerable amplification of the initial direct individual effects.

Moreover, epidemiologists have long recognized 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 these cases only careful toxicological examination or deep knowledge of the deceased person's habits can inform the report on the specific causes of death. This information is rare and typically collected only if the death is part of a criminal event. Therefore, even in larger samples than ours, a decline in drugs- or alcohol-induced deaths could easily reflect into a lower number of deaths by natural causes.

[28]Other leading death causes can, in principle, 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.

Finally, 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 spare the family a social stigma. This motive is likely to be relevant in the environment we study, characterized by small town size, a markedly traditional culture and strong social ties.

6 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 neighboring towns. The specific research design allows us to assume away the presence of omitted confounding factors, possibly related with the quake and the ensuing compensatory interventions, as well as other systematic differences between exempt and non-exempt youths.

We find that cohorts exempt when still of high school age did attend school for longer. The proportion of youths still enrolled in high school shortly after the exemption was granted increased by 3.6 percentage points. This is in line with theoretical arguments that a longer expected working life fosters human capital accumulation. Ten years later the proportion of high school graduates among exempt cohorts was higher by over 2 percentage points; cohorts exempt shortly after high school age instead did not revise their schooling choices. Over the decade 1991-2001, the overall mortality rate of youths exempt before formal eligibility for military service was lower by about 0.3 percentage points; older exemptees display the same mortality rates as comparable non exempt cohorts, suggesting that exemption from military service has no direct effects on subsequent mortality.

The 2SLS estimates of the effects of high school completion on mortality between age 25 and 35 implied by these results suggest that raising high school completion by ten percentage points would reduce subsequent ten-year mortality rates by 1 to 2 percentage points. While the data do not allow us to thoroughly explore the determinants of this causal link, several checks confirm that the estimated effect is unlikely to be due to differences in the degree of labor market attachment induced by higher schooling: employment rates 5 and 10 years after exemption are found to be broadly similar for exempt and comparable non-exempt youths; moreover, conditioning 2SLS estimates also on these intermediate outcomes does not alter the estimated effect of high school completion on subsequent mortality.

In terms of individual probabilities of death, these estimates would imply that completing high school reduces the probability of dying between the mid-20s and mid-30s by between 10 and 20 percentage points. The magnitude of the effect, while at first surprisingly high, is only slightly stronger than that obtained by Lleras-Muney (2005) for older individuals with an additional year of compulsory schooling and roughly in line with that implied by results in Deschenes (2010). The size of the effect can be partly traced to the particular population affected by the specific instrumental variable strategy, namely youths at risk of dropping out, an interpretation consistent with the substantial difference between 2SLS and OLS estimates of the effect of schooling. Additionally, the research design does not allow us to rule out the presence of spillover effects in health behavior which, if present and assumed away, mechanically amplify the estimated individual direct effect. The estimated effect is driven almost exclusively by a decline in the incidence of natural causes. We argue this may reflect a mismeasured decline in drugs and alcohol related deaths, possibly amplified by the presence of peer effects.

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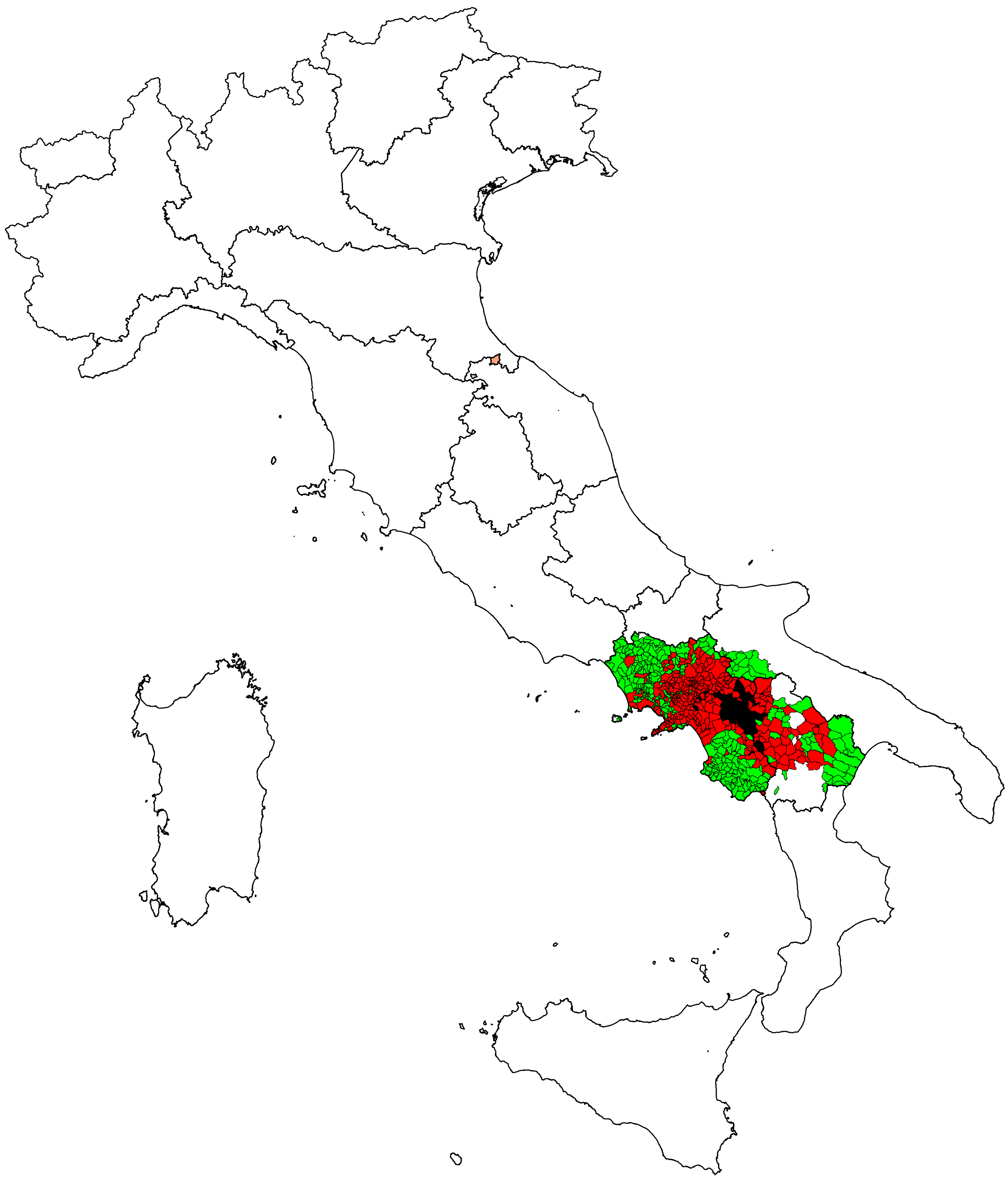
Figure 1: 1980 Earthquake.

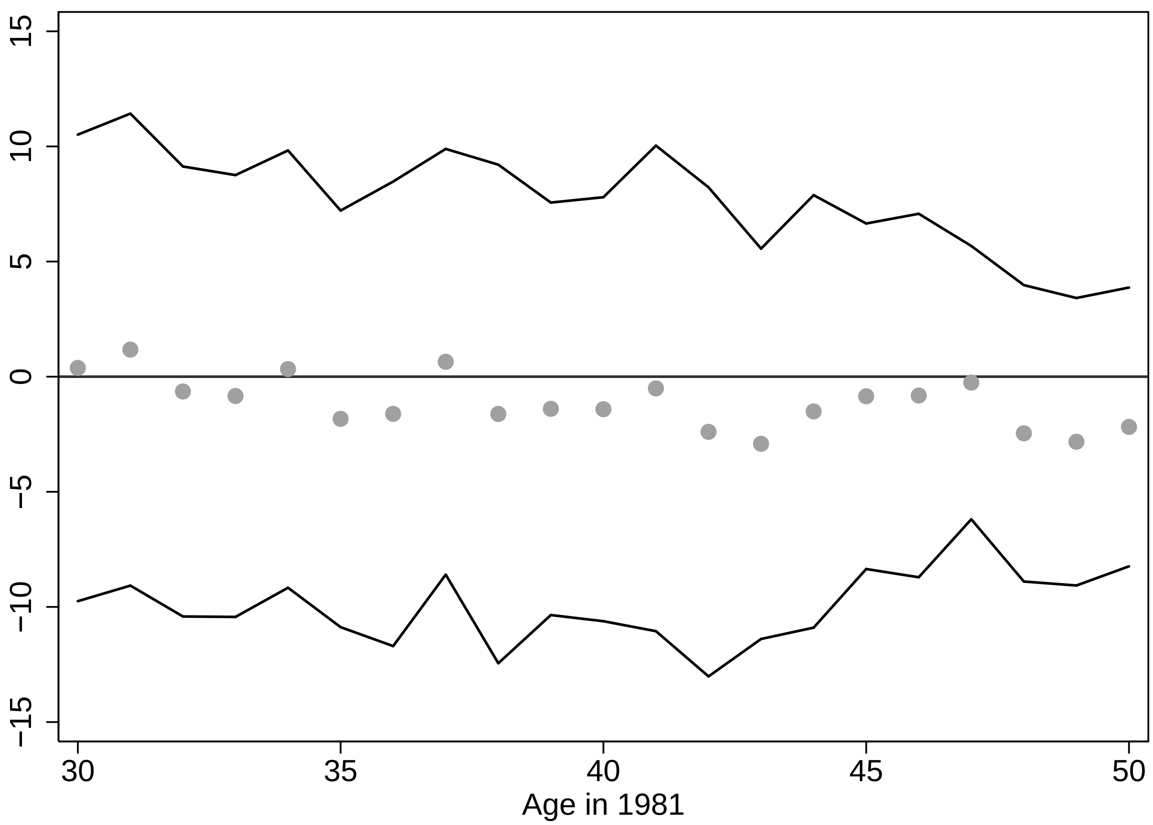


Source: Official report on the 1980 earthquake, Ministero del Bilancio e della Programmazione Economica (1981).
The figure shows towns that were hit by the quake. Darker areas represent more damaged towns.

Figure 2: Treated and control towns

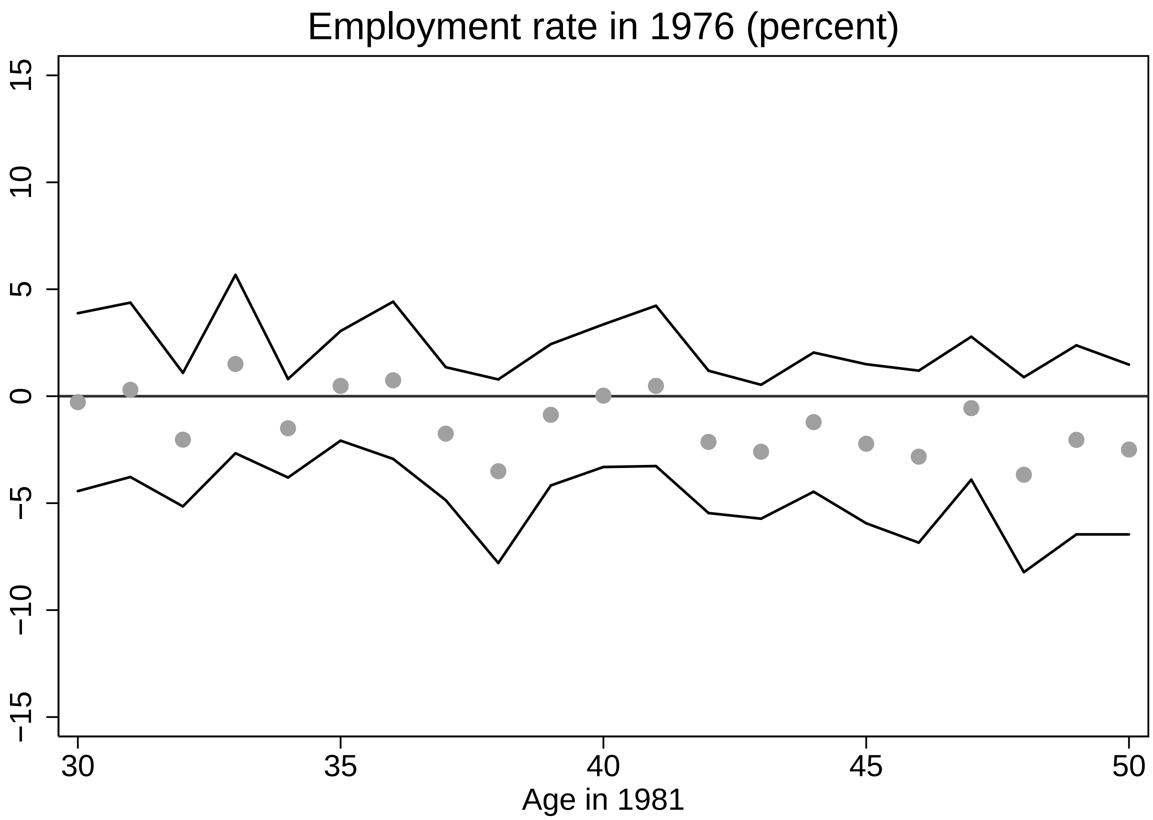
The figure shows inner treated towns (light) and outer control towns (dark). Southern treated towns were granted the exemption although not included in the final list of towns hit by the quake. See text for details.

Figure 3: High-school completion among adults 30-50 years old in 1981.



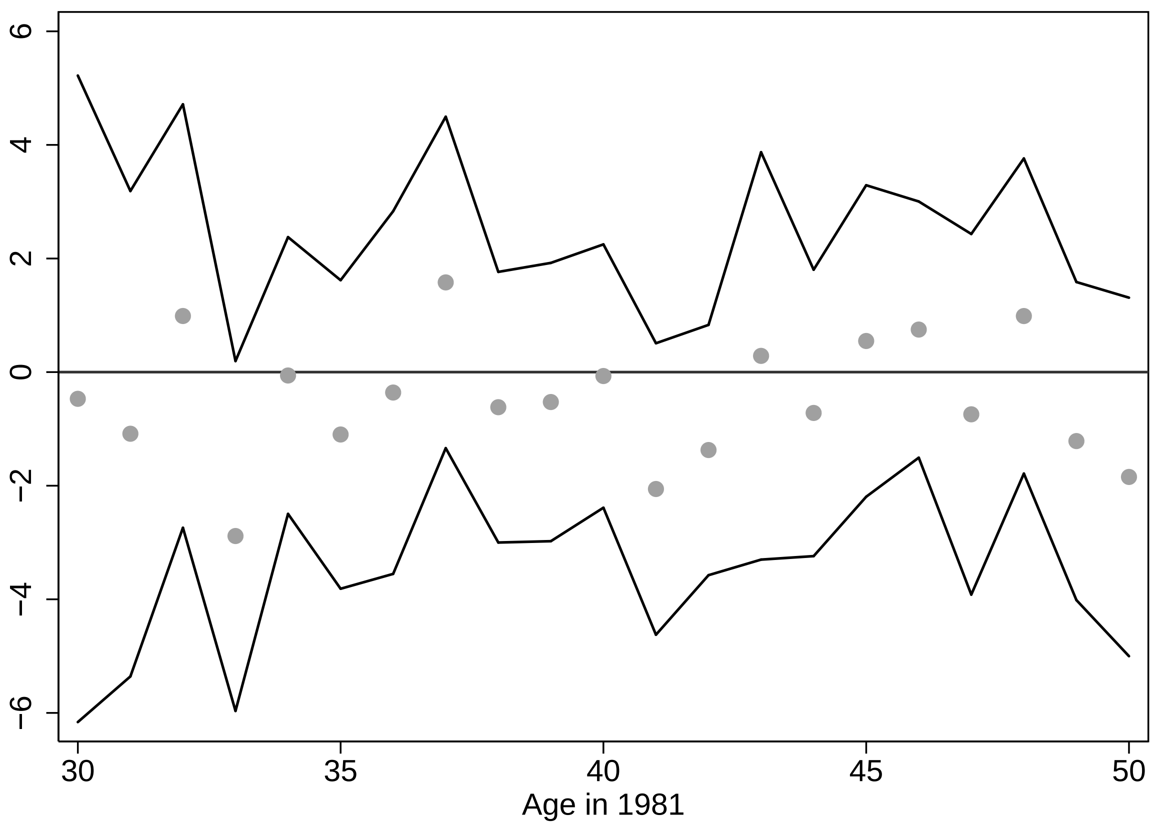
Source: 1981 population census.
The figure reports estimated differences in the share of male population with at least a high-school degree in 1981 and the corresponding 95% confidence intervals based on robust standard errors. Estimates are weighted by the number of observations in the town-age cell.

Figure 4: 1976 adult employment rate by year of birth.



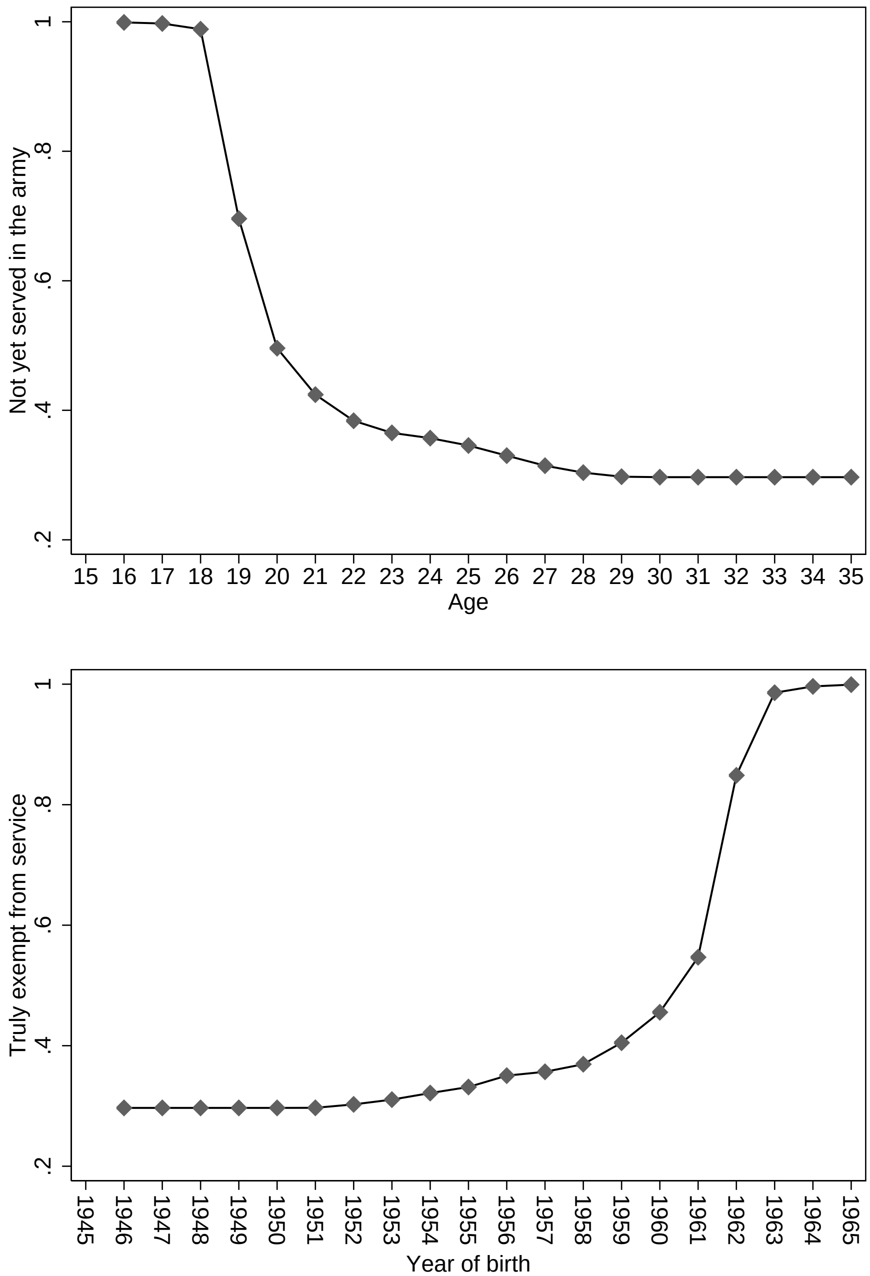
Source: 1981 population census.
The figure reports estimated differences in the share of male population employed in 1976 and in 1981 the corresponding 95% confidence intervals based on robust standard errors. Estimates are weighted by the number of observations in the town-age cell.

Figure 5: Change in employment rates, 1976-81.



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.

Figure 6: Age profile of military service and the size of population affected by the exemption.



Source: 1981 population census.
Panel A reports the age profile of compliance with military service as inferred from the cross-sectional proportion of men from non exempt towns in service at the census date. Panel B reports for each cohort the estimated proportion of men who did not serve because of the exemption (see text for details).

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

	1963-65			1959-61		
	C	T	T-C	C	T	T-C
A. Compulsory military service in 1981	0.4	0.5	0.11	10.5	7.0	-3.51
			(0.13)			(0.90)
B. Student in 1981	45.0	48.6	3.61	17.5	17.9	0.42
			(1.63)			(0.94)
C. In labor market in 1981	53.0	49.3	-3.73	70.4	73.2	2.85
			(1.59)			(1.44)
Obs.	17529	7511		15237	6868	
D. Completed high school in 1991	34.2	36.4	2.27	34.8	34.0	-0.79
			(1.33)			(1.21)
E. Student in 1986	15.5	15.9	0.47	7.6	7.8	0.14
			(0.81)			(0.48)
F. In labor market in 1986	79.2	80.5	1.34	88.0	88.5	0.53
			(0.89)			(0.55)
G. Student in 1991	5.5	6.2	0.70	1.6	1.7	0.10
			(0.45)			(0.21)
H. In labor market in 1991	91.3	91.4	0.10	95.6	95.5	-0.07
			(0.52)			(0.33)
I. Completed college in 1991	4.4	4.3	-0.12	7.0	6.6	-0.42
			(0.34)			(0.48)
I. Dead between 1991 and 2001 by:						
– any cause	1.4	1.1	-0.34	1.5	1.5	0.02
			(0.16)			(0.16)
– natural cause	1.0	0.6	-0.39	1.1	1.1	-0.04
			(0.12)			(0.13)
– accidental cause	0.4	0.5	0.05	0.4	0.4	0.06
			(0.10)			(0.09)
Obs.	18308	8264		18541	8763	

Source: 1981 and 1991 population census.
The table reports percentages for cohorts exempt before and after formal eligibility in treated and control towns and corresponding unconditional differences. Standard errors in parentheses.
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).
Rows A to C are based on town residents at the 1981 census date; rows D-I are based on town of birth as reported in the 1991 population census.

Table 2: The effects of exemption status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Completed high school in 1991	2.04	2.66	2.40	1.87	1.60	2.69	3.53	0.13	0.91
	(1.071)	(0.970)	(0.852)	(0.991)	(1.021)	(1.149)	(1.392)	(1.052)	(0.931)
Completed college in 1991	0.25	-0.14	-0.03	-0.49	-0.63	0.22	0.03	0.21	0.28
	(0.689)	(0.546)	(0.623)	(0.687)	(0.684)	(0.653)	(0.694)	(0.433)	(0.587)
Student in 1986	0.37	1.56	1.56	1.20	1.00	1.99	0.53	0.80	1.03
	(1.975)	(1.020)	(1.142)	(1.250)	(1.252)	(1.115)	(1.204)	(1.015)	(0.771)
In labor market in 1986	1.21	0.01	-0.21	-0.26	-0.10	-0.22	0.88	-0.84	-0.78
	(1.990)	(1.147)	(1.193)	(1.231)	(1.225)	(1.201)	(1.240)	(1.197)	(0.866)
Student in 1991	0.57	1.03	1.04	1.11	1.02	1.07	0.44	0.28	0.41
	(1.064)	(0.619)	(0.678)	(0.734)	(0.736)	(0.569)	(0.717)	(0.534)	(0.382)
In labor market in 1991	0.23	-0.32	-0.40	-0.48	-0.40	-0.44	0.53	0.16	-0.77
	(1.239)	(0.803)	(0.866)	(0.951)	(0.958)	(0.701)	(0.926)	(0.771)	(0.624)
10-year death rate (1991-2001)									
Any cause	-0.30	-0.29	-0.29	-0.38	-0.37	-0.23	-0.53	0.01	0.16
	(0.165)	(0.192)	(0.192)	(0.213)	(0.213)	(0.245)	(0.293)	(0.210)	(0.189)
Natural causes	-0.32	-0.32	-0.33	-0.35	-0.37	-0.31	-0.56	0.00	0.09
	(0.116)	(0.137)	(0.137)	(0.176)	(0.177)	(0.163)	(0.257)	(0.199)	(0.153)
Accidents	0.02	0.03	0.04	-0.02	-0.01	0.08	0.02	0.01	0.07
	(0.092)	(0.100)	(0.108)	(0.113)	(0.111)	(0.145)	(0.170)	(0.080)	(0.109)
Cohort size	N	Y	Y	Y	Y	Y	Y	Y	Y
Parental chars.	N	N	N	N	Y	N	Y	Y	N
Cohort FE	Y	Y	N	N	N	N	Y	Y	N
Geo-Cohort FE	N	N	Y	Y	Y	Y	N	N	Y
Town FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Dependent variables are percentages. Weighted LS; weights are number of observations in town-cohort cell. Robust standard errors in parentheses, clustered at the town level.

Samples - cols. (1)-(3):1956-65, all (T,C) towns; cols.(4)-(5): 1959-1965, all (T,C) towns; col. (6): 1956-58 and 1963-65, all (T,C) towns; col. (7): 1959-65, only least damaged towns and neighbors; col. (8): 1959-65, control and non exempt outer neighbouring towns; col. (9):1953-61, all (T,C) towns. See text for details.

Table 3: LS and 2SLS estimates of the effects of schooling on adult mortality

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
	LS	2SLS	LS	2SLS	LS	2SLS	LS	2SLS	LS	2SLS	LS	2SLS	LS	2SLS	LS	2SLS
Total deaths																
	-0.015	-0.111	-0.020	-0.202	-0.018	-0.233	-0.007	-0.151	-0.014	-0.116	-0.014	-0.115	-0.010	0.042	-0.015	0.174
	(0.007)	(0.077)	(0.011)	(0.158)	(0.011)	(0.195)	(0.011)	(0.101)	(0.007)	(0.107)	(0.007)	(0.106)	(0.009)	(1.739)	(0.009)	(0.224)
		[0.128]		[0.099]		[0.083]		[0.0729]		[0.218]		[0.214]		[0.979]		[0.404]
Natural deaths																
	-0.012	-0.121	-0.017	-0.212	-0.012	-0.229	-0.013	-0.157	-0.012	-0.138	-0.012	-0.138	-0.003	-0.018	-0.014	0.098
	(0.006)	(0.059)	(0.008)	(0.145)	(0.008)	(0.174)	(0.009)	(0.086)	(0.006)	(0.091)	(0.006)	(0.090)	(0.008)	(1.451)	(0.008)	(0.173)
		[0.021]		[0.052]		[0.041]		[0.0341]		[0.045]		[0.042]		[0.990]		[0.562]
Coh. size	Y		Y		Y		Y		Y		Y		Y		Y	
Parental chars.	N		Y		Y		Y		Y		Y		Y		N	
Status in																
-1986	N		N		N		N		Y		Y		N		N	
-1991	N		N		N		N		N		Y		N		N	
Fixed effects for:																
Town	Y		Y		Y		Y		Y		Y		Y		Y	
Cohort	Y		Y		N		Y		Y		Y		Y		N	
Geo*Coh	N		N		Y		N		N		N		N		Y	

The table reports LS and 2SLS estimates of the effect of the percentage with high school degree in the town-year of birth cell on the corresponding mortality rate by all causes and by natural causes over the decade 1991-2001. 2SLS estimates obtained using as instrument for schooling a dummy EX for exemption status. Robust standard errors clustered at town level in parentheses; p-value of weak-instrument robust Anderson-Rubin test statistic ($\chi^2(1)$) in brackets. Cols. 1,5,6: born 1956-65 in treated and control towns, $EX = 1$ if born in 1963-65 in treated towns; cols. 2,3: born 1959-65 in treated and control towns, $EX = 1$ if born in 1963-65 in treated towns; col. 4: born 1959-65 in least damaged treated and neighbouring control towns, $EX = 1$ if born in 1963-65 in treated towns; col. 7: born in 1959-65 in control and outer control towns, placebo exemption assigned to born in 1963-65 in control towns; col. 8: born in 1953-61 in treated and control towns, placebo exemption assigned to born in 1959-61 in treated towns.