SCHOOL CLOSURES DURING THE 1918 FLU PANDEMIC

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Abstract—During the 1918–1919 influenza pandemic, many local authorities made the controversial decision to close schools. We use newly digitized data from newspaper archives on the length of school closures for 165 large U.S. cities during the 1918–1919 flu pandemic to assess the long-run consequences of closing schools on children. We find that the closures had no detectable impact on children's school attendance in 1920, nor on their educational attainment and adult labor market outcomes in 1940. We highlight important differences between the 1918–1919 and COVID-19 pandemics and caution against extrapolating from our null effects to modern-day settings.

I. Introduction

GOVERNMENT responses to pandemics are controversial; proponents argue that government interventions can slow the spread of pandemics, whereas opponents contend that because individuals respond rationally to risk, the negative effects of those interventions outweigh any public health benefits. In this paper, we evaluate the potential costs of one of the most salient and controversial government responses to pandemics: large-scale school closures. Numerous studies find that unplanned school closures and absenteeism negatively impact student achievement (Marcotte, 2007; Marcotte & Hemelt, 2008; Goodman, 2014; Aucejo & Romano, 2016; Gershenson et al., 2017; Jaume & Willén, 2019). Whether these findings predict the impact of planned local policies aimed at pandemic prevention is unknown. Using historical evidence from the 1918–1919 influenza pandemic, we provide one of the first analyses of the long-run effects of local pandemic interventions on affected children.[1]

In the United States, a commitment to federalism caused public health laws to vary significantly across states and municipalities during the 1918–1919 influenza pandemic, with some being strict and others more lax.[2] Many state and local authorities closed schools as the situation worsened, whereas others decided to keep their schools open—mirroring modern-day debates about the costs and benefits of school closures during pandemics. To estimate the impacts of these closures, we construct novel data from newspaper archives on the duration of closures for 165 of the largest U.S. cities in 1910, which we combine with microdata from the 1910 and 1920 decennial censuses to study the short-run effects of school closures in 1918–1919. To investigate the long-run effects of closures on children, we link newborn to 25-year-old males from the 1920 census to their adult records in 1940 to obtain measures of adult outcomes.

Using these data, we first describe the geography of school closures and the city characteristics in 1910 that predict longer closures in 1918. The length of school closures was positively correlated with the number of city workers in medical fields and whether the city had a state order that mandated or recommended a closure, whereas it was negatively correlated with the share of immigrants in a city. One striking feature is a higher school attendance rate of 15- to 18-year-olds in cities that decided to close their schools for a longer period, indicating that stricter cities were positively selected on high school attendance.

Next, we estimate the short-run effects of school closures on attendance rates in the 1920 census. Our identification strategy uses the fact that some of the school-aged population were less likely to have their schooling interrupted because they were either too old or too young to be attending school during the pandemic. We find no effect of closure length on attendance probabilities across different age groups. These null effects persist across groups based on paternal occupational prestige and nativity, as well as students' race and gender. We then show that these null effects on school attendance in the short run extend to the long run. In 1940, the 1918–1919 school closures had no detectable effect on educational attainment, wage income, nonwage income, and hours worked. Again, we find no heterogeneous impacts for individuals with different family backgrounds or demographics. Overall, our results suggest that although the pandemic may have affected the academic performance of school-aged children in 1918–1919, the closures themselves had no measurable effects on the outcomes we study.

To our knowledge, we are the first to study the long-run effects on children of school closures during a pandemic. In the historical context, our paper is most related to Meyers and Thomasson (2020), who study school closures during the 1916 U.S. polio epidemic. They find that children of legal working age living in areas with many polio cases had lower lifetime educational attainment than their peers in less affected locations. The polio epidemic was significantly smaller than the 1918–1919 influenza pandemic and primarily affected children. As a result, no widespread economic disruption occurred, unlike in the 1918–1919 influenza pandemic. Although Meyers and Thomasson (2021)'s findings

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[1]Many papers provide overviews of the use of school closures as a pandemic mitigation strategy, including Ferguson et al. (2006) and Cauchemez et al. (2009).

[2]Ironically, Troesken (2015) notes that the same institutions that led to significant economic growth in the United States also created the patchwork of laws making the country more prone to disease outbreaks when compared to countries with centralized public health systems.

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suggest that children of legal working age may have dropped out of school to work during closures and did not return, this may have been a less attractive option for teenagers during the influenza pandemic, since manufacturing and retail activity declined during the pandemic and employment became harder to find (e.g., Garrett, 2007; Bodenhorn, 2020; Velde, 2022). In addition, Meyers and Thomasson (2021) do not have direct data on school closures and instead rely on geographic variation in polio morbidity rates to identify effects of the polio epidemic on outcomes. They also focus on long-run effects using information on children's state of birth. We use direct measures of school closures at the city level to measure the impacts of school closures on children's short-run and adult outcomes.

Our findings also contribute to an emerging literature on the impacts of the COVID-19 pandemic on children. Although it is too soon to comprehensively measure the long-run impact of COVID-19–related school closures, early results suggest that student outcomes may suffer, at least in the short run (Chetty et al., forthcoming; Maldonado & De Witte, 2022). Evidence also suggests that lower-income children may be more affected than those from higher-income families, thus increasing inequality across children from different backgrounds (e.g., Bacher-Hicks et al., 2021; Chetty et al., forthcoming; Grewenig et al., 2021).

However, important differences are seen between the 1918–1919 and 2020–2021 pandemics and school closures that we think rationalize our null results. Mortality rates in 1918–1919 followed an atypical curve, with death rates highest among young children (0–5), prime-age workers (25–34), and the elderly (70+). This contrasts with COVID-19, which had the highest mortality rate only among older adults. These differences in mortality affect responses to school closures. Even when schools remained open in 1918–1919, absentee rates were extremely high, dampening any potential effects of the closures: many people stayed home independent of local policies on school closures and reopenings.[3] And, ex post, contemporary health officials regarded school closures and other nonpharmaceutical interventions (NPIs) aimed at preventing the spread of the pandemic as largely ineffective (e.g., Byerly, 2010; Tomes, 2010). Our results support the views of these contemporary observers.

Another important contrast is that school closures in the 1918–1919 pandemic were substantially shorter than COVID-19–related school closures, potentially limiting their effects. In our sample of 1918–1919 school closures, the average closure length was 36 days, and some cities decided to make up for missed school days by extending the school year.[4] In 2020, many schools surpassed thirty days of closure in the spring before closing again for in-person class for much of the following academic year.

II. Background and Context

A. *The 1918–1919 Influenza Pandemic*

The 1918–1919 flu was the most severe pandemic in the twentieth century. It was caused by the spread of an H1N1 virus and occurred in three waves: a first mild wave in spring 1918, a second severe wave in fall 1918, and a third less lethal wave in early 1919. Estimates reveal that about one-third of the world's population suffered from influenza during this period (Taubenberger & Morens, 2006). The 1918–1919 virus was extremely lethal compared to other influenza strains. The case fatality rates exceeded 2.5%, and at least 50 million people died from the virus. Most influenza viruses have the largest negative effects on young children and the elderly. But a striking feature of the 1918 pandemic was its high incidence and mortality for those aged 20–40 (Collins, 1931). In the United States, over one-quarter of the population was infected, and about 675,000 individuals died from the virus between 1918 and 1920—0.66% of the total population (Johnson & Mueller, 2002; Crosby, 2003; Taubenberger & Morens, 2006).

In the United States, the pandemic had its first noticeable effect during spring 1918, when it was identified in military personnel (Crosby, 2003; Byerly, 2010; Barry, 2020). Major outbreaks occurred across the country during the second wave, which first emerged in Boston's Commonwealth Pier on August 27, 1918, and, only two days later, the first severely ill soldiers were admitted to the U.S. Naval Hospital in Chelsea, Massachusetts (Byerly, 2010). The pandemic then spread in East Coast cities, including Boston, New York, and Philadelphia, and gradually diffused westward over the next two months. Some cities, including Albany and Chicago, experienced a substantial increase in excess mortality only during the fall of 1918, whereas other cities like San Francisco and New Orleans also experienced a second wave during the first two months of 1919.[5] The severity of the influenza pandemic during the second wave and the first months of the third wave is illustrated by Markel et al. (2007, table 1), who report excess influenza and pneumonia mortality rates over the 24 weeks from September 8, 1918, through February 22, 1919, for 43 cities ranging from 210 excess deaths per 100,000 inhabitants (Grand Rapids, Michigan) to 710 excess deaths per 100,000 inhabitants (Boston, Massachusetts).[6]

B. *School Closures*

Local health authorities responded to increasing mortality numbers during the second wave of the 1918–1919 influenza

[3]For example, in Staten Island, half of students were not in school in mid-October 1918, even while schools were open (The Sun, 1918).

[4]For example, in Atlanta the 1918–1919 school year was extended to June 20 from June 1 because of closures (Influenza Archive, 2020a).

[5]An excellent description of the influenza pandemic in large U.S. cities is provided in Navarro and Markel's digital *Influenza Encyclopedia*; see http://www.influenzaarchive.org/about.html.

[6]Clay et al. (2018) show that air pollution elevated mortality rates in U.S. cities during the pandemic. Other factors such as distance to military camps, differences in prepandemic mortality, poverty rates, and the population composition also contributed to the uneven distribution of excess pandemic deaths across the country (Crosby, 2003; Beach et al., 2020).

pandemic by imposing a wide range of NPIs. These measures included isolation and quarantine, bans on public gatherings, staggered business hours, ventilation of public venues and streetcars, mandated face masking, and school closures. We now have a large and growing literature on the effects of these NPIs on health and economic outcomes. Markel et al. (2007) and Hatchett et al. (2007) suggest that cities that enacted NPIs early delayed peak mortality and had lower mortality, whereas Bootsma and Ferguson (2007) find only modest effects on total mortality. Similarly, Barro (2022) finds that although NPIs slowed the initial acceleration of the pandemic; there is little evidence that the measures reduced overall mortality.[7]

We examine one particularly important NPI in this paper: school closures. In early October 1918, U.S. Surgeon General Rupert Blue issued closure recommendations that included schools along with churches, theaters, and other public institutions. Blue noted that although “there was no way to put a nationwide closing order into effect,” he hoped “that those having the proper authority will close all public gathering places if their community is threatened by the pandemic” (The Boston Globe, 1918). Although the federal government did not have the power to close schools, some states did. New Jersey ordered all schools closed from October 10 to 26, and Louisiana ordered schools closed from October 8 to November 16. But most states did not mandate closures. A few, such as New York and Illinois, made no closure recommendations at all. Others, such as North Carolina, advised communities to consider closing schools if influenza became prevalent in their community (Austin, 2018).

Local authorities had wide latitude in determining whether and when to close schools, with limited oversight from higher levels of government. The earliest school closures occurred around Boston in late September 1918. As the virus spread, other school districts followed. The decision to close schools was controversial, and not all agreed that it would help to slow the spread of the virus. In Chicago schools never closed despite heavy pandemic caseloads, though students who became ill were advised to stay home. In early October, the Chicago Heath Commissioner argued that keeping schools open would reduce the spread of the virus: “[T]he children are better off than they would be if we closed the schools and they were free to roam wherever they chose” (The Chicago Tribune, 1918a). After cases declined in November, Chicago-based public health officials were pleased with their decision to keep schools open. Dr. W. A. Evans, president of the American Public Health Association and a *Chicago Tribune* columnist, summarized this view in a late November column. He argued that the disease was not particularly dangerous for school-aged children, cities that closed schools did not seem to do any better at containing the virus than Chicago, and children were better off supervised in school, where learning could continue (The Chicago Tribune, 1918b). Similar justifications kept schools open in New York City.

In cities that did close their schools, similar pro–school-opening views were common and affected decisions. There was pressure to keep schools open for as long as possible, and after closures, to reopen them quickly. For example, after a local health commissioner ordered schools to close in Minneapolis, Minnesota, the school board and superintendent defied the order, noting that “we shall not close the schools if they arrest us and fine us” (Influenza Archive, 2020b). These officials were supported by the State Board of Health, which praised nearby St. Paul for not closing its schools (Influenza Archive, 2020c). Under pressure, several cities opened their schools too soon, before the pandemic was contained. In Decatur, Illinois, schools opened on November 11. Only seventeen days later, the schools were closed again until December 30.

Even when schools were open, many students did not attend. In Chicago and New York, students who were suspected of having the virus were told to stay home or sent to special quarantine facilities. Many families also appear to have kept children home, fearing infection. In Staten Island, New York, school attendance rates dropped by 50% (The Sun, 1918). In Sacramento, California, 2,237 children were absent on October 21, even as schools remained open and the city reported only forty student cases. The school board attributed absences to “fear” and noted that if absences continued to be high, the city would be forced to close the schools for financial reasons (The Sacramento Bee, 1918b). The next day, absences increased to 2,875 (“apparently due to fright”) and the city closed schools (The Sacramento Bee, 1918a). High levels of absenteeism were not limited to cities. At the peak of the pandemic in Davey, Nebraska (population 123 in 1920), over half of all students did not attend class (The Ceresco Courier, 1918).

III. Data

A. *School Closure Data*

Our main treatment variable is the total number of days a city closed schools during the 1918–1919 school year. We identify 229 cities with a 1910 population greater than 25,000[8] and search historical newspaper archives for mentions of school closures. Markel et al. (2007) collect NPI information for fifty large cities, and for those cities we rely on Markel’s dates of school closure. For all of the smaller cities, as well as an additional nine cities mentioned in newspaper articles relating to those cities, we search for school closure information using newspapers.com and similar

[7]See Stern et al. (2009) for a list of NPIs by type and city. For a detailed discussion of the many papers related to the 1918 pandemic, see the recent surveys by Arthi and Parman (2021) and Beach, Clay et al. (2022). In addition, Almond and Currie (2011), Almond et al. (2018), and Beach, Brown et al. (2022) provide detailed surveys of the long-run effects of early childhood exposure to health shocks, including influenza. But none of these papers measure the causal effect of school closures on children.

[8]To construct this list of 229 cities, we rely on the Bureau of Education’s 1917 annual report describing the school systems of large cities.

FIGURE 1.—MAP OF CITIES BY 1918–1919 SCHOOL CLOSURE LENGTH

This map plots the location of cities in our sample. Dots are colored by the length of school closures during the 1918–1919 pandemic. Darker dots correspond to more days closed. Dot size is weighted by 1910 population, as calculated in the 1910 census.

historical newspaper archives. From our sample of 238 cities, we located school closure and reopening dates for 171 cities.[9] Three of these 171 cities had incomplete school attendance information in the full-count decennial census data, and three could not be matched to a unique census location, so we focus on the remaining 165 cities. If a city closed schools multiple times, we use the total number of days closed across all closures.[10]

Figure 1 plots the distribution of school closures across areas. Very little geographical clustering is found; cities in similar areas often made different closure choices. For example, Chicago and New York were the two largest cities in our sample not to close schools at any point, whereas neighboring cities closed schools for many weeks.[11] Cities closed schools on average for 36 days, with a standard deviation of 21 days.

The length of time schools closed during the pandemic is correlated with several city characteristics. Figure A1 presents the estimated coefficients from a regression of various city characteristics in 1910 on days closed in the 1918–1919 pandemic.[12] An additional day of school closures is associated with a 0.01 standard deviation higher school attendance rate in the city in 1910. These effects are largest among children aged 15–18, who were of legal working age in most states. Unsurprisingly, this pattern is not as strong for children aged 6–10 or 11–14, as school attendance rates for this group are comparably high. The share of individuals working in the medical field in 1910 is positively associated with longer school closures. On the other hand, cities with larger immigrant populations closed their schools for shorter periods. Finally, in states with school closure recommendations, cities closed schools for slightly longer amounts of time.

Figure A3 plots the number of days schools closed versus excess 1918–1919 flu mortality rates. We do not see any evidence that cities with more excess flu deaths were any more likely to close down their schools for longer periods. In our baseline analysis, we do not control for excess mortality because it could have been affected by school closures. But, consistent with the weak relationship observed in figure A3, we show that including mortality as a control does not change the estimated impact of school closures on the school-aged population.

B. *Census Data*

To measure short-run outcomes and city-level covariates, we use individual data from the full-count population censuses in 1910 and 1920.[13] The outcome variable for our short-run regressions is reported school attendance among individuals from newborn to age 25 in each census year.[14] We use other demographic variables to test for possible heterogeneous effects of closures. We assign school closures to each individual's city of residence in the 1910 and 1920 censuses.

To study the long-run impacts of school closures, we link male children in 1920 to their adult observations in the 1940 full-count population census, using the 1920–1940 links provided by the Census Linking Project (Abramitzky et al., 2020), which matches records based on standardized name

[9]For an additional 36 cities, we found sources confirming that the city closed its schools, but we could not confirm the exact dates of closure. We do not use these 36 cities in our analysis. For the final 31 cities, we find no evidence that there was or was not a school closure.

[10]All dates were independently verified by at least two research assistants.

[11]Other cities in our sample that decided not to close schools are Bridgeport, Connecticut; Hartford, Connecticut; New Haven, Connecticut; Lewiston, Maine; and Troy, New York.

[12]City characteristics are standardized to be mean zero with a standard deviation of one.

[13]We use restricted access census data provided by IPUMS (Ruggles et al., 2020).

[14]The variable *school* is not perfectly comparable over time in these two censuses: In 1910 the question asked on April 15, 1910, whether a child had been in school since the previous September 1. In 1920 the question was asked on January 1 and also referred to the period since September 1.

strings, birth state, and birth year.[15] We measure 1940 outcomes for this linked sample, including educational attainment, wage income, existence of nonwage income, and hours worked.[16]

In table A1, we report baseline summary statistics from the three analysis samples we use in this project: newborns to 25-year-olds in the 1910 decennial census, newborns to 25-year-olds in the 1920 decennial census, and newborns to 25-year-olds in the 1920 decennial census whom we link to the 1940 census. In our three samples, 38–41% of these children and young adults report attending school, and they lived, on average, in a city that closed schools for between 25 and 28 days. In our 1920–1940 matched sample, the average newborn to 25-year-old in 1920 obtained 10.3 years of education by 1940 and earned an average annual wage income of $1,151.

IV. Empirical Strategy and Results

A. Estimation Strategy

We use a difference-in-differences approach to show that school closures during the 1918–1919 pandemic had little effect on short-run school attendance and long-run outcomes. The intuition behind the difference-in-differences approach can be seen graphically in figure 2, which plots average attendance rates for cities with longer and shorter school closures by age and census year. Cities are grouped by those that closed schools for (1) 0–21 days, (2) 22–35 days, and (3) 36 days or longer. Panel A shows that 1920 attendance rates are similar for children newborn to age 14 across the three groups of cities, but that students aged 15–21 were more likely to be attending school in cities that closed their schools for a longer time during the pandemic. This does not imply that school closures *increased* school attendance; instead, panel B of figure 2 shows that stricter cities were positively selected on high school attendance rates. Even in 1910 (before school closures could have affected children), children in cities with longer school closures during the 1918–1919 pandemic were more likely to attend school. Comparing panels A and B, no change is seen in that form of selection from 1920 to 1910, indicating that there is no negative effect of 1918–1919 school closures on 1920 school attendance.

To explore these results further and expand our analysis to long-run outcomes, we estimate versions of the following equation:

$$Y_i = \beta_a WeeksClosed_c \times AgeGroup_a + \gamma_c + \Pi \times X_i + \Delta \times V_{c,b} + \omega_{r,b} + \epsilon_i, \quad (1)$$

where the outcome variable Y_i is an indicator measuring whether child i was attending school in 1920 (for the short-run analysis) or an outcome measured in 1940 (for the long-run analysis). The variable $WeeksClosed_c$ describes the number of weeks that schools were closed during the 1918–1919 school year in individual i's city c; we scale this measure so that it is in standard deviation units, and we interpret β_a as measuring the causal effect of a three-week (approximately one standard deviation) increase in school closure days on the outcome of interest for age group a. Age bins a group children into six groups (aged newborn to 5, 6–10, 11–14, 15–18, 19–21, and 22–25 in 1920) so we can separately estimate the effect of school closures on pupils of different ages. γ_c are city fixed effects. X_i is a vector of race-by-gender fixed effects. $V_{c,b}$ contains birth-year fixed effects linearly interacted with the following city characteristics measured in 1910: log population, the fraction of residents who are foreign-born, the average occupational score of 25–54-year-old men, and the school attendance rates of 6–10, 11–14, and 15–18-year-old children. In addition, we include census region-by-birth year fixed effects ($\omega_{r,b}$) to absorb time-varying investment in schooling at the region-level and region-level policy variation related to the 1918 pandemic that may have affected some children differently from others. Robust standard errors are clustered at the city level.

Equation (1) is similar to a difference-in-difference model, because, for example, the $\beta_{age\in[15,18]}$ coefficient is measured relative to the $\beta_{age\in[0,5]}$ coefficient (the omitted category which is zero by construction). To explain our strategy more concretely, if every city in the United States closed schools for either zero or one day in 1918, our method would compare outcomes for 15–18-year-olds in places that closed schools for one day in 1918 against outcomes for 15–18-year-olds in places that did not close schools in 1918 (first difference). We would then compare this difference to the same difference of children newborn to age 5 across those cities (second difference). If $\beta_{age\in[15,18]}$ is negative, this implies that school closures negatively affected outcomes in that age group, relative to the omitted category (newborn to age 5). Our identification assumption is that no city-age specific patterns are correlated with school closures and our outcome variables in a way that our model fails to capture.[17]

B. Short-Run Results

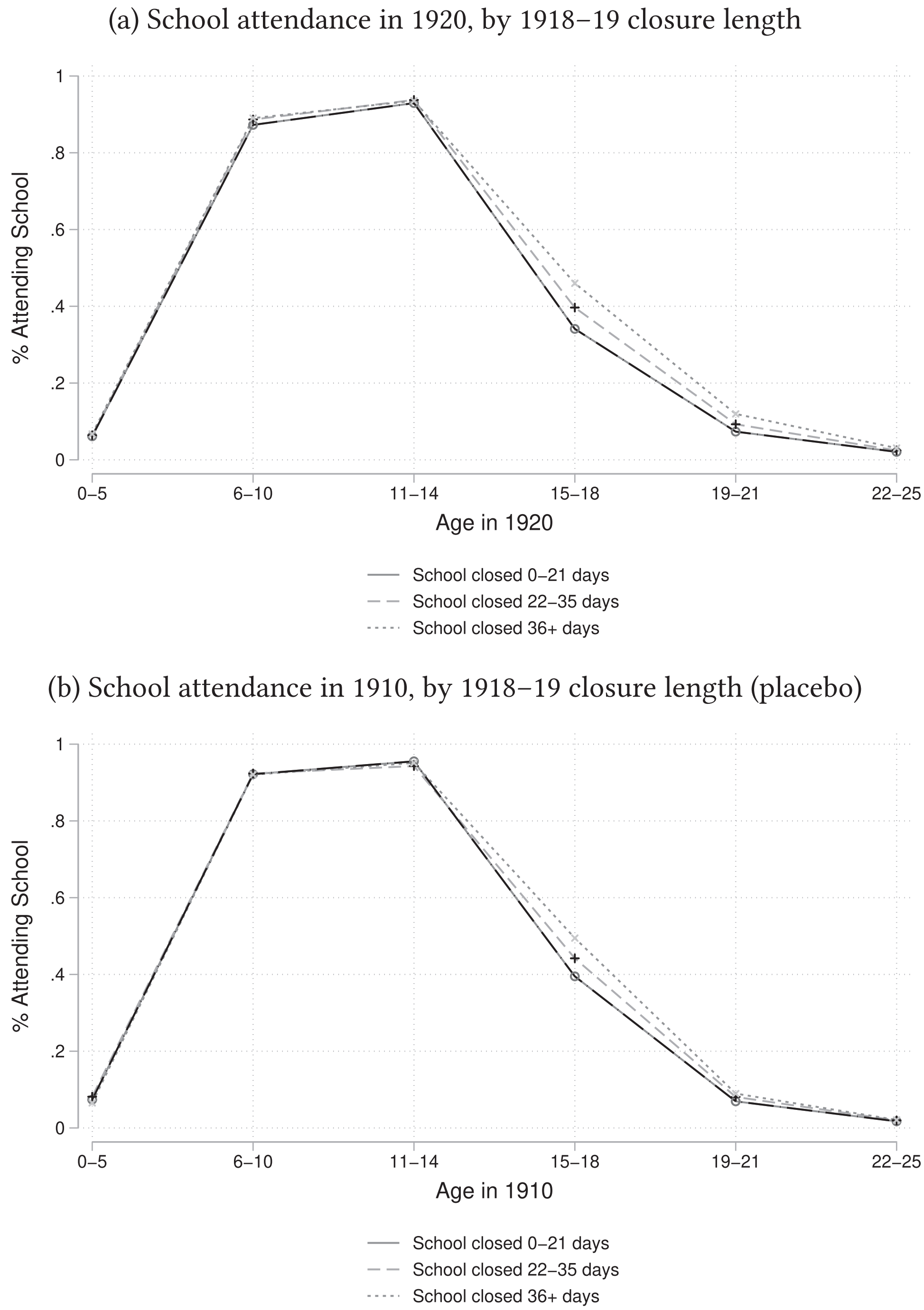
In panel A of figure 3, we plot the estimated β_a coefficients for equation (1) with an indicator of school attendance in 1920 as the outcome variable. We scale the β_a coefficients to measure the effects of a three-week (21-day) school closure. The β_a coefficients are relative to children to age 5 (omitted category), who would not have been affected by school closures. Our estimates reveal no evidence that the number of days a school system closed during the 1918–1919 pandemic

[15] We use the Census Linking Project's standard links with phonetic string cleaning. More information on this linking method can be found in Abramitzky et al. (2019). Our results are robust to using the other linking methods provided by the Census Linking Project. We cannot link women due to name changes upon marriage.

[16] Nonwage income measures income from nonemployer sources, including self-employment. In the 1940 decennial census, this indicator is the only collected measure of nonwage income.

[17] Potential concerns could include city-level availability of schooling for children of different ages, each city's school ventilation quality or other school inputs, or city-age specific preferences for truancy.

FIGURE 2.—AVERAGE SCHOOL ATTENDANCE RATES, BY AGE GROUP AND 1918–1919 SCHOOL CLOSURE LENGTH

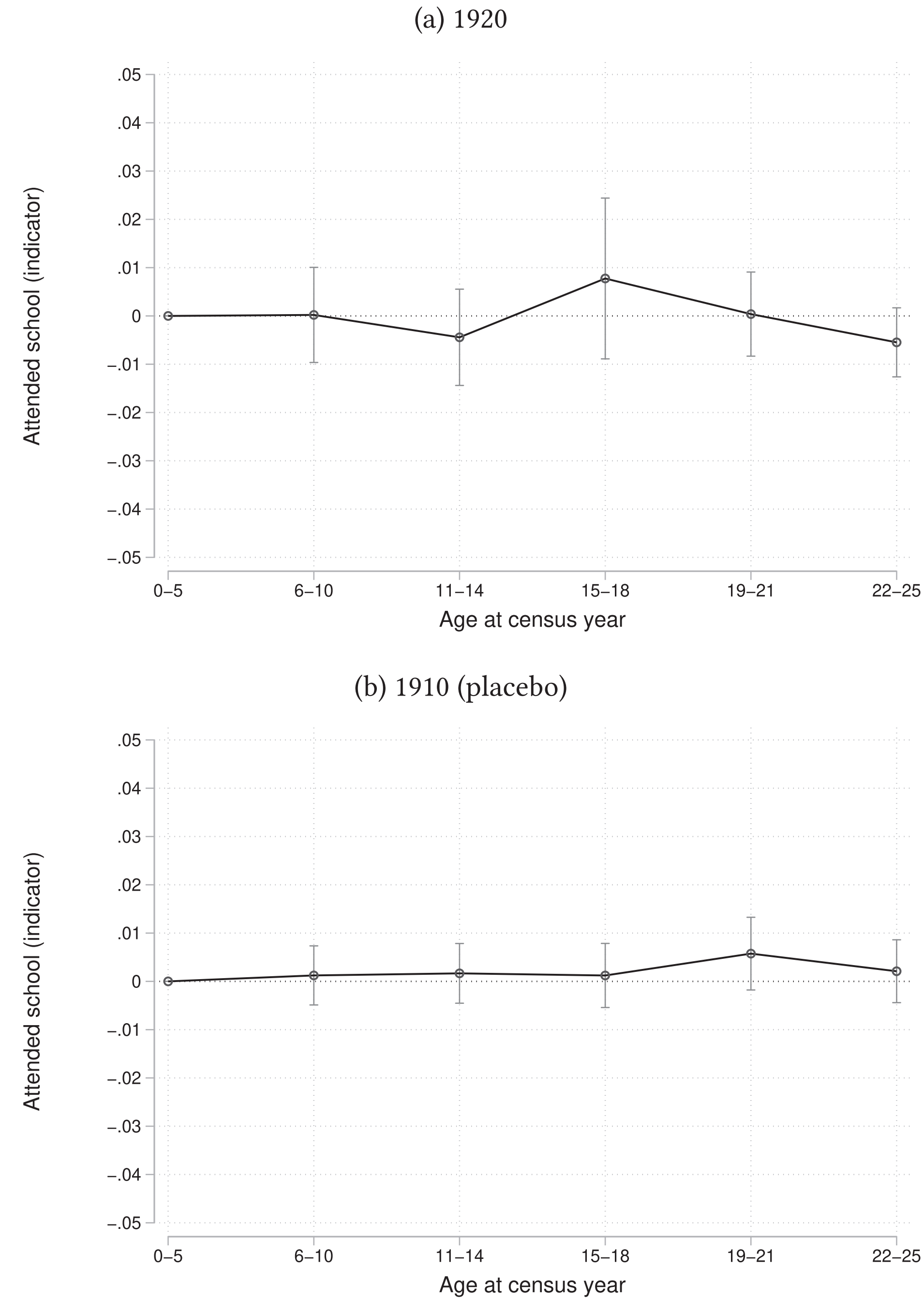


These figures show the fraction of respondents who report school attendance by reported age group in the census. Each subfigure has three lines, separately plotting average school attendance among children who lived in cities that closed schools for 0–21, 22–35, and 36+ days in 1918–1919. Panel a shows average school attendance as reported in the 1920 census; panel b shows average school attendance from the 1910 census. Panel b is a placebo because city closures in 1918–1919 could not have affected school attendance in 1910. The figures show that cities that closed their schools for longer periods had higher rates of school attendance at age 15–21, but the magnitude of that difference is similar in 1910 and 1920.

affected attendance rates for school-aged children in 1920. For example, the estimated coefficient on school closures for the age group 11–14 is almost exactly zero. For each three-week period that a city closed its school system during the pandemic, these children were no less likely to attend school in 1920 due to closures than the omitted category (children newborn to age 5). We find similar null results for other school-aged children in 1920, consistent with the raw school attendance rates plotted in figure 2.

The confidence intervals in panel A of figure 3 are small enough for us to reject even modest differences in school attendance rates for affected cohorts. For example, the 95% confidence interval on the estimated $\beta_{age \in [11,14]}$ coefficient for 11–14-year-old children ranges from −0.014 to 0.006—thus

Figure 3.—Relationship between Days Schools Closed during 1918 Influenza Pandemic and School Attendance, by Age Group and Census Year



These figures plot the β_a coefficients from equation (1) estimated separately for 1920 (panel a) and 1910 (panel b) full-count decennial census data. The estimating equation is

$$Y_i = \beta_a WeeksClosed_c \times AgeGroup_a + \gamma_c + \Pi \times X_i + \Delta \times V_{c,b} + \omega_{r,b} + \epsilon_i,$$

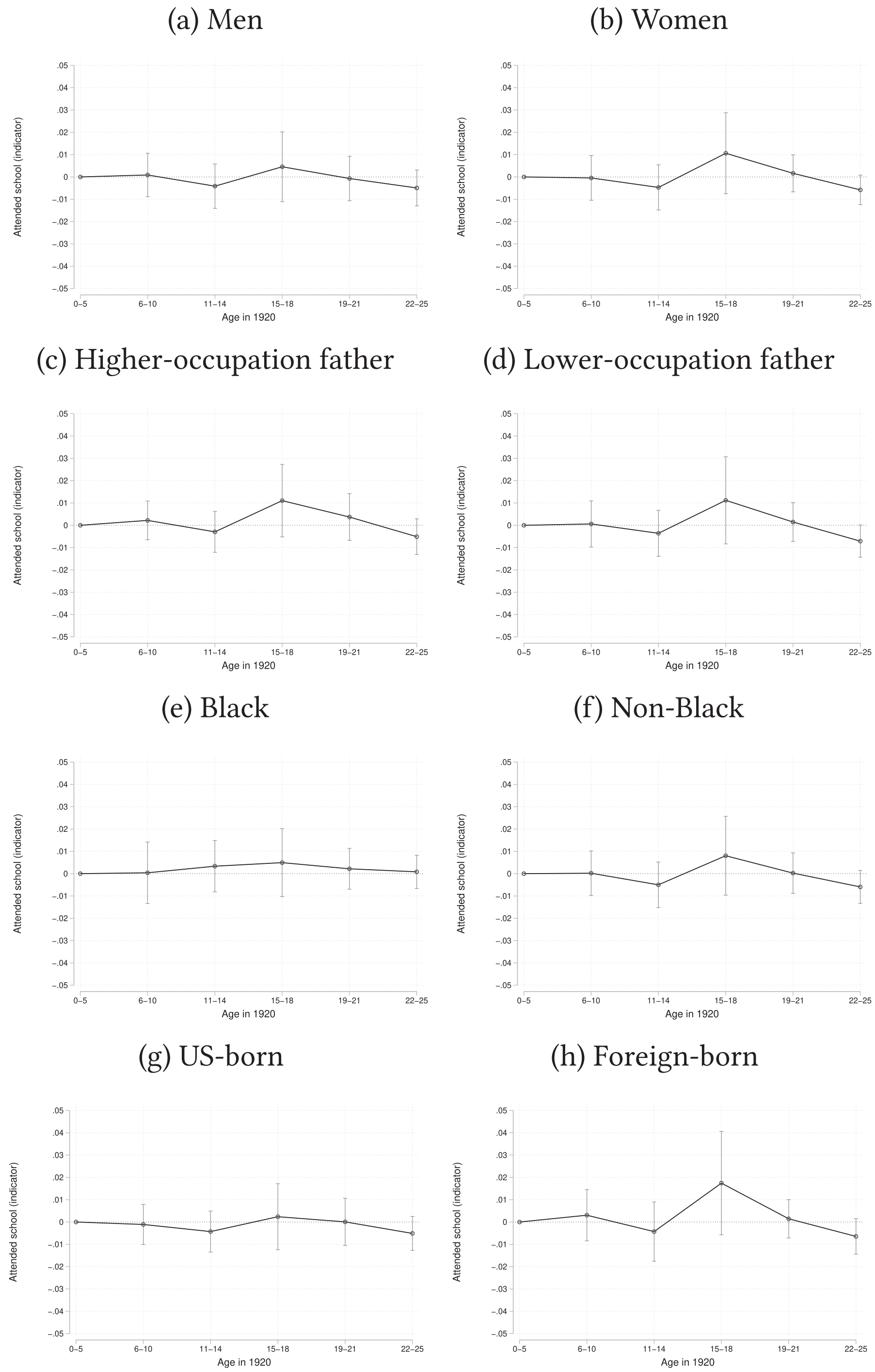
where Y_i is an indicator measuring whether each child i was attending school and $WeeksClosed_c$ describes the number of weeks that schools were closed during the 1918–1919 school year in child i's city c, in three-week increments. Age groups a group children into six age bins in the census (aged newborn to 5, 6–10, 11–14, 15–18, 19–21, and 22–25). X_i is a vector of personal characteristics, $V_{c,b}$ a matrix of controls containing birth-year fixed effects linearly interacted with characteristics of each city in 1910, and $\omega_{r,b}$ are census region-by-birth year fixed effects. Newborns to five-year-olds are the omitted category, and robust standard errors are clustered by city. The figures show the similar relationships between school closure length during the 1918–1919 pandemic and school attendance rates by age group in 1910 (a placebo year) and 1920 (a postpandemic year).

the left edge of the confidence interval implies that 21 days of school closure plausibly caused at most a 1.4% drop in the probability of attending school.[18] This bounding exercise leads to similar conclusions for the other age groups shown in panel A.

In panel B of figure 3, we estimate equation (1) using a placebo 1910 sample. We use the full count census in 1910 to observe 1910 school attendance information, analogous to our baseline 1920 strategy. However, since school closures

[18]Twenty-one days is an approximately one standard deviation increase in the number of days that a city closed its schools in our sample.

FIGURE 4.—RELATIONSHIP BETWEEN DAYS SCHOOLS CLOSED AND 1920 SCHOOL ATTENDANCE, HETEROGENEITY BY STUDENT CHARACTERISTICS



Plots of the β_a coefficients from equation (1) estimated separately for each indicated subgroup of students. The outcome is an indicator measuring whether each child i was attending school.

in 1918 could not have affected school attendance in 1910, we should observe null results. The results in panel B (1910) show similar nulls as our baseline 1920 results. This suggests that the 1920 results are not driven by any time-invariant age-specific selection that our model fails to capture. This regression-based analysis is also consistent with the raw schooling patterns displayed in figure 2 across 1910 and 1920.

In figure 4 we show results from separately estimating equation (1) on subsamples defined by (1) race, (2) gender, (3) parental occupational prestige, and (4) parental nativity. Each figure shows similar null results as our baseline findings, suggesting that there were no heterogeneous effects of school closures on attendance across these dimensions.

We also show that our results are robust to modifications of our baseline specification. First, in figure A4 we show results after conditioning on excess 1918–1919 influenza mortality interacted with age-bin fixed effects. Mortality may be a bad control in this context, because prior work has shown that school closures may directly affect mortality (e.g., Markel et al., 2007). However, figure A4 shows that our results are similar in both 1920 and 1910 if we control for excess mortality. In figure A5, we show results from a specification that conditions on state-by-birth year fixed effects. These models absorb any state-level policies that may have differentially affected some children during this time period relative to others. The resulting estimates are similar to our baseline findings. In addition, figure A5 shows that our results are similar if we cluster standard errors by state instead of city. Figure A6 shows that results are similar if we use the inverse hyperbolic sine transformation of days schools closed as our treatment or if we exclude cities that closed for zero days from our sample.

We also use data on the total length of NPIs imposed by 50 cities compiled by Markel et al. (2007) and Berkes et al. (forthcoming) to test whether our results are robust to conditioning on the length of nonschooling NPIs enacted during the pandemic. Figure A7 estimates our baseline model for this 50 city subsample without (panel A) and with (panel B) controls for nonschooling NPI days interacted with age-bin fixed effects. Results for this subsample are similar to the main sample and are not affected by controlling for nonschooling NPI length.

Finally, we explore the possibility that our null results could have been driven by laws that required students to stay in school. To do so, we use data on compulsory schooling and related child labor laws compiled by Goldin and Katz.[19] In 1920, 43 of 48 states had a work permit age of 14 or lower. None of these states required more than eight years of education in order to receive a work permit, leaving scope for us to see impacts on school-aged children. We show that the remaining sample of children in the five states with more stringent compulsory schooling laws is not driving our null results. First, we exclude children 14 or older who could not work due to state laws from our sample. Next, we include a control variable for whether a given child could work. In both cases, we observe similar results as our main specification, as shown in figure A8.

C. *Long-Run Results*

In this subsection, we estimate whether school closures had any long-run consequences for educational attainment and labor market outcomes based on the sample of linked men described in section IIIB. We estimate versions of equation (1), where Y_i is a 1940 measure of educational attainment or a labor market outcome. We assign the city c as the city where we observe each child in the 1920 census, and other variables are as previously defined.

Figure 5 shows that the 1918–1919 school closures had little effect on years of educational attainment and labor market outcomes in 1940. Our point estimates are close to zero, and our 95% confidence intervals rule out large changes in our outcomes of interest. For example, in all cases our 95% intervals for the effect of a 21-day school closure are bounded ±0.1 years of schooling. Relative to a separate literature on pandemics and long-run schooling (Almond, 2006; Meyers & Thomasson, 2021; Beach, Brown et al., 2022; Li & Malmendier, 2022),[20] our results are small and suggest little impact of school closures on children.[21] These findings are consistent with our null short-run results on school attendance, which would be the main mechanism that we believe could lead to long-run effects. Our short-run results are more precisely estimated because we can use all relevant respondents in 1910 and 1920 without linking to 1940.

As with the short-run results, we look for evidence of heterogeneous long-run effects by estimating our model on subsamples of the population (figures A9–A11). Except for Black men (figure A9), where we find imprecisely estimated evidence that school closures could have mattered, point estimates are generally statistically insignificant and close to zero. Our null long-run effects are also robust to controlling for excess pandemic mortality (figure A12) or state-by-birth year fixed effects (figure A13). Figure A14 shows that our results are similar if we cluster standard errors by state.[22] Finally, our long-run results are not affected by using the inverse hyperbolic sine transformation of our treatment variable (figure A15), excluding zero closure cities (figure A16), accounting for differences in child labor laws across states (figures A17 and A18), or using binary milestone measures of schooling (e.g., completing eight or twelve years of school) as outcomes (figure A19).

V. Conclusion

During pandemics, governments implement a variety of NPIs to combat infectious agents, including limiting the size of gatherings, curtailing business activities, mandating masking and social distancing, and closing schools. These interventions implemented during the COVID-19 pandemic reignited interest in evaluating the short- and long-term effects of NPIs.

In this paper, we analyze the NPI most likely to affect children: school closures. We estimate how school closures during the 1918–1919 pandemic affected school attendance, educational attainment, and labor market outcomes as adults. Using new data on the timing of 1918–1919 school closures across U.S. cities, we find null effects of school closure length

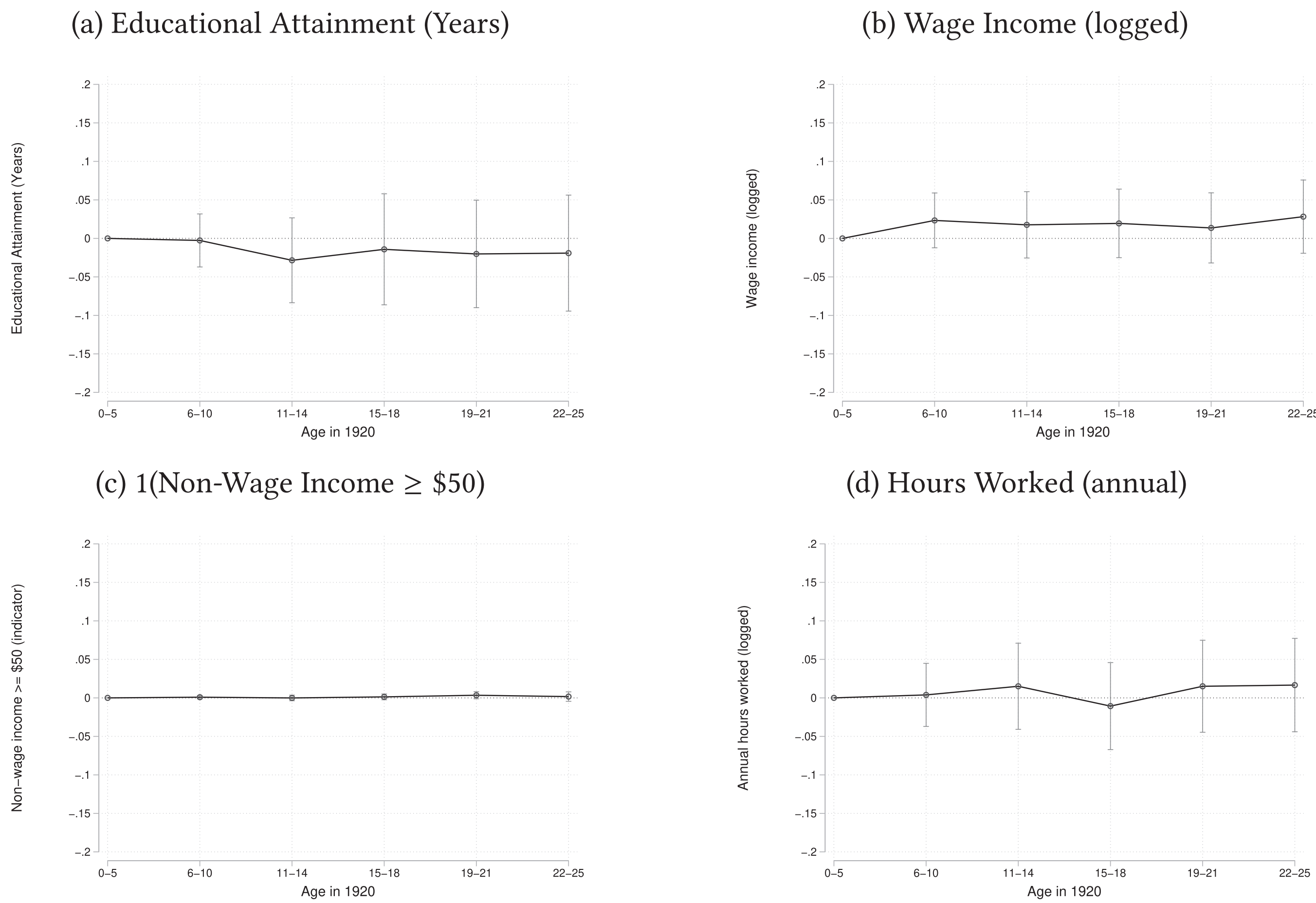
[19]These data are located at https://scholar.harvard.edu/goldin/pages/data.

[20]Li and Malmendier (2022) go beyond an analysis of the effect of the 1918–1919 pandemic on children and examine the effects of 1918–1919 school closures on the long-run outcomes of children in a smaller sample of fifty cities. They rely on an instrumental variable approach using pandemic severity as an instrument for school closure duration.

[21]Our 95% confidence levels allow us to reject the majority of "analogous" effects from these papers, though it is difficult to directly compare magnitudes because we are studying different treatments.

[22]As in the short-run results, our long-run findings are not affected by controlling for nonschooling NPI length for the subsample of fifty cities with available data.

FIGURE 5.—RELATIONSHIP BETWEEN DAYS SCHOOLS CLOSED DURING 1918 INFLUENZA PANDEMIC AND 1940 OUTCOMES



Plots of the β_a coefficients from equation (1). Outcomes are each measured in the 1940 census. The estimating equation is

$$Y_i = \beta_a WeeksClosed_c \times AgeGroup_a + \gamma_c + \Pi \times X_i + \Delta \times V_{c,b} + \omega_{r,b} + \epsilon_i,$$

where Y_i is the indicated outcome and $WeeksClosed_c$ describes the number of weeks that schools were closed during the 1918–1919 school year in child i's city c, in three-week increments. Age bins a groups children into six age bins (aged newborn to 5, 6–10, 11–14, 15–18, 19–21, and 22–25). X_i is a vector of personal characteristics, $V_{c,b}$ a matrix of controls containing birth-year fixed effects linearly interacted with characteristics of each city in 1910, and $\omega_{r,b}$ are census region-by-birth year fixed effects. Newborns to five-year-olds are the omitted category, and robust standard errors are clustered by city. The figures show null effects of school closures in 1918–1919 on 1940 human capital and labor market outcomes.

on 1920 school attendance. Linking affected children to the 1940 census, we also find little evidence of long-run schooling or labor market impacts; point estimates are close to zero with standard errors that rule out sizable effects. We find no evidence that these null short- and long-run effects differ across student characteristics, including socioeconomic status, race, and parental nativity.

Our results illustrate important differences between school closures in 1918–1919 and 2020–2021. While school closures in 2020 often lasted for months, the average school closed in 1918–1919 for many fewer days. Moreover, the 1918 virus led to high absentee rates—in some cases over 50%—in schools that stayed open, in part because the 1918–1919 virus was a serious health risk to children and young parents. This highlights that the effects of pandemic mitigation policy are difficult to forecast when agents simultaneously change their behavior in response to threats. Finally, the lack of effective remote learning in 1918 may have limited the scope for heterogeneous effects to emerge. Unlike today, children in 1918 with more household resources did not necessarily have the ability to continue to learn at a higher rate. Given these differences, it is unsurprising that we find little effects of 1918–1919 school closures on the school-age population.

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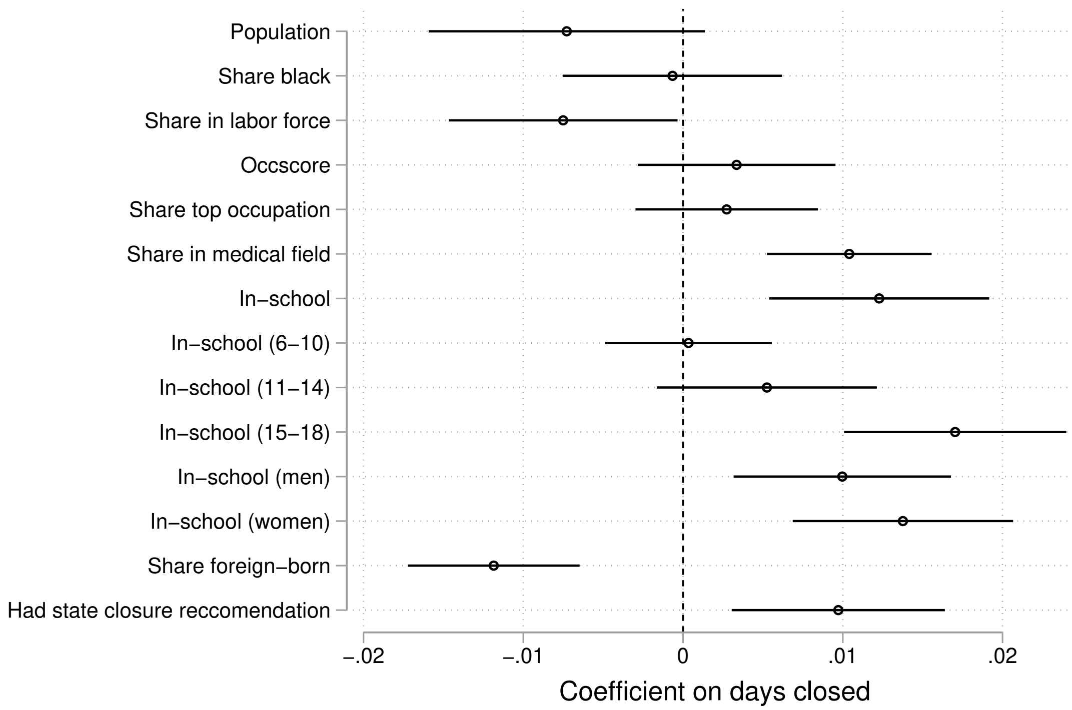
Table A1: Summary statistics: decennial census samples

	Short-Run 1910 Sample		***Short-Run 1920 Sample***		***Matched 1920–1940 Sample***	
	Mean	Obs	Mean	Obs	Mean	Obs
Male (%)	0.50	12,685,646	0.49	15,030,821	1.00	2,310,150
Variables from 1910–1920						
White (%)	0.95	12,685,646	0.94	15,030,821	0.96	2,310,150
Age (in Childhood Census)	12.97	12,685,646	12.52	15,030,821	12.22	2,310,150
Attending School (%)	0.38	12,685,646	0.38	15,030,821	0.41	2,310,150
Attending School Age 0–5 (%)	0.07	2,961,281	0.06	3,698,605	0.07	571,017
Attending School Age 6–10 (%)	0.92	2,176,404	0.88	2,847,966	0.87	468,592
Attending School Age 11–14 (%)	0.95	1,706,601	0.93	2,101,684	0.94	339,291
Attending School Age 15–18 (%)	0.44	1,884,066	0.39	2,018,221	0.45	316,478
Attending School Age 19–21 (%)	0.08	1,617,893	0.09	1,694,269	0.13	250,092
Attending School Age 22–25 (%)	0.02	2,339,401	0.02	2,670,076	0.04	364,680
Number of Days Closed	25.05	12,685,646	25.56	15,030,821	27.71	2,094,082
Variables from 1940						
Educational Attainment (years)					10.33	2,266,496
Wage Income ($)					1,150.87	2,214,413
1(Non-Wage Income $\geq$ $50)					0.19	2,238,024
Hours Worked (Annual)					1,576.75	2,310,150
Weeks Worked (Annually)					39.80	2,310,150
Hours Worked (Weekly)					34.40	2,310,150

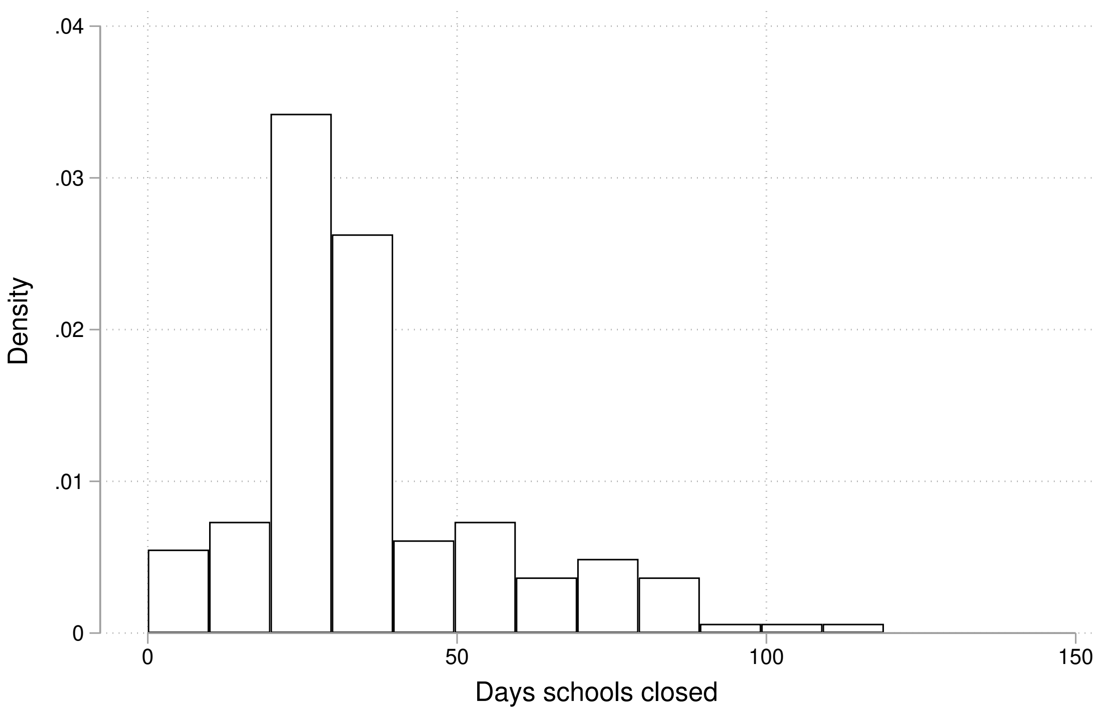
Notes: This table shows summary statistics describing the three samples of decennial census data that we use in our analysis: (a) the set of all 0–25-year-olds in the 1910 decennial census, (b) the set of all 0–25-year-olds in the 1920 decennial census, and (c) the set of all male 0–25-year-olds in the 1920 decennial census who we match forward to adult records in the 1940 decennial census. Samples are restricted to children who resided in one of the cities that comprise our final analysis sample.

Figure A1: Relationship between 1918–19 school closure length, 1910 city demographics, and state closure orders



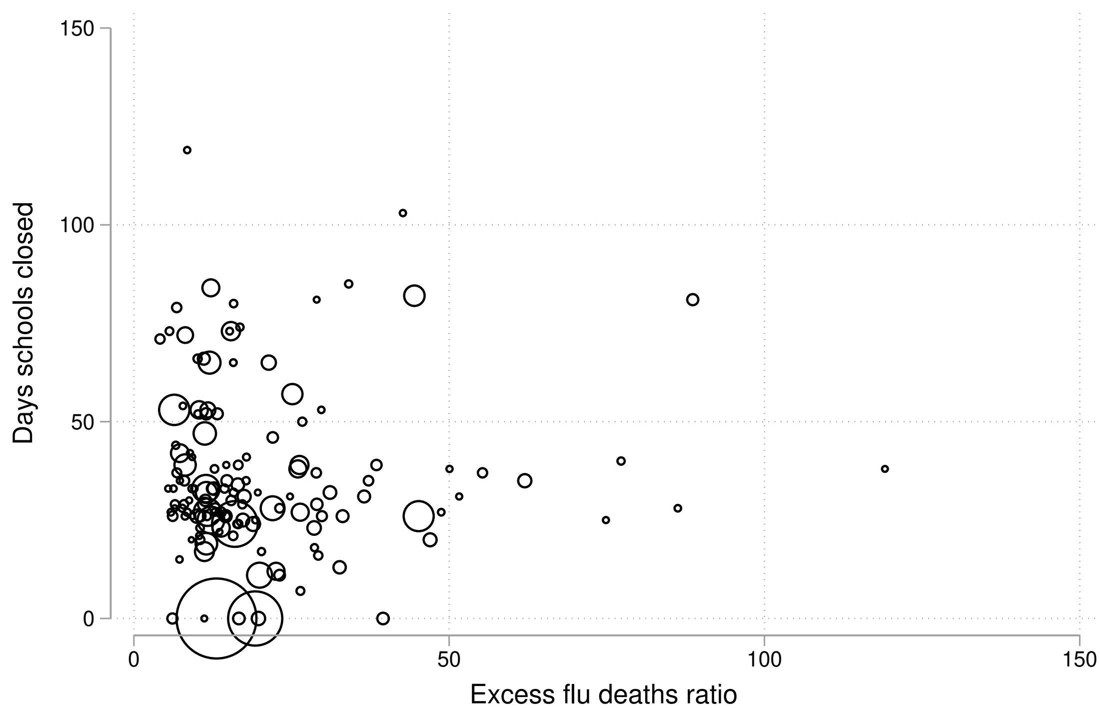
This figure plots coefficients from separate regressions with the indicated variable as the outcome and days schools closed in 1918 and 1919 as the independent variable. Each outcome variable is standardized to have mean zero, standard deviation one. All demographics and individual characteristics are calculated from the 1910 full-count decennial census and each observation is a city. State closure recommendations indicate states that had a direct order or a recommendation for schools to close at some point during the 1918–19 pandemic. Overall school attendance rates are calculated for 6-18-year-olds. 95 percent confidence intervals calculated with robust standard errors.

Figure A2: Histogram of days schools closed in 1918–19, by city



This figure shows the distribution of days that schools where closed during the pandemic in 1918–19 for our sample of cities.

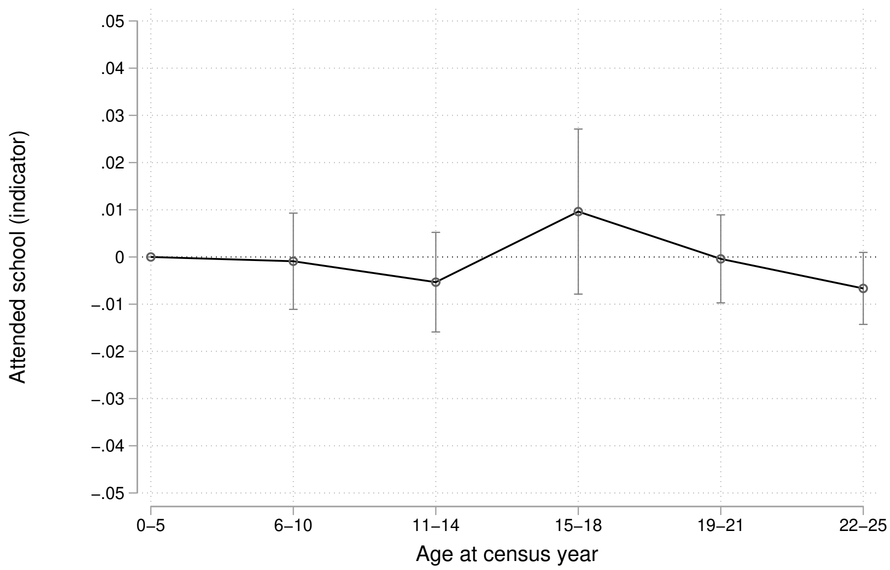
Figure A3: Scatterplot of school closure length against excess pandemic flu death ratios



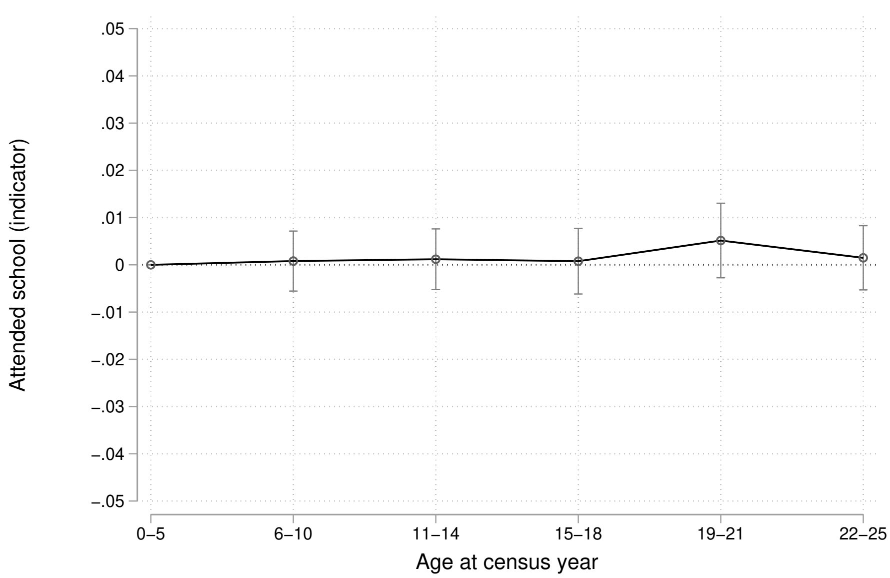
This figure shows the relationship between 1918–19 school closures and excess 1918–19 pandemic flu deaths ratios. Each dot is a city and dot sizes are weighted by population.

Figure A4: Relationship between days schools closed during 1918 influenza pandemic and school attendance by census year, with mortality controls

(a) 1920



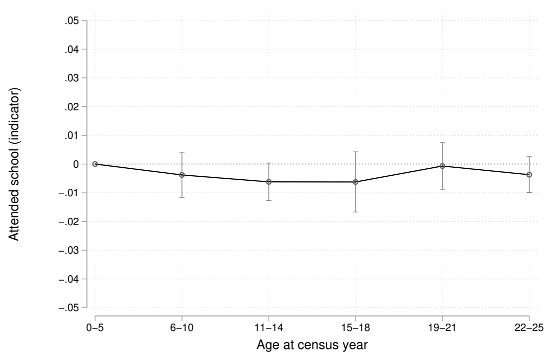
(b) 1910 (placebo)



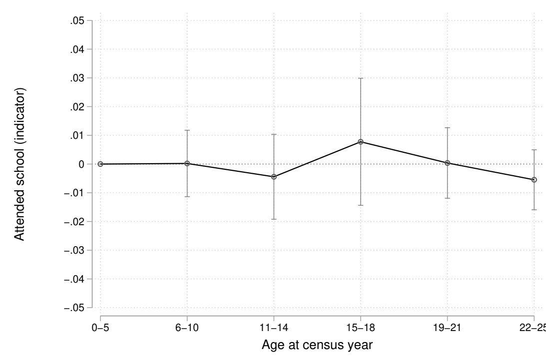
These figures plot the β_a coefficients from Equation 1 estimated separately for 1920 (Panel a) and 1910 (Panel b). These models also include include controls for excess mortality ratios interacted with age at census year fixed effects.

Figure A5: Relationship between days schools closed during 1918 influenza pandemic and school attendance by census year, specification robustness

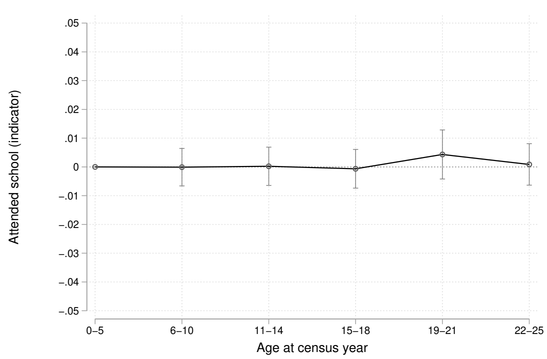
(a) 1920, state-by-birth year fixed effects



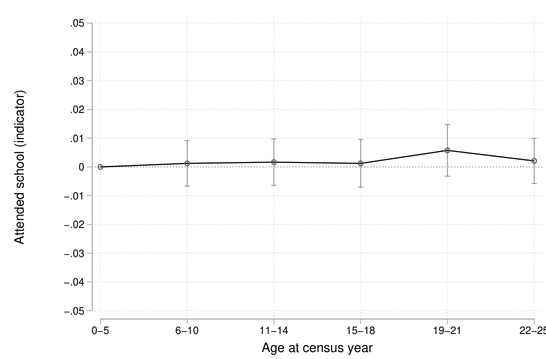
(b) 1920, state-clustered standard errors



(c) 1910, state-by-birth year fixed effects



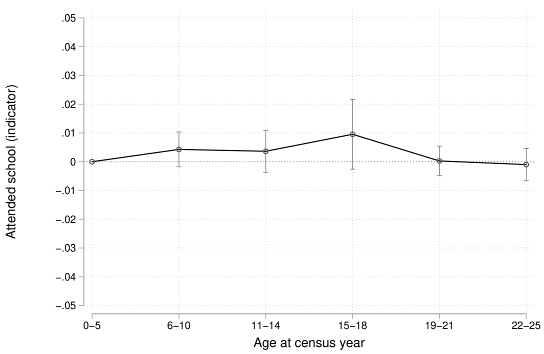
(d) 1910, state-clustered standard errors



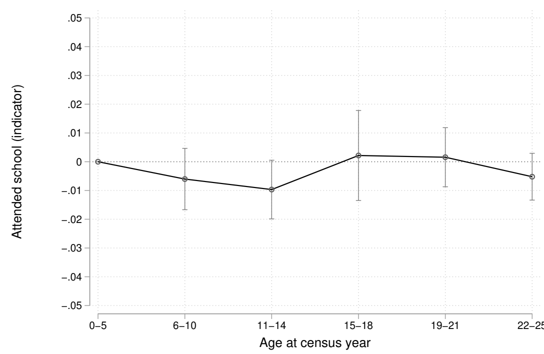
These figures plot the β_a coefficients from Equation 1. Panels A and C include state-by-birth year fixed effects in addition to the baseline covariates. Panels B and D cluster standard errors by state.

Figure A6: Relationship between days schools closed during 1918 influenza pandemic and school attendance by census year, treatment measurement robustness

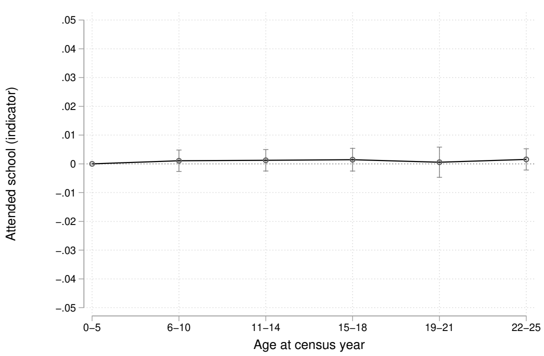
(a) 1920, inverse hyperbolic sine transformation



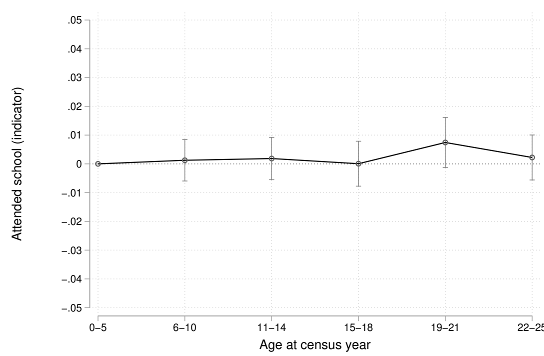
(b) 1920, exclude zero-closure cities



(c) 1910, inverse hyperbolic sine transformation



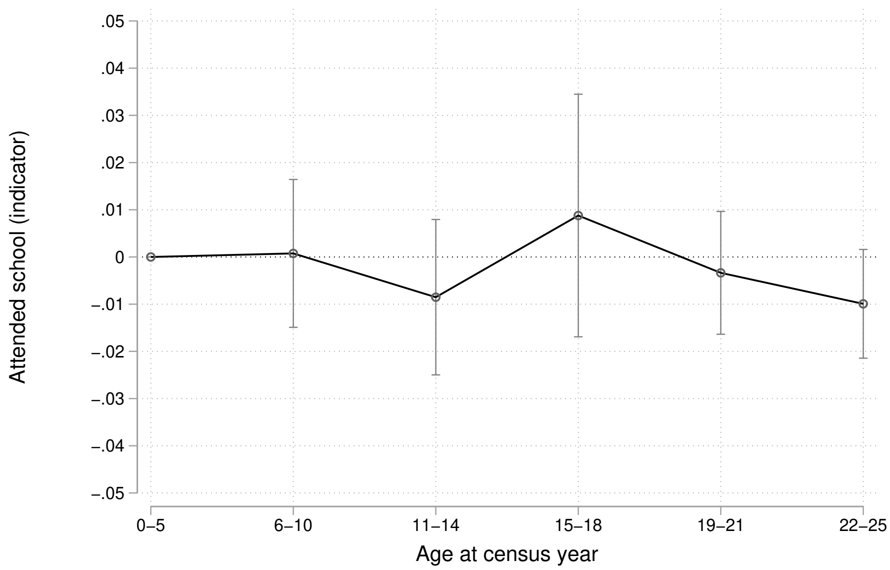
(d) 1910, exclude zero-closure cities



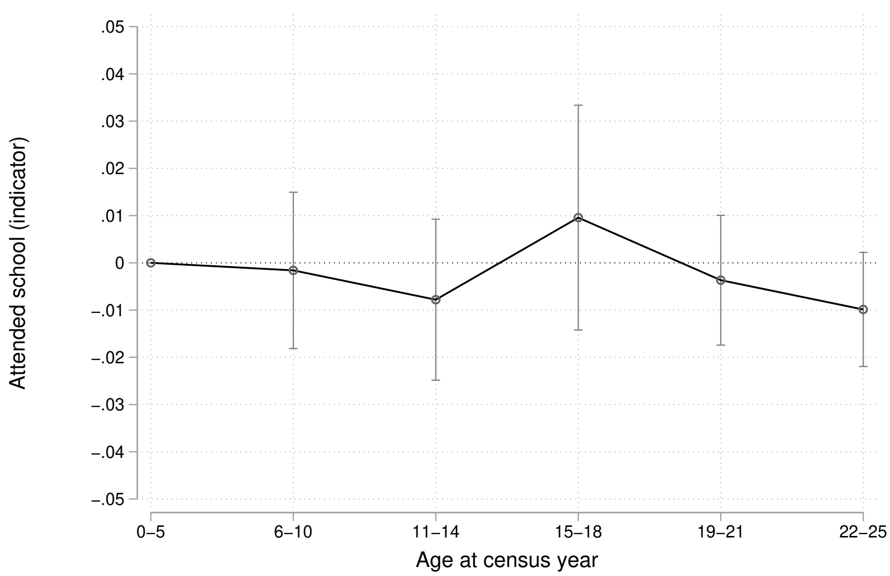
These figures plot the β_a coefficients from Equation 1. Standard errors are clustered by city. Panels A and C use the inverse hyperbolic sine transformation of the treatment variable. Panels B and D exclude cities that closed for 0 days from the estimation sample.

Figure A7: Relationship between days schools closed during 1918 influenza pandemic and school attendance in 1920, with and without other NPI controls

(a) 1920, sub-sample of cities with other NPI info



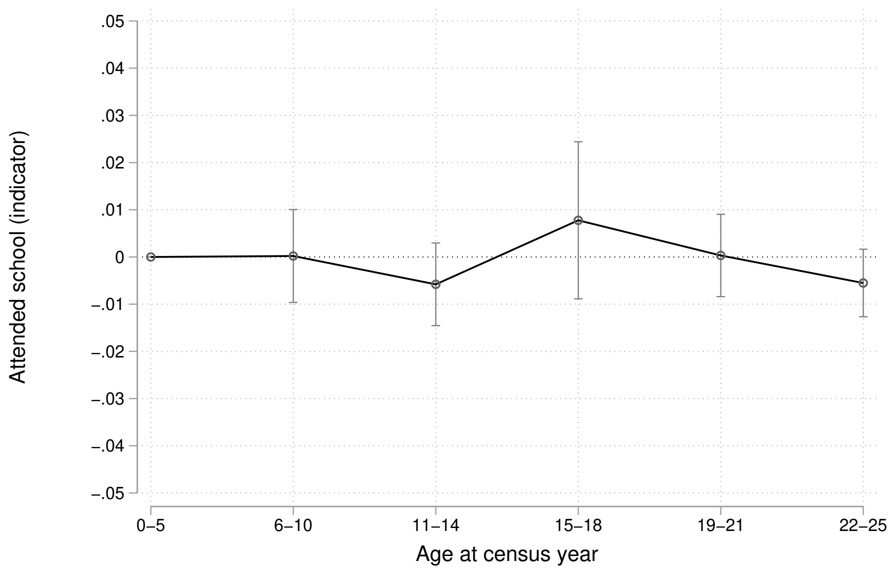
(b) 1920, controlling for other NPI days



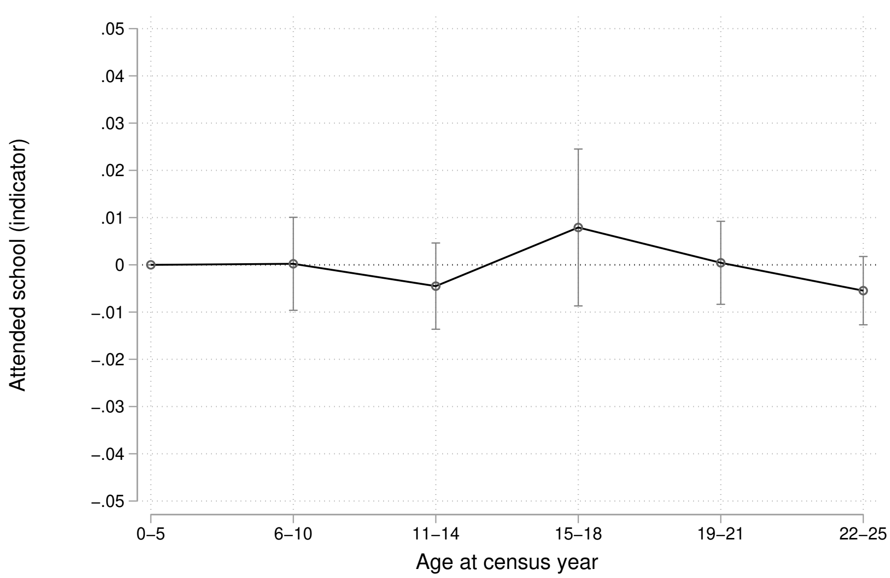
These figures plot the β_a coefficients from equation 1 estimated separately on a subset of cities with data on other NPIs beyond school closures. Panel A shows the baseline results for this sample; Panel B shows the results after conditioning on additional NPI days interacted with age bins.

Figure A8: Relationship between days schools closed during 1918 influenza pandemic and school attendance in 1920, compulsory schooling robustness

(a) 1920, excluding youth 14 or older who could not receive work permits



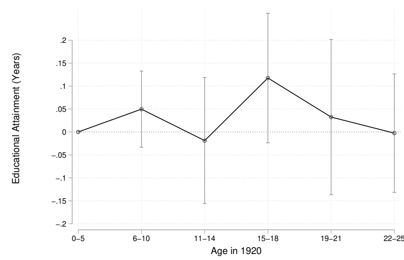
(b) 1920, controlling for whether one can get a work permit



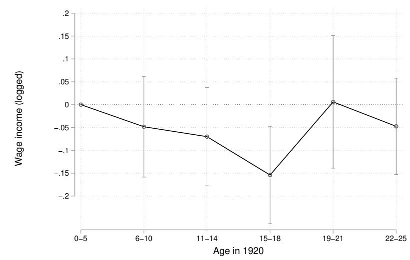
These figures plot the β_a coefficients from equation 1. Panel A excludes youth 14 or older who could not work due to state laws; Panel B includes a control for whether the given youth could work.

Figure A9: Relationship between days schools closed and 1940 outcomes, heterogeneity by student race

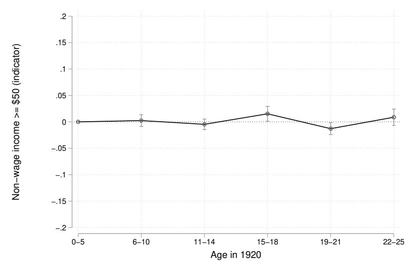
(a) Educational Attainment (Years)
Black



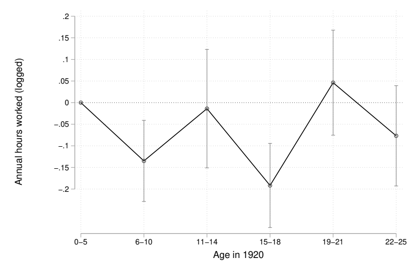
(b) Wage Income (logged)
Black



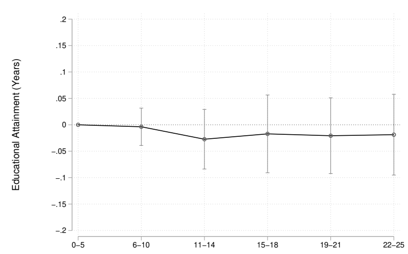
(c) 1(Non-Wage Income ≥ $50)
Black



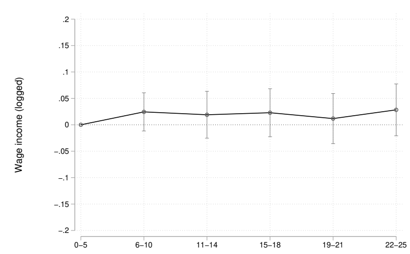
(d) Annual Hours Worked (logged)
Black



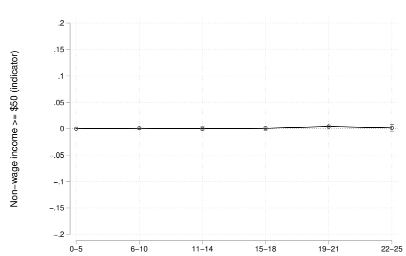
(e) Educational Attainment (Years)
Non-Black



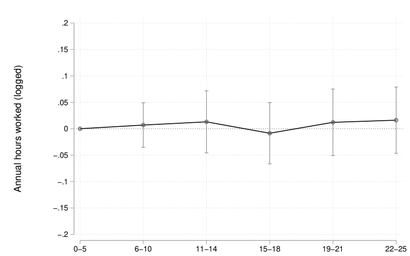
(f) Wage Income (logged)
Non-Black



(g) 1(Non-Wage Income ≥ $50)
Non-Black



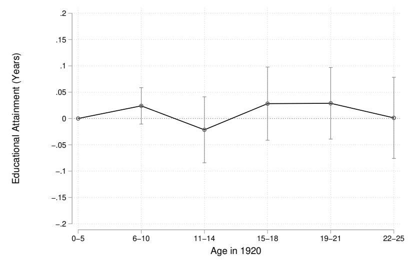
(h) Annual Hours Worked (logged)
Non-Black



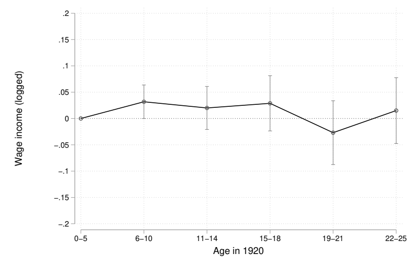
These figures plot the β_a coefficients from Equation 1 estimated separately for each indicated subgroup of students for each indicated outcome.

Figure A10: Relationship between days schools closed and 1940 outcomes, heterogeneity by parental occupational prestige status

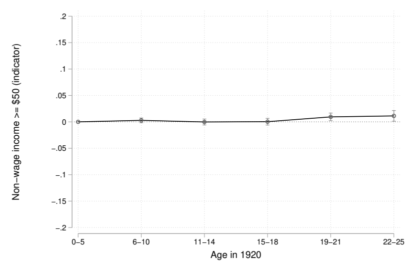
(a) Educational Attainment (Years)
Higher-prestige



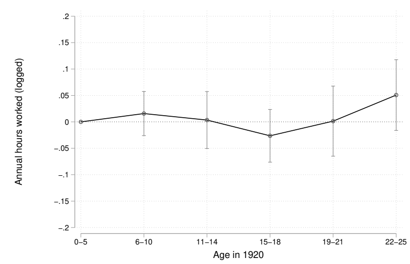
(b) Wage Income (logged)
Higher-prestige



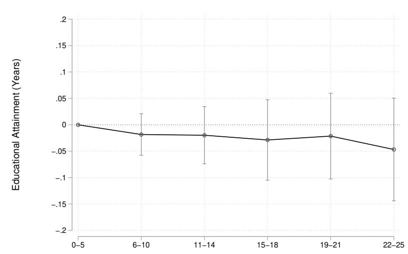
(c) 1(Non-Wage Income ≥ \$50)
Higher-prestige



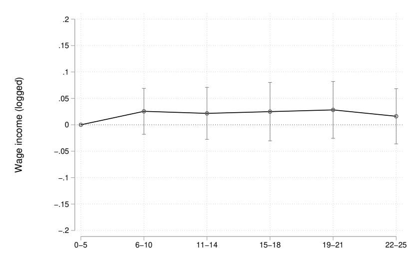
(d) Annual Hours Worked (logged)
Higher-prestige



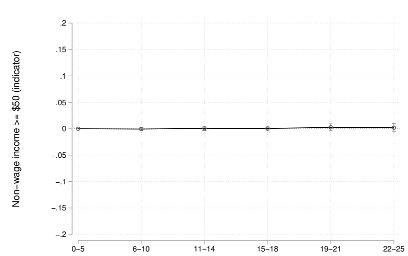
(e) Educational Attainment (Years)
Lower-prestige



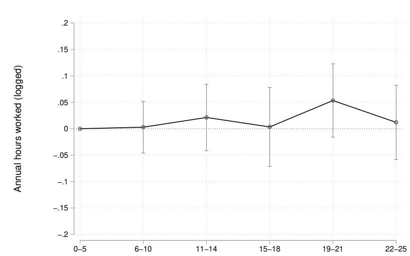
(f) Wage Income (logged)
Lower-prestige



(g) 1(Non-Wage Income ≥ \$50)
Lower-prestige



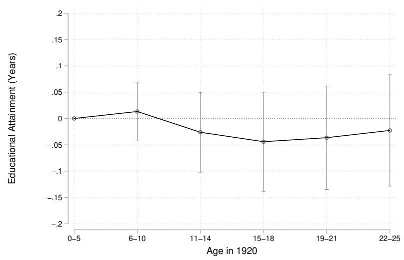
(h) Annual Hours Worked (logged)
Lower-prestige



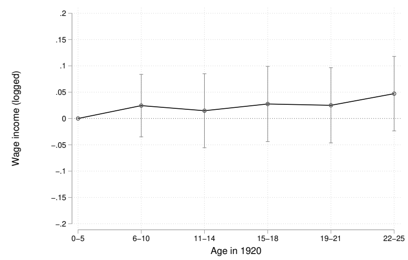
These figures plot the β_a coefficients from Equation 1 estimated separately for each indicated subgroup of students for each indicated outcome.

Figure A11: Relationship between days schools closed and 1940 outcomes, heterogeneity by parental nativity

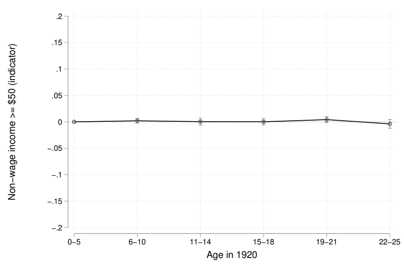
(a) Educational Attainment (Years)
Foreign-born



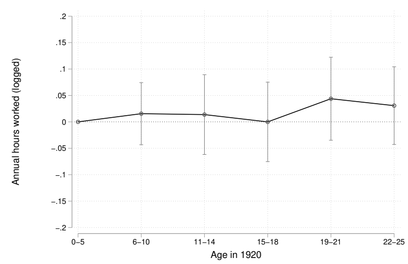
(b) Wage Income (logged)
Foreign-born



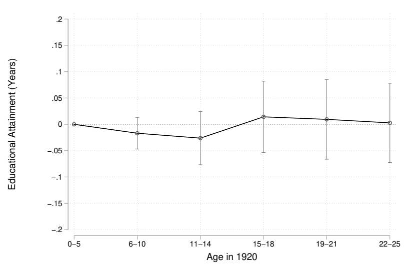
(c) 1(Non-Wage Income ≥ \$50)
Foreign-born



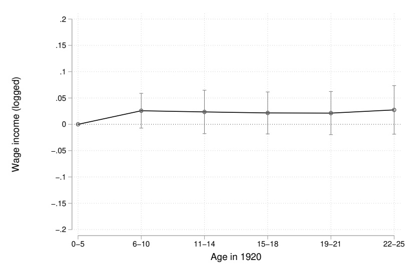
(d) Annual Hours Worked (logged)
Foreign-born



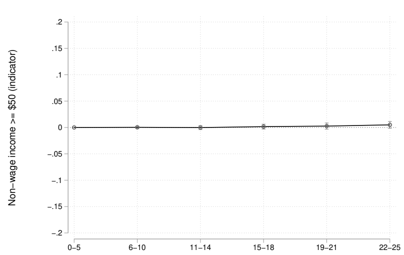
(e) Educational Attainment (Years)
US-born



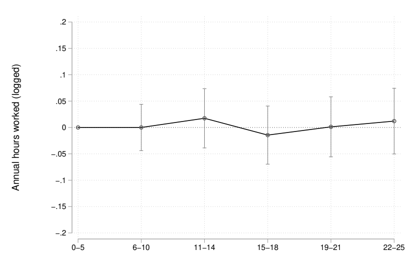
(f) Wage Income (logged)
US-born



(g) 1(Non-Wage Income ≥ \$50)
US-born



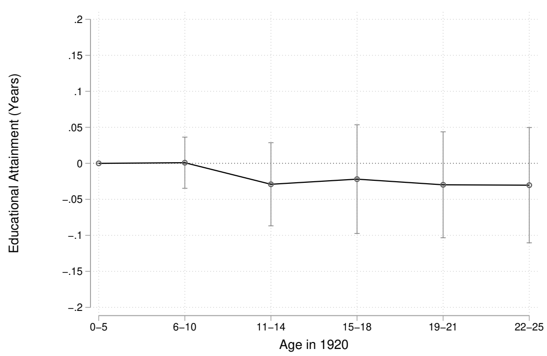
(h) Annual Hours Worked (logged)
US-born



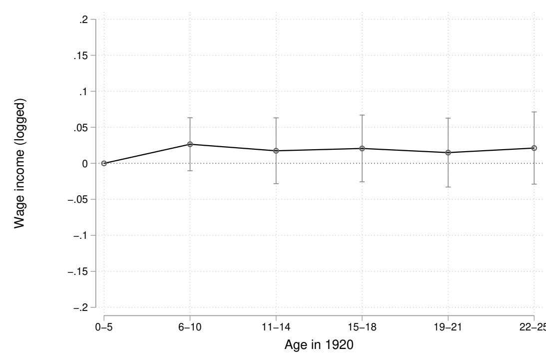
These figures plot the β_a coefficients from Equation 1 estimated separately for each indicated subgroup of students for each indicated outcome.

Figure A12: Relationship between days schools closed during 1918 influenza pandemic and 1940 outcomes, with mortality controls

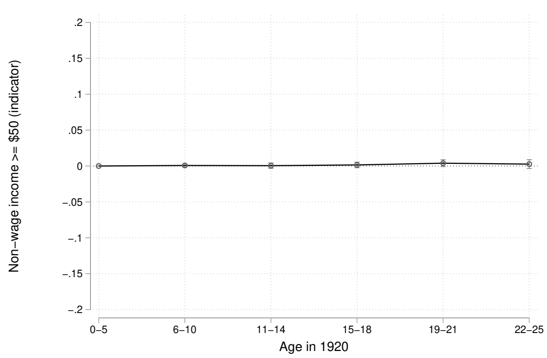
(a) Educational Attainment (Years)



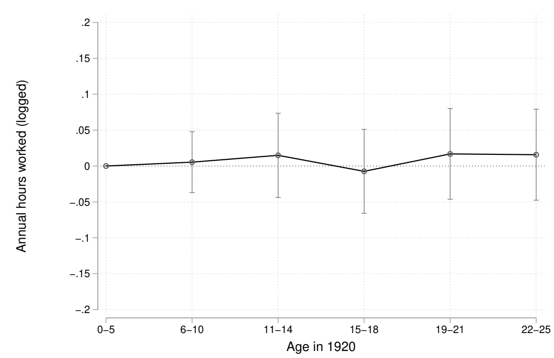
(b) Wage Income (logged)



(c) 1(Non-Wage Income ≥ $50)



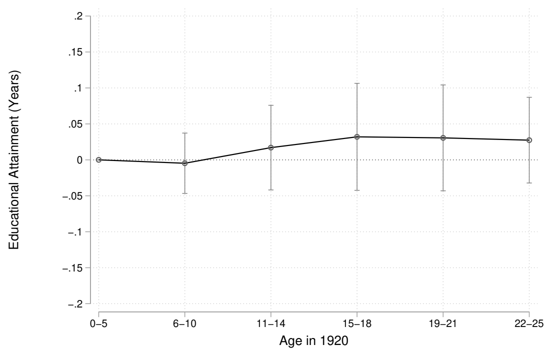
(d) Annual Hours Worked (logged)



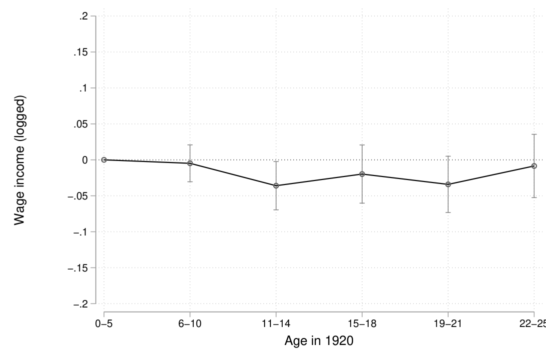
These figures plot the β_a coefficients from a version of Equation 1 that also controls for excess influenza mortality ratios interacted with age bin fixed effects.

Figure A13: Relationship between days schools closed during 1918 influenza pandemic and 1940 outcomes, controlling for state-by-birth year fixed effects

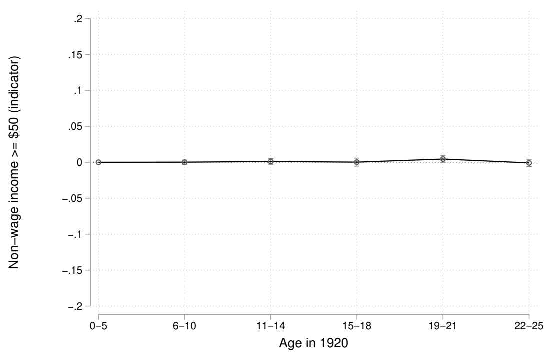
(a) Educational Attainment (Years)



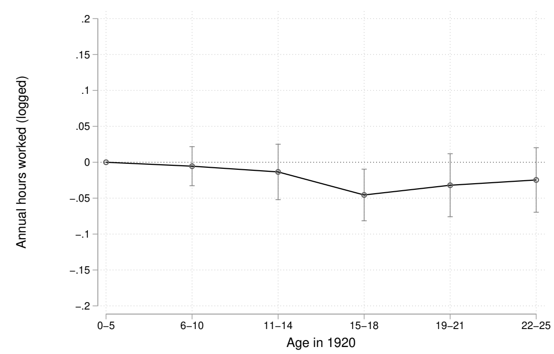
(b) Wage Income (logged)



(c) 1(Non-Wage Income ≥ $50)



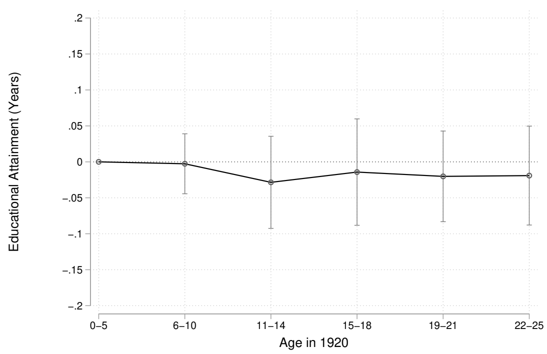
(d) Annual Hours Worked (logged)



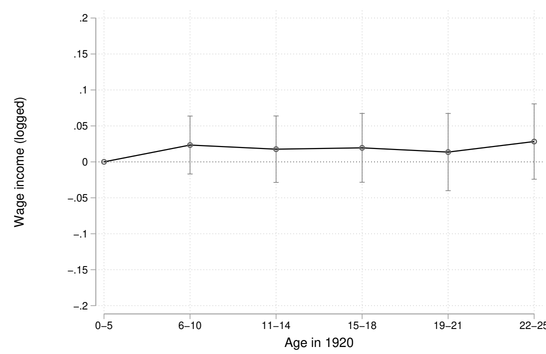
These figures plot the β_a coefficients from a version of Equation 1 that also controls for state-by-birth year fixed effects.

Figure A14: Relationship between days schools closed during 1918 influenza pandemic and 1940 outcomes, with state-clustered standard errors

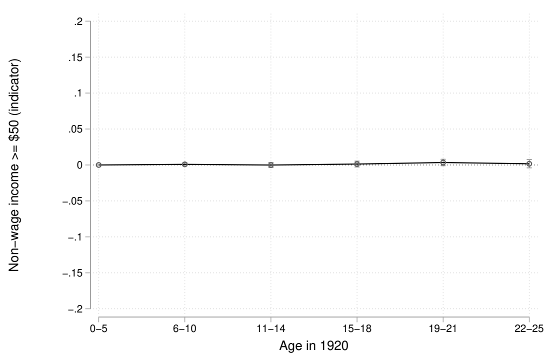
(a) Educational Attainment (Years)



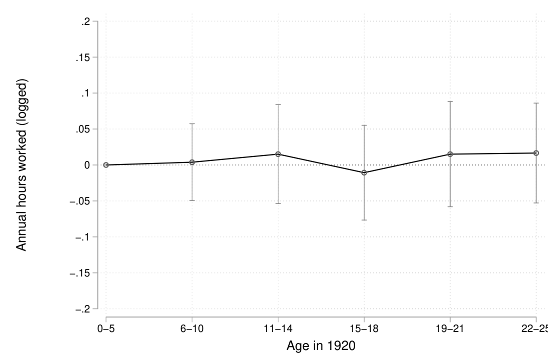
(b) Wage Income (logged)



(c) 1(Non-Wage Income ≥ $50)



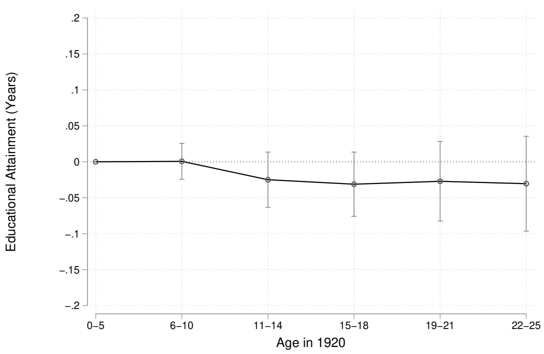
(d) Annual Hours Worked (logged)



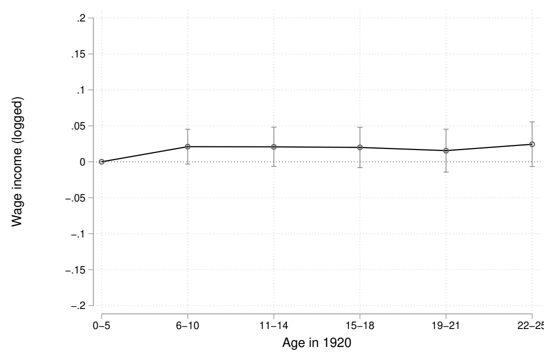
These figures plot the β_a coefficients from Equation 1. Standard errors are clustered by state.

Figure A15: Relationship between days schools closed during 1918 influenza pandemic and 1940 outcomes, inverse hyperbolic sine transformation

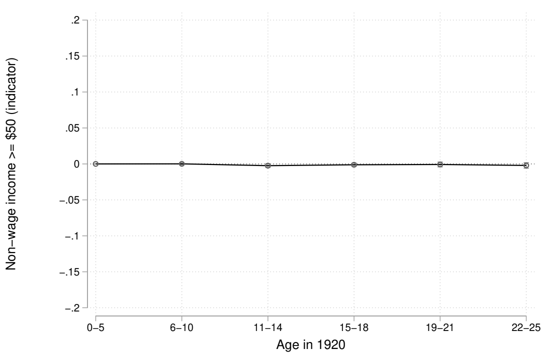
(a) Educational Attainment (Years)



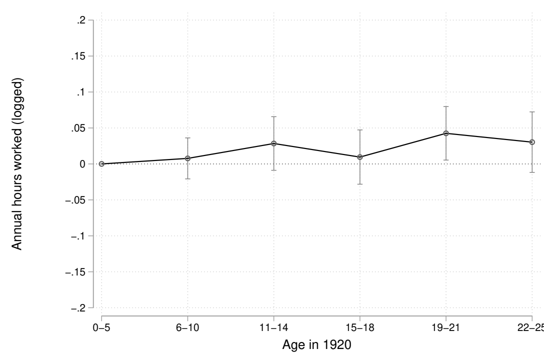
(b) Wage Income (logged)



(c) 1(Non-Wage Income ≥ $50)



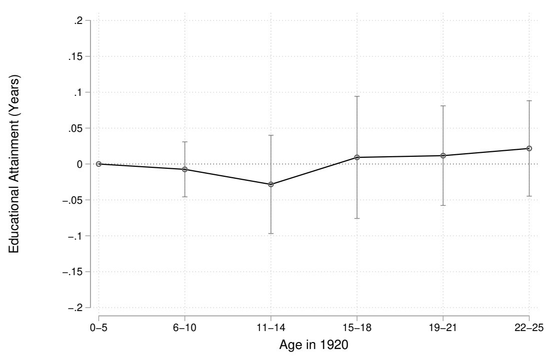
(d) Annual Hours Worked (logged)



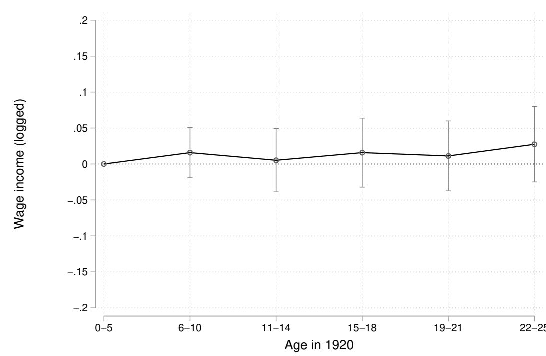
These figures plot the β_a coefficients from Equation 1. Standard errors are clustered by city and we use the inverse hyperbolic sine transformation of days schools closed as the treatment variable.

Figure A16: Relationship between days schools closed during 1918 influenza pandemic and 1940 outcomes, exclude zero-closure cities

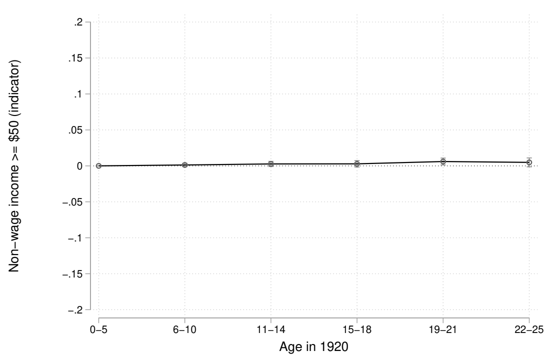
(a) Educational Attainment (Years)



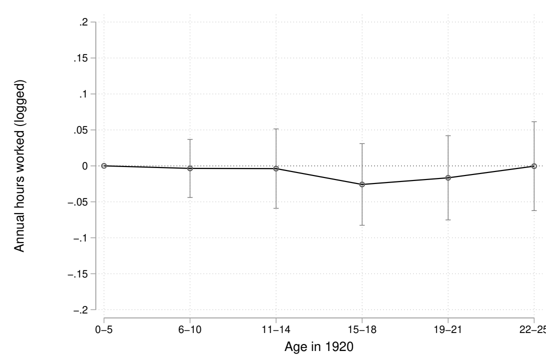
(b) Wage Income (logged)



(c) 1(Non-Wage Income ≥ $50)



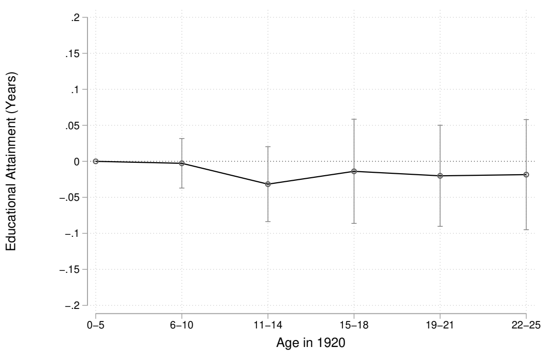
(d) Annual Hours Worked (logged)



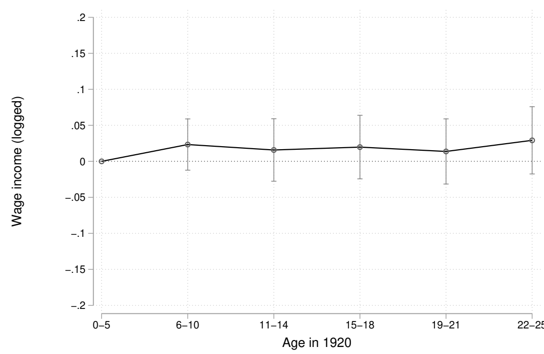
These figures plot the β_a coefficients from Equation 1. Standard errors are clustered by city. We exclude cities that closed for zero days from the estimation sample.

Figure A17: Relationship between weeks schools closed during 1918 influenza pandemic and 1940 outcomes, excluding youth 14 or older who could not work due to state laws

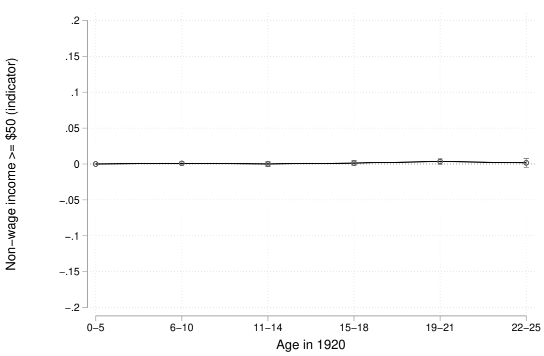
(a) Educational Attainment (Years)



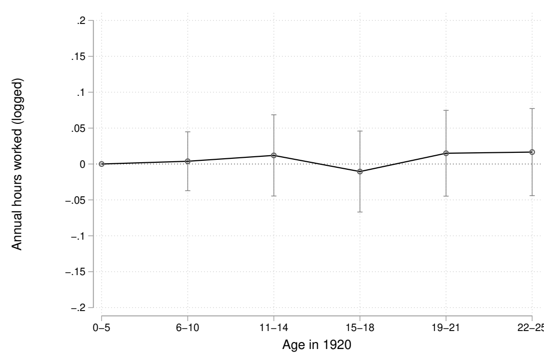
(b) Wage Income (logged)



(c) 1(Non-Wage Income ≥ $50)



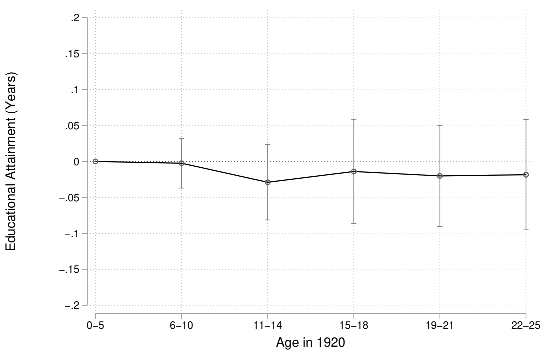
(d) Annual Hours Worked (logged)



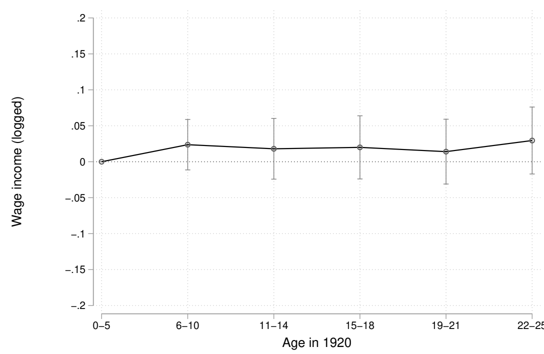
These figures plot the β_a coefficients from Equation 1. Standard errors are clustered by city. We exclude youth from the sample who were 14 or older and could not work due to state laws.

Figure A18: Relationship between weeks schools closed during 1918 influenza pandemic and 1940 outcomes, controlling for whether one can get a work permit in 1920

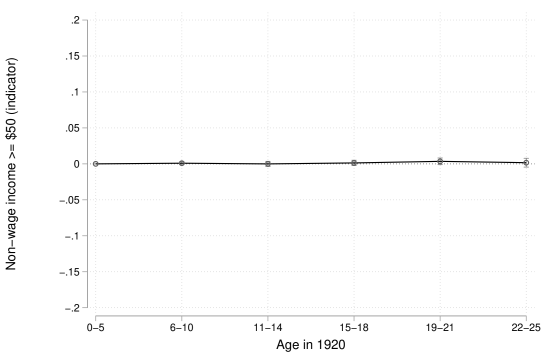
(a) Educational Attainment (Years)



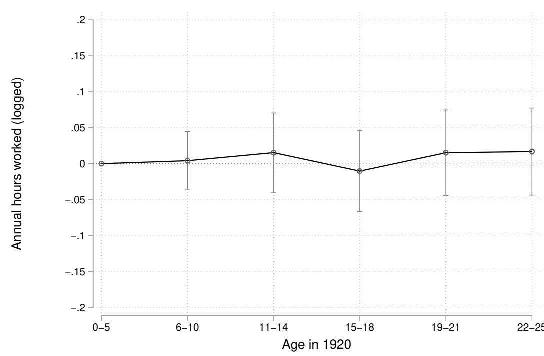
(b) Wage Income (logged)



(c) 1(Non-Wage Income ≥ \$50)



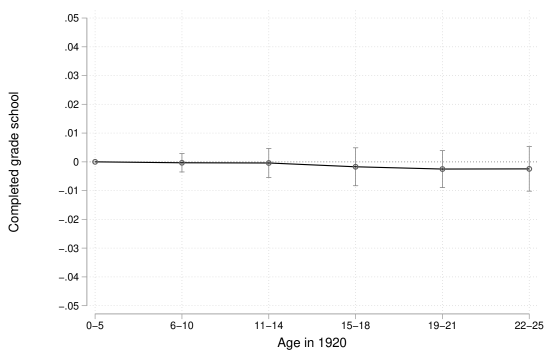
(d) Annual Hours Worked (logged)



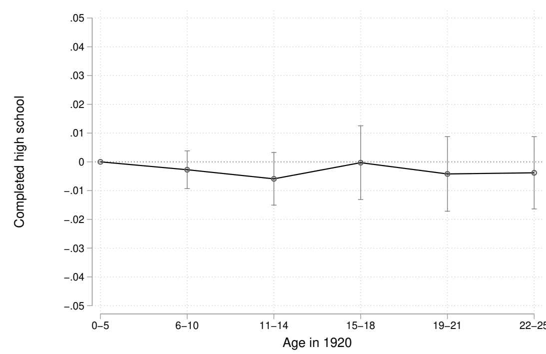
These figures plot the β_a coefficients from Equation 1. Standard errors are clustered by city. Each model also includes a control for whether the given youth could work.

Figure A19: Relationship between weeks schools closed during 1918 influenza pandemic and alternative 1940 education outcomes

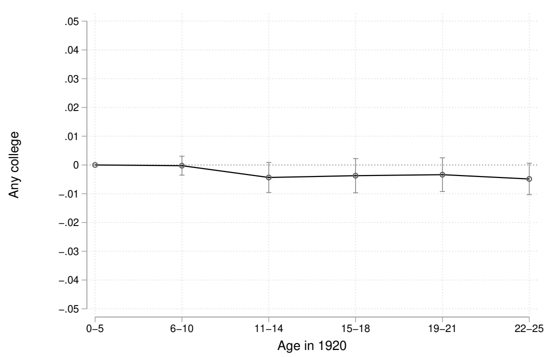
(a) ≥ 8 years of education



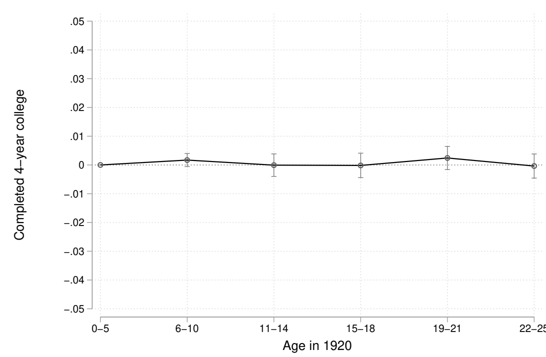
(b) ≥ 12 years of education



(c) > 12 years of education



(d) ≥ 16 years of education



These figures plot the β_a coefficients from Equation 1 for each indicated outcome. Standard errors are clustered by city.