Education and Later-life Mortality: Evidence from a School Reform in Japan[*]

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Abstract

We examine the mortality effects of a 1947 school reform in Japan that extended compulsory education from primary to secondary school by up to 3 years and eliminated secondary school fees. The children affected by the reform were mainly from disadvantaged families, and would likely have benefited from additional schooling. Even in this favorable environment, we fail to find that the reform improved later life mortality up to age 87, even though it significantly increased years of schooling. Furthermore, there is little effect on hospitalization. These findings suggest limited health returns to schooling at the lower levels of educational attainment.

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1. Introduction

Economists argue that educated people live longer than uneducated people because education expands access to resources, increases investment in health, and improves the efficiency of health production through better knowledge and information acquisition (Grossman 1972; Cutler and Lleras-Muney 2008). Consistent with this argument, a positive association between educational attainment and health status has been observed across time and space. Several studies have used compulsory education reforms to estimate the causal impact of schooling on later-life mortality, but the evidence is highly mixed, with effects of varying sizes, including precisely estimated zero effects.[1]

We examine the impact of a 1947 school reform in Japan, which extended compulsory schooling from primary to secondary education, on later-life mortality. We adopt two empirical approaches to estimate the impact of schooling on mortality. We first employ a regression discontinuity (RD) design at the monthly level to compare individuals born just before the school entry cutoff with those born just after, who were almost identical in age but were subject to school reform. However, because students born immediately before and after the cutoff were also the oldest and youngest within their respective academic cohorts, we use the academic cohorts born before and after the school reform as the control group in a difference-in-regression discontinuity (Dif-in-RD) design to isolate the effect of additional schooling from that of relative age effects.

Using the 100% census in 2020, we show that the school reform led to a significant increase in the secondary school completion rate and thus, in years of schooling by 0.12–0.21 years. However, we do not find that additional schooling reduced mortality of the affected cohorts up to age 87 years. Our reduced-form mortality estimate is precisely 0, and we can rule out with 95% confidence that the school reform reduced mortality by more than 0.7 percentage points in the 50 years between 1972 and 2021, during which up to 67.2% of the affected cohorts died. In addition, we find no evidence of reductions in mortality from more specific causes of death or in rates of outpatient visits and inpatient admissions that could affect quality of life. We

[1] See studies for the United States (Lleras-Muney 2005; Mazumder 2008; Lleras-Muney et al. 2022), the United Kingdom (Clark and Royer 2013; Davies et al. 2018), France (Albouy and Lequien 2009), the Netherlands (Van Kippersluis et al. 2011), Sweden (Lager and Torssander 2012; Meghir et al. 2018; Fischer et al. 2021), Romania (Malamud et al. 2023), and Taiwan (Kan 2016). Grossman (2006), Mazumder (2012), Galama et al. (2018), and Lleras-Muney (2022) provide literature reviews.

also examine several socioeconomic outcomes available from the 1980 census. While the effect of the school reform on labor force participation and employment is very limited, the affected cohorts shift from the primary to the tertiary sector, suggesting greater access to white-collar jobs. However, the magnitude of this shift is small.

Our results echo the null findings of education on mortality in the recent literature by Clark and Royer (2013) for the United Kingdom, Meghir et al. (2018) for Sweden, and Malamud et al. (2023) for Romania. Our study complements the extensively studied, but inconclusive, literature in several ways.

First, the cohorts affected by the school reform (i.e., "compliers") were likely to be mainly from disadvantaged families. Because the pre-reform secondary school completion rate was already as high as 92% and the reform abolished secondary school fees, those who could not attend secondary school before the reform seem to have faced financial constraints. Such children might have benefited more from improved labor market opportunities, knowledge gained from additional schooling, and better peers than those from more advantaged families.[2]

Second, the incremental years of schooling are up to 3 years from grades seven to nine, unlike most previous studies that mostly examine compulsory schooling extended by 1 year, such as the school reform in the United Kingdom. While the non-linear return to education by the initial level of education (e.g., primary vs. college education) has been widely discussed, the potential non-linearity by a different incremental margin in years of schooling has received less attention in the literature.

Third, we can track mortality of affected cohorts up to age 87 years, while other studies track mortality at best to the early 70s. Clark and Royer (2013) for the United Kingdom, Meghir et al. (2018) for Sweden, and Malamud et al. (2023) for Romania track mortality until ages 74, 75, and 71 years, respectively.[3] An additional 10–15 years of follow-up is critical, as mortality for the affected cohorts in our setting is 14% between ages 65 and 74 years, but as high as 31% between ages 75 and 84 years.

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[2] Numerous studies show that disadvantaged children benefit more than advantaged children from government interventions and welfare programs (e.g., Deming 2009; Havnes and Mogstad 2011; Herbst 2017; Cornelissen et al. 2018; Cattan et al. 2021; Kose 2023).

[3] Van Kippersluis et al. (2011) examine the effect of a 1928 Dutch reform that increased compulsory schooling from 6 to 7 years on mortality between the ages of 81 and 87 years, but a limitation of their study is the selective mortality before the age of 81 years. By contrast, we observe the mortality of the affected cohorts between the ages of 37 and 87 years.

Finally, this is the first study to estimate the causal effect of schooling on later-life mortality outside of the United States and Europe, except for Taiwan, which did not extend compulsory schooling until 1968 (Kan 2016). Because the intervention must have occurred many years ago for later-life mortality to be observed, Japan is ideal, given that its school reform took place in 1947, the same year as in the United Kingdom, which has been extensively studied (e.g., Oreopoulos 2006; Clark and Royer 2013).

In summary, we examine the impact of education on mortality among individuals who might have benefited from additional schooling until very late in life in the non-Western setting. Even in this favorable setting, we fail to find evidence of a positive return to schooling on mortality. While the discrepancy across studies could be due to different populations being affected by the reforms, our results show that the return to education on mortality was likely limited at the lower levels of schooling.

The remainder of the paper is organized as follows. Section 2 provides the institutional background. Section 3 describes the data. Section 4 presents our empirical strategy. Section 5 reports the results. Section 6 concludes.

2. Background

2.1. 1947 Education reform

Prior to the postwar education reform, all students in Japan attended a 6-year common primary school from ages 6 to 12 years under the National School Order enacted in 1941. After completing the sixth grade, post-primary education was not compulsory; students either opted out of the formal education system or continued their schooling. There were three tracks of post-primary education: general, academic, and vocational. Students who wished to continue their education attended a 2-year general secondary school, as long as they could afford the tuition. Alternatively, students who passed the exams could attend either an academic or a vocational track school, both of which were classified as "high school" (not "secondary school") in the national census.

Primary school fees were abolished in 1900, but post-primary school fees were not. Each municipality set the fee for public schools. For example, in 1946 a general secondary school charged 12 yen per academic year (Yubetsu-cho 1982), a large amount for a poor household when the average annual household income among the non-rich in the prewar period was about

237 yen (Minami et al. 1993).

In 1947, the Japanese government, with the support of the General Headquarters, implemented a school reform that was legally defined in the School Education Law (SEL). Because all laws governing compulsory education were enacted at the national level, the timing of the reform was the same for all regions.

The main changes brought about by the reforms are as follows.

(i) Compulsory full-time education was extended from age 6 years (primary) to 9 years (primary and secondary).[4]

(ii) Early selection of different tracks after the sixth grade was abolished. Under the new system, all students were kept in a common general secondary school until the ninth grade.

(iii) Secondary school fees were abolished.

Two types of compliers were affected by the school reform. First, those who would have stopped at primary school in the absence of the law now completed secondary school under the new education system. Because the completion rate of secondary education was already high before the reform (84%–92%), as shown in Subsection 5.1, room for improvement was limited. Nonetheless, their schooling increased by 3 years. Importantly, these compliers might have come from disadvantaged families who could not afford the fee before the reform. By contrast, in the United Kingdom, the fee had already been abolished when the education reform was introduced in 1947 (Oreopoulos 2006). Second, those who would have stopped at secondary school without the change in the Japanese law now enjoyed an additional year of schooling, from 2 years of secondary school in the old system to 3 years in the new system. To the extent that students attended an extra year of secondary school because of the abolition of school fees, they might have come from disadvantaged families.

The academic year in Japan begins in April, and Article 22 of the SEL requires parents to send their children to elementary school once they turn 6 years old before the following April. Consequently, children born between April and March of the following year form an academic cohort.

As the school reform applied to the cohort that started secondary school in 1947, the cohort

[4] Households whose children evade compulsory education first receive a warning from the government and then a fine of 1,000 yen for failure to comply (Cabinet Office 1947).

born after April 1934 (i.e., 12 years before the reform) was the first academic cohort directly affected by the reform. However, older academic cohorts, especially those born in 1932 and 1933, at the end of the first and second years of the "old" secondary school at the time of the reform, could have been partially affected, because the abolition of fees applied not only to entering students but also to current students. Although they were technically exempt from compulsory education, they might have chosen to continue their secondary education under the new system, probably because the remaining years of secondary education were free. Alternatively, they (or their parents) might have feared being disadvantaged in the future labor market by competing against peers with more years of schooling.

One interpretation of our setting is that the April 1934 cutoff largely reflects the effect of *forcing* individuals who could not afford the fees to attend secondary school[5], while the additional April 1932 and 1933 cutoffs reflect the effect of *voluntarily* attending secondary school by lowering the cost of attending school through fee removal (Lleras-Muney 2022). An increase in secondary schooling among the latter group may indicate that they perceived the marginal returns to attending additional years of secondary school to be greater than the marginal costs following fee removal.

2.2. Post-reform learning environment

One may consider that students could not learn meaningfully in the turbulent environment immediately after WWII, but this does not seem to be supported by much historical evidence. First, the men affected by the reform were too young to fight in the war. Second, the school reform does not seem to have resulted in lower per capita resources owing to overcrowded classrooms, in part because the proportion of students affected by the reform is not particularly large. Appendix Figure A1 plots the student–teacher ratio (panel A) and student–classroom ratio (panel B) in secondary schools from 1943 to 1951. While the reform significantly increased secondary school enrollment, both measures remained approximately the same or improved slightly over time. See Appendix Table D1 for the data sources of the many historical archives we have newly digitized.

[5] As Figure 2 shows, the secondary school completion rate did not reach 100% just above the April 1934 cutoff, suggesting that the April 1934 cutoff partially reflects the effect of voluntary secondary school attendance.

Third, and more directly, Figure 1 shows the relationship between the student–teacher ratio and two measures of wartime destruction[6]—the per capita number of buildings destroyed (panel A) and the per capita number of deaths, including the missing (panel B), at the prefecture level (N=47). There is no strong relationship between the level of destruction and school quality, either before the end of the war (1943) in the first rows or after the war (1949) in the second rows. As a result, the last rows, which show the differences between the two, suggest that prefectures that were more affected by the war did not experience greater changes in school quality than prefectures that were less affected by the war. The slopes with robust standard errors in parentheses for the last rows of panels A and B are -7.10 (27.90) and 26.44 (58.44), with p-values of 0.800 and 0.653, respectively, both of which are far from statistically significant.

Fourth, Appendix Figure A2 shows no systematic relationship between the two measures of wartime destruction and secondary school attendance in 1948 (if anything, the relationship is positive for panel B).[7] Moreover, the attendance rate was quite high, exceeding 90 % in almost all prefectures. Thus, it is unlikely that students worked during school hours and did not attend school. Fifth, Appendix Table A1 compares the curriculum for the seventh grade (first year of secondary school) before and after the reform (Ministry of Education 1941; Ministry of Education, Science and Culture 1980). Not surprisingly, nationalistic and militaristic curriculum content was removed, and more meaningful subjects, such as reading, science, and foreign languages, were added. By contrast, Clark (2023) shows that the vast majority (over 95%) of students affected by the 1947 British reform attended lower-track schools, whose curriculum emphasized practical education at the expense of vocational or traditional academic education.[8]

Finally, as shown in Subsection 5.5, we document some positive returns, albeit of a small magnitude, of education in the labor market. Nearly 90% of the cohorts just before the reform were already enrolled in secondary school, when it was not compulsory and required a fee, suggesting that students (or their parents) perceived some potential return from secondary education even then. Taken individually, each piece of evidence is not sufficient, but taken together, we do not have strong reasons to believe that students were not learning meaningfully

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[6] We obtained a very similar relationship between class size and two measures of wartime destruction (not shown). These two measures are the same variables used in Davis and Weinstein (2002). We thank David Weinstein for providing the data.

[7] The slopes with robust standard errors in parentheses for panels A and B are 6.98 (10.45) and 33.52 (15.53), with p-values of 0.508 and 0.036, respectively.

[8] Japan's 1947 school reform abolished tracking after primary school.

in part because of the unique circumstances of WWII.

3. Data

Our primary sample consists of individuals born in Japan between April 1929 and March 1936 (96 birth year-month cohorts), including the 3 academic years before and after the affected cohorts (1932–1934).

3.1. Education

To estimate the impact of school reform on the level of completed education, we used the 100% 2020 Japanese census, which has become available recently. Notably, this census is the first in the history of Japanese censuses to distinguish between primary and secondary schools in the categories for the highest level of completed education. Until the 2010 census, the lowest category was "primary/secondary school," which combined primary and secondary schools. As a result, to the best of our knowledge, this study is the first to document the impact of the 1947 school reform not only on educational attainment ("first stage") but also on any reduced-form outcomes, such as labor and health outcomes.

With more than 50,000 observations in each birth year-month cohort, for a total of 4.8 million observations in the sample period, we have substantial power to implement an RD design. The obvious drawback is that the affected cohorts, born between 1932 and 1934, were already 86–88 years old at the time of the census. To the extent that individuals who were induced to obtain more schooling came from disadvantaged families, they might have died early and thus, would not have been included in the census. This may attenuate the estimate of educational attainment.

Specifically, the 2020 census asked about the highest level of education completed in the following categories: primary school, secondary school, high school, junior college, college, and graduate education. We created a dummy variable for each level of education completed or higher. We also assigned a number of years of schooling to each level of education. Because the census does not distinguish between the old and new education systems, we used the 1985 and 1995 pooled Social Stratification and Social Mobility Survey, which separately records old 2-year and new 3-year secondary schools, along with birth year (not birth year/month) information, to impute years of schooling for those who completed secondary school.[9]

Appendix Table A2 shows the imputed years of schooling for each academic cohort. It confirms that some fraction of the 1932 and 1933 academic cohorts, who were not subject to compulsory secondary education, completed secondary education under the new system, implying that we should expect discontinuities in the probability of completing secondary education at the April 1932 and 1933 cutoffs, in addition to the April 1934 cutoff.

3.2. Mortality

We used vital statistics for 1972–2021 to estimate the impact of the school reform on mortality. This administrative dataset, collected by the Ministry of Health, Labour and Welfare, records all deaths in Japan, including the exact date of death and the year-month of birth of the deceased. January 1972 is the earliest year-month for which individual-level death records are available, and December 2021 is the latest available at the time of writing. Vital statistics include deaths of Japanese nationals that occurred outside Japan. Therefore, migration bias is not a serious concern in our setting.

Because the 2000 census is the first to report birth year-month (not birth year-quarter), our running variable in RD, we used the 2000 census, which captures the population as of October 2000, as our starting point. We subtracted the cumulative deaths between January 1972 and October 2000 to construct the at-risk population for January 1972.[10] Our main outcome is the mortality rate between 1972 and 2021, where the total number of deaths between January 1972 and December 2021 is divided by the population in January 1972 for each birth year-month cohort.[11]

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[9] See Appendix Table D2 for details on the dataset. We do not know whether those who completed high school or higher attended either the old 2-year or the new 3-year secondary schools before moving on to high school. To be conservative, we uniformly assigned 12, 14, 16, and 18 years to those who completed high school, junior college, college, and graduate education, respectively, because the post-secondary education system in Japan after the reform adopted 3 years of high school followed by either 2 years of junior college or 4 years of college and 2 additional years of a master’s degree. To the extent that recent cohorts who graduated from high school or above attended a new secondary school, our estimate of years of schooling represents the lower bound.

[10] A similar approach to calculating mortality is taken by Clark and Royer (2013) for the United Kingdom and Malamud et al. (2023) for Romania.

[11] Following Lleras-Muney (2005), we calculated the cohort mortality rate by the change in cohort size between the 2020 census and the 2000 census, the first census with birth month information available, and obtained similar null results (not shown).

In this way, we followed each cohort for 50 years (ages 37 to 87 years) for the 1932–1934 birth cohorts. Approximately 59%–67% died between 1972 and 2021, conditional on surviving to 1972, providing a nearly complete picture of the effect of education on mortality in later life. For reference, the life expectancy conditional on surviving to age 40 years for those born in 1935 was 34.41 years (a total of 74.41 years) and 38.76 years (a total of 78.76 years) for men and women, respectively.[12]

One limitation is that individual-level mortality data were available only after 1972. Here, we tested for selective mortality up to 1972 using birth counts, the 2000 census, and deaths recorded between 1972 and 2000. Appendix Figure C1 shows no suggestive evidence of selective mortality at any of the relevant school entry cutoffs in the month of April.

Vital statistics provide detailed information on the leading causes of death (ICD9 until 1994 and ICD10 after 1995), allowing us to examine the cause-specific mortality associated with health behaviors. We examined two leading causes of death, cancer and circulatory diseases, which accounted for 33.7% and 24.3% of all deaths, respectively, in the 1932–1934 birth cohorts. In addition, we classified certain causes of death as preventable or treatable based on epidemiological literature, following Meghir et al. (2018) and Malamud et al. (2023). Preventable causes of death (e.g., accidents) should reflect risk-taking behavior and investment in health, whereas treatable causes of death (e.g., asthma) could be related to access to health care.

3.3. Healthcare utilization

As mortality is the ultimate outcome, we also estimated the impact of the school reform on healthcare utilization, which might have affected quality of life but not necessarily its length. These outcomes have received less attention in the literature (Behrman et al. 2011; Yue et al. forthcoming) than later-life mortality studies. We used the Patient Survey for 1984–2017 collected by the Ministry of Health, Labour and Welfare. This dataset is a nationally representative repeated cross-sectional survey that collects administrative data from hospitals and clinics. Because the survey is conducted every 3 years, it has individual patient-level data for 12

[12] The life expectancy *at birth* for the same cohort is only 46.92 (men) and 46.54 (women). However, this figure is close to 48.6 (men) and 52.0 (women) for the US-born white in 1910, as studied in Lleras-Muney (2005).

survey rounds between 1984 and 2017.

The Patient Survey consists of two types of data: outpatient data and discharge data. We used the former to examine outpatient visits and the latter to examine inpatient admissions. Discharge data include ICD codes, allowing us to examine cause-specific inpatient admissions. The dataset is also used by Shigeoka (2014). See Appendix Table D2 for details on the dataset.

4. Empirical strategy

We adopt two empirical methods: 1) RD with a linear spline to identify the effect on each academic cohort separately and 2) Dif-in-RD to identify the average effects of all affected cohorts. Throughout the study, we collapse the outcomes at the birth year-month level (denoted by b), as this is the level of our analysis in both designs.

4.1. Regression discontinuity design

Let Y_b be the outcome of interest for birth year-month cohort b. We run the following regression:

$$Y_b = \sum_{k=1932}^{1933,1934} \beta_k \mathbf{1}\{R_b \geq c_k\} + \gamma_0 R_b + \sum_{k=1932}^{1933,1934} \gamma_k (R_b - c_k)\mathbf{1}\{R_b \geq c_k\} + \delta_m + \varepsilon_b, [1]$$

where R_b is the birth year-month cohorts, our running variable, which takes the value 1 in April 1929, 2 in May 1929, and 96 in March 1936.[13] c_k (k = 1932, 1933, and 1934) are the April cutoffs for 1932, 1933, and 1934, respectively. $\mathbf{1}\{R_b \geq c_k\}$, the first term in [1], takes one for cohorts born after each school entry cutoff, so each β_k—our coefficient of interest—captures the *additional* effect of passing each April cutoff.

We allow each linear spline of running variable (R_b and $(R_b - c_k)\mathbf{1}\{R_i \geq c_k\}$, the second and third terms in [1]) to differ before the reform, for each of the three affected academic cohorts, and after the reform to capture the underlying relationship between the birth cohort and outcomes. δ_m is the calendar birth-month FEs to address the relative age effects, as children born just after the school entry cutoff are the oldest within their respective grades (e.g., Bedard and Dhuey 2006; Black et al. 2011; Cascio and Schanzenbach 2016), and seasonality of births

[13] Our results are quantitatively similar when we include two or four cohorts (instead of three) before and after the school reform (not shown).

(Buckles and Hungerman 2013; Shigeoka 2015) can have an independent effect on mortality. Here, we further interact each birth-month FE with a dummy for cohorts born after the school reform to allow for different patterns of seasonality before and after the reform (Clark and Royer 2013).

We estimate equation [1] by ordinary least squares using the number of observations in each cell (i.e., cohort size) as the weights. Heteroskedastic-robust standard errors, unless specified otherwise, are reported.

4.2. Difference-in-regression discontinuity design

While we account for relative age effects and seasonality of births by including birth-month FEs in equation [1], we can also explicitly address them by using the academic cohorts born before and after the reform, as a comparison group ("control cohorts" hereafter). This can be done by estimating an analogous regression model to equation [1] for the discontinuities of being born just after the cutoff in the control cohorts and then subtracting these discontinuities of the control cohorts from those of the treatment cohorts (Malamud et al. 2023).

We first organize each birth year-month cohort in relation to the month of April to form "synthetic" cohorts (i.e., from October of the previous year to September). Let Q_b be the *normalized* birth year-month cohorts in the range of -6 to 5, taking 0 in April, 1 in May, and 5 in September, and -1 in March, -2 in February, and -6 in the previous October. We then estimate the following "difference-in-discontinuities" model:

$$Y_b = \beta_0 + \gamma 1(Q_b \geq April) + \delta\, Treat_b + \theta\, Treat_b * 1(Q_b \geq April) + f(Q_b) + \varepsilon_i, [2]$$

where $1(Q_b \geq April)$ is an indicator of individuals born after April. $Treat_b$ is an indicator equaling 1 for treated cohorts born during the 1932–1934 "synthetic" cohorts and 0 otherwise.[14] $f(Q_b)$ now includes the interactions of our normalized running variable with both $1(Q_b \geq April)$ and $Treat_b$, allowing for different relationships between the outcome and birth year-month both before and after the school entry cutoff and in the treatment and control cohorts. In this way, we flexibly control for an increasing trend in schooling and lower mortality for younger

[14] $Treat_b$ takes 1 for three treated "synthetic" cohorts, namely, those born between October 1932 and September 1933, October 1933 and September 1934, and October 1934 and September 1935, and 0 otherwise. In practice, the $Treat_b$ dummy is replaced by each "synthetic" cohort dummy to be more flexible.

cohorts. Our coefficient of interest is the interaction term, θ, which estimates the impact of being born just after the cutoff among treatment cohorts over and above the effects in control cohorts before and after the school reform, which is captured by γ.[15]

The advantage of this approach is that it directly addresses relative age effects and provides the average effect for all three affected cohorts. The disadvantage is that, by construction, the running variable can take only 6 months on either side of the cutoffs. To obtain balanced data around April, we remove 6 months from both ends of the sample used in equation [1], which spans October 1929 to September 1935 (or 84 birth year-month cohorts). The underlying assumptions for this empirical strategy are that (i) relative age effects and seasonality are stable across the treatment and control cohorts and (ii) school cohort-specific shocks, other than the school reform, are balanced across the treatment and control cohorts (Grembi et al. 2016). We use this approach to complement the RD specification.

We do not report the two-stage least squares estimates, because the mortality data do not include education, and there may be changes in school quality that are not captured by our imputed measure of schooling years. Instead, following Clark and Royer (2013), Meghir et al. (2018), and Malamud et al. (2023), we report the reduced-form estimates of school reform on mortality rather than the split-sample instrumental variable estimates.

4.3. Balance tests

The key to our identification is that birth cohorts born before and after the relevant school entry cutoffs are comparable, except for changes in schooling. Panels A–C of Appendix Figure A3 plot the predetermined characteristics at birth: cohort size (i.e., the density test by McCrary 2008), percentage of stillbirths, and sex ratio at birth. Except for strong seasonality, none of the three outcomes changes suddenly at each cutoff. Appendix Table A3 shows that, once we control for calendar birth-month FEs, most estimates at the cutoffs are neither statistically significant nor economically large.

[15] Our results are robust to using the data only *after* the school reform as the control cohorts (not shown). We choose the current approach to keep the samples as similar as possible for estimating equations [1] and [2].

5. Results

5.1. Effects on educational attainment

We begin by presenting graphical evidence of the first stage in Figure 2. Panel A plots the proportion of individuals who completed secondary school or higher between April 1929 and March 1937. We add vertical lines for April to identify each educational cohort. Each point represents the average outcome for each year-month of birth.

The left graph of panel A in Figure 2 shows that the proportion of individuals with secondary education or higher increased discontinuously in the April of 1932, 1933, and 1934, with solid vertical lines. Not only were the cohorts born after April 1934 directly affected by the reform, but the cohorts born after April 1932 and 1933, which were at the end of the first and second years of secondary school at the time of the reform, were partially affected. These findings mitigate concerns that our compliers might have perceived low returns to completing secondary school but were forced to remain in school. Although these cohorts were technically exempt from compulsory schooling, they still chose to continue in secondary school, suggesting that the compliers perceived that the marginal benefit of attending additional years of secondary school outweighed its marginal cost after the fee was removed.

In addition, the secondary school completion rate did not reach 100% immediately after the reform, posing the challenge of achieving universal coverage in a short period of time. To account for seasonality, the right-hand figure of panel A plots the outcome "residualized" by calendar birth-month FEs. It is reassuring to observe that the results are similar. In particular, we do not observe similar discontinuities in April of other years used as control cohorts in Dif-in-RD.

Panel B of Figure 2 plots years of schooling as the outcome. The left graph shows that while seasonality is observed every April[16], the discontinuities at the relevant cutoffs are larger than those of the surrounding cutoffs. The right graph, which partials out the birth-month FEs, clearly shows large discontinuous jumps at the three relevant cutoffs, while the discontinuous jumps at the other cutoffs are mostly muted.

Table 1 reports a separate estimate at each of the three cutoffs from equation [1] in panel A and a pooled estimate from equation [2] in panel B. Column (1) of panel A shows that the share

[16]Similar discontinuities in years of schooling at each school entry cutoff, likely reflecting relative age effects, are observed in other settings, such as Sweden (panel B of Figure 2 in Meghir et al. 2018).

of those who completed secondary school or higher increased by 2.8, 2.5 and 3.4 percentage points (p-value<0.01) at each cutoff in 1932, 1933, and 1934, from the means of 0.84, 0.88, and 0.92, respectively. In columns (2) and (3), we find little spillover effect on higher education than on secondary education, probably because fees for higher education were not eliminated by the reform, except for a 0.8 percentage point increase in college or above for the 1934 cutoff, which is the cohort directly affected by the reform.

Column (4) of Table 1 reports that years of schooling increased by 0.12, 0.21, and 0.18 at the 1932, 1933, and 1934 cutoffs, respectively. As mentioned in Section 2, such increased schooling years reflect both 1) the higher completion rate of secondary school among those who would have stopped at primary school and 2) a 1-year extension of secondary schooling for those who would have stopped at secondary school without the law change. We compute the contribution of the former by multiplying the estimates of secondary school or higher in column (1) by 3 years (as little spillover is observed) and dividing it by the estimate of years of schooling in column (4). The former accounted for 71.8%, 35.9%, and 57.3% of the increase in schooling years at the 1932, 1933, and 1934 cutoffs, respectively.

Panel B of Table 1 reports the Dif-in-RD estimates, where we also report γ, which captures the relative age effects for the control cohorts, along with θ, our coefficient of interest for the interaction term in equation [2]. Column (1) shows that there is a 2.6 percentage point increase in the proportion with a secondary education or higher, which translates into an average of 0.11 additional years of schooling in column (4). Appendix Figure B1 visually illustrates these relationships by plotting the outcomes for the treatment cohorts (first row), control cohorts (second row), and their differences (third row) around April, normalized to 0. While we observe a sharp increase in both secondary education or higher and years of schooling at the cutoff for the treatment cohorts, there are no similar jumps for the control cohorts, resulting in positive Dif-in-RD estimates.

We compare the magnitude of educational attainment with the previous literature, which also uses changes in compulsory schooling in the RD framework. Our estimates of 0.12–0.21 additional years of schooling are much smaller than those of Clark and Royer (2013) of 0.45 and 0.35 years for the 1947 and 1972 reforms in the United Kingdom, smaller or close to those of Meghir et al. (2018) of 0.25 years in Sweden, but larger than those of Malamud et al. (2023) of 0.12 years in Romania. The reform in this study affected a smaller fraction of students but their schooling increased by 3 years. Other reforms, notably the British reform of 1947, occurred in the same years as our reform and extended schooling by 1 year for a large proportion of cohorts, making them average population effects. However, the affected cohorts might have included those who perceived low monetary and health returns to more schooling (Lleras-Muney 2022), and those who associated the reform with a deterioration in school quality, that is, large classes, and relatively inexperienced teachers (Clark 2023).

Appendix Figure B2 and Table B1 show the heterogeneity by sex. We find similar impact sizes on the completion rate of secondary education or higher for men and women.

5.2. Compliers' characteristics

Census data do not allow us to examine the socioeconomics of compliers, as all relevant outcomes are post-determined. We find that the Statistical Yearbook of Aomori prefecture, located in the northern part of Japan's main island of Honshu, is rich enough to provide some indication of the characteristics of compliers.[17] See Appendix Table D1 for the sources of all historical data mentioned in this subsection.

Figure 3 shows the relationship between the transition rate to secondary education and the share of agricultural households among all households at the municipal level in Aomori (N=11). The transition rate, on the y-axis, is the proportion of new entrants to the first year of secondary education (grade seven) in a given year divided by the number of students enrolled in the last year of primary education (grade six) in the previous year. The share of agricultural households on the x-axis is used as a proxy for the economic wealth of municipalities, as agricultural households are on average poorer than non-agricultural households (Minami 2008).[18]

Panel A of Figure 3 for the pre-reform period (year 1943) shows that while the transition rate is close to 1 (or 100%) in the more prosperous municipalities (i.e. with a lower share of agricultural households), the corresponding rate is much lower and far from one in the less prosperous municipalities, with a slope of -0.288 (p-value of 0.001). However, after the reform (year 1949) in panel B, the transition rate is close to 1 in all municipalities and thus, the slope is

[17] Specifically, the following three sets of information were available at the *municipal* level: (i) the number of enrollments in each grade at each municipal level for both before and after the reform, which allowed us to calculate the transition rate to secondary school; (ii) information on school quality (student–teacher ratio and student–classroom ratio; and (iii) the number of agricultural households and total households.
[18] For example, the ratio of agricultural to non-agricultural per capita income was 0.37 in 1935 (Minami 2008).

close to zero (0.027) and far from statistically significant (p-value of 0.715).

As a result, in panel C of Figure 3, which takes the change between the pre- and post-reform years, most of the increase in secondary enrollment comes from the less affluent municipalities. The slope of 0.314 (p-value<0.0001) suggests that a 10% increase in the share of agricultural households leads to a 3.14% increase in the transition rate from primary to secondary. Although the evidence is limited to one prefecture, the result suggests that, owing to the elimination of fees for secondary education in the 1947 school reform, those who complied were more likely to come from low-socioeconomic status (SES) households.

Moreover, at the national level, collaborative evidence from another historical archive shows that those who did not attend secondary school before the reform were indeed poor on average. Appendix Figure A4 plots household wealth by secondary school enrollment status in 1938. Clearly, those who did not enroll in secondary school came from poorer households than those who did enroll before the reform.

Finally, one remaining concern is that even though enrollment in secondary schools increased disproportionately among low SES students after the reform, they might not have learned much, because their schools were of low quality. Again, using data for Aomori prefecture, Figure A5 shows the relationship between school quality and the share of agricultural households at the municipal level. If anything, school quality (proxied by the student–teacher ratio and student–classroom ratio) was higher in less economically prosperous municipalities, mitigating such a concern.[19]

5.3. Effects on mortality

We now examine the impact of school reform on mortality, which is our main outcome. Panel A of Figure 4 plots the all-cause mortality rate between 1972 and 2021 for the corresponding cohorts. The negative slope reflects a younger population along the horizontal axis. However, we do not find large or significant breaks in this trend at the relevant cutoffs, providing visual evidence that the reform did not affect mortality. Appendix Figure C2 plots the all-cause mortality rate for the treatment cohorts, control cohorts, and their differences around April in the spirit of Dif-in-RD; no obvious discontinuities are observed at the cutoffs for any of

[19] The slopes with robust standard errors in parentheses for panels A and B are -12.09 (4.66) and -13.53 (5.48), with p-values of 0.029 and 0.036, respectively, which are both statistically significant at the 5% level.

the three plots. Some recent work suggests that the effects of education on health and mortality may vary substantially over the life cycle (e.g., Kaestner et al. 2020; Lleras-Muney and Moreau 2022). Panel B of Figure 4 plots the mortality by age groups (ages 50–59, 60–69, and 70–79 years), but we observe no discernible patterns for any of the groups.

Table 2 reports the estimates from RD in panel A and Dif-in-RD in panel B. Column (1) for all-cause mortality shows that the mortality estimates are far from statistically significant. The estimates for the 1932, 1933, and 1934 cutoffs are −0.0025, 0.0002, and 0.0005, respectively, from the means of 0.672, 0.628, and 0.585, respectively. While the natural question is about how much statistical power we have to detect the mortality effect, our reduced-form mortality estimate is precisely close to 0. For example, the lower bound of the 95% CI of the 1932 cutoff estimate, the largest of all three estimates, is −0.74 percentage points, which is only a 1.1% reduction from the average mortality rate of 67.2% between 1972 and 2021.[20]

Cause-specific mortality.— Figure 5 shows the mortality rates for cancer and circulatory diseases in panels A and B, followed by preventable or treatable causes of death in panels C and D. Again, there are no obvious jumps at the relevant cutoffs, and columns (2)–(5) in Table 2 confirms these patterns.

Heterogeneity.— The health returns to education can be heterogeneous within the same study (Barcellos et al. 2023; Lleras-Muney et al. 2022). Appendix Figure C3 and Table C3 examine heterogeneity by sex, as men and women are expected to follow different underlying health processes as they age. We find no effects on total mortality for either sex.

5.4. Effects on healthcare utilization

We next examine the effect of school reform on healthcare utilization, which may affect quality of life, but not necessarily length of life. Figure 6 plots the log rate of outpatient visits (panel A) and inpatient admissions (panel B). We observe no meaningful breaks in either outcome at any of the school entry cutoffs. Panels C–E plot the cause-specific inpatient admission rate by cancer, circulatory disease, and respiratory disease, respectively, but again we see no discernible breaks at any relevant cutoffs. Table 3 confirms the visual patterns.

[20] The estimates on all-cause mortality by age groups are summarized in Appendix Table C2 and confirm the patterns in panel B of Figure 4.

5.5. Effects on labor market outcomes

The primary focus of this study is to examine the effects of school reform on mortality. However, examining whether the expansion affected other relevant outcomes helps us understand the underlying mechanisms between education and mortality.

We examine several labor market outcomes in the 1980 census, the earliest census available at the individual level. The 1932–1934 birth cohorts were 46–48 years old in 1980; thus, they were still in the labor force, as the mandatory retirement age was 60 years. Note that birth is reported only at the quarter level in 1980, and thus, we estimate a variant of equation [1] in which birth year-month, our running variable, is replaced by birth year-quarter.

Table 4 reports the results. Columns (1) and (2) show no change at any of the cutoffs in labor force participation or employment, both of which are already quite high and thus, have little room for improvement. Columns (3)–(5), which examine the choice of industry, show that the affected cohorts shift from the primary to the tertiary sector, suggesting increased access to white-collar jobs. However, the magnitude is relatively small compared to the mean. For example, the share of those working in the tertiary sector increased by 1.2 percentage points at the 1934 cutoff, compared to a mean of 0.50 or 2.4%. Unfortunately, the Japanese census does not collect income or earnings.

In summary, while we find that the affected cohorts appeared to have obtained better jobs, the magnitude of the improvement in labor market outcomes appears to be limited. It is possible that the signaling effect of secondary education was limited, as nearly 90% of the academic peers had already completed secondary education before the reform.

Indeed, the effects of compulsory schooling on labor market outcomes in similar settings are highly mixed. While Oreopoulos (2006) finds that the 1947 British reform raised income, Devereux and Hart (2010) and Clark (2023) find much smaller or no effects. Similarly, Meghir and Palme (2005) find that the 1948 Swedish reform led to a small income increase for men (2%), but no effect for women. In addition, Malamud et al. (2023) find limited returns to education on most labor market outcomes in Romania. Importantly, the effect of education on health may operate through channels other than labor market outcomes, such as better health knowledge or better social networks (Galama et al. 2018), but data limitations prevent us from investigating such outcomes.

6. Conclusion

Whether policy-induced changes in education can improve the longevity of a population is still a widely debated but unresolved issue. We exploit sharp differences in educational attainment across cohorts due to Japan's 1947 school reform, which extended compulsory schooling from 6 to 9 years. The reform's abolition of school fees suggests that the children affected by the reform were from disadvantaged families, and would likely have benefited from schooling. Even in this relatively desirable environment, we fail to find an improvement in later life mortality up to age 87 years for the affected cohorts. This finding suggests that the health returns to schooling were limited, at least at the lower levels of educational attainment.

In terms of external validity, while Japan's high-quality universal health insurance system, introduced in 1961 (Kondo and Shigeoka 2013), has the potential to reduce the education-health gradient, financial resources could still improve health outcomes, because people have to pay for coinsurance. Moreover, the other benefits of education, such as better knowledge and better peers, should still be present.

We should emphasize that this result does not imply that the return to education on mortality is insignificant across the education distribution. Some studies document that the health returns to schooling can be positive (Buckles et al. 2016; Connolly 2021; Fletcher and Noghanibehambari 2021; González et al. 2023). More research is needed to understand whether this heterogeneity arises from the different roles of schooling in improving health, different characteristics of compliers, or a combination of both.

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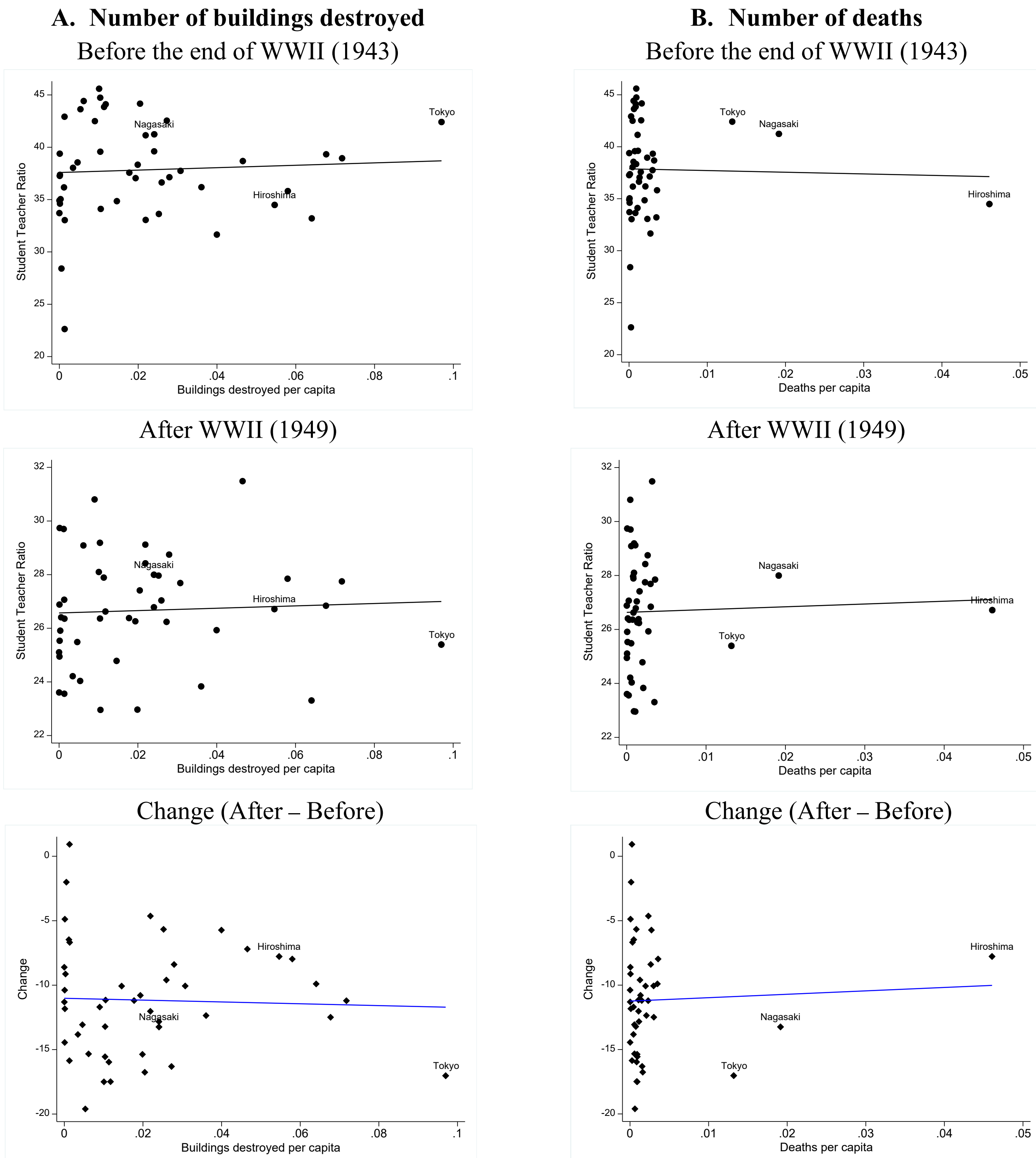
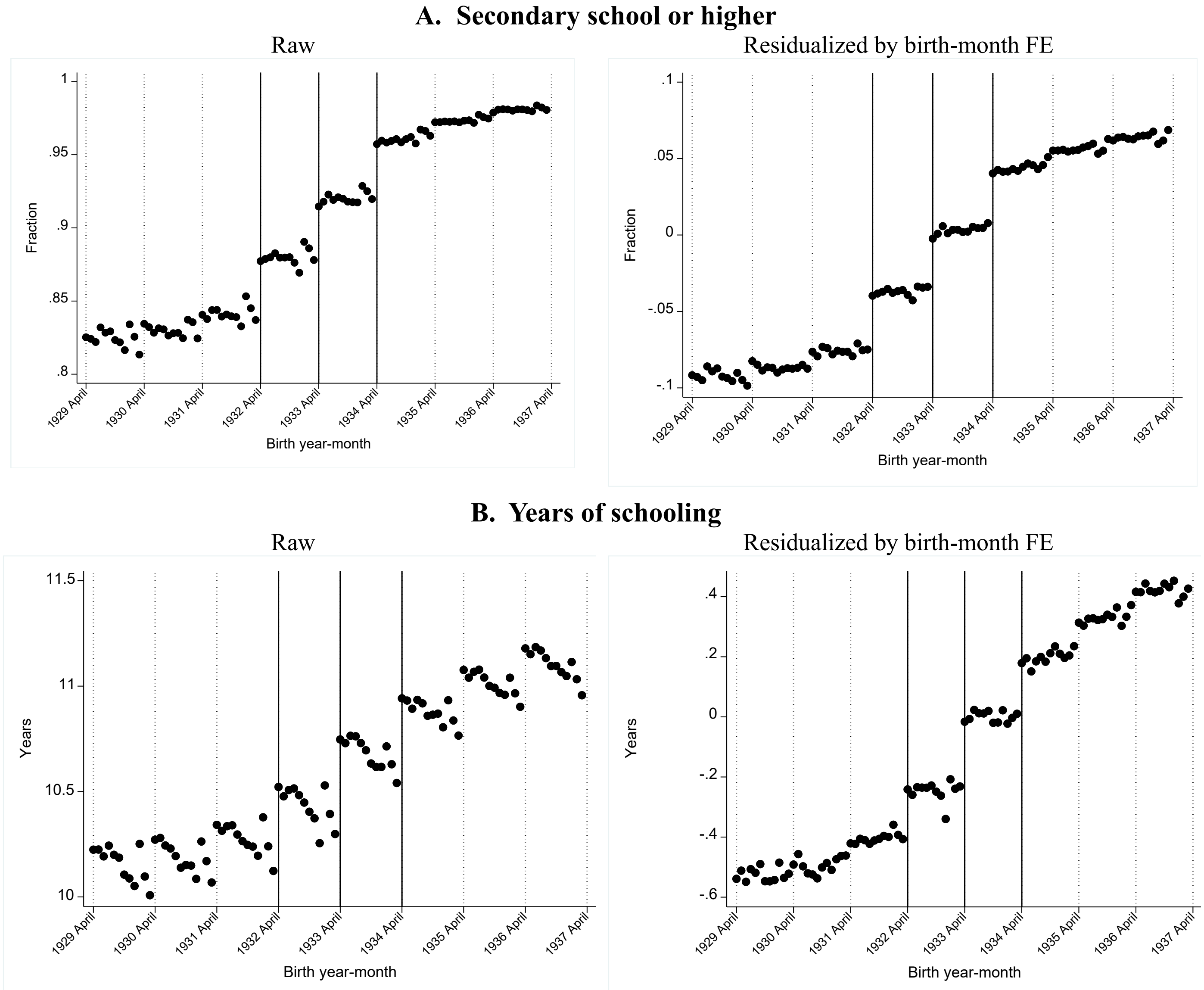
Figure 1—Destruction by WWII and the student–teacher ratio



Notes: The data are from the 1943 and 1949 annual reports of the Ministry of Education. See Appendix Table D1 for data sources. The student–teacher ratio is the number of students divided by the number of teachers in secondary education. The horizontal axis is the per capita number of buildings destroyed by WWII (panel A), and the per capita number of deaths in WWII (panel B), as used in Davis and Weinstein (2002). For panel A, the slopes with robust standard errors in parentheses for the first, second, and third rows are 11.50 (26.83), 4.40 (12.99), and -7.10 (27.90), with p-values of 0.670, 0.736, and 0.800, respectively. Similarly, for panel B, the slopes with robust standard errors in parentheses for the first, second, and third rows are -16.15 (65.05), 10.30 (16.70), and 26.44 (58.44), with p-values of 0.805, 0.541, and 0.653, respectively.

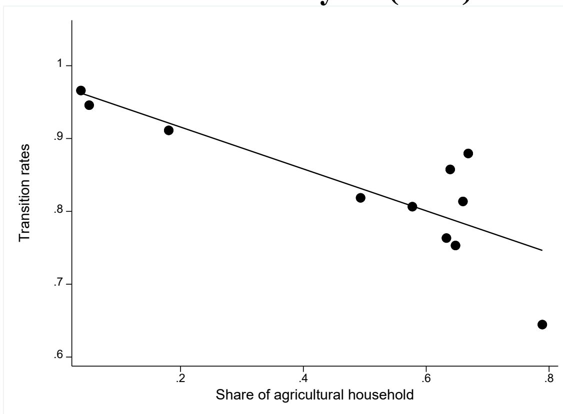
Figure 2—Educational attainment



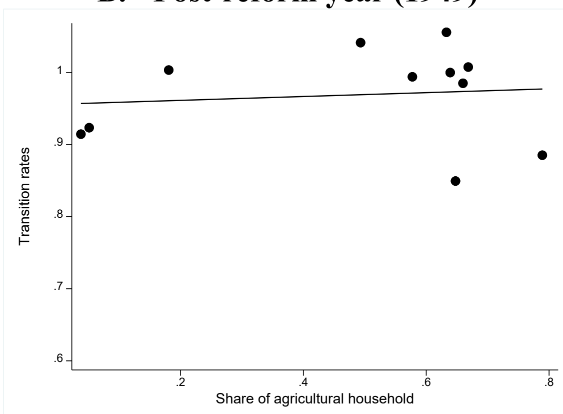
Notes: The data are from the 2020 census. The points represent the means of the outcomes in birth year-month cells from April 1929 to March 1936 (96 months). The outcomes in panels A and B are the proportion with a secondary education or higher and years of schooling, respectively. The figures on the left plot the outcomes from the raw data, and the figures on the right plot the same outcomes residualized by calendar birth-month FEs to account for seasonality. Each vertical line represents the month of April, which defines the academic cohort in Japan (April–March), and the solid lines correspond to April 1932, 1933, or 1934.

Figure 3—The transition rate to secondary school and economic wealth (Aomori prefecture)

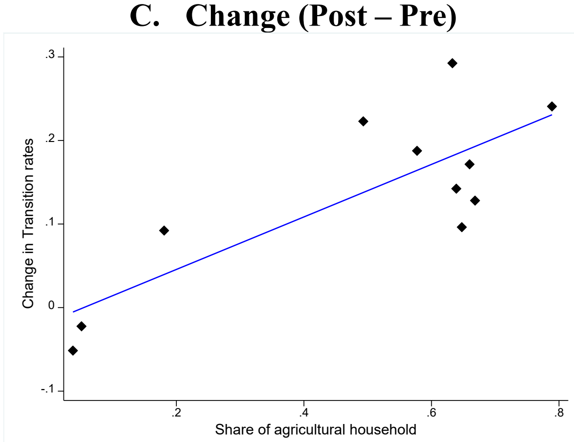
A. Pre-reform year (1943)



B. Post-reform year (1949)



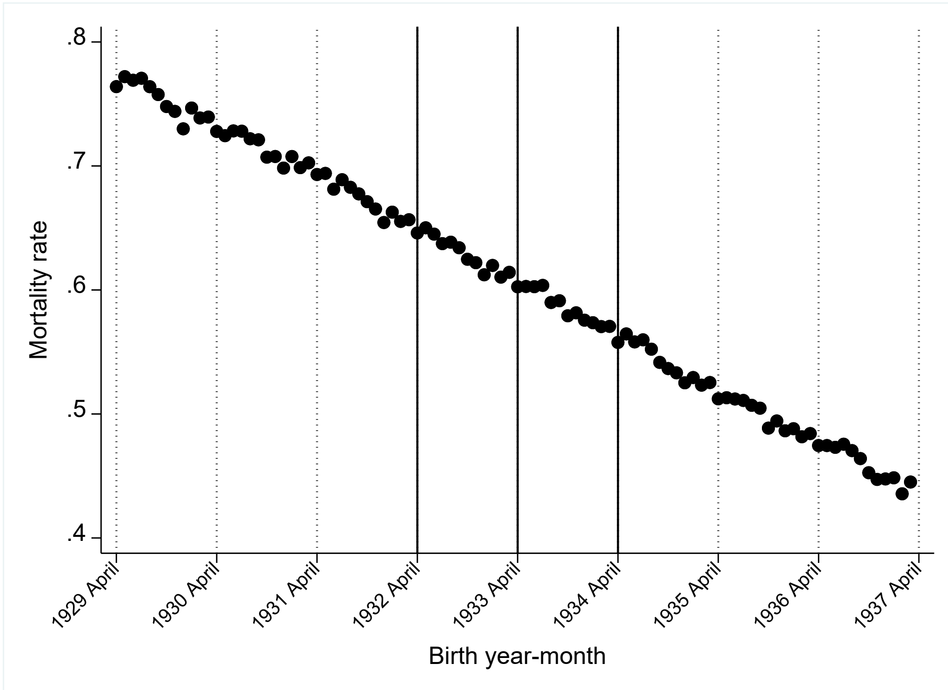
C. Change (Post – Pre)



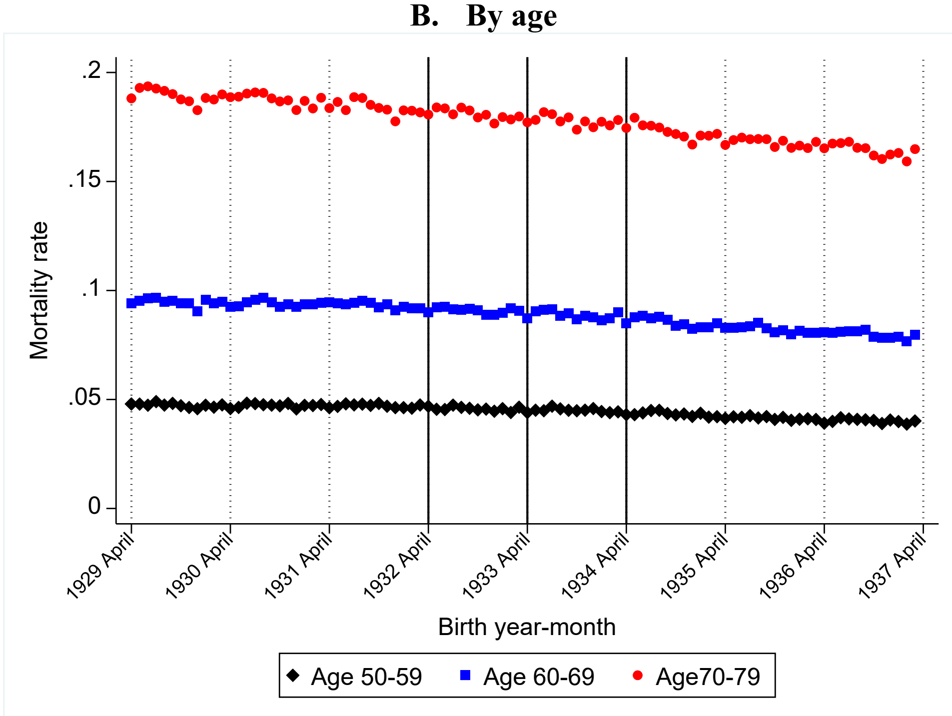
Notes: The data are from the 1943 and 1949 Statistical Yearbook in Aomori prefecture. See Appendix Table D1 for data sources. The outcome is the transition rate to secondary education, which is the proportion of new entrants to the first year of secondary education (grade 7) in a given year divided by the number of students enrolled in the last year of primary education (grade 6) in the previous year. To avoid counting the enrollment from outside of Aomori prefecture, enrollment in private schools (4% of total enrollment) is excluded from the sample. Before the reform year (1947) in panel A, these figures are from *kokumin gakko koto ka* (a general track secondary school). After the reform year (1949) in panel B, they are from *shinsei chugakko* (a secondary school in the new education system). The points represent the means of the outcomes in each municipality in Aomori (N=11). Panel C shows the difference between panels A and B. The horizontal axis is the share of agricultural households among the total number of households, which is used as a proxy for the economic wealth of a municipality. The slopes with robust standard errors in parentheses for panels A–C are -0.288 (0.059), 0.027 (0.071), and 0.314 (0.058), with p-values of 0.001, 0.715, and 0.000, respectively.

Figure 4—Mortality

A. All

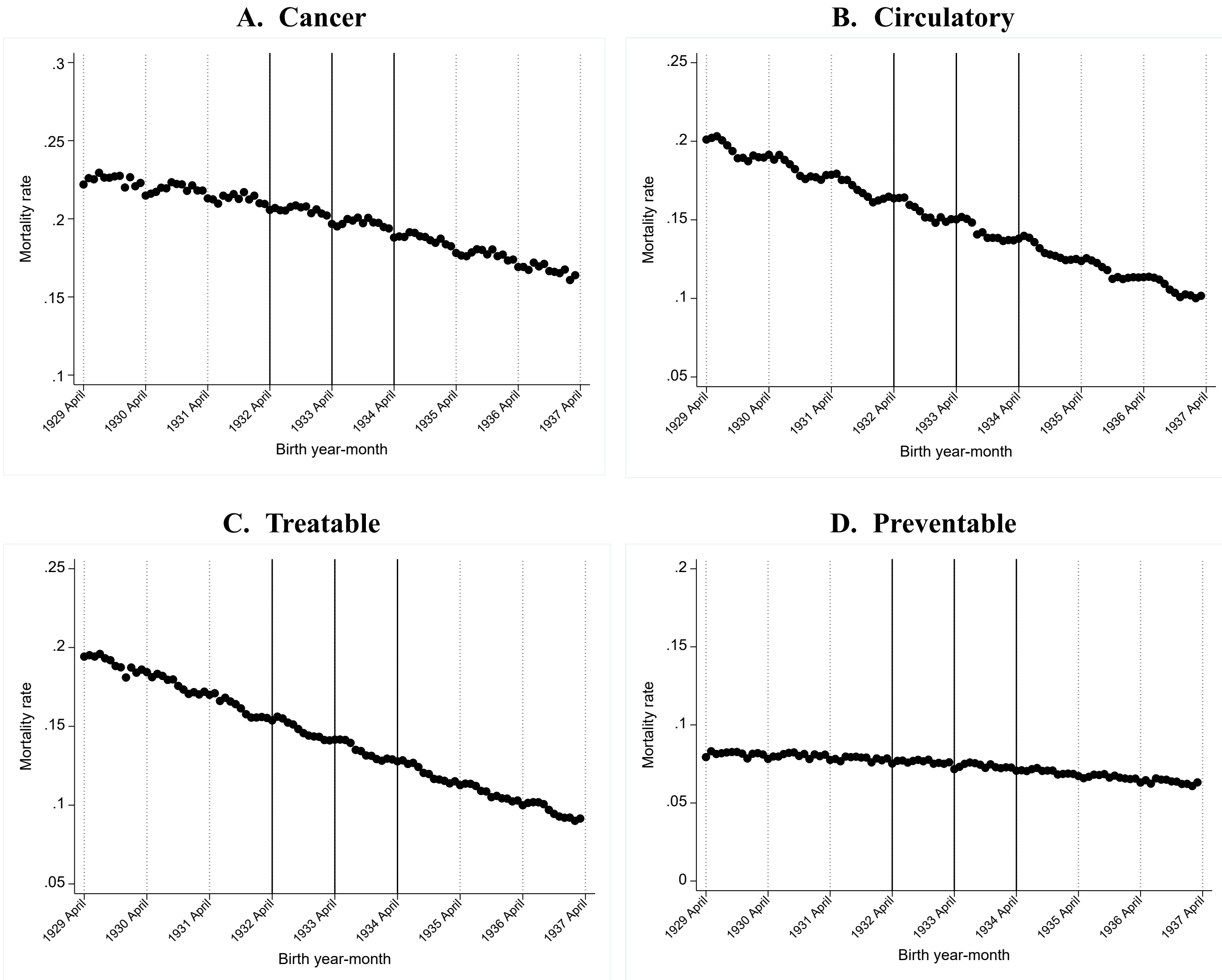


B. By age



Notes: The data are from the 2000 census and 1972–2021 vital statistics. The outcome is the mortality rate between 1972 and 2021. The points represent the means of the outcomes in birth year-month cells from April 1929 to March 1936 (96 months). The vertical lines represent the cohorts born in April 1932, 1933, or 1934.

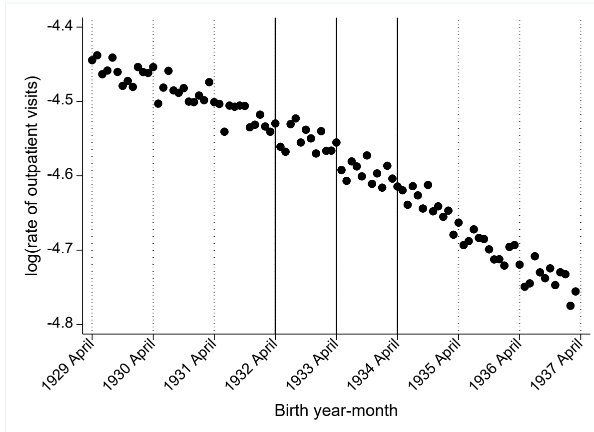
Figure 5—Cause-specific mortality



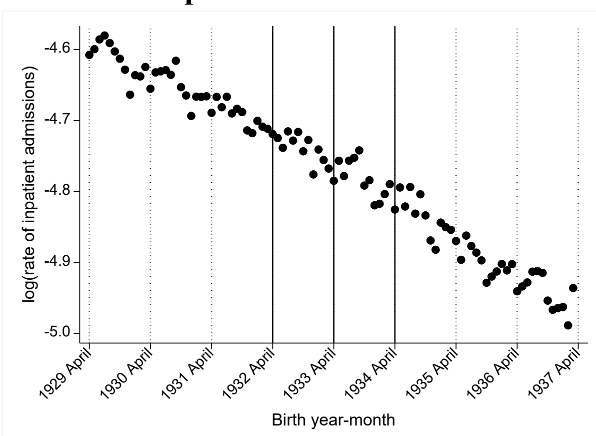
Notes: The data are from the 2000 census and 1972–2021 vital statistics. The outcome is the mortality rate between 1972 and 2021. The points represent the means of outcomes at birth year-month cells from April 1929 to March 1936 (96 months). The vertical lines represent the cohorts born in April 1932, 1933, or 1934. See Table C1 for the list of ICD9 and ICD10 diagnoses for each panel.

Figure 6—Healthcare utilization

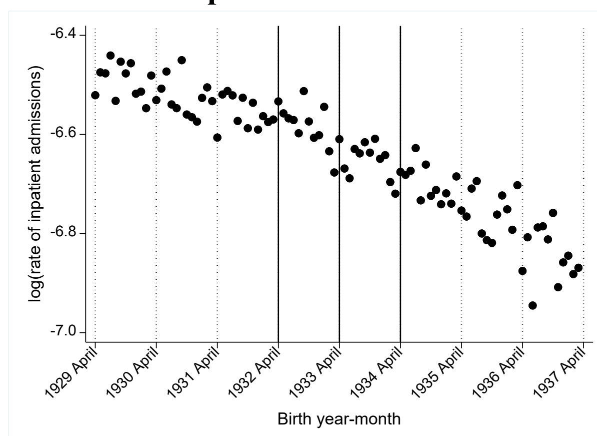
A. Outpatient care



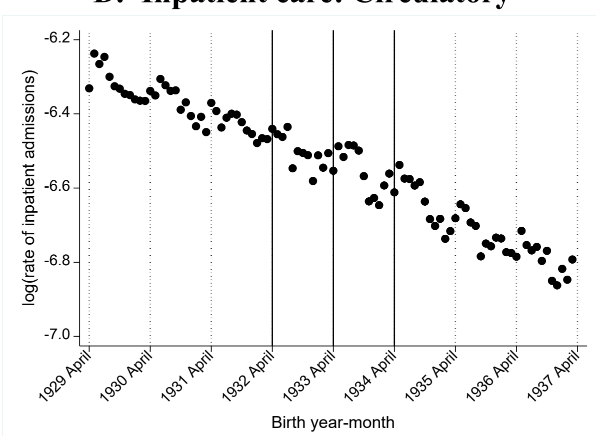
B. Inpatient care: All causes



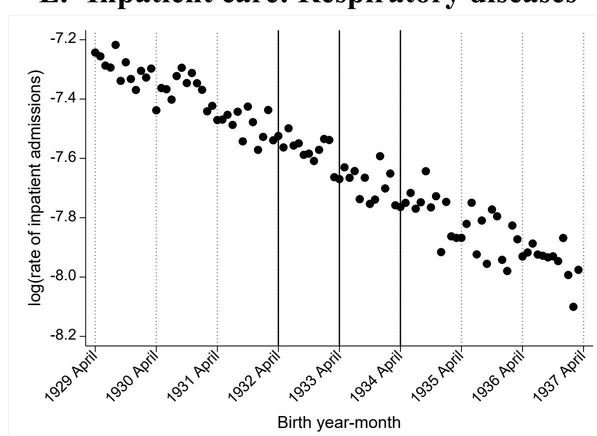
C. Inpatient care: Cancer



D. Inpatient care: Circulatory



E. Inpatient care: Respiratory diseases



Notes: The data are from outpatient data (panel A) and discharge data (panels B–E) in the 1984–2017 Patient Survey conducted every 3 years (12 rounds). See Appendix Table D2 for details of the dataset. The outcome is the log rate of outpatient visits (panel A) and inpatient admissions (panels B–E). The points represent the means of outcomes at birth year-month cells from April 1929 to March 1936 (96 months). The vertical lines represent the cohorts born in April 1932, 1933, or 1934. See Table C1 for the list of ICD9 and ICD10 diagnoses corresponding to panels C–E.

Table 1—Effects on educational attainment

	Secondary or higher	High school or higher	College or higher	Years of schooling
	(1)	(2)	(3)	(4)
A. RD with multiple cutoffs				
1932 cutoff	0.028***	-0.011***	0.001	0.117***
	(0.002)	(0.002)	(0.001)	(0.012)
1933 cutoff	0.025***	-0.004	0.001	0.209***
	(0.002)	(0.003)	(0.002)	(0.019)
1934 cutoff	0.034***	-0.011*	0.008***	0.178***
	(0.003)	(0.006)	(0.002)	(0.035)
Mean below 1932 cutoff	0.842	0.550	0.073	10.270
Mean below 1933 cutoff	0.881	0.539	0.078	10.431
Mean below 1934 cutoff	0.921	0.527	0.080	10.673
No. of birth year-month	96	96	96	96
No. of observations	4,818,576	4,818,576	4,818,576	4,818,576
B. Dif-in-RD (pooled cutoffs)				
1(After April)	0.007***	0.049***	0.015***	0.248***
	(0.002)	(0.006)	(0.002)	(0.029)
1(After April)×Treated	0.026***	-0.020**	-0.001	0.111**
	(0.004)	(0.010)	(0.003)	(0.050)
Mean	0.884	0.529	0.075	10.431
No. of birth year-month	84	84	84	84
No. of observations	4,204,000	4,204,000	4,204,000	4,204,000

Notes: The data are from the 2020 census collapsed at birth year-month cells from April 1929 to March 1936 (96 months) in panel A and from October 1929 to September 1935 (84 months) in panel B. Panel A reports the estimates from equation [1], with robust standard errors in parentheses. The 1932, 1933, and 1934 cutoffs are dummies that take the value 1 if the cohort is above April in each year. Panel B reports the estimates of equation [2], with robust standard errors in parentheses. 1(After April) is an indicator that takes 1 for individuals born after April and 0 otherwise. Treated is an indicator equal to 1 for treated cohorts born between October 1932 and September 1935, and 0 otherwise. See Table A1 for the assignment of years of schooling to each birth cohort in column (4). Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 2—Effects on mortality

	All causes	Cause of deaths			
		Cancer	Circulatory diseases	Treatable	Preventable
	(1)	(2)	(3)	(4)	(5)
A. RD with multiple cutoffs					
1932 cutoff	-0.0025	0.0002	-0.0012	0.0001	-0.0001
	(0.0025)	(0.0011)	(0.0009)	(0.0011)	(0.0006)
1933 cutoff	0.0002	-0.0000	-0.0007	0.0011	0.0001
	(0.0027)	(0.0013)	(0.0013)	(0.0014)	(0.0008)
1934 cutoff	0.0005	-0.0009	0.0015	0.0013	0.0007
	(0.0038)	(0.0014)	(0.0015)	(0.0012)	(0.0009)
Mean below 1932 cutoff	0.672	0.213	0.169	0.161	0.078
Mean below 1933 cutoff	0.628	0.206	0.155	0.147	0.076
Mean below 1934 cutoff	0.585	0.197	0.142	0.134	0.074
No. of birth year-month	96	96	96	96	96
No. of observations	13,237,894	13,237,894	13,237,894	13,237,894	13,237,894
B. Dif-in-RD (pooled cutoffs)					
1(After April)	-0.0071***	-0.0054***	0.0019***	0.0001	-0.0022***
	(0.0016)	(0.0008)	(0.0008)	(0.0008)	(0.0004)
1(After April)×Treated	0.0033	0.0006	0.0013	0.0020*	0.0002
	(0.0026)	(0.0011)	(0.0013)	(0.0011)	(0.0007)
Mean	0.617	0.205	0.152	0.143	0.076
No. of birth year-month	84	84	84	84	84
No. of observations	11,624,394	11,624,394	11,624,394	11,624,394	11,624,394

Notes: The data are from the 2000 census, and 1972–2021 vital statistics collapsed at birth year-month cells from April 1929 to March 1936 (96 months) in panel A, and October 1929 to September 1935 (84 months) in panel B. Panel A reports the estimates from equation [1], with robust standard errors in parentheses. The 1932, 1933, and 1934 cutoffs are dummies that take 1 when the cohort is above April each year. Panel B reports the estimates from equation [2] with robust standard errors in parentheses. 1(After April) is an indicator equaling 1 for individuals born after April and 0 otherwise. Treated is an indicator equaling 1 for treated cohorts born between October 1932 and September 1935 and 0 otherwise. The number of observations reflects the number of survivors up to January 1972, our population at risk. See Table C1 for the list of ICD9 and ICD10 diagnoses for each cause of death. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 3—Effects on healthcare utilization

	Outpatient care	Inpatient care			
		All causes	Cancer	Circulatory diseases	Respiratory diseases
	(1)	(2)	(3)	(4)	(5)
A. RD with multiple cutoffs					
1932 cutoff	-0.004	-0.007	0.040	-0.023	0.021
	(0.012)	(0.011)	(0.038)	(0.024)	(0.039)
1933 cutoff	-0.014	-0.006	0.010	0.001	-0.010
	(0.015)	(0.014)	(0.046)	(0.030)	(0.052)
1934 cutoff	0.009	0.005	0.042	-0.005	-0.040
	(0.018)	(0.016)	(0.046)	(0.034)	(0.063)
No. of birth year-month	1,152	1,152	1,152	1,152	1,152
No. of observations	1,358,720	1,385,572	1,385,572	1,385,572	1,385,572
B. Dif RD (pooled cutoffs)					
1(After April)	-0.018	-0.018	-0.058	0.059	-0.003
	(0.071)	(0.163)	(0.225)	(0.184)	(0.203)
1(After April)×Treated	0.013	0.008	0.084	-0.035	-0.037
	(0.108)	(0.250)	(0.344)	(0.276)	(0.313)
No. of birth year-month	1,008	1,008	1,008	1,008	1,008
No. of observations	1,196,791	1,221,317	1,221,317	1,221,317	1,221,317

Notes: The data are from outpatient data (column 1) and discharge data (columns 2–5) from the 1984–2017 Patient Survey, conducted every 3 years (12 rounds). See Appendix Table D2 for details on the dataset. The outcome is the log rate of outpatient visits (column 1) and inpatient admissions (columns 2–5). The data are collapsed to birth year-month cells for each round of the survey from April 1929 to March 1936 (96 months) in panel A and from October 1929 to September 1935 (84 months) in panel B. Panel A reports estimates from an equation [1] with robust standard errors in parentheses. The 1932, 1933, and 1934 cutoffs are dummies that take the value 1 if the cohort is above April in each year. Panel B reports the estimates from equation [2], with standard errors clustered by birth year-month in parentheses. 1(After April) is an indicator that takes 1 for individuals born after the month of April and 0 otherwise. Treated is an indicator that takes 1 for treated cohorts born between October 1931 and September 1934, and 0 otherwise. See Table C1 for a list of ICD9 and ICD10 diagnoses for each cause of death. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 4—Effects on labor market outcomes

	Labor force participation	Employment	Work in primary sector	Work in secondary sector	Work in tertiary sector
	(1)	(2)	(3)	(4)	(5)
RD with multiple cutoffs					
1932 cutoff	0.001	0.001	-0.001	-0.004*	0.005**
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
1933 cutoff	0.003	0.003	-0.004***	-0.001	0.005
	(0.002)	(0.002)	(0.001)	(0.004)	(0.003)
1934 cutoff	0.002	0.002	-0.008***	-0.004	0.012***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
Mean below 1932 cutoff	0.797	0.785	0.142	0.364	0.493
Mean below 1933 cutoff	0.801	0.790	0.133	0.370	0.497
Mean below 1934 cutoff	0.803	0.791	0.123	0.376	0.500
No. of birth year-quarter	32	32	32	32	32
No. of observations	12,823,297	12,823,297	10,121,690	10,121,690	10,121,690

Notes: The data are from the 1980 census collapsed at birth year-quarter cells from April 1929 to March 1936 (32 quarters). The table reports the estimates of equation [1] with birth year-month replaced by birth year-quarter, with robust standard errors in parentheses. The 1932, 1933, and 1934 cutoffs are dummies that take on the value of 1 if the cohort is above April each year. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Online Appendix
(Not for Publication)

Education and Later-life Mortality:
Evidence from a School Reform in Japan

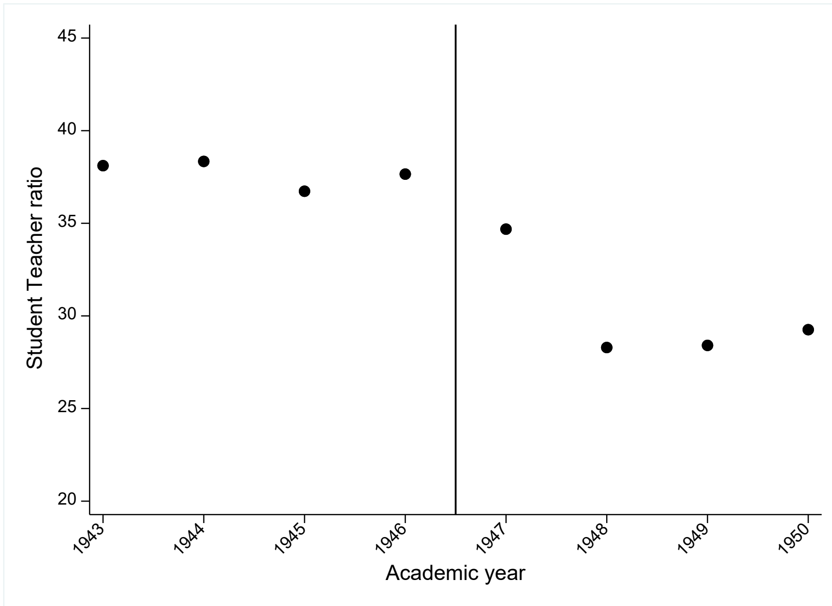
By Kazuya Masuda and Hitoshi Shigeoka

Table of Contents

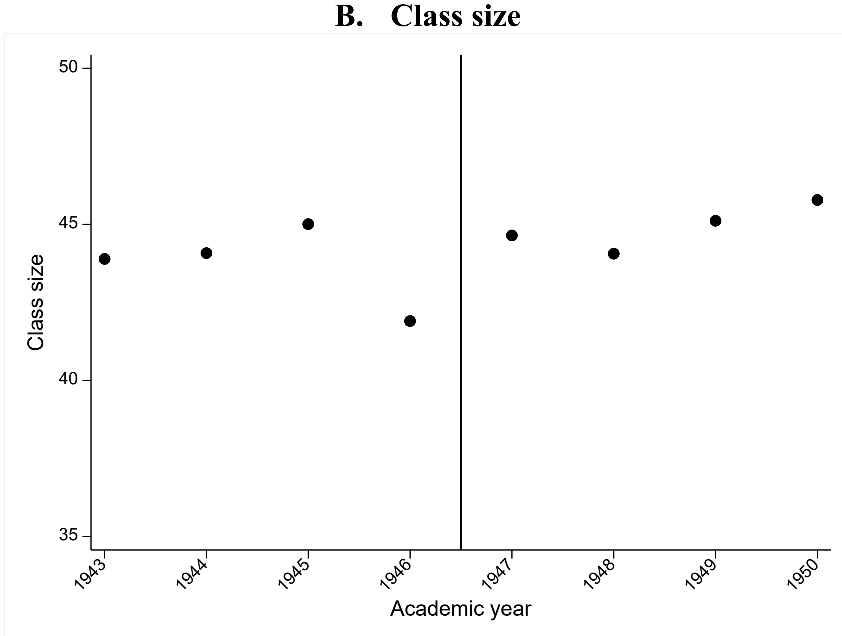
Appendix A: Other figures and tables

Figure A1—Quality of secondary education over time

A. Student–teacher ratio



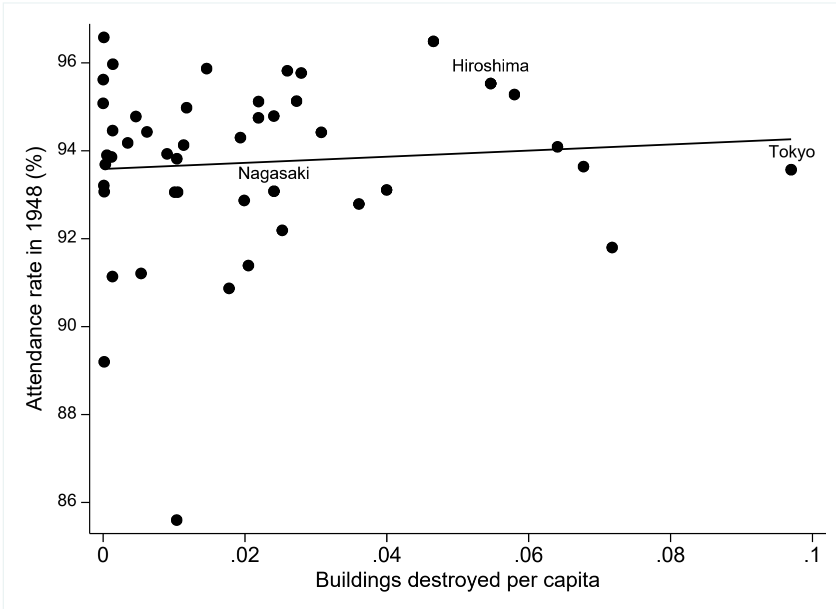
B. Class size



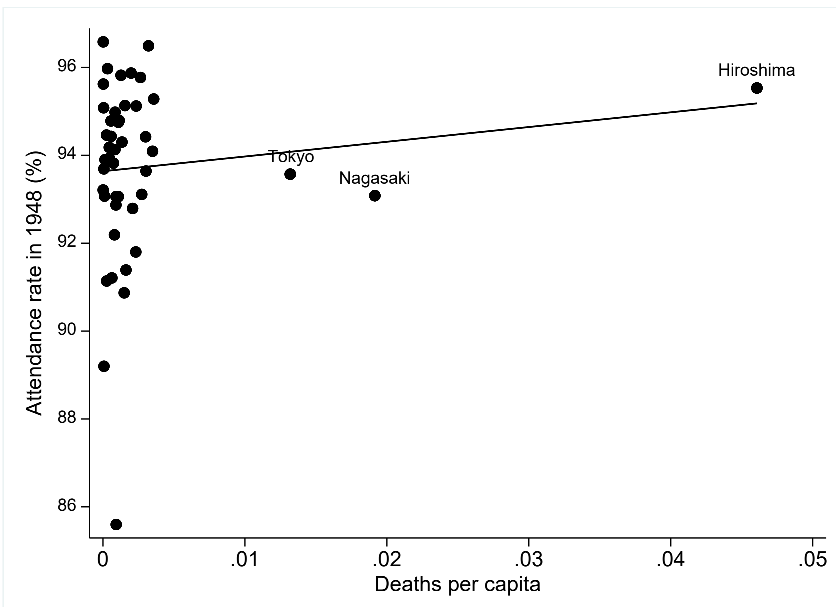
Notes: The data are from the 1943–1950 annual reports of the Ministry of Education. See Appendix Table D1 for data sources. The outcomes in panels A and B are the student–teacher ratio and class size of secondary education, respectively. The student–teacher ratio is the number of students divided by the number of teachers, while class size is the number of students divided by the number of classrooms. Before the reform year of 1947, these figures are from *kokumin gakko koto ka*, a general track secondary school, and after the reform year, they are from *shinsei chugakko,* a secondary school. The solid lines correspond to the reform year 1947.

Figure A2—Destruction by WWII and school attendance

A. Number of buildings destroyed



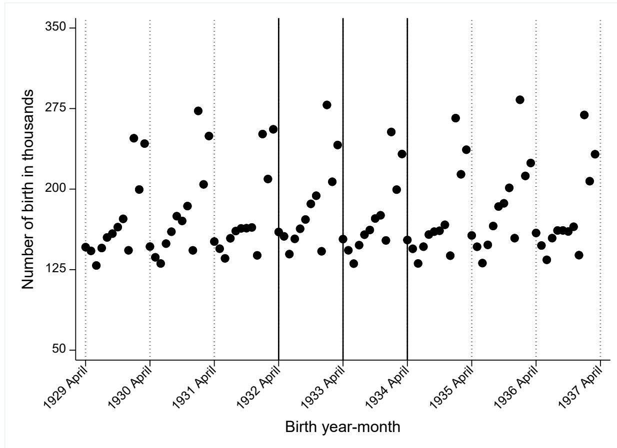
B. Number of deaths



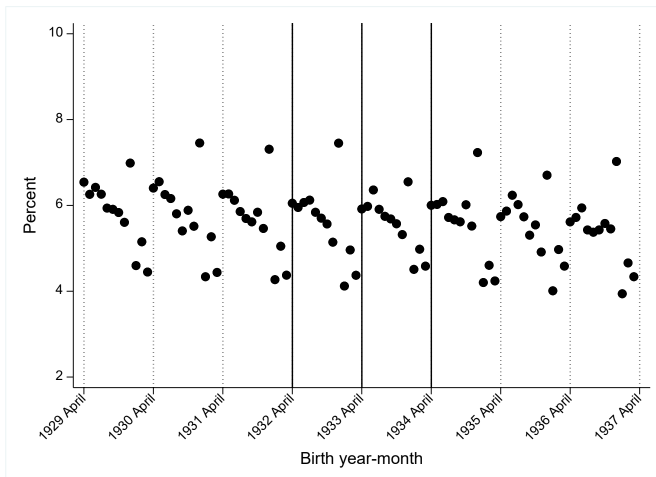
Notes: The data are from the 1948 annual reports of the Ministry of Education. See Appendix Table D1 for data sources. The outcome is the secondary school attendance rate in 1948 (%). The points represent the means of the outcomes in each prefecture. The horizontal axes in panels A and B are the per capita number of buildings destroyed by WWII and the per capita number of deaths by WWII, respectively, as used by Davis and Weinstein (2002). The slopes with robust standard errors in parentheses for panels A and B are 6.98 (10.45) and 33.52 (15.53), with p-values of 0.508 and 0.036, respectively.

Figure A3—Predetermined outcomes

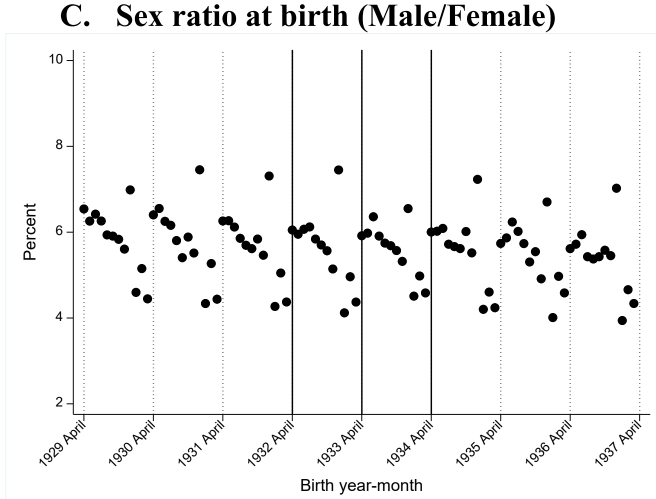
A. Cohort sizes at birth



B. Percent of stillbirths

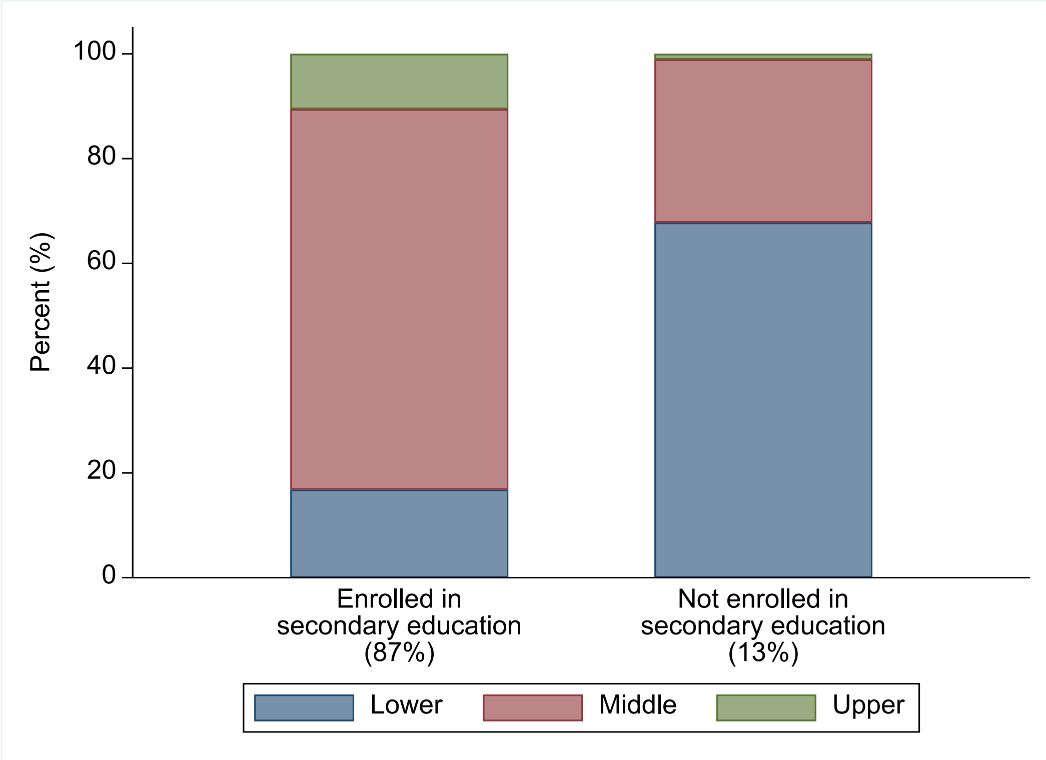


C. Sex ratio at birth (Male/Female)



Notes: The data are from 1929–1937 vital statistics in Japan. See Appendix Table D1 for data sources. The points represent the means of outcomes at birth year-month cells from April 1929 to March 1936 (96 months). The outcomes in panels A, B, and C are the number of births per 1000 (i.e., density), the percentage of stillbirths, and the sex ratio at birth (male/female), respectively. Each vertical line represents the month of April, which defines the academic cohort in Japan (April–March), and the solid lines correspond to April 1932, 1933, or 1934.

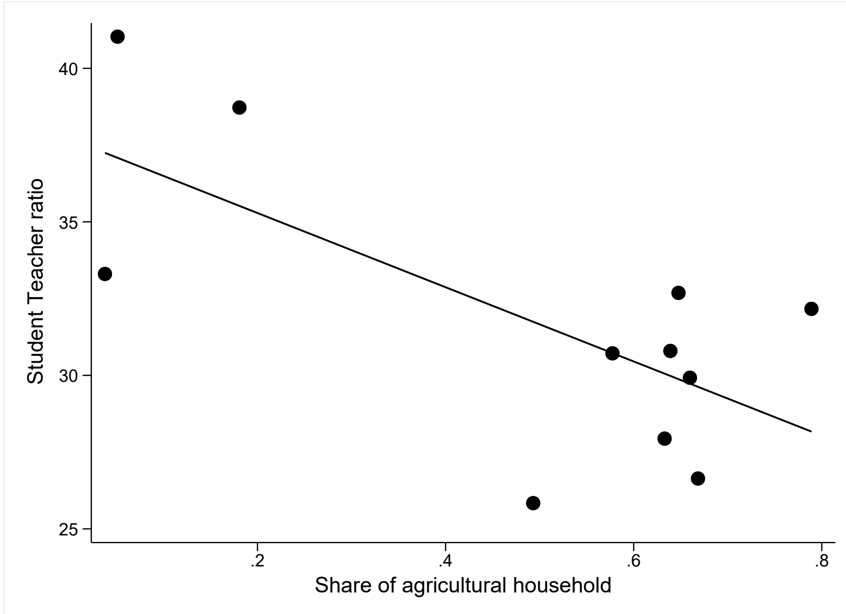
Figure A4—Household assets by the enrollment status in secondary education



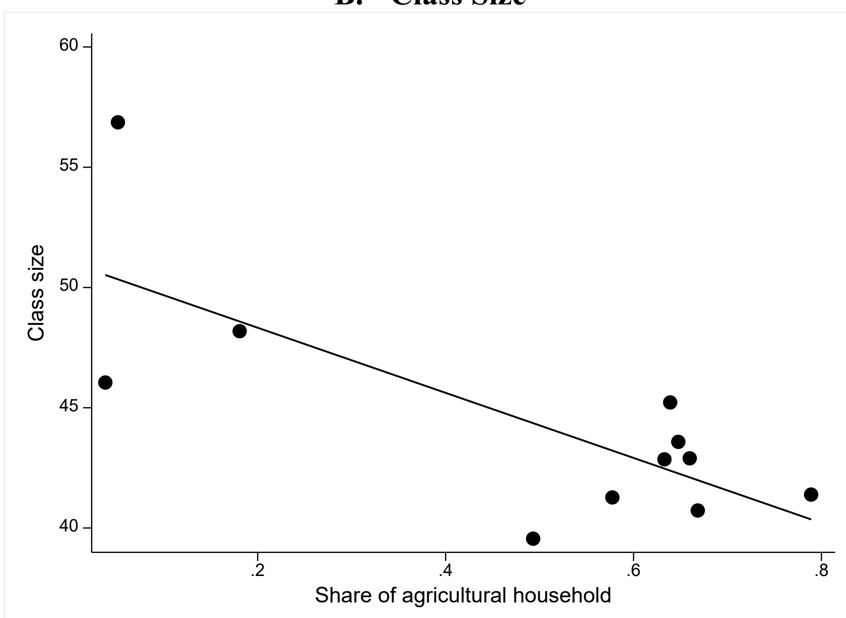
Notes: The data are from the Ministry of Education in 1938 (before the school reform). See Appendix Table D1 for data sources. The outcome is the level of household assets by secondary school enrollment status. The numbers in parentheses on the horizontal axis are the share of children.

Figure A5—Quality of secondary education and economic wealth (Aomori prefecture)

A. Student–Teacher Ratio



B. Class Size



Notes: The data are from the 1950 Statistical Yearbook in Aomori prefecture. See Appendix Table D1 for data sources. The outcomes in panels A and B are the student–teacher ratio and class size, respectively. The student–teacher ratio is the number of students divided by the number of teachers, while class size is the number of students divided by the number of classes. To avoid counting the enrollment from outside of Aomori prefecture, enrollment in private schools (4% of total enrollment) is excluded from the sample. The points represent the means of the outcomes in each municipality in Aomori (N=11). The horizontal axis is the share of agricultural households among the total number of households, which is used as a proxy for the economic wealth of the municipalities. The slopes with robust standard errors in parentheses for panels A and B are -12.09 (4.66) and -13.53 (5.48), with p-values of 0.029 and 0.036, respectively.

Table A1—Curriculum of grade 7 before and after the school reform

	Old system	New system	Change
Instructions per week			
“*Shushin*” (Ethics)	2	0	-2
Reading	4	6	2
Social studies	4	5	1
Mathematics	3	4	1
Science	2	4	2
Physical Education	6	3	-3
Fine arts	4	4	0
Vocational studies	5	5–8	0–3
Foreign language	0	1–4	1–4
Free study	0	1–4	1–4
Elective	3–5	0	-3–-5
Total	33–35	30–34	similar
Instruction days	>235	>235	same
Maximum class size	<50	<50	same

Notes: The data for the old and new systems are from the Ministry of Education (1941) and the Ministry of Education, Science and Culture (1980), respectively. “Elective” may be allotted to one or more existing subjects and/or to the original subject (e.g., foreign language) determined by each school. Each instruction period is 40/50 minutes.

Table A2—Assigned years of schooling for secondary school completion

Birth year	Old system (8 years)	New system (9 years)	Assigned years of schooling
1930	100.0%	0.0%	8.00
1931	91.9%	8.1%	8.08
1932	62.3%	37.7%	8.38
1933	17.1%	82.9%	8.83
1934	0.0%	100.0%	9.00

Notes: The data are from 1985 and 1995, and come from Social Stratification and Social Mobility (SSM). See Appendix Table D2 for details on the dataset.

Table A3—Balance test

	Cohort size at births (in thousands)	Percent of stillbirths	Sex ratio at birth
	(1)	(2)	(3)
A. RD with multiple cutoffs			
1932 cutoff	7.087*	0.028	0.006
	(3.753)	(0.094)	(0.007)
1933 cutoff	-4.720	0.010	0.018*
	(5.108)	(0.128)	(0.010)
1934 cutoff	5.792	-0.073	0.005
	(4.999)	(0.153)	(0.018)
Mean below 1932 cutoff	174.373	5.677	1.049
Mean below 1933 cutoff	182.718	5.612	1.050
Mean below 1934 cutoff	173.365	5.592	1.057
No. of birth year-month	96	96	96
No. of observations	-	16,893,206	16,893,206
B. Dif-in-RD (pooled cutoffs)			
1(After April)	-114.627***	2.049***	0.061***
	(11.809)	(0.199)	(0.008)
1(After April)×Treated	4.097	-0.165	0.005
	(17.402)	(0.268)	(0.014)
Mean	200.950	5.302	1.046
No. of birth year-month	84	84	84
No. of observations	-	14,844,291	14,844,291

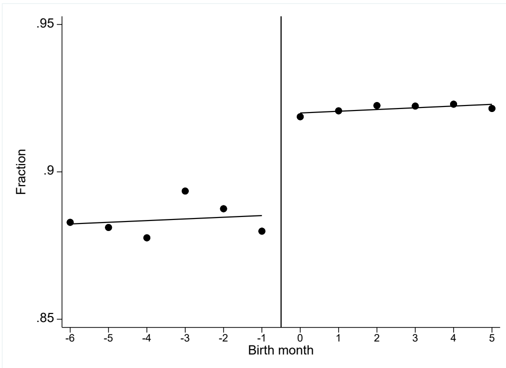
Notes: The data are from vital statistics in Japan collapsed at birth year-month cells from April 1929 to March 1936 (96 months) in panel A and from October 1929 to September 1935 (84 months) in panel B. Panel A reports the estimates from equation [1], with robust standard errors in parentheses. The 1932, 1933, and 1934 cutoffs are dummies that take on a value of 1 if the cohort is above April each year. Panel B reports the estimates of equation [2] with robust standard errors in parentheses. 1(After April) is an indicator that equals 1 for individuals born after April and 0 otherwise. Treated is an indicator equal to 1 for treated cohorts born between October 1932 and September 1935, and 0 otherwise. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Appendix B: Results on educational attainment

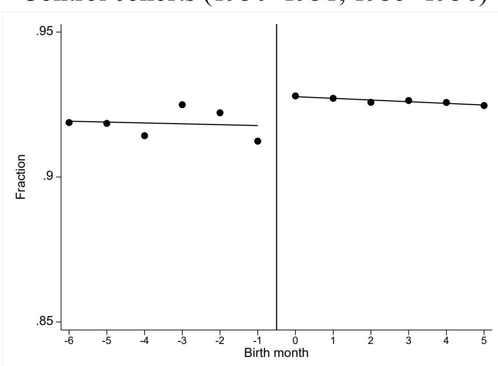
Figure B1—Educational attainment by Dif-in-RD

A. Secondary or higher

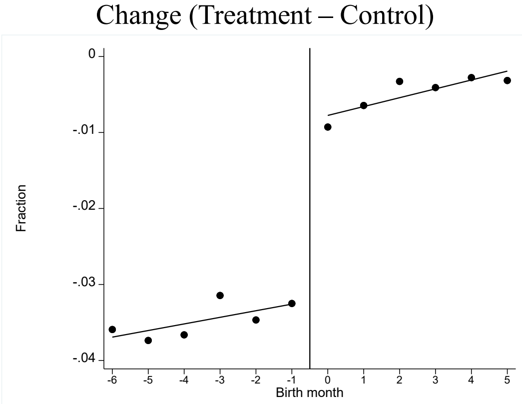
Treatment cohorts (1932–1934)



Control cohorts (1930–1931, 1935–1936)

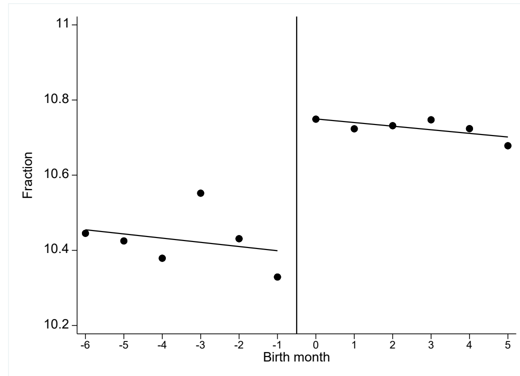


Change (Treatment – Control)

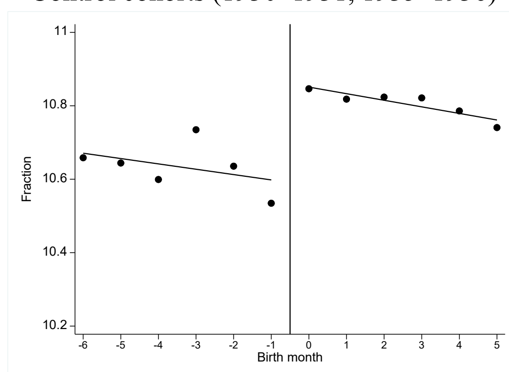


B. Years of schooling

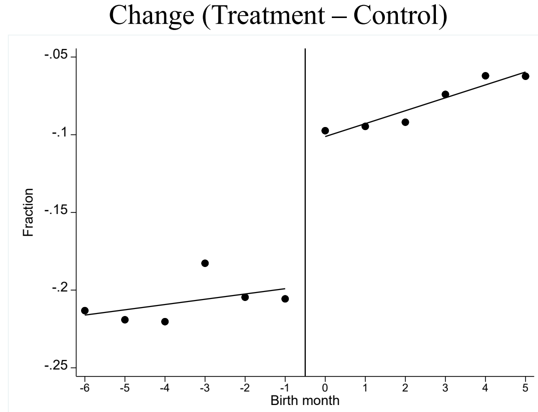
Treatment cohorts (1932–1934)



Control cohorts (1930–1931, 1935–1936)



Change (Treatment – Control)

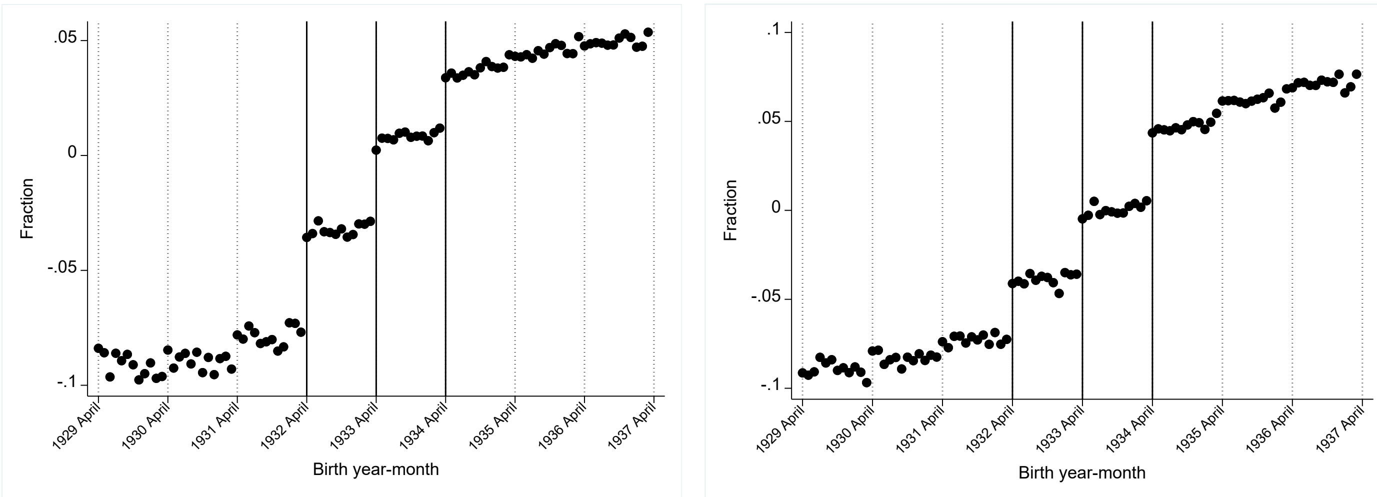


Notes: The data are from the 2020 census from October 1929 to September 1936 (84 months). The outcomes in panels A and B are the fraction with a secondary education or higher and years of schooling, respectively.

Figure B2—Educational attainment by sex

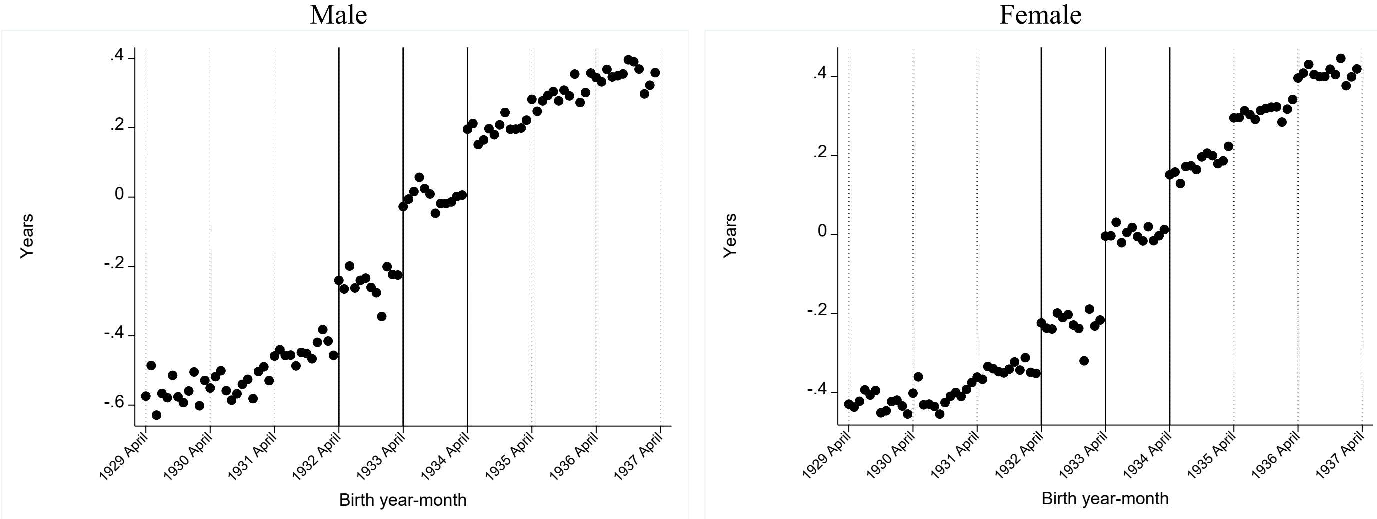
A. **Secondary school or higher** (residualized)

Male | Female



B. **Years of Schooling** (residualized)

Male | Female



Notes: The data are from the 2020 census. The points represent the means of the outcomes in birth year-month cells from April 1929 to March 1936 (96 months). The outcomes in panels A and B are the fraction with a secondary education or higher and years of schooling, respectively. The outcomes are residualized by birth-month FEs to account for seasonality, such as relative age effects. Each vertical line represents the month of April, which defines the academic cohort in Japan (April–March), and the solid lines correspond to April 1932, 1933, or 1934.

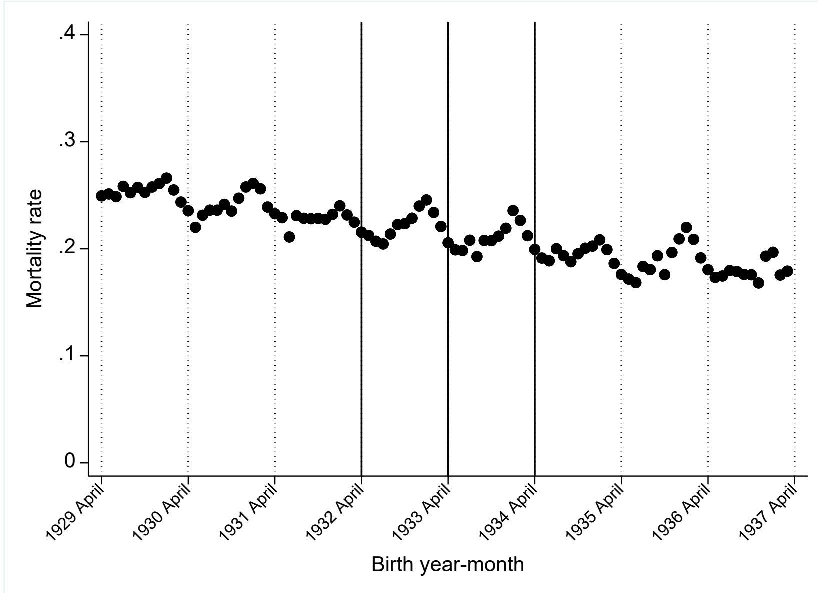
Table B1—Educational attainment by sex

	Secondary or higher		Years of schooling	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
A. RD with multiple cutoffs				
1932 cutoff	0.036***	0.023***	0.157***	0.085***
	(0.002)	(0.002)	(0.021)	(0.014)
1933 cutoff	0.026***	0.023***	0.213***	0.189***
	(0.002)	(0.003)	(0.030)	(0.022)
1934 cutoff	0.025***	0.039***	0.218***	0.138***
	(0.003)	(0.003)	(0.040)	(0.028)
Mean below 1932 cutoff	0.861	0.846	10.825	9.994
Mean below 1933 cutoff	0.907	0.886	11.022	10.109
Mean below 1934 cutoff	0.948	0.926	11.268	10.339
No. of birth year-month	96	96	96	96
No. of observations	1,774,317	3,044,259	1,774,317	3,044,259
B. Dif-in-RD (pooled cutoffs)				
1(After April)	0.005***	0.008***	0.314***	0.192***
	(0.002)	(0.003)	(0.031)	(0.029)
1(After April)×Treated	0.027***	0.025***	0.149***	0.088*
	(0.004)	(0.005)	(0.056)	(0.051)
Mean	0.905	0.867	10.957	10.108
No. of birth year-month	84	84	84	84
No. of observations	1,539,314	2,664,686	1,539,314	2,664,686

Notes: The data are from the 2020 census collapsed at birth year-month cells from April 1929 to March 1936 (96 months) in panel A and from October 1929 to September 1935 (84 months) in panel B. Panel A reports the estimates from equation [1], with robust standard errors in parentheses. The 1932, 1933, and 1934 cutoffs are dummies that take the value 1 if the cohort is above April in each year. Panel B reports the estimates of equation [2], with robust standard errors in parentheses. 1(After April) is an indicator that takes the value 1 for individuals born after April and 0 otherwise. Treated is an indicator that takes 1 for treated cohorts born between October 1932 and September 1935, and 0 otherwise. See Table A1 for the assignment of years of schooling to each birth cohort in columns (3) and (4). Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

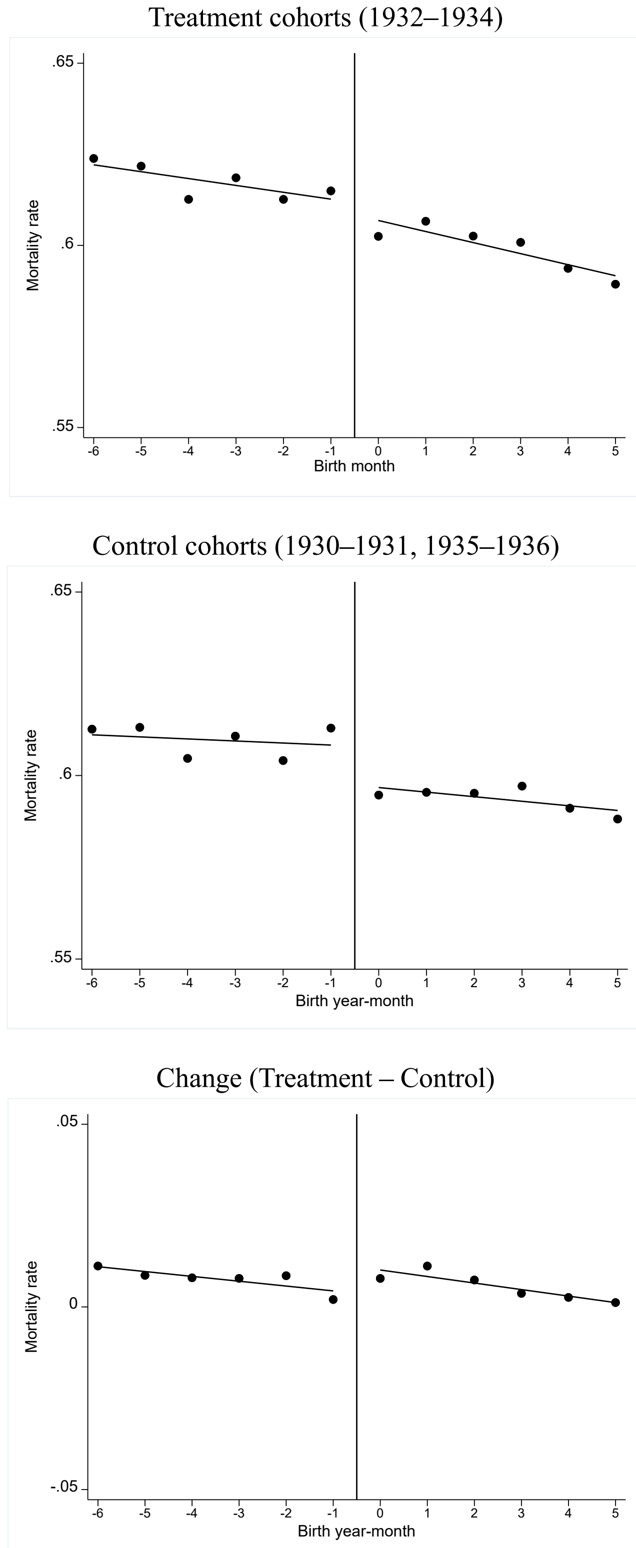
Appendix C: Results on mortality

Figure C1—Mortality rate between birth and 1972



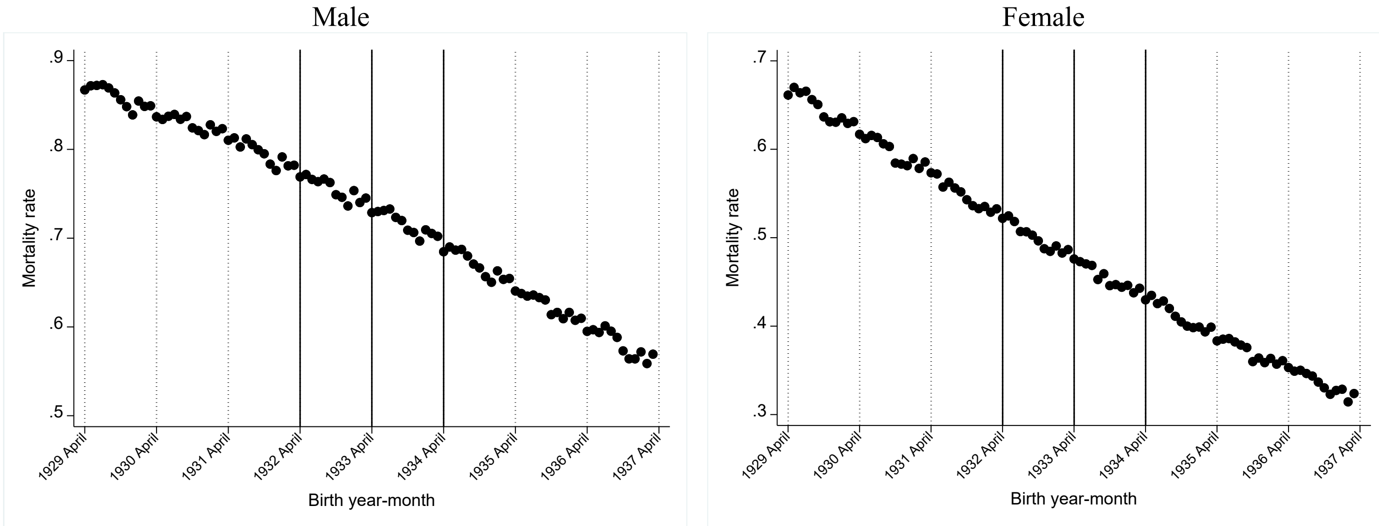
Notes: The data are from the 2000 census, 1972–2000 vital statistics, and birth count data from the 1929–1937 vital statistics. The outcome is the mortality rate between birth and 1972. The points represent the means of the outcomes in birth year-month cells from April 1929 to March 1936 (96 months). The vertical lines represent the cohorts born in April 1932, 1933, or 1934.

Figure C2—All-cause mortality by Dif-in-RD



Notes: The data are from the 2000 census, and 1972–2021 vital statistics collapsed at birth-year-month cells from October 1929 to September 1935 (84 months).

Figure C3—All-cause mortality by sex



Notes: The data are from the 2000 census and 1972–2021 vital statistics. The outcome is the mortality rate between 1972 and 2021. The points represent the means of the outcomes in birth year-month cells from April 1929 to March 1936 (96 months). The vertical lines represent the cohorts born in April 1932, 1933, or 1934.

Table C1—ICD diagnoses and causes of death/hospitalization

	ICD9	**ICD10**
	1971–1995	1996–2019
Cancer	140–209	C00–C97
Circulatory diseases	390–459	I00–I99
Respiratory diseases	460–519	J00–J99
Treatable causes of death		
Tuberculosis	010–018, 137	A15–A19, B90
Malignant neoplasm of cervix uteri	180	C53
Chronic rheumatic heart disease	393–398	I05–I09
All respiratory diseases	460–519	J00–J99
Asthma	493	J45, J46
Appendicitis	540–543	K35–K38
Abdominal hernia	550–553	K40–K46
Hypertensive and cerebrovascular disease	401–405,430–438	I10–I15, I60–I69
Chollelthiasis and cholecystitis	574, 575.0, 575.1	K80–K81
Preventable causes of death		
Lung cancer	162	C33–C34
Cirrhosis of liver	571.0–571.3,571.5–571.6	K70, K74.3–K74.6
External causes of death	800–999	V, W, X, Y

Notes: “Treatable causes of death” and “Preventable causes of death” are from Meghir et al. (2018).

Table C2—All-cause mortality by age groups

	By ages		
	50–59	60–69	70–79
	(1)	(2)	(3)
A. RD with multiple cutoffs			
1932 cutoff	-0.0008	-0.0018**	-0.0007
	(0.0006)	(0.0008)	(0.0011)
1933 cutoff	-0.0001	-0.0015	-0.0014
	(0.0007)	(0.0012)	(0.0014)
1934 cutoff	-0.0002	-0.0013	-0.0007
	(0.0010)	(0.0012)	(0.0017)
Mean below 1932 cutoff	0.047	0.093	0.184
Mean below 1933 cutoff	0.046	0.091	0.181
Mean below 1934 cutoff	0.045	0.089	0.178
No. of birth year-month	96	96	96
No. of observations	12,849,617	12,190,490	10,956,628
B. Dif-in-RD (pooled cutoffs)			
1(After April)	-0.0008**	-0.0009*	-0.0015
	(0.0003)	(0.0005)	(0.0009)
1(After April)×Treated	0.0001	-0.0007	0.0011
	(0.0006)	(0.0010)	(0.0014)
Mean	0.046	0.090	0.179
No. of birth year-month	84	84	84
No. of observations	11,281,888	10,700,457	9,612,811

Notes: The data are from the 2000 census, and 1972–2021 vital statistics collapsed at birth year-month cells from April 1929 to March 1936 (96 months) in panel A, and October 1929 to September 1935 (84 months) in panel B. Panel A reports the estimates from equation [1], with robust standard errors in parentheses. The 1932, 1933, and 1934 cutoffs are dummies that take 1 when the cohort is above April each year. Panel B reports the estimates from equation [2] with robust standard errors in parentheses. 1(After April) is an indicator equaling 1 for individuals born after April and 0 otherwise. Treated is an indicator equaling 1 for treated cohorts born between October 1932 and September 1935 and 0 otherwise. The number of observations reflects the number of survivors up to January 1972, our population at risk. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table C3—All-cause mortality by sex

	All causes	
	Male	Female
	(1)	(2)
A. RD with multiple cutoffs		
1932 cutoff	-0.0068***	0.0013
	(0.0024)	(0.0027)
1933 cutoff	-0.0024	0.0004
	(0.0023)	(0.0029)
1934 cutoff	-0.0012	-0.0019
	(0.0027)	(0.0026)
Mean below 1932 cutoff	0.795	0.546
Mean below 1933 cutoff	0.755	0.499
Mean below 1934 cutoff	0.715	0.454
No. of birth year-month	96	96
No. of observations	6,665,444	6,572,450
B. Dif-in-RD (pooled cutoffs)		
1(After April)	-0.0119***	-0.0096***
	(0.0017)	(0.0020)
1(After April)×Treated	0.0007	0.0045
	(0.0028)	(0.0030)
Mean	0.745	0.489
No. of birth year-month	84	84
No. of observations	5,855,484	5,768,910

Notes: The data are from the 2000 census, and 1972–2021 vital statistics collapsed at birth year-month cells from April 1929 to March 1936 (96 months) in panel A and October 1929 to September 1935 (84 months) in panel B. Panel A reports the estimates from equation [1], with robust standard errors in parentheses. The 1932, 1933, and 1934 cutoffs are dummies that take 1 if the cohort is above April in each year. Panel B reports the estimates of equation [2], with robust standard errors in parentheses. 1(After April) is an indicator that takes 1 for individuals born after the month of April and 0 otherwise. Treated is an indicator equal to 1 for treated cohorts born between October 1931 and September 1934, and 0 otherwise. The number of observations reflects the number of survivors up to January 1972, our population at risk. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Appendix D: Data Appendix

Table D1—Sources of historical data

Figure number	Variables	Source
Figure 1	- Student–teacher ratio at the prefecture level	Ministry of Education, eds. "1943 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979. Ministry of Education, eds. "1949 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979.
	- Per capita death by WWII - Per capita destroyed buildings by WWII	Davis and Weinstein (2002)
Figure 3	- Transition rate to secondary school - Share of agricultural households	Aomori Ken Naisei Bu Toukei Ka, eds. "1943 *Aomori Ken Toukeisyo* [Statistical Yearbook of Aomori Prefecture]," Aomori: Touou Insatsu, 1945. Aomori Ken Naisei Bu Toukei Ka, eds. "1949 *Aomori Ken Toukeisyo* [Statistical Yearbook of Aomori Prefecture]," Aomori: Touou Insatsu, 1951.
Figure A1	- Student–teacher ratio - Student–classroom ratio at national level	Ministry of Education, eds. "1943 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979. Ministry of Education, eds. "1944 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979. Ministry of Education, eds. "1945 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979. Ministry of Education, eds. "1946 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979. Ministry of Education, eds. "1947 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979. Ministry of Education, eds. "1948 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979. Ministry of Education, eds. "1949 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979. Ministry of Education, eds. "1950 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979.
Figure A2	- Secondary school attendance rates at the prefecture level	Ministry of Education, eds. "1948 *Monbusyou Nenpou* [Annual report of the minister of state for education]," Tokyo: Insatsukyoku Choyokai, 1979.
	- Per capita death by WWII - Per capita destroyed buildings by WWII	Davis and Weinstein (2002)
Figure A3	- Cohort size at birth - Percent of stillbirths - Sex ratio at birth	Ministry of Health, eds. "1929 *Jinkou Doutai Toukei* [Annual report of the vital statistics]," Tokyo: Tokyo Toukei Kyokai, 1930. Ministry of Health, eds. "1930 *Jinkou Doutai Toukei* [Annual report of the vital statistics]," Tokyo: Tokyo Toukei Kyokai, 1931.

		Ministry of Health, eds. "1931 *Jinkou Doutai Toukei* [Annual report of the vital statistics]," Tokyo: Tokyo Toukei Kyokai, 1932. Ministry of Health, eds. "1932 *Jinkou Doutai Toukei* [Annual report of the vital statistics]," Tokyo: Tokyo Toukei Kyokai, 1933. Ministry of Health, eds. "1933 *Jinkou Doutai Toukei* [Annual report of the vital statistics]," Tokyo: Tokyo Toukei Kyokai, 1934. Ministry of Health, eds. "1934 *Jinkou Doutai Toukei* [Annual report of the vital statistics]," Tokyo: Tokyo Toukei Kyokai, 1935. Ministry of Health, eds. "1935 *Jinkou Doutai Toukei* [Annual report of the vital statistics]," Tokyo: Tokyo Toukei Kyokai, 1936. Ministry of Health, eds. "1936 *Jinkou Doutai Toukei* [Annual report of the vital statistics]," Tokyo: Tokyo Toukei Kyokai, 1937. Ministry of Health, eds. "1937 *Jinkou Doutai Toukei* [Annual report of the vital statistics]," Tokyo: Tokyo Toukei Kyokai, 1938.
Figure A4	- The level of household assets by the enrollment status in secondary education	Ministry of Education, eds. "*Jinjo Shogakkou Sotsugyosha No Dokounikansuru Shirabe* [Survey on primary school graduates]," Tokyo: Ministry of Education, 1938.
Figure A5	- Transition rate to secondary school - Share of agricultural households	Aomori Ken Naisei Bu Toukei Ka, eds. "1943 *Aomori Ken Toukeisyo* [Statistical Yearbook of Aomori Prefecture]," Aomori: Touou Insatsu, 1945. Aomori Ken Naisei Bu Toukei Ka, eds. "1949 *Aomori Ken Toukeisyo* [Statistical Yearbook of Aomori Prefecture]," Aomori: Touou Insatsu, 1951.

Table D2—Details of microdata

Name	Source	Description
Social Stratification and Social Mobility Survey (SSM)	(A1) (A2)	The nationwide survey on social stratification and social mobility, also known as the SSM Survey, is one of the most traditional large-scale social surveys in Japan, and has been carried out every 10 years since the first survey in 1955 by the Japan Sociological Society. It covers around 2,000 individuals residing in Japan per survey round. Individual-level data for male samples are available from 1955, and data for female samples first became available in 1985. Information on the individual's highest level of education under either the old or new education system, along with information on birth year (not birth year-month), is available only for 1995 or earlier. Therefore, we use 1985 and 1995 surveys to compute the years of schooling for secondary school completion at the birth-year level and assign it to each academic cohort (allowing for measurement error), as shown in Table A1.
Patient Survey	(B)	Started in 1948, the Patient Survey is a national sample survey of hospitals and clinics that gathers information on the utilization of medical institutions in Japan. A comprehensive version of the Patient Survey has been conducted by the Ministry of Health, Labour and Welfare every 3 years since 1984. It covers approximately 2,000–7,000 hospitals and 3,000–6,000 clinics per survey year. The sample size has become larger in recent years. The survey collects information on the International Classification of Diseases (ICD) codes; patients' principal sources of payment; and limited sociodemographic characteristics, such as sex and patients' place of residence. Individual-level patient microdata files are available from 1984. There are two datasets in the Patient Survey, namely, 1) outpatient data and 2) discharge data, which we use to examine outpatient visits and inpatient admissions, respectively. The outpatient data in the Patient Survey are collected on a day in mid-October (normally a weekday in the second week) and include information on all patients who visit hospitals or clinics as outpatients (i.e., visits to hospitals or clinics not culminating in hospitalization). The datasets contain 364,000–1,020,000 outpatient visitors. The discharge data in the Patient Survey report all the inpatients discharged from the surveyed hospitals and clinics in the month of September of the survey year. The datasets contain about 180,000–1,140,000 inpatient records per survey year.

Source:
(A1) https://ssjda.iss.u-tokyo.ac.jp/Direct/gaiyo.php?eid=0762&lang=eng (1985)
(A2) https://ssjda.iss.u-tokyo.ac.jp/Direct/gaiyo.php?eid=0763&lang=eng (1995)
(B) http://www.mhlw.go.jp/english/database/db-hss/dl/sps_2008_06.pdf