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THE IMPACT OF EDUCATION ON MORTALITY: EVIDENCE FROM A COMPULSORY EDUCATION REFORM*

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A negative relation between schooling and health have been observed by social scientists. However, these associations may not necessarily represent causal effect due to the presence of omitted variable or reverse causality from health to education. Taking advantage of a compulsory education reform in Taiwan, we attempt to identify the impact of education on health, as measured by mortality. The data used in this research come from the 2000 Population Census and the 1999–2008 death records of Taiwan, where there was an extension of compulsory education from six to nine years. We find that education does have an impact on mortality for men, but not for women.

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Very preliminary and incomplete

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1 Introduction

Social scientists have long been interested in the relationship between education and health outcomes. The importance of this relationship mainly arises from its policy implications. From the perspective of education policy makers, if education is associated with an improvement in health, the health benefits of education should be taken into account when the cost-benefit analysis with respect to education-related programs are conducted. Moreover, from the public health policy makers' perspective, education could be an effective tool for enhancing the population's health.

There are several possible channels through which education has a direct effect on health. For example, education may enhance an individual's health production efficiency and allocative efficiency because of the knowledge advantage of the educated (see Grossman, 2004, for a review and exposition). Moreover, better educated individuals may have better health because education may reduce an individual's rate of time preference such that better educated individuals are more future-oriented and more likely to invest in health. See Becker and Mulligan (1997) for a discussion of endogenous time preferences and some empirical evidence with respect to education. Education may also enhance health via the income effect. That is, better educated individuals usually have higher income, which gives them better access to higher quality inputs of health production, e.g., nutrition and living conditions, etc.

It is documented in the literature that there exists positive associations between education and health. See Grossman (2006) for a recent review. However, the nature of these associations is still controversial. Omitted variables or reverse causality between education and health may lead to a spurious effect of education on health. There are variables such as an individual's time preferences and ability which affect both education and health. If they are not controlled for, the estimated association between education and health may be just non-causal correlation, which captures some effect of these omitted variables' effect on health. Moreover, healthier individuals may be better able to obtain more education because they are more efficient learners. Healthier individuals may also have more incentive to invest in education because their longer life expectancies enable them to enjoy greater payoffs to this investment. The causal effects of health on education, which if not accounted for will lead to a simultaneity bias when estimating the effect of education on health.

This study reports new evidence on the impact of education on health by looking at the case of Taiwan. For identification, we exploit the exogenous variation in education induced by the 1968 compulsory education reform, which extended compulsory education from six to nine years. Individuals born before September 1, 1955 (i.e., younger than 12 at the beginning of the 1968 school year) were affected by this reform.

Our empirical work is based on the 100% sample of the 2000 Population Census of Taiwan and Taiwan's 1999–2008 death records, which covers all individuals deceased during that period. Using the total number of individuals (by gender) of a specific birth place (county) born during 1951–1959 from the 2000 Population Census and the number of deaths of individuals of a specific birth place (county) from the death records, we obtain the county level mortality rates during 1999–2008. Moreover, we obtain the gender specific county-level schooling years from the 2000 Population Census. Thus, our empirical analysis is based on all individuals in Taiwan, rather than a representative sample.

We use two groups of instruments for our two-stage least squares estimation. The first pertains to the dummy variable indicating whether or not an individual were born in 1956 or after. When using this dummy variable as a instrument, we exclude individuals born in 1955. This is because we do not have an individual's month of birth in the census data. This piece of information is not released for confidentiality reasons. The second group of instruments consists of measures of program intensity pertaining to the compulsory education reform in Taiwan. From 1965 onward, to prepare for the extension of compulsory education from six to nine years, the government built a substantial number of new schools and increased the number of qualified junior high school teachers. Thus, the number of schools and number of qualified junior high school teachers per thousands of children aged 12 in a county could be used as instruments too.

Our results suggests that education does have a causal effect on men's mortality. Estimation results obtained by regressing men's mortality rates on a dummy variable indicating whether or not an individual was affected by the compulsory education law reform suggests that the reform did have a negative effect on their mortality. When employing the dummy variable indicating whether or not an individual was affected by the reform as an instrument, our two-stage least squares results indicate that education has a negative impact on men's mortality. Finally, two-stage least squares estimations using program intensity as instruments also yield results suggesting a negative impact of education on men's mortality. Overall, our estimation delivers a consistent set of results suggesting that education has a negative impact on men's health, as measured by mortality. But the results generated by different estimation strategies all suggest that women's mortality are not affected by their education attainment.

2 Previous Studies

A positive association between education and health is extensively documented by a vibrant body of literature. For example, a negative association between education and mortality is obtained by Kitagawa and Hauser (1973), Christenson and Johnson (1995), and Elo and Preston (1996), and Lleras-Muney (2005). All these studies look at the US population.

Education is also found to be positively associated with health conditions and behavior, e.g., Arendt (2005), Cowell (2006), Grimard and Parent (2007), Tenn *et al.* (2010), Silles (2009), and Webbink *et al.* (2010). Arendt (2005) examines the relation between education and smoking in Danmark. Cowell (2006) looks at the impact of education on some health behavior (i.e., binge drinking and smoking) in the U.S. Grimard and Parent (2007) and Tenn *et al.* (2010) investigates the relationship between education and being a smoker in the U.S. Silles (2009) explores the impact of education on self-reported health in Britain. Webbink *et al.* (2010) inspects the associated between education and being overweight in Australia. See Grossman (2006) for a survey.

There do exist some studies obtaining results indicating the absence of an effect of education on health though. For example, Albouya and Lequien (2009) find that education does not lower mortality in France, and Clark and Royer (2010) do not obtain a positive effect of education on health outcomes or behavior in Britain.

As mentioned earlier, due to the possible presence of omitted variables and reverse causality, education may be endogenous and the estimated association between education and health may not represent a causal impact. Recognizing the potential problem of endogeneity, for identification some of the older studies use instrumental variables, which are not very convincing though. For example, Berger and Leigh (1989) use variables such as per-capita state expenditures on education, per-capita disposable income, IQ test scores as instruments.

To address the problem of endogeneity of education, recent studies mostly use variations in education caused by quasi-experiments for identification. Such instruments are more likely to be exogenous so that the empirical strategy is more convincing. The studies by Lleras-Muney (2005), Arendt (2005), Oreopoulos (2006), Grimard and Parent (2007), and Albouya and Lequien (2009), Silles (2009), Clark and Royer (2010) are some examples.

Estimating the impact of education on mortality in the U.S., Lleras-Muney (2005) employs the variation in (a) the minimum school leaving age as required by the compulsory attendance laws and (b) the minimum age of getting a work permit as required by the child labor laws as instruments. A negative effect is found. Arendt (2005) uses school reforms in Denmark in 1957 (examination for qualification to enter the 6th to 10th grades abolished) and 1975 (compulsory education extended from 7 to 9 years) and finds that more education is associated with better health and lower likelihood of smoking in Danmark. Using the extension of minimum school leaving age from 14 to 15 for England, Scotland, and Wales in 1947, and for Northern Ireland in 1957, Oreopoulos (2006) obtain a strong beneficial effect of education on health (as measured by activity limitations).

Taking advantage of two French education reforms, which extended the minimum school leaving from 13 to 14 (the Zay reform, targeting individuals born after 1923) and 14 to 16 years (the Berthoin reform, targeting individuals born after 1953), Albouya and Lequien (2009) finds that education does not lower mortality in France. The instrument used by Grimard and Parent (2007) is the increase in education of Vietnam war draft avoiders in the U.S. and only weak evidence of a negative relationship between education and smoking is obtained. A positive relationship between education and self-reported health in Britain is found by Silles (2009), who exploits the raising of the minimum school-leaving age in 1947 from 14 to 15 throughout and free universal secondary education for all pupils starting from 1944 the United Kingdom for identification. No evidence of the health benefit of education is obtained by Clark and Royer (2010), who use the 1947 extension of the minimum school leaving age from 14 to 15 and the 1972 extension from 15 to 16 for identification.

3 Compulsory Education Reform in Taiwan

Compulsory education was first implemented in Taiwan in 1943, i.e., during the Japanese-occupied colonial era, for the purpose of assimilation and producing more skilled workers. See Tsurumi (1979). Children between 6 and 12 had to receive six years of free elementary school education. Compulsory education was expanded from six years to nine years in 1968. The post-1968 compulsory education consists of six years of elementary school and three years of junior high school education (i.e., grades 7–9).

After a period of rapid growth in the early 60s, Taiwan's government realized the need to upgrade the industrial base and strengthen its industrial foundation, and further promoting exports. These goals and strategies to achieve them are outlined in the fourth Economic Development Plan (1965-68), which was drafted by the Taiwan's Council for Economic Development and Planning (CEDP, formerly the Council for U.S. Aid).[1] The expansion of compulsory education was one of these measures.

In addition to measures such as establishing the National Science Council (which is similar to the U.S.'s National Science Foundation) and export processing zones, there were other policies to enhance the quality of Taiwan's human resources through training and education. For example, to improve the quality of Taiwan's human resources the government expanded vocational education and occupational training, and launching a nine-year compulsory education program. These policies are outlined in the first Manpower Development Plan (1966–1970).[2]

After a short preparation period, where a substantial number of new schools were built and a large number of qualified teachers were trained, the nine-year compulsory education program was launched in the 1968 school year. During 1966–1968, 243 new junior high schools were opened and the number of junior high school teachers per thousands of ele-

[1] Taiwan's Economic Development Plans are a sequence of soviet-style programs to promote economic development and growth. The first plan was launched in 1953, emphasizing reconstruction and expansion of the agricultural sector. The four-year Economic Development Plans were replaced by six-year plans starting from 1976.

[2] The Manpower Development Plans are a series of centralized programs drafted by the CEDP aiming to enhances human resources. After the end of the twelfth Manpower Development Plan in 2008, Taiwan's government discontinued this sequence and delegate the responsibilities of human resources development to different ministries.

Table 1: Program intensity*

Year	Population aged 12	High school teachers	High schools	Elementary school graduates	Teachers per 1000 elementary school graduates	Schools per 1000 elementary school graduates
1962	50449	17879	318	289634	61.730	1.098
1963	60082	20190	359	309087	65.321	1.161
1964	60497	22422	393	313845	71.443	1.252
1965	61751	24382	411	325076	75.004	1.264
1966	64478	26230	425	343924	76.267	1.236
1967	69464	28428	450	359223	79.137	1.253
1968	72067	34335	654	348953	98.394	1.874
1969	70350	38615	691	371552	103.929	1.860
1970	74285	43618	726	378685	115.183	1.917

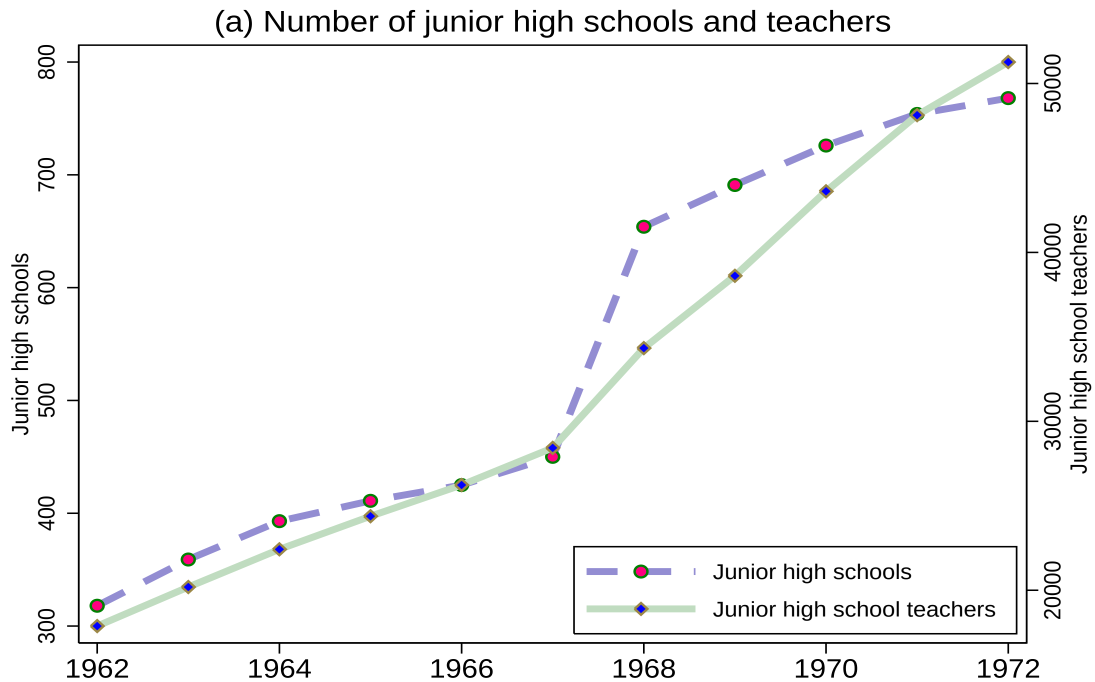
*Information on the number of junior high schools and teachers, and number of elementary school graduates are extracted from Ministry of Education (1962–1970). Data concerning Taipei City (municipality) for the period 1967–1970 come from Department of Budget, Accounting and Statistics (1968–1972). Data on the size of population aged 12 are obtained from Ministry of Interior (1962–1971).

mentary school graduates increased by 31.2 percent from 1966 to 1968. See Figure 1 and Table 1.

The 1968 compulsory education reform was one of the most dramatic reforms in post-colonial Taiwan. Although elementary school education was compulsory and its enrollment rate reached almost 100%, restricted by the limited number of junior high schools, in the mid 1960s only 60% of elementary school graduates continued to their education. With the extension of compulsory education from six to nine years, the junior high school enrollment rate of elementary school graduates jumped from 62.29% to 74.66% between 1967 and 1968. This enrollment rate increased gradually and reached 83.86% in 1972. See Figure 2.

We also see in Figure 2 that the proportion of junior high school graduates entering a higher level of education was mildly increasing up to 1970, reaching 90.13%, it dropped sharply to 71.35% in 1971. This is because while there was a substantial increase in the number of junior high school students due to the extension of compulsory education, the capacity of senior high schools in Taiwan did not change very much such that when the first batch of junior high school students under nine-year compulsory education graduated in 1971 many of them did continue their education.

Figure 1: Number of junior high schools and teachers, and program intensity



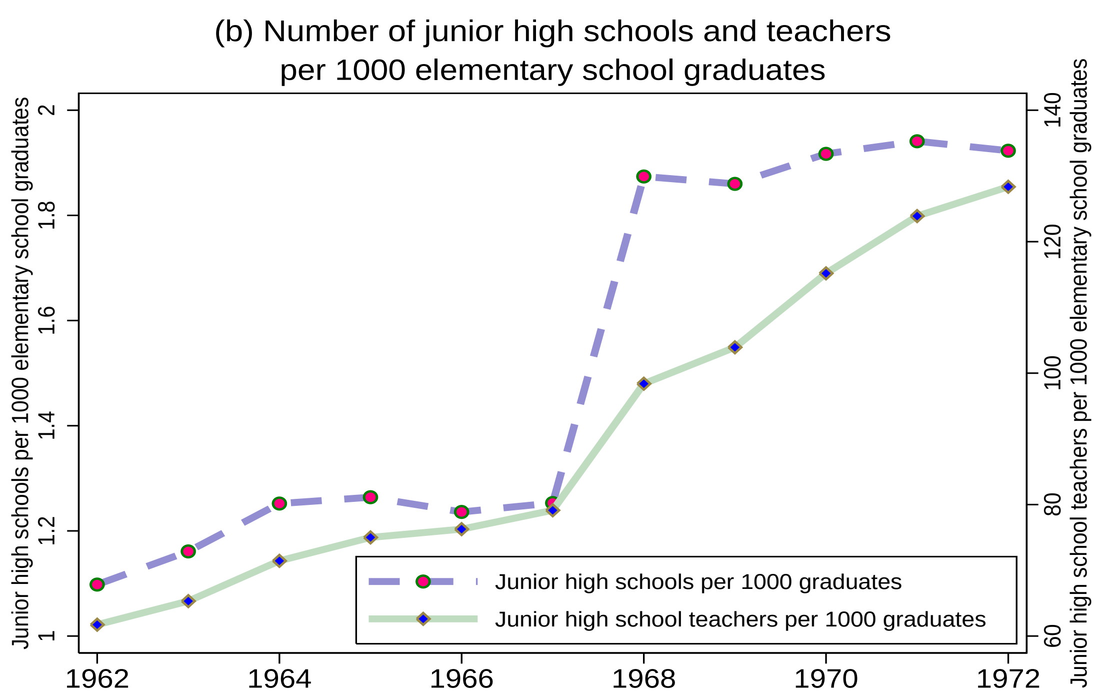
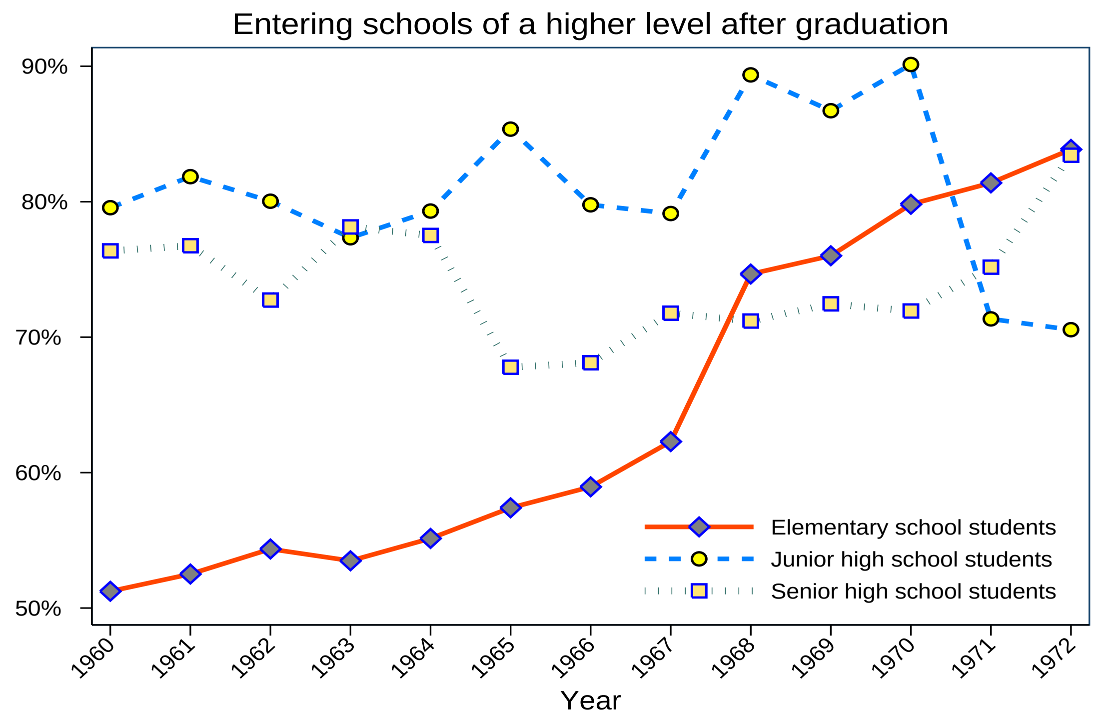


Figure 2: Effect of compulsory education laws on continuing education
Source: Website of the Ministry of Education



4 Data

The data that we use come from two sources: the 2000 Population Census of Taiwan and death records compiled by the Department of Health of Taiwan's Executive Yuan.[3] While the public use version of the census data includes all individuals in Taiwan in December 2000, the death records include all individuals deceased in Taiwan during 1999–2008.

At the individual level, there is no linkage between the census data and the death records. This is because we do not have national identification numbers for individuals in the census data. However, we have information on each individual's county of birth in both the census data and the death records. This enables us to track the group of individuals who were born in a specific county and have a given set of time-invariant characteristics (e.g., year of birth and gender). Accordingly, we categorize individuals in the census data and the death records into cells according to their gender, county of birth, and year of birth, and

[3]The Executive Yuan is the executive branch of the government of the Republic of China (Taiwan).

compute their average characteristics and mortality outcomes. In particular, we compute mortality rates for the period 1999–2008 as follows.

$$\overline{d}_{gcbt} = \frac{\#d_{gcbt}}{\#p_{gcbt}}, \tag{1}$$

where $\#p_{gcbt}$ is the number of individuals in group gcb (i.e., individuals of gender g, and born in county c, year b) who were alive at the beginning of year t and $\#d_{gcbt}$ is the number of individuals in group gcb who died by the end of year t. We obtain $\#d_{gcbt}$ for year 2001 from the 2000 Population Census, for which the enumeration took place in December. The values of $\#d_{gcbt}$ for other years are computed as follows.

$$\begin{aligned} \#p_{gcbt} &= \#p_{gcb\,2001} - \sum \#d_{gcbt} \quad \text{for } t \geq 2002 \\ \#p_{gcbt} &= \#p_{gcb\,2001} + \sum \#d_{gcbt} \quad \text{for } t \leq 2000. \end{aligned} \tag{2}$$

During our study period (1999–2008) there were 24 counties and municipalities (i.e., cities with equal status to a province). In our empirical analysis, we exclude two counties: Kinmen and Lienchiang Counties (both are archipelagos). This is because the population sizes of these two counties were small. In the 2000 Population Census, in Kinmen County each of the 1951–1959 birth cohorts has 496.11 individuals (291.22 men and 204.89 women) on average. In Lienchiang County, each of the 1951–1959 birth cohorts has only 149.89 individuals (83.56 men and 66.33 women) on average. The use of these two counties' data may lead to small-sample bias. After excluding Kinmen and Lienchiang Counties, on average each cell consists of 7128.14 individuals.

Descriptive statistics of our data are displayed in Table 2. The descriptive statistics suggests that the average years of schooling for men are women are 10.5 and 9.2. Moreover, men's mortality rate (on average 0.568%) is much higher than women's (i.e., 0.227%). These figures are in line with the average annual mortality rates of 0.561% (men) and 0.227% (women) for the 1951–1959 birth cohorts during 1999–2008 computed using data reported in Ministry of Interior (2009). In Ministry of Interior (2009) data on the number of deaths are from the death records compiled by the Department of Health and the total number of individuals born in 1951–1959 are from the household registry.[4] The average years of schooling and mortality rates for different cohorts are exhibited in Figures 3 and 4.

[4]An electronic version of Ministry of Interior (2009) is available at `http://sowf.moi.gov.tw/stat/year/elist.htm`.

Table 2: Descriptive statistics

County Level Variables*	All	Male	Female
Mortality rate (%)	0.399	0.568	0.227
(1999–2008, for individuals born 1951–1959)	(0.004)	(0.004)	(0.002)
Years of schooling	9.877	10.504	9.238
(Individuals born 1951–1959)	(0.023)	(0.001)	(0.001)
Number of junior high schools	1.510		
(per 1000 of population aged 12)	(0.37e-03)		
Number of junior high school teachers	89.190		
(per 1000 of population aged 12)	(0.028)		
Hospital density	0.0617		
(1963–1971, per square kilometer)	(0.004)		
Population Density	1.533		
(1963–1971, 1000 per square kilometer)	(0.001)		
# Elementary school graduates in 1962	16.900		
(in thousands)	(0.016)		
% Junior high school graduates of the 1950 birth cohort	50.061		
	(0.032)		
Number of individuals in the 2000 Population Census			
Born in 1951	308339	154686	153653
Born in 1952	305853	154004	151849
Born in 1953	310347	155978	154369
Born in 1954	322126	162139	159987
Born in 1955	341991	172752	169239
Born in 1956	348195	175630	172565
Born in 1957	336702	169671	167031
Born in 1958	353134	178377	174757
Born in 1959	367132	185501	181631

*Sample mean weighted by population size in each cell. Standard errors in parentheses. Information on the number of junior high schools and teachers, and number of elementary school graduates are extracted from Ministry of Education (1962–1970). Data concerning Taipei City (municipality) for the period 1967–1970 come from Department of Budget, Accounting and Statistics (1968–1972). Data on the size of population aged 12 are obtained from Ministry of Interior (1962–1971).

In addition to education, we use hospital density (i.e., the number hospitals per square kilometer) and population density (thousands of population per square kilometer), the number of elementary school graduates of the 1950 birth cohort (interacted with birth cohort dummies or birth year), and percentage of junior high school graduates of the 1950 cohort (interacted with birth cohort dummies or birth year) are used to explain mortality rates.[5] While hospital and population densities control for current county level characteristics that may affect county resident's health, the number of elementary school graduates and percentage of junior high school graduates of the 1950 cohort account for the initial stock of human capital.

To have a preliminary examination of he effect of the extension of compulsory education from six years to nine years, we look at Figure 3 which shows the average years of schooling for men and women born during 1951–1959 based on the 2000 Population Census of Taiwan. The dotted lines represent the projected years of schooling using a quadratic time trend for the cohorts not affected by the compulsory education law reform (i.e., born during 1951–1955), while the dash lines are the quadratic-time-trend projected average years of schooling for cohorts affected by the reform (i.e., born during 1956–1959). In the projections, we leave out individuals born in 1955 because the public use version of the census data does not contain information on individuals' exact date of birth for confidentiality reasons and without this information it is uncertain whether or not an individual born in 1955 was affected by the compulsory law change. Comparing the projected years of school for men and women aged born during 1951–1954 (the cohorts affected by the compulsory education law change) and those born during 1956–1959 (the cohorts not affected), we see that there is a marked increase in schooling years, especially for men.

Next, we explore the impact of the 1968 compulsory education law change on mortality rates. See Figure 4, where the dotted lines trace the quadratic-time-trend projected average mortality rates for the cohorts not affected by the compulsory education law reform (i.e., born during 1951–1955), while the dash lines are the projections for cohorts affected by the reform (i.e., born during 1956–1959). The graphs show that there is a sharp drop in the

[5] Data on the number of hospitals come from Provincial Department of Health (1962–1973). Information on population size for the period 1962–1970 come from Ministry of the Interior (1963–1971). For Taipei City, the relevant data for the period 1967–1971 come from Department of Budget, Accounting and Statistics (1968–1972).

Figure 3: Years of schooling of cohorts born 1951–1959 (Source: 2000 Population Census)

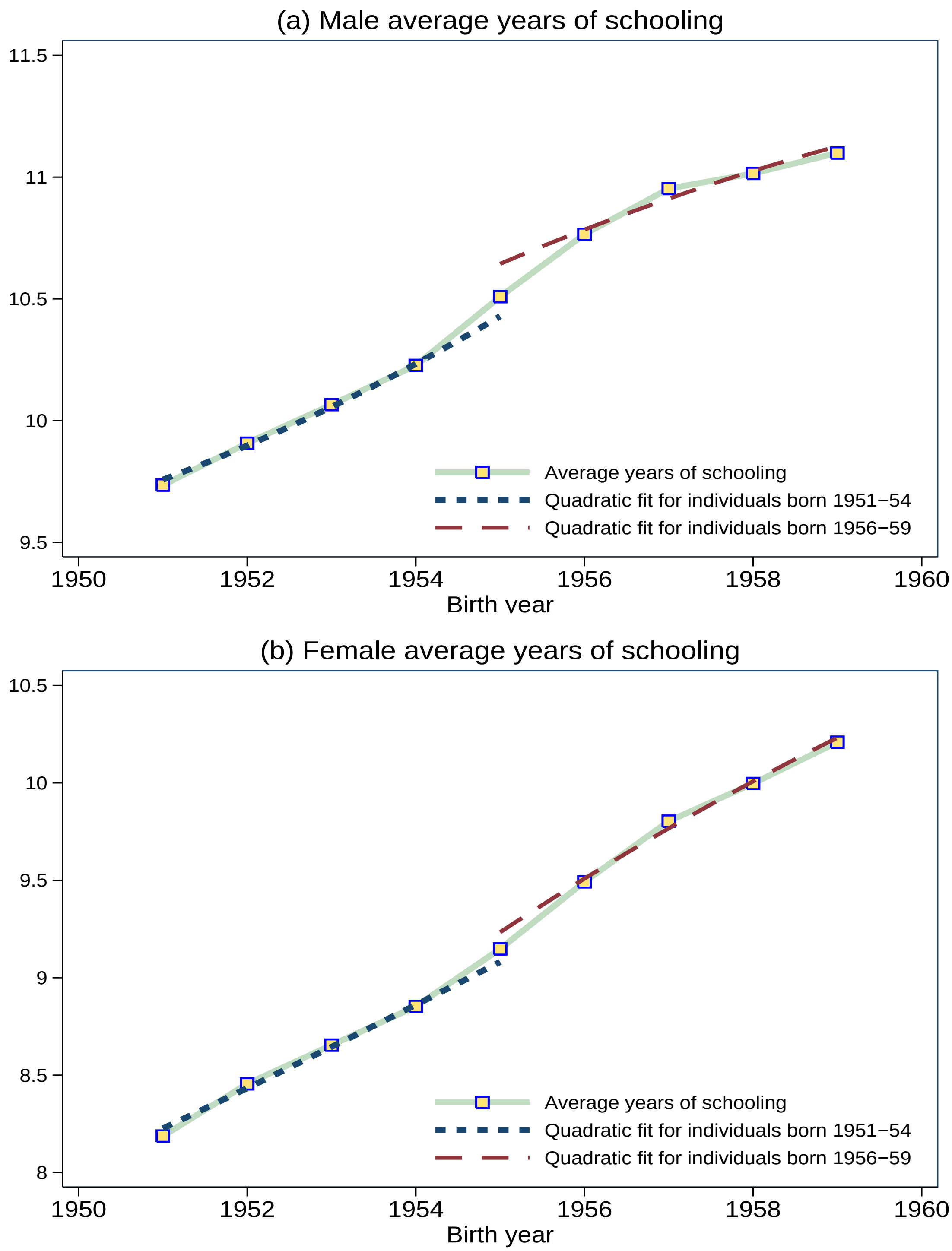
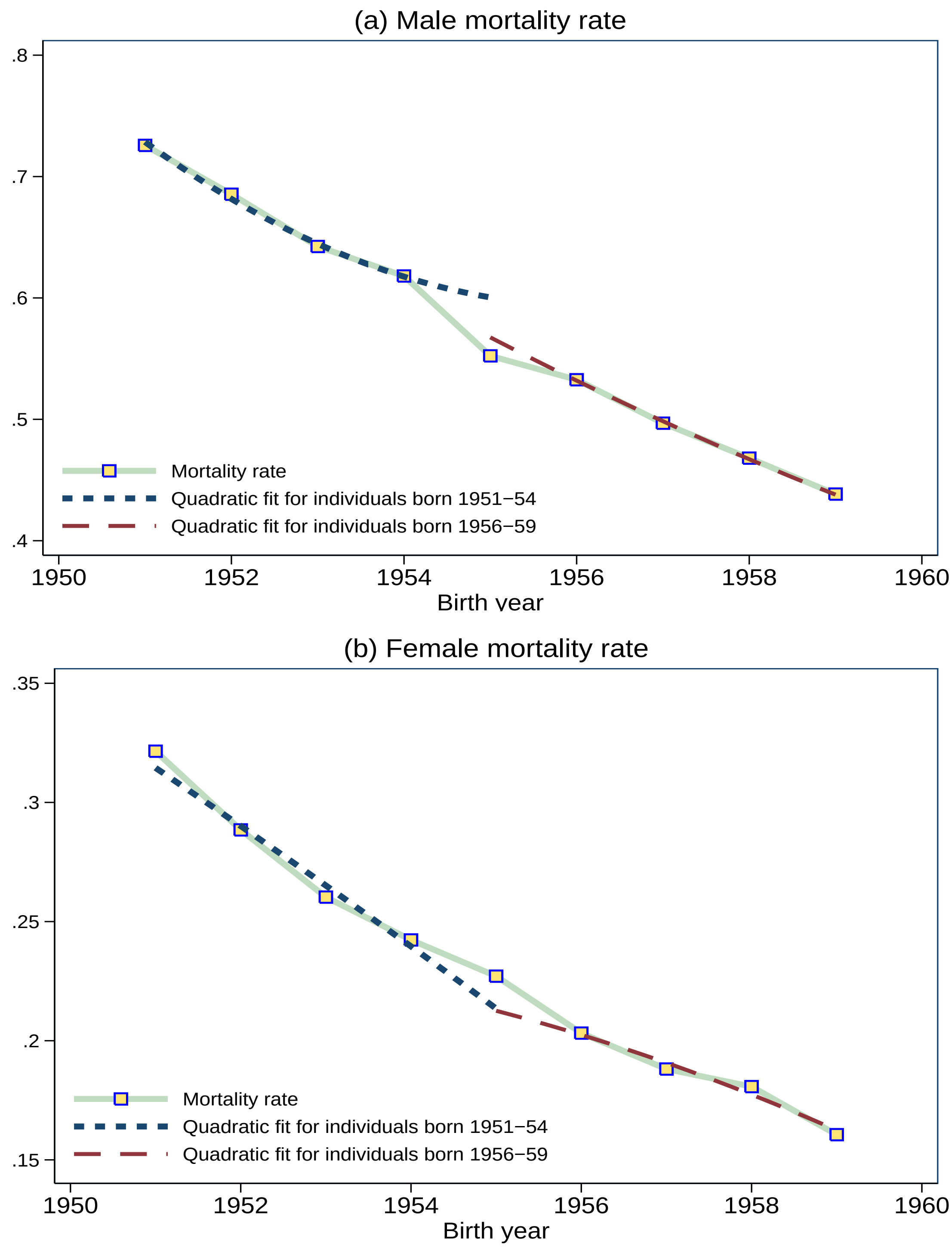


Figure 4: Mortality rates (Source: 2000 Population Census and 1999–2008 death records)



mortality rate of men affected by the compulsory law change. However, the mortality rate of women seems to be unaffected by the extension of compulsory education.

5 Empirical Strategy

We aim to estimate the relationship between education on health. This relationship can be written as

$$h_i = s_i\widetilde{\alpha} + \boldsymbol{x}_i\widetilde{\boldsymbol{\beta}} + \widetilde{\epsilon}_i, \tag{3}$$

where h_i is individual i's stock of health, s_i is her years of schooling, $\boldsymbol{x}_i$ is a vector of her characteristics, and $\widetilde{\epsilon}_i$ is an error term embodying unobservable factors. Our task is to estimate $\widetilde{\alpha}$, which represents the impact of education on health. Omitted variables (e.g., ability, time preference) or reverse causality may results in a correlation between s_i and $\widetilde{\epsilon}_i$, i.e., s_i is endogenous.

An individual's health stock is not observable. In the literature, different measures are used, e.g., activity limitations (self-reported), body mass index, self-rated health status, etc. In this study, we use mortality as our measure of an individual's health stock. The advantage is that it is subject to less measurement errors compared with other indicators, which are mostly self-reported and sometimes lack consistent definition. For example, self-rated health status may not be measured consistently across individuals since different individuals may interpret a category (e.g., "good") differently. With death records for the whole population, mortality can be measure with very little error. The relationship between an individual's mortality and her education can be represented by the following linear probability model.

$$d_{icbt} = s_{icbt}\alpha + \boldsymbol{x}_{icbt}\boldsymbol{\beta}_1 + \boldsymbol{h}_{icbt}\boldsymbol{\beta}_2 + \sum_{b=1951}^{1959} d_b\eta_b + \sum_{c=1}^{21} d_c\delta_c + \sum_{t=1999}^{2008}\sum_{c=1}^{21} d_t d_c\gamma_{ct} + \epsilon_{icbt}, \tag{4}$$

where d_{icbt} is the year t mortality status (i.e., equals one if deceases and zero otherwise) of individual i, who was born in county c belonging to birth cohort b; s_{icbt} is his years of schooling, $\boldsymbol{x}_{icbt}$ is a vector of time-invariant socioeconomic characteristics, $\boldsymbol{h}_{icbt}$ is a vector of county of birth characteristics when the individual was 12, d_c and d_b, respectively, are county of birth and year of birth dummies, η_b and δ_c, respectively, are parameters capturing year of birth fixed effects and county of birth fixed effects, and ϵ_{icbt} is an error term capturing all unobserved variables affecting individual i's mortality.

With the census data and death records that we have, we are not able to estimate (4). This is because we are not able to identify individuals in the death records to those in the census. Instead, work with aggregate data, which allows us to track a group of individuals born in a given county. we aggregate our census data by county of birth and track mortality of individuals born in a given county.

We categorize individuals in the census by gender, birth year and county of birth, and compute the mortality rate $\overline{d}_{gcbt}$ by dividing the total number of individuals deceased in year t, who were of gender g, born in county c in year b (i.e., belong to group gcb), by the total number of individuals of group gcb, who were present at the beginning of year t. That is,

$$\overline{d}_{icbt} = m_{gsct}/p_{gcbt},$$

where m_{gsct} is the total number of group gcb individuals who died in year t (as computed from the death records), and p_{gcbt} is the total number of group gcb individuals who were alive at the beginning of year t.

The model that we are estimating becomes

$$\overline{d}_{gsct} = \overline{s}_{gcb}\alpha + \overline{\boldsymbol{x}}_{gcb}\boldsymbol{\beta}_1 + \boldsymbol{h}_{gcb}\boldsymbol{\beta}_2 + \sum_{b=1951}^{1959} d_b\eta_b + \sum_{c=1}^{21} d_c\delta_c + \sum_{t=1999}^{2008}\sum_{c=1}^{21} d_t d_c\gamma_{ct} + \overline{\epsilon}_{gcbt}, \qquad (5)$$

where $\overline{s}_{gcb}$ and $\overline{\boldsymbol{x}}_{gcb}$, respectively, represent average years of schooling and average characteristics of individuals belonging to group gcb. Since each cell $gsct$ represents different number of individuals. To make the estimation results representative of the population of Taiwan, the population size in a cell is used as weight in our estimations.

It is noted that endogeneity of s_i at the individual level, as in model (3), implies that $\overline{s}_{gcb}$ is also endogenous in (4). To account for endogeneity of $\overline{s}_{gcb}$, we use instrumental variables. We have two types of instruments. The first pertains to the timing of the compulsory education reform and the other pertains to the program intensity of the reform.

5.1 Reduced form estimation

We first explore the impact of education on mortality by estimating a reduced form model as follows.

$$\overline{d}_{gsct} = \lambda_0 + R_{gcb}\lambda_1 + \overline{\boldsymbol{x}}_{gcb}\boldsymbol{\lambda}_2 + \boldsymbol{h}_{gcb}\boldsymbol{\lambda}_3 + y_{gc}\lambda_4 + y_{gc}^2\lambda_5 + y_{gc}\boldsymbol{h}_c^0\lambda_5 + \sum_{c=1}^{21} d_c\delta_c + \sum_{t=1999}^{2008}\sum_{c=1}^{21} d_t d_c \gamma_{ct} + \overline{\omega}_{gcbt}, \tag{6}$$

where the dummy variable R_{gcbt} indicates whether or not the 1968 compulsory education extension applies to group gcb, y_{gc} stands for year of birth, and d_c, denoting a dummy variable for county c, is to control for county level heterogeneity. It equals 1 if yes and zero otherwise. In addition, we use $\boldsymbol{h}_c^0$ to control for county specific pre-existing conditions that may affect both mortality and education attainment. These variables are interact with year of birth. The vector $\boldsymbol{h}_c^0$ includes the number of junior high schools and the percentage of elementary school graduates in county c in 1962.

Model (6) follows a regression discontinuity design, where we allow the mortality rate to follow a smooth time trend (i.e., a quadratic function of year of birth) and a discrete jump in the mortality rate following the timing of the reform (i.e., a non-zero λ_1) embodies the effect of compulsory education law reform. A negative and statistically significant estimate of λ_1 constitutes evidence of a negative impact of education on mortality.

Since mortality rate being a smooth function of time is assumed to achieve identification, birth cohort specific effects $\{\eta_1, \ldots \eta_9\}$ are not allowed. Moreover, since we do not have information on the month of birth of individuals in the census, for individuals born in 1955, we are not sure whether they were affected by the compulsory education extension. Thus, we exclude individuals born in 1955 in our estimation.

5.2 Timing of reform as instrument

To account for endogeneity of education, we use the timing of the compulsory education law reform as an instrument (denoted R_{gcb}). The first-stage regression is specified as

$$\overline{s}_{gsct} = \pi_0 R_{gcb}\pi_1 + \overline{\boldsymbol{x}}_{gcb}\boldsymbol{\pi}_2 + \boldsymbol{n}_{gcb}\boldsymbol{\pi}_3 + y_{gc}\pi_4 + y_{gc}\boldsymbol{h}_c^0\pi_5 + \sum_{c=1}^{21} d_c\kappa_c + \sum_{t=1999}^{2008}\sum_{c=1}^{21} d_t d_c \zeta_{ct} + \overline{v}_{gcbt}. \tag{7}$$

The corresponding second stage regression model is as follows.

$$\overline{d}_{gsct} = \beta_{10} + \overline{s}_{gcb}\beta_{11} + \overline{\boldsymbol{x}}_{gcb}\boldsymbol{\beta}_{12} + \boldsymbol{h}_{gcb}\boldsymbol{\beta}_{13} + y_{gc}\beta_{15} + y_{gc}\boldsymbol{h}_c^0\beta_{16} + \sum_{c=1}^{21} d_c\delta_c + \sum_{t=1999}^{2008}\sum_{c=1}^{21} d_t d_c \gamma_{ct} + \overline{\epsilon}_{gcbt}, \tag{8}$$

where y_{gc} represents year of birth of individuals of gender g and born in county c. In (8) and (7), we assume mortality and education to be a smooth function of time, and the reform in compulsory education leads to a jump in years of education without having a direct impact on mortality. These identification assumptions preclude the incorporation of cohort fixed effects $\{\eta_1, \ldots \eta_9\}$ in (8) and (7). Moreover, due to the absence of information on individuals' month of birth, in the estimation we exclude individuals born in 1955, because we are sure whether these individuals were affected by the compulsory education law reform or not.

5.3 Program intensity as instruments

The second type of instruments that are going to use pertains to program intensity of Taiwan's 1968 reform in compulsory education. In preparation for the extension of compulsory education from 6 to 9 years, the government started building junior high schools and increase the training of quality teachers from 1965. For example, more than 243 new schools were opened in between 1965 and 1968, increasing the number of junior high schools per 1,000 primary school graduates from 1.264 to 1.874 between 1967 and 1968. In addition, enrollment in teacher colleges was expanded in the mid-1960s such that the number of junior high school teachers increased by over forty percent between 1965 and 1968. The expansion of the capacity of junior high education in Taiwan continued even after 1968, but at a smaller scale. See Table 1.

Following Duflo (2001), this study uses the program intensity of Taiwan's 1968 compulsory education reform as instruments to identify the effect of education on health. We have two measures of program intensity, i.e., (a) the number of junior high schools per thousands of elementary school graduates, and (b) the number of junior high schools teachers per thousands of elementary school graduates. Using these two sets of instruments separately, the first-stage of our two-stage least squares regressions are written as

$$\begin{aligned} \overline{s}_{gsct} \quad = \quad & \phi_0^z + \textstyle\sum_{c=1}^{21}\sum_{b=1951}^{1959} d_b p_{cb}^z \phi_{1cb}^z + \overline{\boldsymbol{x}}_{gcb}\boldsymbol{\phi}_2^z + \boldsymbol{h}_{gcb}\boldsymbol{\phi}_3^z + \sum_{b=1951}^{1959} d_b \boldsymbol{h}_c^0 \phi_{0b}^z \\ & + \textstyle\sum_{b=1951}^{1959} d_b\psi_b + \sum_{c=1}^{23} d_c\kappa_c + \sum_{t=1999}^{2008}\sum_{c=1}^{21} d_t d_c \zeta_{ct} + \overline{\xi}_{gcbt}^z, \end{aligned} \tag{9}$$

where p^z_{cb} pertains to a measure of program intensity (i.e., either the number of junior high school per 1000 elementary school graduates, for $z = s$, or the number of junior high school teachers per 1000 elementary school graduates, for $z = t$) in county c for birth cohort b, and h^0_c denotes the number of junior high schools and the percentage of elementary school graduates in county c in 1962.

The second stage regressions are specified as

$$\begin{aligned}\overline{d}_{gsct} &= \alpha^z_0 + \overline{s}_{gcb}\alpha^z_1 + \overline{\boldsymbol{x}}_{gcb}\boldsymbol{\alpha}^z_2 + \boldsymbol{h}_{gcb}\boldsymbol{\alpha}^z_3 + \textstyle\sum_{b=1951}^{1959} d_b \boldsymbol{h}^0_c \eta_{0b} \\ &\quad + \textstyle\sum_{b=1951}^{1959} d_b \eta_b + \sum_{c=1}^{21} d_c \delta_c + \sum_{t=1999}^{2008}\sum_{c=1}^{21} d_t d_c \pi_{ct} + \overline{\xi}^z_{gcbt}.\end{aligned} \tag{10}$$

Contrary to the reduced form model (6), the use of program intensity p^z_{bc} as instruments allows us to control for birth cohort specific effects in both the first stage and second stage regressions (9)–(10). Moreover, to control for pre-existing conditions which may affect both mortality and the education attainment, we use the set of time-varying variables $\boldsymbol{h}^0_c$ as regressors in the first stage and second stage regressions, which pertains to the percentage of junior high school graduation for the 1950 birth cohorts and the number of elementary school graduates in 1962. These variables are interacted with birth cohort dummies to allow for different effects for different cohorts.

6 Estimation Results

6.1 Reduced form results

The results pertaining to the estimation of the reduced form model (6) are reported in Table 3. The results suggest that after controlling for the effect of time trend, the mortality rate of men born in 1956 or later is 0.018 percentage point lower than those born earlier. Given that average mortality rate of men born during 1951–1959 being 0.568%, 0.018 is not a very small number. However, for women, there is no difference those affected by the compulsory education reform and those not affected. The coefficient estimate is 0.006, which is statistically insignificant though. These results are consistent with the graphs in Figure 4 examined in Section 4.

Table 3: Reduced form results

	Men	Women
Reform (born in 1956-1959)	-0.018*	0.005
	(0.009)	(0.009)
Hospital density	-4.83e-05**	1.48-e05
	(1.93e-05)	(1.08e-05)
Population density	-0.001	-0.004
	(0.010)	(0.006)
Birth year	-2.590	-5.045***
	(1.876)	(0.845)
Birth year square	0.654	1.296***
	(0.480)	(0.214)
Birth year×% junior high school graduates of the 1950 cohort	-1.71e-06	-1.15e-05
	(2.07e-05)	(8.57e-06)
R^2	0.799	0.656
Observation	1680	1680

†Standard errors (in parentheses) are clustered at the county of birth level. *, ** and *** denotes statistical significance at the 10%, 5% and 1% levels. The regressions include county of birth dummies, dead year dummies, and interaction of county of birth and dead year dummies as regressors. Individuals born in 1955 are excluded.

6.2 Timing of reform as instrument

We next examine the results pertaining to the use of the timing of the compulsory education law reform as instrument. The first stage results are reported in the first two columns of Table 4.[6] These results suggest that the reform has a substantial effect on the education attainment for both men and women. For men, the reform boosted the years of education by 0.316 years. The F-statistic associated with the instrument is 61.565, which shows that the timing of the compulsory education law reform has substantial explanatory power toward men's education attainment. For women, the reform raised the average years of education by 0.215. The instrument's F-statistic is 14.866, which is regarded to be strong enough for identification of the parameter of interest.

The magnitude of the impact of compulsory education law reform for the case of Taiwan is comparable with the 0.19–0.28 years increase caused by the Germany's introduction of a compulsory ninth grade (Pischke and von Wachter, 2008). It is also similar to Oreopoulos'

[6] In the first stage regression, a second order polynomial for birth year was intended. However, due to multicollinearity the quadratic term is not specified.

Table 4: First stage results†

	(1) Men	(2) Women	(3) Men	(4) Women	(5) Men	(6) Women
Reform	0.316***	0.215***				
	(0.040)	(0.056)				
PIS×Born in 1959			0.016	-0.097		
			(0.079)	(0.108)		
PIS×Born in 1958			0.035	-0.043		
			(0.081)	(0.092)		
PIS×Born in 1957			0.053	-0.098		
			(0.091)	(0.093)		
PIS×Born in 1956			0.112*	-0.036		
			(0.065)	(0.055)		
PIS×Born in 1955			0.141*	0.136		
			(0.072)	(0.092)		
PIS×Born in 1954			0.155*	0.043		
			(0.089)	(0.088)		
PIS×Born in 1953			0.151	0.019		
			(0.095)	(0.121)		
PIS×Born in 1952			0.175	0.117		
			(0.124)	(0.125)		
PIS×Born in 1951			0.177*	0.070		
			(0.087)	(0.084)		
PIT×Born in 1959					-0.001	-0.003
					(0.002)	(0.002)
PIT×Born in 1958					0.001	-0.001
					(0.002)	(0.002)
PIT×Born in 1957					0.002	0.001
					(0.002)	(0.002)
PIT×Born in 1956					0.004*	0.001
					(0.002)	(0.002)
PIT×Born in 1955					0.004*	0.003
					(0.002)	(0.002)
PIT×Born in 1954					0.005**	0.003
					(0.002)	(0.002)
PIT×Born in 1953					0.006**	0.003
					(0.002)	(0.002)
PIT×Born in 1952					0.008***	0.006**
					(0.003)	(0.002)
PIT×Born in 1951					0.07e-05***	0.05e-05*
					(0.02e-05)	(0.02e-05)
Hospital density	7.48e-04***	8.35e-04***	7.69e-04***	4.49e-04**	4.00e-04**	1.95e-04
	(1.48e-05)	(1.83e-04)	(1.80e-04)	(1.76e-04)	(1.68e-04)	(1.97e-04)
Population density	-0.195***	-0.213**	-0.115**	-0.145**	-0.027	-0.038
	(0.055)	(0.087)	(0.045)	(0.055)	(0.018)	(0.027)
Birth year	0.132***	0.222***				
	(0.010)	(0.013)				
Birth year×% junior high school graduates of the 1950 birth cohort	-0.05e-05	0.04e-04**				
	(0.01e-04)	(0.02e-04)				
Instruments	Timing of reform (born 1956–1959)		Schools per 1000 graduates		Teachers per 1000 graduates	
F-statistic	61.565	14.866	4.422	2.309	32.983	14.509
R^2	0.990	0.994	0.995	0.996	0.996	0.997
Observation	1680	1680	1890	1890	1890	1890

†Standard errors (in parentheses) are clustered at the county of birth level. *, ** and *** denotes statistical significance at the 10%, 5% and 1% levels. All regressions include county of birth dummies, dead year dummies, interaction of county of birth and dead year dummies, interaction of year of birth dummies and proportion of elementary school graduates in 1962, and interaction of year of birth dummies and number junior high schools in 1962 as regressors. Regressions (3)–(6) include birth cohort dummies as regressors and their interactions percentage of junior high school graduates of the 1950 birth cohort as regressors. Individuals born in 1955 are excluded in regression set (1)–(2).

(2007) estimate of 0.236, 0.260 and 0.458 years increase caused by the U.S. school leaving age requirements of 14, 15, and 16 years, respectively. But our estimates are lower than the 0.405 and 0.643 years increase caused by Canada's school leaving age requirements of 14 and 15 years, and the 0.425 years increase in schooling caused by UK's school leaving age of 15 years as reported by Oreopoulos (2007).

In the first stage regression results, we also see that hospital density has the expected sign. It is positively correlated with education attainment for both men and women. This association may be due to the fact that hospital density reflects the amount of county-specific public resources and facilities, e.g., public kindergartens or day-care centers, which may improve the chance for an individual to pursue better education. On the contrary, population density is negatively associated with education attainment for both men and women. This may arise from the fact that higher population density implies stiffer competition for education resources. The interaction between birth cohorts and percentage junior high school graduation rate for the 1950 birth cohort is statistically insignificant for men, but is positive and statistically significant for women. The positive association is expected. It may reflect the persistence of county specific factors which is favorable to education attainment, e.g., social norm and level of economic development. This factors are important for women but not for men though.

The second stage results with the timing of the compulsory education law reform as instrument are reported in Table 5's third and fourth columns. For men, the estimate of the impact of education on mortality is negative and statistical significant. The results implies that for men an increase in one year of education will reduce their mortality rate by 0.059 percentage points. The magnitude of this impact is economically significant given that the mortality rate of men born 1951–1959 is around 0.568% (see Table 2).

Our estimation results suggest that IV estimate is larger in magnitude than the OLS estimate. This seems unreasonable. Given that the possible omitted variables are mostly likely to improve health (i.e., reduce mortality rate) and boost education attainment (e.g., initial stock of human capital such as health or intelligence), the OLS estimate is likely to be larger in magnitude that the IV estimate. However, the Hausman test for endogeneity suggests that the IV estimate is not different from the OLS estimate (i.e., -0.050). This suggests that education is not an endogenous variable. This is consistent with Lleras-Muney's

(2005) finding.

Our estimate of the impact of education of -0.059 is lower than Lleras-Muney's (2005) estimates of the impact of schooling years on the 10-year mortality rate, which are in the range [-0.017 , -0.061]. Lleras-Muney's (2005) estimates implies an impact of an extra year of schooling of between -0.17 and -0.61 percentage points on the annual mortality rate. Given the average annual mortality rate of 1.06% for individuals aged 50.4 on average (based on the census data for whites aged 35–80) and 2.54% for individuals aged 62.9 on average (based on the National Health and Nutrition Examination Survey data for whites aged 40–85), these estimates are very large. Without accounting for education's endogeneity, Lleras-Muney's (2005) estimates in terms of annual mortality rate are in the range [-0.11, -0.17] (in percentage points). Relative to the average mortality rates in the data, these estimates are comparable to our estimates IV estimate of -0.059 or the OLS estimate of -0.050 for men.

For women, our estimate (either the IV estimate or the OLS estimate) of the impact of education on mortality is statistically insignificant. The OLS coefficient estimate of 0.002 (see the first column of Table 5) and the IV estimate of 0.020 (see the fourth column of Table 5) are both statistically insignificant. This suggests that for women education does not have any impact on the mortality rate. This piece of result is also in contrary to those obtained by Lleras-Muney's (2005), who finds that education's (absolute) impact on mortality are the same for men and women even though women's mortality rates are much lower in her samples.[7]

As for the effects of other variables, while hospital density does not have any effect on the mortality rate for men and women, population density has a negative effect on men's mortality rate and no effect on women's.

6.3 Program intensity as instruments

The estimation results pertaining to the use of program intensity as instruments are displayed in columns 3–6 of Table 4 (first stage) and columns 4–8 of Table 5 (Second stage). We

[7] According to the regression results of Lleras-Muney (2005), women's mortality rate are around 13.7 and 7 percentage points lower than men's in the NHEFS individual and census aggregate samples.

Table 5: Second stage results: Timing of reform as instrument

	OLS		IV-1		IV-2		IV-3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men	Women	Men	Women	Men	Women	Men	Women
Education years	-0.050**	0.009	-0.059**	0.021	-0.119	0.043	-0.087***	-0.006
	(0.022)	(0.015)	(0.025)	(0.039)	(0.075)	(0.058)	(0.021)	(0.024)
Hospital density	0.44e-04	-0.32e-04	-0.02e-04	0.05e-04	0.85e-04**	-0.46e-04**	0.66e-04**	-0.25e-04
	(0.36e-4)	(0.30e-04)	(0.14e-04)	(0.32e-04)	(0.41e-04)	(0.20e-04)	(0.32e-04)	(0.33e-04)
Population density	-0.021***	-0.006	-0.013*	-0.001	-0.029***	-0.002	-0.025***	-0.008**
	(0.006)	(0.004)	(0.007)	(0.008)	(0.009)	(0.006)	(0.005)	(0.004)
Birth year			-0.025***	-0.024**				
			(0.004)	(0.010)				
Birth year×% junior high school graduates of the 1950 birth cohort			0.021e-04	0.010e-04				
			(0.014e-04)	(0.010e-04)				
Instruments	—		Timing of reform (born 1956–1959)		Schools per 1000 graduates		Teachers per 1000 graduates	
R^2	0.794	0.644	0.800	0.649	0.793	0.644	0.793	0.644
Hansen's J-statistic					11.909	6.570	7.882	11.104
					[0.155]	[0.584]	[0.445]	[0.196]
Hausman test statistic			0.007	0.326	0.381	1.968	2.168	0.677
			[0.934]	[0.568]	[0.537]	[0.161]	[0.141]	[0.410]
Observation	1890	1890	1680	1680	1890	1890	1890	1890

†Standard errors (in parentheses) are clustered at the county of birth level. *, ** and *** denotes statistical significance at the 10%, 5% and 1% levels. All regressions include county of birth dummies, dead year dummies, interaction of county of birth and dead year dummies, interaction of year of birth dummies and proportion of elementary school graduates in 1962, and interaction of year of birth dummies and number junior high schools in 1962, and regressions in sets (1)–(3) include birth cohort dummies as regressors. Individuals born in 1955 are excluded in regressions (3)–(4).

discuss the results when the number of junior high schools per thousand elementary school graduates are used as instruments for estimation.

The first stage regression results pertaining to the use of number of junior high school per thousand elementary school graduates as instruments are reported in columns 3–6 of Table 4. See equation (9), where junior high school per thousand elementary school graduates is interacted with birth cohort dummies. The number of school per thousand graduates variables are statistically significant only for men born in 1956, 1955, 1954, and 1951 (i.e., aged 12, 13, 14, and 17 when nine year compulsory education was implemented in 1968). The coefficient estimates are small and statistically insignificant for those born in 1957 or after. This suggests that it was mostly men attending junior high school in the year when the compulsory education law reform is implemented and those born before that year benefited from having more junior high schools. This may be due to the fact that prior to 1968 and even in 1968, the shortage in schools prevented some students from attending junior high school and the availability of schools was critical to the attainment of junior high school education. The number of schools became less important after 1968 probably because, after the large scale school building campaign in the mid 1960s, there were sufficient number of schools to absorb students who are willing to attending junior high school. The F-statistic of the instruments is 4.4, suggesting that the instruments are not strong in explaining men's years of education.

For women, the number of schools per thousand graduates variables are statistically significant for those born in 1955. For other birth cohorts, the coefficient estimates are statistically insignificant and some of them are even negative. The F-statistic of 2.3 also suggests that for women these instruments are weak. Thus, the second stage results may not be reliable.

The results of the second stage regression using the number of schools per thousands of elementary school graduates as instruments are reported in Table 5's columns 5–6. For men, the results suggest that an additional year of education reduces the mortality rate by 0.119 percentage points. Hansen's J-statistic of 11.909 suggests that the over-identification restriction test does not reject the null hypothesis that the instruments are not correlated with the second stage error term. This estimate is much larger in magnitude than the OLS estimate -0.050 and the IV estimate -0.059 with the timing of the compulsory law reform as the instrument. However, the standard error of the estimate is also much larger, probably because the instruments are not strong. This renders the coefficient estimate statistical insignificant at conventional levels. Furthermore, the Hausman test statistic suggests that this IV estimate is no different from the OLS estimate.

For women, the results in column (6) of Table 5 indicate that the estimate of the impact of education on mortality is positive, but statistically insignificant. This is consistent with the OLS estimate and the estimate using the timing of the compulsory education law reform as instrument.

Now we turn to the results with variables pertaining to the number of qualified junior high school teachers per thousand of elementary school graduates as instruments for estimation. The first stage regression results are displayed in columns 5 and 6 of Table 4. For men, the effect of teachers per thousands of graduates on years of education is positive and statistically significant only for those who were born in 1956 and after (i.e., aged 12 or older when the extension of compulsory education from six to nine years was implemented). The effect is statistically insignificant for men born in 1957 and older, and turns negative and remains statistically insignificant for men born in 1959. This pattern for the effect of number of teachers per thousand of graduates is consistent with the first stage regression results when the number of schools per thousands of graduates variables were used as instruments. We attribute this to the fact that elementary school graduates were prevented from attending junior high school due to insufficient capacity of junior high school education, so that the county ratio of qualified junior high school teachers, as an indicator of school capacity in the county, determined whether or not an elementary school graduate can enter junior high school. Nonetheless, the expansion of teacher training programs has produced sufficient qualified junior high school teachers when the extension of the compulsory education from six to nine years was implemented in 1968. As such the ratio of qualified junior high school teachers did not play an important role in determining education attainments in later years.

According to the F-statistic of the instruments of 33.0, variables pertaining to the teachers to elemental school graduates ratio have very strong explanatory power toward men's education attainment. This also shows that the instruments are probably strong enough for identification of the impact of education on mortality.

For women, these variables do not have as much explanatory power toward education attainment as in the case for men. The F-statistics of the instruments is 19.7, which is sufficient for identification of the causal impact though. Similar to the case of men, the teachers to elementary school graduates ratio is more important for education attainment in earlier years. Women's first stage regression results show that the teacher ratio is statistically significant in affecting the education attainment of the 1951 birth cohort only.

The second stage results are reported in columns (7)–(8) of Table 5. The results shows that for men the impact of education on mortality is -0.087, implying that an extra year of education reduces men's mortality rate by 0.087 percentage points. This is again larger than the OLS estimate of 0.050. However, the Hausman test statistic suggests that difference between the OLS and IV estimates are statistically significant, indicating that education is not endogenous. The Hansen's J-statistic of 7.882 suggests that the instruments are likely to be uncorrelated with the second stage regression's error term.

The impact of education on women's mortality is positive, but statistically insignificant. This suggests that women's health is not affected by education. This is consistent with results that we obtained by using the reduced form model and using other instruments.

7 Conclusion

This study investigates the impact of education on health, using mortality as a measure. Economists and other social scientists are interested in this impact for the purpose of policy evaluation. If education has an impact on health, then this indirect benefit of education should be taken into account when education policies are evaluated.

It is likely that there are omitted variables which affect both the health and education attainment of individuals. This suggest that without accounting for these omitted variables, the results will be biased. There are numerous attempts to control for these omitted variables in order to estimate the causal impact of education on health. One of the most convincing strategy is to take advantage of quasi-experiments generated by reforms. This study is another attempt to use a quasi-experiment to control for omitted variables. We explore the case of Taiwan which extended its compulsory education from six to nine years in 1968, such that there was a surge in education attainment for those born in 1956 and after.

Our empirical study uses the 2000 Population Census of Taiwan and the 1999–2008 death records. While the former consists of information of all individuals present in Taiwan in 2000, the latter contains records of death of all individual residing in Taiwan during 1999–2008. There is no individual identification information allowing us to link the two datasets at the individual level. Thus, instead of working with individual level data, we work with aggregate data. We aggregate individual in the two datasets at the level of cells (defined by gender, county of birth and birth year) and link the cells in two dataset using the county of birth.

Taking advantage of the 1968 reform in compulsory education in Taiwan, we use several empirical strategies. The first is reduced form estimation, which looks at the impact of the timing of the reform on the mortality rates. The second uses the timing of the reform as an instrument for two-stage least square estimation. The third takes advantage of the regional variation in the number of schools and teachers, which were expanded by the government beginning from 1965 in anticipation of the vast increase in junior high school students.

Our estimation results also indicate that education is not endogenous. Hausman test statistics indicate that the differences between the OLS and instrumental variable estimates are not statistically significant.

The results obtained by different estimation strategies unanimously suggests that there is a statistically significant impact of education on mortality for men. It is statistically insignificant for women. In terms of the magnitude, the impact of education on mortality for men is moderate. An extra year of education reduces men's annual mortality rate by around 0.05 percentage points (based on OLS estimate), which is not small given that the average annual mortality rate of men born in 1951–1959 is 0.568%. In relative terms this implies that an extra year of education reduces mortality rate by around 0.088%.

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