School Closures, Inequality, and Politics – Evidence from the 1918 Spanish Flu*

Karin Li† Ulrike Malmendier‡

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Abstract

We assess the long-term consequences of the 1918 pandemic for school-aged children in the U.S., with an emphasis on the heterogeneous policy measures taken across different states and cities. We document a significant negative effect of the pandemic and pandemic-induced school closures on school attendance after schools re-opened, and on the high-school graduation rates of the affected cohorts. We also estimate significant heterogeneity in the adverse consequences by socio-economic background. We then show that idiosyncratic decisions of local politicians led to measurable differences in the length of school closures and, as a result, differently signed effects of the marginal day of school closure across cities. The estimated effects of the pandemic on educational attainment have a measurable impact on the return to schooling.

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†University of California at Berkeley, karin.li@berkeley.edu.
‡University of California at Berkeley, and NBER, ulrike@berkeley.edu.

1 Introduction

In 2020, the COVID-19 pandemic devastated countries all over the globe, claiming millions of lives, shutting down economies, and upending social life for months on end. The health crisis also led to extended school closures. In many places, children were confined to distance learning for over a year, with severe implications for their learning and mental health and, at least in the U.S., widening inequalities across socio-economic, ethnic, and racial strata.[1] When schools started to re-open, some of the inequalities persisted as educators observed a significant racial gap in children's return to school.[2] In Black and Latino families, parents' reluctance appeared to reflect the pandemic's disproportionate health impact on their communities, as well concerns about their schools' funding and ability to ensure proper sanitation (Gilbert et al., 2020).[3] Since remote learning conditions at home are often worse for minority groups, the health crisis exacerbated inequality in education and, as a result, in future earnings and possibly other outcomes (such as health, crime, and social capital). Not surprisingly, a vehement public debate about political decisions and policy measures ensued.

About one hundred years earlier, the world faced a similar situation. A string of the H1N1 virus triggered the most severe infectious-disease health crisis of the 20th century. The so-called "Spanish Flu" took an estimated 50 million lives (Taubenberger and Morens, 2006). In the U.S. alone, a third of the population were infected and for about 675,000 of those, the infection was fatal (Taubenberger et al., 1997). The epidemic hit in three waves (Spring 1918, Fall 1918, Spring 1919), wherein the second wave in the fall of 1918 was the most severe and deadly.

Like COVID-19, the 1918 influenza was a respiratory disease whose main transmission vector were droplet infections. As such, many policy measures from today mirror those undertaken back in 1918. They included school closures, mask wearing, and shut-down of places of public gathering (e.g. theaters).[4]

In this paper, we assess the long-term consequences of the 1918 pandemic and of

[1] See, for example, Son et al. (2020), Green et al. (2021), Pizarro-Ruiz and Ordóñez-Camblor (2021), or reports such as "COVID-19 and student learning in the United States: The hurt could last a lifetime," by McKinsey & Company (2020)

[2] Cf. reports such as "Missing in School Reopening Plans: Black Families' Trust", The New York Times (02/01/2021) or "The racial divide in returning to the classroom", Axios (02/06/2021)

[3] In Asian families, whose children continued with remote learning at the highest rate across all groups, the high fraction of households with elderly people as well the community's past experiences with viruses played role. Cf. "10,000 CPS students who chose in-person classes later opted out; racial disparities seen in attendance rates", Chicago Tribune (01/28/2021)

[4] Detailed information can be found in the "Influenza Encyclopedia" of the University of Michigan.

the heterogeneous policy measures taken at the time across different states and cities in the U.S. for school-aged children. We assess the effect of the pandemic on school attendance, educational attainment, and future earnings. We also document significant heterogeneity in the adverse consequences of pandemic exposure by socio-economic family background. Finally, we show that idiosyncratic decisions of local politicians about school closures translate into significantly different outcomes.

Compared to other countries, the U.S. is a particularly suitable candidate to study the long-term effects of the 1918 pandemic because of the availability of high-quality data on the health consequences and economic implications, both on the state- and the city-level. Another reason is that daily life in the U.S. was comparatively little affected by World War I, at least compared to other industrialized countries, minimizing the role of confounding factors.

We use four main sources of data. First, we transcribe data on age- and region-specific death counts from the U.S. influenza mortality data tables obtained from the historical records of the Centers for Disease Control and Prevention (CDC). Combined with region-specific population gathered from census data this information allows us to calculate a location- and age-specific measure of pandemic exposure, henceforth referred to as *specific mortality rate*. The average mortality across all age groups within a given state ranges from 0.4% (Washington) to 1.0% (Pennsylvania). When restricting the sample to children below the age of nine, specific mortality ranges from 0.1% (Utah/Minnesota) to 3.6% (Connecticut/Rhode Island/Pennsylvania). When looking at the sample of children older then nine but not older than 18, specific mortality ranges from 0.2% to around 0.5%. Second, we use city-specific information on policy interventions in response to the pandemic, including school closures, days of school closures, and reports about city health officers (CHOs) and local politicians that allow us to identify CHOs that took the existence or severity of the pandemic (too) lightly ("taking the pandemic lightly") and conflict among local politicians. Our third main source of data is the U.S. decennial census. We extract microdata on (individual) economic outcomes from the 1920–1970 census waves. Fourth, we use state-specific data on compulsory schooling laws from 1918, provided by Goldin and Katz (2008). The data allows us to group individuals into school-age cohorts.

We start from documenting a significant negative effect of severe pandemic exposure on the education of school-aged children. While school-aged children were largely spared from influenza-induced mortality, which exhibited a distinct "W"-shaped pattern across age cohorts,[5] children in areas with higher pandemic-specific mortality rates were significantly less likely to to return to school after school reopening in 1919. We find that a one standard deviation increase in the specific mortality rate is associated with a 30% reduction in school attendance rates the year following the pandemic, controlling for individual-specific characteristics and state fixed effects. Moving further in time, we estimate a significant negative relationship between the specific mortality rate and high-school completion for individuals that were of schooling age in 1918. A one standard deviation increase in sample-specific mortality rate predicts a 6% reduction in the probability of high school graduation.

We next investigate whether parents' socioeconomic characteristics influence the effect of pandemic exposure on children's educational outcomes. We utilize the 1920 Census, which was conducted immediately following the 1918 pandemic, to link school-aged children to their parents' education and occupation. Specifically, we use parents' occupational education scores, which measure the percentage of people in the individual's occupation with one or more years of college, and the Siegel (1971) occupational prestige score, which evaluates the general and social standing of occupations. Interacting these scores with our measure of pandemic severity, we estimate a significantly positive effect of both parents' occupational education and the father's occupational prestige on school attendance post pandemic, in 1919. This indicates socioeconomic status of parents can alleviate the negative effect of the pandemic on children's school attendance. For instance, having a parent with an occupational education score in the top reduces the negative pandemic effect by 3.8–7.2 pp for each standard deviation increase in sample-specific mortality rate. Having a father with an occupational prestige score in the top decile, drives down the negative pandemic effect by 3.7 pp per standard deviation increase in sample-specific specific mortality rate.

We then turn to the city-level data on school closures to investigate potential underlying mechanisms. We first assess the magnitude of the effect of pandemic-induced school closures on educational outcomes. We relate data on school closures from 50 cities, instrumented with the average influenza mortality in a city, to high-school completion rates. Our results indicate a 0.6–0.9% decrease in high-school graduation probability with every additional day of school closure, around 7–9% overall.

We then investigate in which cities children were more versus less affected by these school closures in terms of their educational attainment. For this analysis, we cre-

[5]People at very young, middle, and higher ages were disproportionately at risk to die from an infection (Taubenberger et al., 1997).

ate a new data set on local health officers and politicians, based on qualitative data from the "City Stories" of the University of Michigan's "Influenza Encyclopedia." We construct an index that captures (1) how often the local city health officers (CHOs) are described as making decisions based on beliefs rather than data, and their actions and statements are described as taking the pandemic (too) lightly (e.g., "no Influenza, only bad colds") and "not adhering" to certain measures (e.g., publicly not wearing a mask); and (2) how often local political decision-making is described with terms of conflict, such as "disagree," "clash," or "bitterness," rather than terms such as "cooperation," "compliance," or "agreement." We combine these measures into two proxies for pandemic-concerned and cooperative cities (CHO taking the pandemic seriously, no politician conflict), on the one hand, and cities that were pandemic-denying and conflict-rich (CHO taking the pandemic lightly, politician conflict), on the other end of the spectrum.

Using this measure, we first show that cities that were pandemic-denying and conflict-rich were more reluctant to close schools, in particular cities with higher levels of mortality rates. The opposite is true for cities that were pandemic-concerned and cooperative during the pandemic.

We then re-estimate the relationship between the pandemic and individual educational attainment separately in cities that were pandemic-concerned and cooperative during the pandemic vs. those that were not. We find that the probability of high-school completion is *increasing* in the number of days schools closed for cities that were pandemic-denying and conflict-rich (CHO taking the pandemic lightly, politician conflict), while the opposite is true for cities that were pandemic-concerned and cooperative (CHO taking the pandemic seriously, no politician conflict). These result suggest that pandemic-denying and conflict-rich cities could have benefited from a stricter policy intervention. Likewise, in pandemic-concerned and cooperative cities, the policy measures undertaken appear to have been too strict on the margin, at least in terms of educational attainment. That is, fewer days of school closure could have improved the overall likelihood of high-school completion for school-children. These results reveal that local policy measures can make a significant difference in how individual outcomes are shaped by the pandemic experiences.

Finally, given this negative impact of the pandemic on secondary schooling, we investigate whether these adverse effects translate into income losses later in life. We find evidence of increased returns to high-school completion for the affected age cohort, which also implies increased income inequality among those individuals. In particular, cohorts that were just younger than 18 at the time of the pandemic, experienced a higher return to high school completion than their adjacent, slightly older cohorts. This result indicates that the pandemic briefly interrupted the overall trend of declining returns to education in the early 20th century. Nonetheless, it also squares with standard labor market explanations of declining returns to education since the education premium only rises for the cohorts with potentially lower graduation probabilities.

Overall, the empirical evidence suggests that the strength of pandemic exposure and pandemic-induced school closures affect the educational attainment of children to a measurable extent. Thus, school closures are a policy measure that has to weigh the trade-off between containing disease spread and limiting longterm collateral damage to specific cohorts. Furthermore, policies have to account for the evidence that heterogeneity in parents' socioeconomic backgrounds play a significant role in how much the pandemic affects educational attainment of the cohorts in question. Lastly, the results suggest that local policy measures make a difference in how individual outcomes are shaped by pandemic experiences.

This paper makes contributions to several strands of literature. First, it contributes to the research on the economic effects of the 1918 Spanish Flu pandemic. Garrett (2009) estimates a positive relationship between influenza mortality rates and wage growth from 1914 to 1919. Brainerd and Siegler (2003) argue that the 1918 pandemic and in particular high mortality rates predict higher per-capita economic growth in the 1920s, reflecting mean reversion. Almond (2006), shows that cohorts that were in utero during the pandemic suffered significant losses in educational attainment, income, socioeconomic status, and physical health later in life. Parman (2015) provides an alternative interpretation of the observed cohort differences between those in utero and those not, namely, a shift in intra-family resource allocation towards alive older siblings.

Recent contributions by Beach et al. (2020) and Ager et al. (2020) study the economic impact of the 1918 pandemic with a focus on topics of interest in the COVID-19 context. Beach et al. (2020) surveys the literature on global health and economic consequences and highlights the importance of long-lasting effects of the 1918 pandemic on human-capital accumulation, also pointing to school closures. Ager et al. (2020), instead, suggests that school closures had no effect on educational outcomes. A related study by Meyers and Thomasson (2017) suggests that short-term school closures during the U.S. 1916 polio epidemic resulted in reduced educational attainment of affected children. They propose that older students refused to return to school once they re-opened. The difference in findings of Ager et al. (2020) relative to the other papers (and to ours) likely reflects sample differences and differences in the treatment of (endogenous) school closures. Specifically, Ager et al. (2020) focus on a male-only sample with a very broad age band (0-25 years), while our analysis uses data on compulsory schooling from 1918 in order to precisely define distinct groups of school-aged children. Differently from Ager et al. (2020), we treat school closures as endogenous and instrument for the length of school closures using mortality rates.[6] Additionally, we also look at heterogeneity in effects by parental socioeconomic status and analyze directly the impact of local politicians' characteristics on city-level educational policies and educational outcomes.

Second, this paper contributes to the literature on secondary schooling in the U.S. at the beginning of the 20th century (see for example Goldin (1998), and Goldin and Katz (2008) as well as the interaction between education and health. Lleras-Muney (2005) uses compulsory schooling laws as an instrument for education in order to estimate the causal impact of education on mortality. Using consecutive U.S. census data from 1960, 1970 and 1980, the paper identifies a negative effect of education on adult mortality. Similarly, Clark and Royer (2013) investigate the causal relationship between education and health outcomes in Great Britain and obtain similar results. Research on the causal relationship between education and health outcomes has also gained considerable traction in the recent medical literature (see for example the reply by Meyerowitz-Katz and Kashnitsky (2020), to Christakis et al. (2020)).

Third, in terms of methodology, we combine individual-level data with the regional and age-specific variation in pandemic exposure. A similar methodology is adopted by Falk et al. (2019) where the authors combine individual-level data on patience with regionally varying information on longevity in their analysis. The identification of the long-lasting impact of a one-time event on individuals relates to the literature on experience effects (see for example Malmendier and Shen (2018), or Malmendier and Nagel (2011)). Lastly, the usage of variation in health variables as an instrument for endogenous factors that influence outcome variables relates to Kotschy (2021), which

[6]Research on the High School Movement during the beginning of the 20th century, for example, points towards a considerable role of organizational-level factors for driving a surge in high-school attendance and graduation rates. "In order to extend education to the secondary level, schools had to be built and teachers had to be hired. These actions were not based simply on the aggregation of individual family choices concerning whether or not to allow children to attend school. Rather the decision was whether a school district, township, county, or state would tax everyone regardless whether they had children who would attend the school." (Goldin and Katz, 1997)

uses the decline in cardiovascular disease mortality as an instrument for increases in life expectancy, which are then related to economic outcomes (e.g. per capita income).

The remainder of the paper proceeds as follows. In Section 2 we describe our data. Section 3 presents the empirical evidence on effects of pandemic exposure on educational attainment, both in terms of post-pandemic school attendance and in terms of post high-school graduation. Also, it presents the differences in pandemic effects on school attendance by parental background. Section 4 shows the results related to school closures and assesses the influence of local political decisions. Section 5 presents suggestive evidence on the long-term earnings implications of education effects on earnings. Section 6 concludes.

2 Data

2.1 Census Data

The micro data is obtained from the U.S. decennial censuses 1920–1970 via IPUMS (Ruggles et al., 2020). IPUMS variables used in the analysis include education, income, individual-level characteristics, and regional identifiers. We use the IPUMS variable SCHOOL from the 1920 Census to determine whether an individual attended school in 1919.[7] We construct the binary measure "High School Completion" based on the IPUMS variable EDUC, which records the highest grade that they have completed.[8]

The income variable INCWAGE reports a person's total pre-tax wage and salary income for the year prior to each census. We replace top-coded entries as missing. Both EDUC and INCWAGE are available from the 1940 census onward. Control variables include SEX, RACE, URBAN and METRO, which are re-coded to dummy variables indicating whether a person is female, non-white, and living in an urban or metropolitan area, respectively. These variables are available throughout for all census years, with the exception of URBAN not being available for 1950. For the analysis of long-term effects, we also include the variable AGE as a control variable.

We further calculate each individual's occupational education score to measure the educational intensity of a given occupation. The occupational education score reports

[7] The original questionnaire text reads: "Attended school any time since September 1, 1919."

[8] The IPUMS-variable EDUC encodes the value "0" as "N/A or no schooling". Therefore, there are two ways to interpret this value: either as "N/A" or "0". In the main analysis, we assume "0" to be "N/A". In Appendix A.2 we show that the results are not sensitive to the alternative specification of interpreting the value "0" as "no schooling".

the percentage of people in the individual's occupation that have completed one or more years of college. We also calculate the Siegel prestige score for each occupation. The Siegel score is a three-digit numeric variable that is based on a series of surveys conducted at the National Opinion Research Center during the 1960s. In all surveys, respondents were asked to evaluate either the "general standing" or the "social standing" of occupations. Details about the methodology and construction of the occupational prestige index are in Siegel (1971). It captures the societal view towards given occupations in terms of their prestige, using large numbers of survey respondents. It has been used, among others, in studies of inter-generational mobility (see, e.g., Borjas (1992)).

Finally, we use regional identifiers STATEICP and CITY to combine the micro data with the pandemic data (which varies by region). The IPUMS variable CITY is not available in the 1970 census, which is the reason why the city-level analysis cannot be implemented for that year.

2.2 Pandemic Data

We construct several measures to capture the severity of the pandemic: (1) region- and age-specific mortality rates, (2) a binary measure of school closures, and (3) a numeric measure of the length of school closures. Whereas (1) is available on the state- and on the city-level, (2)-(3) are only available on the city-level. At the city-level we also measure of city governance during the pandemic utilizing (4) a numeric measure for the degree of the City Health Officer taking the pandemic lightly, and (5) a binary measure of local conflict between politicians.

2.2.1 State-Level

We calculate the state- and age-specific Influenza mortality rate by dividing the death count in 1918 due to "Influenza & Pneumonia" by the corresponding (sub-)population size of 1910:

$$MortalityRate_{s,a,1918} = \frac{\text{Death Count}_{s,a,1918}}{\text{Population}_{s,a,1910}} \quad \text{by state and age cohort} \tag{1}$$

where s indicates the state and a the age cohort. Values of the variable Death Count$_{s,a,1918}$ are manually transcribed from the mortality tables of the Centers for Disease Control and Prevention (CDC), which report the number of people that died due to a specific cause within a region and within specific age groups. The regions in this data set are either specific states or cities. Age groups are given in ten-year intervals for ages ten and higher and in more stratified intervals at younger ages. Appendix A.1 spells out all the regions and age groups. For example, the tables report 4,948 deaths recorded in the state of California in 1918 due to Influenza and Pneumonia for the age group of 30 to 39 year-olds. This data is available for 24 states and 50 cities. $\text{Population}_{s,a,1910}$ is from the 1910 census. In order to retrieve age-group- and region-specific data of the population count, we rely on population tables that are reported analogously to the mortality tables of the CDC. We use the 1910 rather than the 1920 census numbers (even though 1920 is closer to the 1918/1919 years of the pandemic), to avoid confounds due to the influenza death numbers.

We then combine this data set of pandemic exposure with the micro data in the following way: First, we restrict the sample to individuals that were born in 1918 or earlier, i. e., who were alive during the pandemic. Second, we restrict the sample to observations that have matching entries for state of birth and state of residence. We make the implicit assumption that people who were born in a certain state and are still living there, have also experienced the pandemic in this state.

After applying these sample restrictions, we assign all sample observations a region- and age-specific mortality rate. Thus, we differentiate between individual exposure to the pandemic based on region and on age. A 55-year old from California, for example, has a different "specific mortality rate" than a 5-year old from California and than a 55-year old from Pennsylvania.

Table 1 shows the summary statistics for this variable on the state-level. The average mortality rate is around 0.6%, which is in line with estimates on the Influenza death rate in the U.S. in 1918 (Noymer and Garenne, 2000). Note that there is considerable variation in the specific mortality rate, as indicated by the standard deviation. The average mortality across all age groups within a given state ranges from 0.4% (Washington) to 1.0% (Pennsylvania). When restricting the sample to children below the age of nine, specific mortality ranges from 0.1% (Utah/Minnesota) to 3.6% (Connecticut/Rhode Island/Pennsylvania). When looking at the sample of children older then nine but not older than 18, specific mortality ranges from 0.2% to around 0.5%.

Figure 1 plots the distribution of the specific mortality rate, both unconditional and conditional on age, during the pandemic. There are two interesting aspects to be mentioned. First, most of the density is contained within the 0-1% range, as indicated by the density plot in the left panel, which is in line with present-day Influenza mortality rates. Second, the right panel exhibits the typical "W"-shape across the age spectrum,

Table 1: Summary Statistics: Age- and location-specific Mortality Rate (State-Level)

	Observations	Mean	Std. Dev.	Min	Max
Mort.Rate	1.631,935	0.640	0.583	0.124	3.608
Mort.Rate (0-9 yr. olds)	574,664	0.767	0.864	0.124	3.608
Mort.Rate (10-18 yr. olds)	374,887	0.288	0.065	0.205	0.472

which is a well-known stylized fact from the medical literature on the 1918 pandemic (Taubenberger and Morens, 2006).

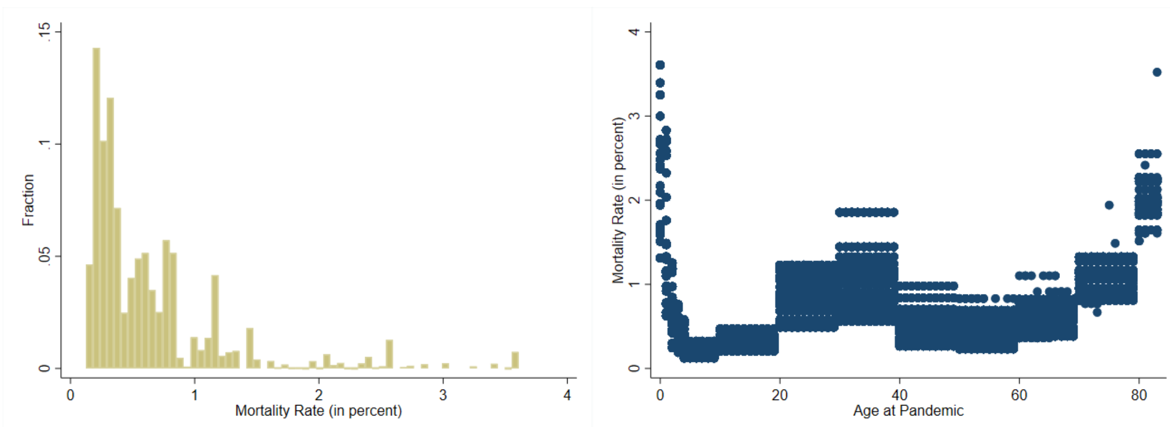

Figure 1: Distribution of the specific mortality rate: histogram (left) and conditional on age at the pandemic (right)

2.2.2 City-Level

Data on the city-level includes detailed historical reports with information on pandemic severity for 50 cities (inhabitants over 100,000). We extract additional information on whether schools closed during the pandemic in a given city, the days and weeks school closed, and the compliance of the public with non-pharmaceutical interventions such as the use of masks and social distancing.

While the place of living is reported on both state- and city level, the place of birth of a specific person is only reported on the state-level. Therefore, it is impossible to restrict the data analogously to the state-level analysis. We keep the practice of dropping every observation for which the state of birth differs from the state of living at the time of each census. Additionally, we also drop individuals who have moved 5 years prior to each census.[9]

The specific mortality rate is then obtained analogously to the state-level mortality rate: we obtain the death counts by city and age group from the mortality table of the CDC and the population data from the decennial census 1910. Dividing the former by the latter, this yields the city- and age specific mortality rate, which we assign to each individual based on their city of living and age during the pandemic. The availability of pandemic-related data on the state- and city level, as well as the variation in the mortality rate by state are shown in Figure 2. Appendix A.1 spells out all the regions and age groups.

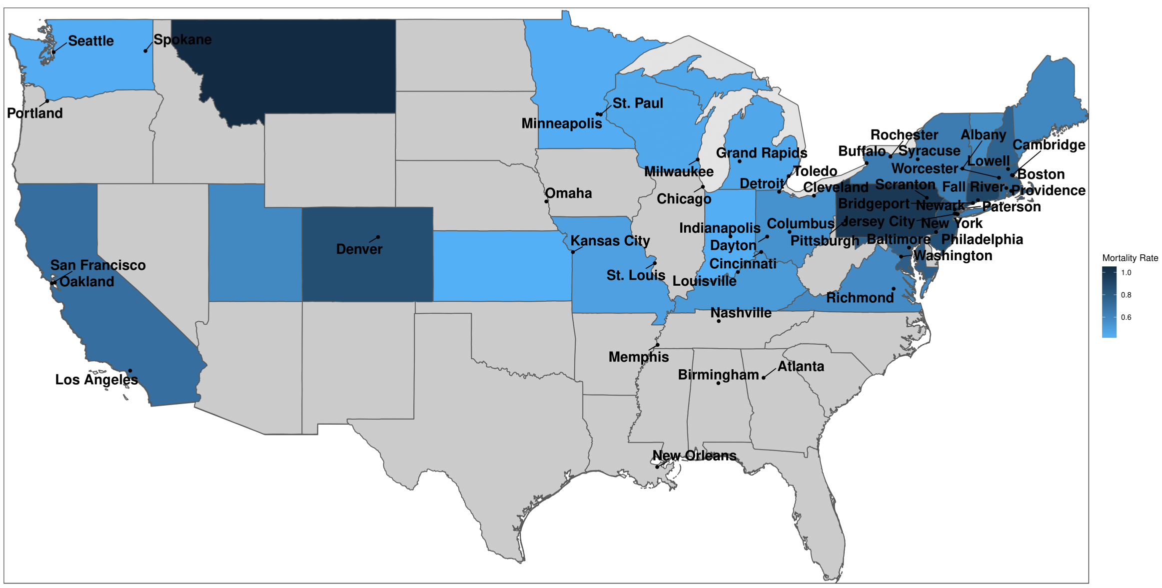

Figure 2: Availability of pandemic-related data and variation in state-wide mortality rate. Appendix A.1 spells out all the regions and age groups.

Table 2 shows the summary statistics for the specific mortality rate on the city-level. The average mortality rate is around 0.8%, thus slightly higher than on the state level. Again note that there is considerable variation in the specific mortality rate, as indicated by the standard deviation. When restricting the sample to children below the

[9]The census data includes the variable "MIGRATE5" that indicates the migration status of an individual five years before every census and is available from 1940 onward. The IPUMS code provides a numerical value in the range of 0 to 9. We use this measure to identify whether a person has moved to a different city five years prior to observation and drop all observations that imply that this has occurred.

age of nine, specific mortality ranges from 0.1% (Spokane, WA) to 5.0% (Detroit, MI). When looking at the sample of children older then nine but not older than 18, specific mortality ranges from 0.1% (Grand Rapids, MI) to around 0.6% (Birmingham, AL).

Table 2: Summary Statistics: Age- and location-specific Mortality Rate in percent and Average city-specific mortality before school closures in deaths per 100,000 inhabitants (City-Level)

	Observations	Mean	Std. Dev.	Min	Max
Mort.Rate	787,409	0.787	0.749	0.109	4.980
Mort.Rate (0-9 yr. olds)	190,661	0.940	1.032	0.109	4.980
Mort.Rate (10-18 yr. olds)	132,229	0.277	0.090	0.135	0.556
Av.Mort.before School Closure	977,446	4.992	11.280	0.767	61.567

Figure 3 plots the distribution of the specific mortality rate, both unconditional and conditional on age during the pandemic at the city-level. Again, most of the density is contained within the 0-1% range, as shown by the density plot in the left panel. Also the right panel again depicts the typical "W"-shape across the age spectrum as on the state-level.

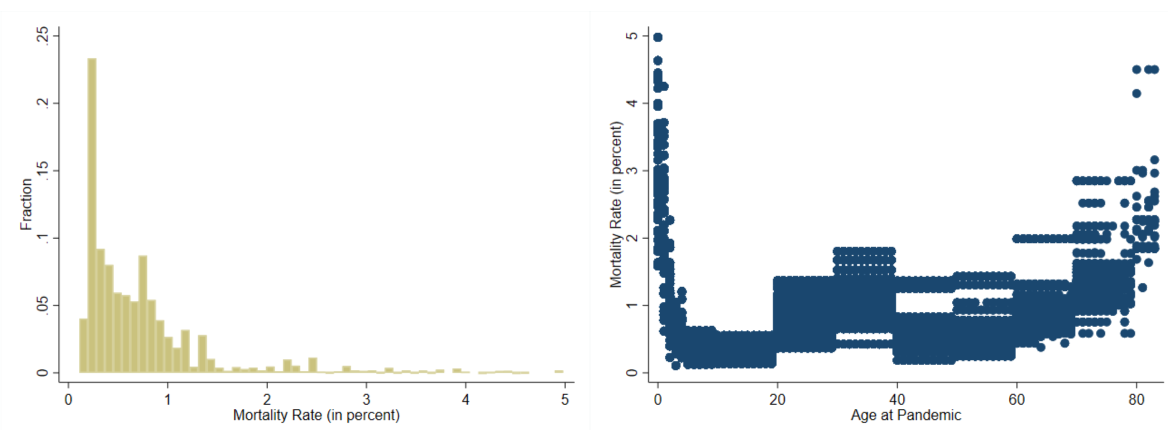

Figure 3: Distribution of the specific mortality rate: histogram (left) and conditional on age at the pandemic (right)

In addition to the overall mortality rate of 1918, we are also able to use weekly mortality data on the city level from the beginning of the pandemic in order to measure the severity of the pandemic before any schools closed. Specifically, the mortality rates from September 14$^{\text{th}}$ to October 5$^{\text{th}}$ 1918 are compiled into an average mortality before school closure with linearly declining weights from the last to the first week. This measure reports deaths per 100,000 inhabitants. The summary statistics are also shown in Table 2.

Another measure of pandemic severity is school closures. In terms of data availability, the major difference to the mortality rate is that this measure is only obtainable on the city-level, as the decision of school closures was made by individual cities during the pandemic. For measuring school closure, there are two distinct measures: one binary variable "schoolclosed" (0 = No school closure due to pandemic; 1 = School closure due to pandemic) and a continuous variable that indicates how many days schools were closed within one city. These measures are obtained from various sources. For most cities in the data set, we refer to the numerical indicators provided by Stern et al. (2009). The cities that are not captured in the data set by Stern et al. (2009) include Atlanta (Georgia), Detroit (Michigan), Bridgeport (New Haven), Jersey City (New Jersey), Paterson (New Jersey), Scranton (Pennsylvania) and Memphis (Tennessee). For the former two, we refer to the reports provided by the "City Stories" from the "Influenza Encyclopedia" of the University of Michigan.For Bridgeport, we gather data on school closures from Winslow and Rogers (1920). Similarly, data for Scranton comes from Higgins (2018). For the other cities, we refer to various newspaper articles.

Lastly, we collect information on the cities' local politicians. Thereby, we construct two measures that reflect certain characteristics pertaining to the political environment of a given city: the city health officer's degree of taking the pandemic lightly and conflict among politicians with regards to policy responses to the pandemic. The information is obtained from the "City Stories" of the "Influenza Encyclopedia" of the University of Michigan.

City Health Officer (CHO) taking the pandemic lightly. City Health Officers were of great importance during the pandemic, serving as a central point of consultancy and policy recommendation. Like today, any type of pandemic-indemnifying measure was ordered on a local, city-level. Given this decentralized public-health system, the heterogeneity in decision-making abilities among local politicians, heavily influenced by the recommendations of the respective CHO's, arguably altered different cities' inhabitants' lives.[10] To document those differences, we construct a new data set that captures different cities' CHOs either taking the pandemic lightly or seriously.

First, we identify all articles in the city stories describing the course of pandemic developments of each city. The resulting set of 44 articles captures the experiences each city's citizens and politicians went through during the pandemic. It should be mentioned here that the "City Stories" present a thorough description of each city's pandemic story, building on a total of 1296 historical newspaper articles.

We extract from these articles all paragraphs that address the CHO, either by his name or title (e.g. "Sautter" or "[Albany's] city health officer), separately for each city in our data set. The resulting set of a total of 409 paragraphs, on average 9.3 per city and CHO, captures a thorough description of each city's CHO in terms of his statements/views, actions and foundation of his arguments that came up in the policy debates around the responses to the 1918 pandemic.

We then extract information on the CHO by classifying each paragraph into one of four categories: (1) the CHO's action, (2) description of the CHO, (3) statements/views of the CHO towards the pandemic situation or (4) foundation of an argument made by the CHO. For each of those categories, we use a list of keywords that indicate "taking the pandemic lightly" or "taking the pandemic seriously". Examples for "taking the pandemic lightly", for instance, include: basing their arguments on beliefs rather than data, making pandemic-denying statements (e.g., "no Influenza, only bad colds"), being described as "sanguine" and not adhering to certain measures (e.g., publicly not wearing a mask). Examples for "taking the pandemic seriously" indicators include: basing their arguments on data, taking precaution, being described as "alarmed" or "worried" and using the word "serious" or "gravity" in their statements.[11]

In the next step, we count all keywords that indicate "taking the pandemic lightly" and "taking the pandemic seriously" for each city's CHO. If the number of keywords attributed to "taking the pandemic lightly" is greater than the number of keywords attributed to "taking the pandemic seriously", we assign the value 1, i.e. "taking the pandemic lightly" to the CHO. If the reverse holds, the number 0 is assigned. If the numbers are equal, a value of 0.5 is assigned.

Politician Conflict. The procedure for constructing the variable for politician conflict is similar to the one for CHO taking the pandemic lightly : For each city in

[10] Cf. article "How a Fragmented Country Fights a Pandemic" by Polly J. Price, *The Atlantic* (03/19/2020)

[11] For the complete list of the keywords, see Table A10 in Appendix Section A.6.

our data set, we extract each paragraph that addresses the dynamics between the local politicians. The resulting set of a total of 123 paragraphs, on average 2.8 per city, captures a thorough description of each city's politician's collaboration in response to the 1918 pandemic. We then apply a list of keywords that indicates "conflict" or "no conflict". Examples of keywords for "conflict" include "disagree", "clash" or "bitterness". Examples of keywords for "no conflict" include "cooperation", "compliance" or "granted full authority". In the next step, we count all keywords that indicate "conflict" and "no conflict" for each city's politicians. If the number of keywords attributed to "conflict" is greater than the number of keywords attributed to "no conflict", we assign the value 1, i. e., "conflict" to the city. If the reverse holds, the number 0 is assigned.[12]

2.3 Data on Schooling Laws

In order to group individuals into cohorts based on schooling ages, we use data on Compulsory Education and Child Labor Laws provided by Goldin and Katz (2008). The data provide state-specific mandatory schooling ages during the 1900–1939 time period for each state and year. Specifically, it contains both an official "school-entry age," at which a person is required to enter school, and "school-exit age," at which an individual is allowed to exit the schooling system and to obtain a work permit.

We use this information to construct state-specific cohorts of children at "mandatory schooling age," "plausible schooling age," and "plausible college age" as of 1918. Children of "mandatory schooling age" were older than the legal maximum school-entry age but younger than the minimum school-exit age; children in the "plausible schooling age" group were older than the minimum school-exit age but at most 18 years old; and "plausible college age" is assigned to individuals who were older than 18 but not older than 24 in 1918. Figure 4 provides an illustration of division into different groups based on the data on schooling laws. The two groups "mandatory schooling age" and "plausible schooling age", framed in bold, form the (plausibly affected) "schooling age" group, i. e., individuals that were of schooling age during the 1918 pandemic.

3 Educational Attainment

Using the 1940 census micro data, we plot high-school graduation rates by birthyear, highlighting the different cohorts. The IPUMS-data provides a variable EDUC which

[12] For the complete list of the keywords, see Table A11 in Appendix Section A.6.

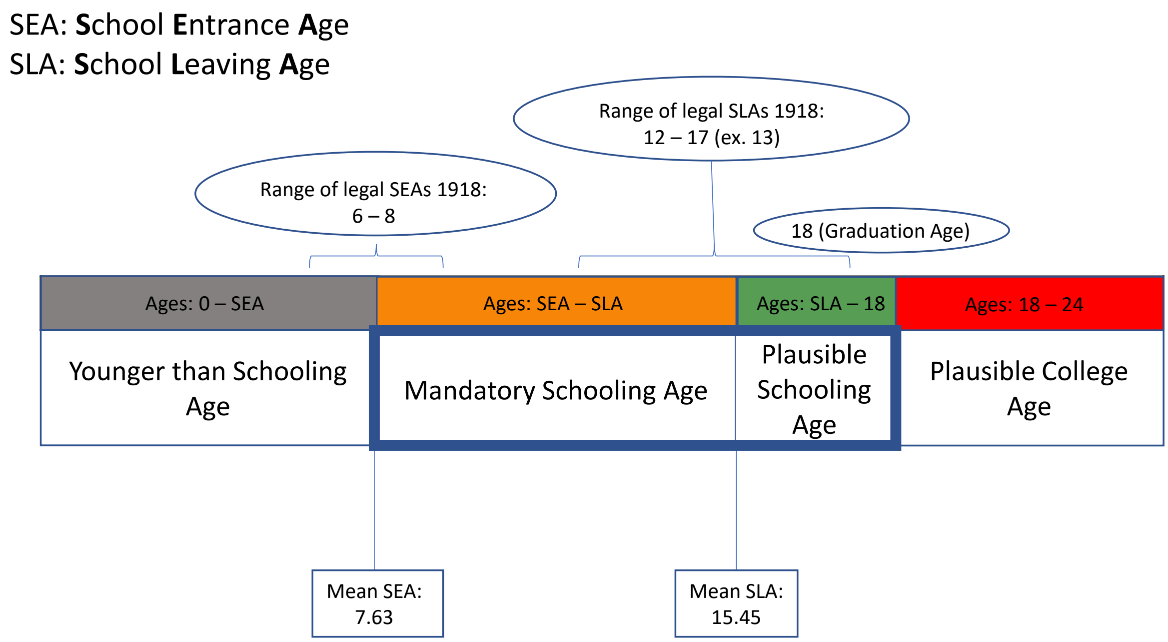

Figure 4: Age group definition based on data on Compulsory Education and Child Labor Laws provided by Goldin and Katz (2008)

indicates the highest level of education completed by an individual by grade level. We use this to extract all people that have at least completed grade 12 for each birth year and divide the number of people satisfying this criterion by the total number of people that were born in the respective birth year.

The colors indicate the cohorts that are determined by ages during the 1918 pandemic. Red colored bars refer to people that were of plausible college age, green of plausible high-school age and orange of mandatory schooling age in 1918.

As the graph in Figure 5 shows, there is a general upward trend from older to younger generations. This confirms the developments of the so-called "U.S. High School Movement" at the beginning of the 20th century. However, when looking at the transition from the red- to the green-colored bars, it is apparent that there is a sudden drop in high-school graduation rates. Thus, people that were just in the plausible high-school age during the pandemic experienced a change in the general trend of education relative to the slightly older cohort. Moreover, while the shape of the general trend seems to be largely unaffected, the discontinuous drop from one cohort to the next is striking.

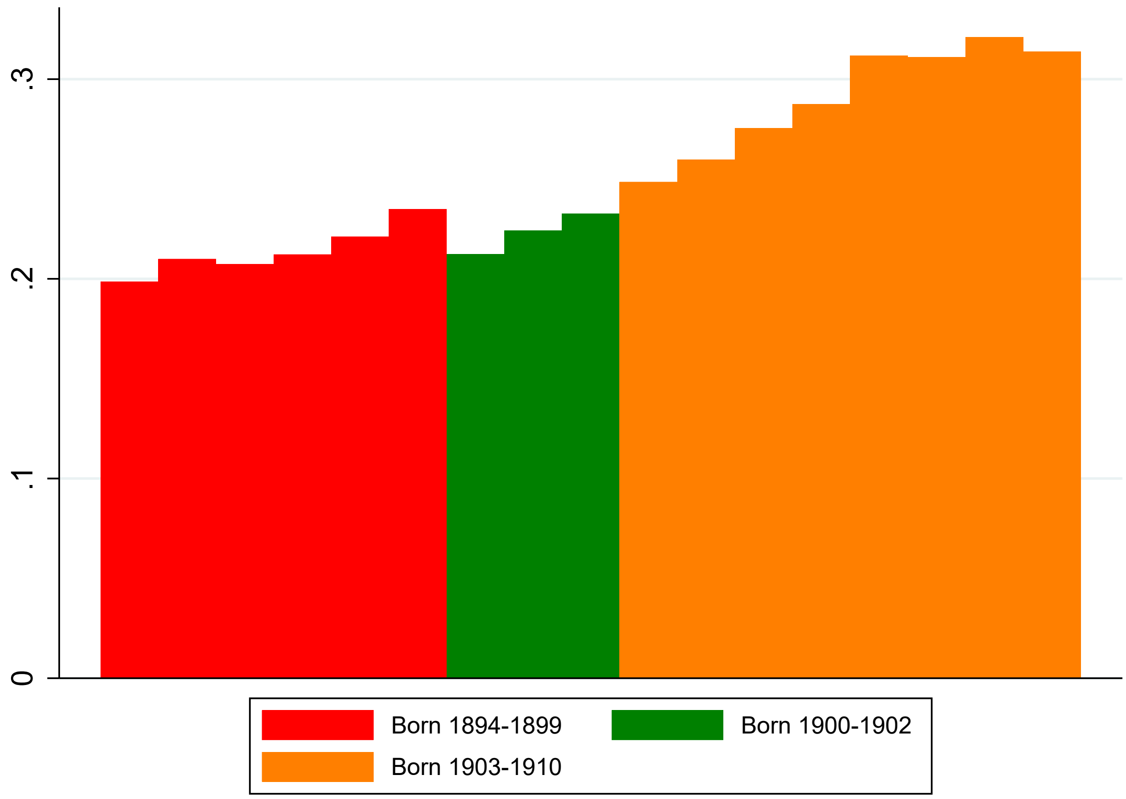

Figure 5: High School Graduation Rate by birth year and cohort (1940 Census). Cohort-specific colors applied according to the group definition pictured in figure 4

3.1 School Attendance and High School Graduation

Table 3 presents a set of OLS regressions of school attendance on the specific mortality rate. Thereby, the data set is restricted to people that were of schooling age at the time of the pandemic in a given state, i. e., from the legal school entry age to 18. Thus, for an individual i in state s and age group a, the regression equation of school attendance on their specific mortality rate is given by:

$$SchoolAttendance^{1919}_{i,s,a} = \alpha + \beta \cdot \text{Mort.Rate}_{s,a,1918} + \gamma' \boldsymbol{X}_i + \delta_s + \varepsilon_{i,s,a} \qquad (2)$$

where the vector of individual controls $\boldsymbol{X}$ includes variables for sex, race, urban, metro. A full set of state fixed effects δ_s is also included in some specifications, standard errors are clustered at the state-age level.

Table 3: Regression Table: School Attendance 1919 and specific mortality rate

	Dependent variable: School Attendance in 1919			
	(1)	(2)	(3)	(4)
Mort.Rate	-1.021***	-1.010***	-4.305***	-4.303***
	(0.256)	(0.259)	(0.425)	(0.426)
Controls	No	Yes	No	Yes
State FE	No	No	Yes	Yes
Observations	102964	102964	102964	102964

Standard errors clustered by state and age group.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All the columns of table 3 show a negative coefficient of the specific mortality rate, i. e., the higher the age- and state-specific exposure to the pandemic, the less likely it was for a (surviving) individual of that age-group and state to attend school one year after the pandemic. Columns (1) and (2) show the variation between states while the coefficients in (3) and (4) indicate the pandemic effect on school attendance stemming from within-state variation in specific mortality rates by including state fixed effects. Adding controls leaves the results largely unaffected. The within-state coefficient estimate of -4.303 from the specification with controls (column 4) implies that a one standard deviation increase in the sample-specific mortality rate leads to a 30% reduction in probability of attending school the year following the pandemic.

We next investigate the effect of pandemic exposure on high school graduation.[13] Table 4 shows the estimates, separately for each age cohort, from regressing high-school completion on the specific mortality rate with a full set of fixed effects and controls, as well as without state-fixed effects. We estimate regression equations of the following structure:

$$HighSchoolCompletion_{i,s,a,t} = \alpha + \beta \cdot \text{Mort.Rate}_{s,a,1918} + \gamma' \boldsymbol{X}_i + \delta_s + \mu_t + \varepsilon_{i,s,a,t} \quad (3)$$

[13] In Appendix A.3 we show that when looking at completion of 9th grade instead of high school graduation, the results exhibit the same pattern in terms of direction and significance of the coefficients.

where the vector of individual controls $\boldsymbol{X}$ again includes variables for sex, race, urban, metro. A full set of state- and year-fixed effects (δ_s and μ_t, respectively) are also included in some specifications.

We restrict the data set to only those people that were alive during the pandemic and below the age of 25. Within this age range, there are three distinct, mutually exclusive subgroups. The first subgroup consists of people that were younger than the legally defined school entry age in each state. This first group also serves as the reference group for this exercise. The second subgroup are people that were of schooling age in 1918, i. e., at least as old as the legal school entry age and at most as old as 18 ("School-Aged"). The oldest group ("College-Aged") are people that were older than the school-aged and at a plausible college age during the time of the pandemic (18-24). Columns (1), (3) and (5) display the within-state and columns (2), (4) and (6) the between-state effect of the specific mortality rate on high-school completion.

As column (3) indicates, the coefficient of the specific mortality rate on high-school completion for the school-aged is large and statistically significant at the 1% level. This can be interpreted as an 8.6% reduction in the probability of high school graduation with a 0.1% increase in specific mortality rates, or 6% reduction in the probability of high school graduation with a standard deviation increase in sample-specific mortality rates.

Note that the coefficient of interest is about twenty times large in absolute value in the school-aged group (columns (3) and (4)) compared to those of the surrounding cohorts. Also note that the coefficient is weakly positive, as well as weakly negative for the younger and older cohorts, respectively. This result indicates that the pandemic did not have an economically significant direct effect on cohorts other than the immediately affected school-aged children.

3.2 School attendance and Parental Background

As shown above, the specific mortality rate appears to have a systematic effect on school attendance immediately after the pandemic and high-school completion in general for the cohort of the school-aged. In order to explore how socioeconomic status influences this overall effect we take the 1920 Census and link people of schooling age to their parents. This allows us to retrieve information on parental occupational education and the parental occupational prestige. We then run the same regression as described by equation (2) but include a dummy variable that indicates whether the occupational

Table 4: Regression Table: High School Completion and specific mortality rate

	Dependent variable: High School Completion					
	Younger SA		School-Aged		Plausible College-Aged	
	(1)	(2)	(3)	(4)	(5)	(6)
Mort.Rate	0.0361***	0.0206**	-0.856***	-0.496***	-0.0343***	-0.0245
	(0.00324)	(0.0101)	(0.0858)	(0.100)	(0.00847)	(0.0290)
Constant	0.319***	0.374***	0.699***	0.385***	0.314***	0.182***
	(0.0405)	(0.0170)	(0.0734)	(0.0311)	(0.0113)	(0.0208)
State FE	Yes	No	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	179127	179127	193281	193281	72926	72926

Standard errors clustered by state and age group.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

education score or the occupational prestige score of the mother or the father is in the top decile of the respective census. This dummy is also interacted with the specific mortality rate of the child. Table 5 shows the results for this exercise. As can be observed, the coefficient of the specific mortality rate is comparable to the results of table 3 indicating that overall pandemic effect is relatively stable. However, including either parent's occupational education scores yields a positive significant coefficient on the interaction terms. For the occupational prestige, a similar pattern arises on the father's side. This indicates that higher socioeconomic status is an alleviating factor via partly counteracting the negative pandemic effect.[14] Quantitatively, having a parent with an occupational education score in the top decile drives down the negative pandemic effect by 3.8–7.17 pp for each standard deviation increase in sample-specific specific mortality rate. Having a father with an occupational prestige score in the top decile, drives down the negative pandemic effect by 3.67 pp, again for each standard deviation increase in sample-specific specific mortality rate. This provides evidence that

[14] Note that we cannot relate parental status to high-school completion because the information on the highest grade obtained is only available from the 1940 Census onwards. That, in turn, poses potential sample selection problems when linking children to their parents.

Table 5: Regression Table: Parents' Occupational Education Score & Occupational Prestige Score with Interactions, TOP10

	Dependent variable: School Attendance in 1919			
	(1) Mother	(2) Father	(3) Mother	(4) Father
Mother's occ. education score Top 10 x Mort.Rate	1.174*** (0.258)			
Mother's Occ. Educ. Score Top 10	-0.162** (0.0772)			
Father's occ. education score Top 10 x Mort.Rate		0.536** (0.210)		
Father's Occ. Educ. Score Top 10		0.0119 (0.0679)		
Mort.Rate	-4.365*** (0.394)	-3.715*** (0.376)	-4.358*** (0.393)	-3.714*** (0.376)
Mother's occ. prestige score Top 10 x Mort.Rate			-0.707 (1.781)	
Mother's Occ. Prestige Score Top 10			0.345 (0.398)	
Father's occ. prestige score Top 10 x Mort.Rate				0.518** (0.222)
Father's Occ. Prestige Score Top 10				0.0233 (0.0711)
Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	7066	81754	7066	81754

Standard errors clustered by state and age group.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

different socioeconomic groups were affected differently by the pandemic.[15]

[15] In Appendix A.4 we also show the results for an alternative specification using the top quartile. Also, we include the results of the "plain" regressions without grouping the parents' occupational education & prestige scores into percentiles.

4 School Closures and Local Politics

The results presented above suggest that the drop in education outcomes in 1918 is not random. Rather, it seems like exposure to the 1918 Spanish flu pandemic strongly correlates with a decline in school attendance right after the pandemic and a negative effect on high school graduation for those directly affected. In order to investigate the underlying mechanism that leads to these outcomes, we next turn to a detailed analysis on the city level with enriched information about non-pharmaceutical interventions.

In order to gain a more detailed insight into the underlying mechanism that drives the decline in high school graduation rates in the context of the 1918 Spanish flu pandemic, we use an instrumental variables approach. Therein, we make use of the fact that school closures are observable on the city-level.

As mentioned before, the city-level data on pandemic exposure provides a set of pandemic-related variables in addition to the mortality rate. Information on school closures and their length are available for 50 cities. We want to identify the potential effect of school closures on education for the children that have been in school during the pandemic. However, directly regressing high-school completion on school closures can result in fallacious results since this variable arguably suffers from high levels of endogeneity: school closures are a policy measure that have been undertaken by local administrations in response to the pandemic. However, educational attainment of a city's population strongly depends on policy measures (see for example Goldin and Katz (1997)).

As a solution to this endogeneity problem, we propose to use a measure of exogenously given pandemic severity before any school closures were ordered to instrument the duration of school closures. For this measure, we use city-specific Influenza mortality rates during the first few weeks of the pandemic on the city level, specifically from September 14$^{\text{th}}$ to October 5$^{\text{th}}$ 1918. Thereby, we calculate an average of the mortality in deaths per 100,000 inhabitants over those weeks, with linearly declining weights in reverse chronological order. For the average mortality rate to be a valid instrument, several conditions have to be met. First, the mortality rate has to be (quasi-) randomly assigned, given covariates. This means that neither the outcome variable (high-school completion) nor the instrumented variable (duration of school closure) affects the assignment of "average mortality rate". This arguably holds as both outcome and instrumented variables are chronologically occurring after mortality rates have been assigned.

Second, the exclusion restriction has to hold. That is, the average mortality rate has to affect high-school completion only through the channel of school closure duration. This condition is not testable. However, there are several arguments that support this idea. First, the sample is restricted to only those that were of schooling age during the pandemic. Thus, failure to obtain a high school degree because of the pandemic is unlikely to be caused by the mortality rate through anything else but policy measures directly targeted at schools for containing the pandemic. Second, if the former was violated, the only likely channel would be through death. However, naturally, we only observe people that were alive in the census.

Third, the instrument must sufficiently correlate with school closure duration. The first-stage (with F-Statistics included) in table A9 confirms that this condition is satisfied.

As a result, we estimate two stage least squares regressions of the following form:

$$\begin{aligned} \text{Days Closed}_{i,c,s,t} =& \kappa + \theta_1 \cdot \text{Average Mortality Rate}_{c,1918} \\ &+ \theta_2 \cdot \text{Average Mortality Rate}^2_{c,1918} + \gamma' \boldsymbol{X}_i \\ &+ \lambda' \boldsymbol{Z}_c + \delta_s + \mu_t + v_{i,c,t} \end{aligned} \tag{4}$$

$$\begin{aligned} \text{High School Completion}_{i,c,s,t} =& \alpha + \beta_1 \cdot \widehat{\text{Days Closed}}_{i,c,t} \\ &+ \beta_2 \cdot \widehat{\text{Days Closed}}^2_{i,c,t} + \gamma' \boldsymbol{X}_i + \lambda' \boldsymbol{Z}_c + \delta_s + \mu_t + \varepsilon_{i,c,t} \end{aligned} \tag{5}$$

for individual i in city c and state s at time t, where "Days Closed" is the duration of school closure in days and $\boldsymbol{Z}_c$ is a vector of city-specific controls, in particular population density and enrollment rate in 1920.

Table 6 shows the estimation results with this methodology separately for the baseline specification, one including a control for whether a city's City Officer took the situation seriously, as well as one including a control for whether there was conflict between local politicians in a city.

In general, we find a significant, non-linear effect of instrumented school closure duration on high school completion across all specifications. This suggests the existence of an optimal number of days closed at approximately 30 to 34 days (33.1, 32.5, 34.5 in columns (1), (2) and (3), respectively). Among the individuals observed in our data set (i.e. people that have survived the pandemic), it appears that closing schools for

Table 6: Regression Table: High School Completion (City-level) 2SLS

	Dependent variable: High School Completion		
	(1) Baseline	(2) Seriously	(3) Conflict
Days Closed	0.0296***	0.0139**	0.185***
	(0.00351)	(0.00600)	(0.0171)
Days Closed^2	-0.000447***	-0.000214**	-0.00268***
	(0.0000578)	(0.0000986)	(0.000251)
Population Density	0.00000756***	0.00000394***	0.00000361**
	(0.00000173)	(0.00000131)	(0.00000158)
Enrollment rate 1920	0.0291***	0.0325***	-0.00124
	(0.00339)	(0.00396)	(0.00239)
Seriously		-0.0485***	
		(0.0121)	
Conflict			0.615***
			(0.0551)
Year FE	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
First-Stage (linear) F	25788.6	28436.2	32028.2
First-Stage (square) F	6392.9	6900.5	6989.2
Observations	67266	67266	65287

Bootstrapped standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

less than 30 days was beneficial for their educational development, while school closures beyond 34-35 days could have borne adverse consequences.

The results of columns (2) and (3) suggest that the pandemic-handling of local politicians had a significant influence on the education attainment of their residents. The coefficient of "Seriously" in column (2) points towards an interesting observation: cities with a "strict" city health officer tend to perform worse with respect to high school completion. This implies that the decision for overburdening non-pharmaceutical interventions may have been counterproductive. On the contrary, "Conflict" (column (3)) among local politicians could have had a beneficial effect on high school completion. This might arise out of a more discussion-driven environment or through mechanically decreasing the number of days closed closer to the optimum.

Lastly, note that the first stage is reported in table A9 in Appendix section A.5. All relevant coefficients exhibit strong significance.

5 Earnings

In order to identify the possible long-term economic consequences of pandemic-related schooling losses, we next perform an analysis focusing on uniformly defined groups just around the cut-offs defining the affected group (school-aged). Figure 6 illustrates the distribution of the groups. Two groups surround the cut-off value defining the school entry age (state-specific) and two other groups are defined around the graduation age (18).

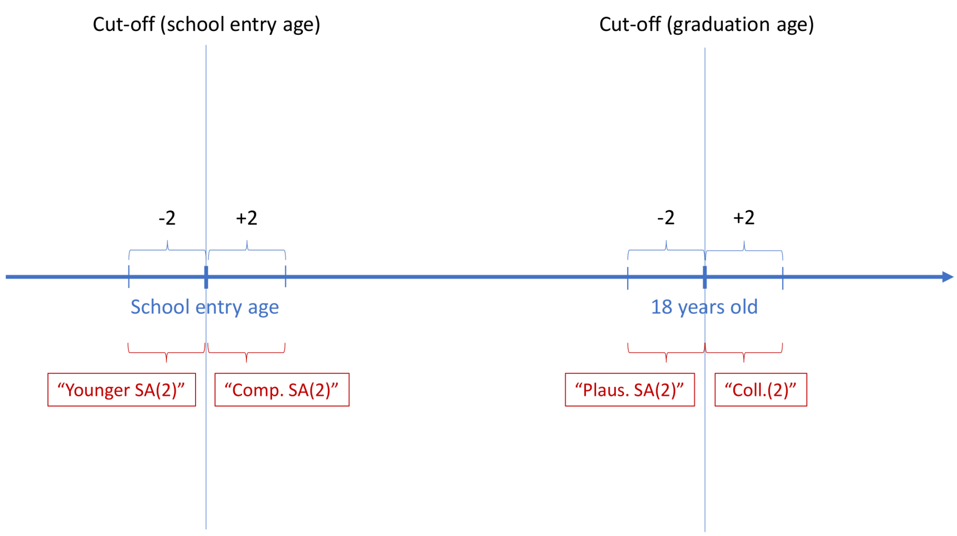

Figure 6: Definition of cut-off groups

In the absence of any pandemic effects, the evidence from Goldin (1999) or Goldin and Katz (2000) documents a secular decline of returns to education in the time between 1910 and 1960 (as a result of the increased supply of skilled labor). This implies the

Table 7: Regression Table: Log (Income) and High School Completion for groups right around the cut-offs (City-level)

	Dependent variable: Log (Income)			
	(1) YoungerSA(2)	(2) Comp. SA(2)	(3) Plaus. SA(2)	(4) Coll.(2)
High School Completion	0.255***	0.276***	0.350***	0.338***
	(0.0264)	(0.0135)	(0.0209)	(0.0241)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	10804	9954	7104	6032

Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

existence of a monotonic time trend in returns to education, indicating that older cohorts should continuously have higher returns to high school compared to younger cohorts.

Table 7 shows OLS regressions of log income on high-school completion with city- and year fixed effects as well as controls. The controls also include age and its squared term. The results indicate a higher return to high-school completion for the groups experiencing the pandemic while in school compared to the groups directly adjacent to them. Especially the comparison between columns (3) and (4) exhibit a pattern that contradicts evidence on the general time trend of the returns to high school during the early 20th century. This is tentative evidence for a higher return to education for the pandemic-affected individuals, which in turn implies potentially higher within-cohort inequality.

6 Conclusion

The consequences of the 1918 pandemic for school-aged children in the U.S. were severe. Using newly constructed data sets across states and cities, we estimate a significantly negative impact on school-attendance and high-school completion post-pandemic. The economic magnitude of the estimated effects is large. A one standard deviation increase in sample-specific mortality rates leads to a 6% reduction in the probability of high school graduation. Importantly, children from higher social-status families, as proxied by education and occupational prestige, fare significantly better. Using city-level data, we identify pandemic-induced school closures as the channel for this adverse effect, and also show that local politics matter. Lastly, we also find tentative evidence of an increased high school premium for the affected groups hinting at potentially increased within-cohort income inequality.

Taken together, our results show a strong significant effect of the 1918 pandemic on individual outcomes. The results also highlight the important role of policy responses as well as the large heterogeneity across socioeconomic strata. This historical evidence can provide guidance in answering questions that are present in the contemporaneous debates around adequate educational policy measures in response to the ongoing COVID-19 crisis.

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A Appendix

A.1 Regions and age groups of pandemic-related data

Age groups: 0, 1, 2, 3, 4, $5-9$, $10-19$, $20-29$, $30-39$, $40-49$, $50-59$, $60-69$, $70-79$, $80-89$, $90-99$, >100

States: California, Colorado, Connecticut, Indiana, Kansas, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Missouri, Montana, New Hampshire, New Jersey, New York, Ohio, Pennsylvania, Rhode Island, Utah, Vermont, Virginia, Washington, Wisconsin

Cities: Birmingham (AL), Los Angeles (CA), Oakland (CA), San Francisco (CA), Denver (CO), Bridgeport (CT), New Haven (CT), Washington D.C., Atlanta (GA), Chicago (IL), Indianapolis (IN), Louisville (KY), New Orleans (LA), Baltimore (MD), Boston (MA), Cambridge (MA), Fall River (MA), Lowell (MA), Worcester (MA), Detroit (MI), Grand Rapids (MI), Minneapolis (MN), Saint Paul (MN), Kansas City (MO), Saint Louis (MO), Omaha (NE), Jersey City (NJ), Newark (NJ), Paterson (NJ), Albany (NY), Buffalo (NY), New York City (NY), Rochester (NY), Syracuse (NY), Cincinnati (OH), Cleveland (OH), Columbus (OH), Dayton (OH), Toledo (OH), Portland (OR), Philadelphia (PA), Pittsburgh (PA), Scranton (PA), Providence (RI), Memphis (TN), Nashville (TN), Richmond (VA), Seattle (WA), Spokane (WA), Milwaukee (WI)

A.2 Robustness check: Alternative specification of EDUC-variable

The IPUMS-variable EDUC, from which the variable for high school completion is calculated encodes the value "0" as "N/A or no schooling". Therefore, there are two ways to interpret this value: either as "N/A" or "0". In the main analysis, we assume "0" to be "N/A". The tables below replicate the main analysis with the alternative specification of interpreting the value "0" as "no schooling". The results are not sensitive to the alternative specification.

Table A1: Regression Table: High School Completion and specific mortality rate

	Dependent variable: High School Completion					
	Younger SA		School-Aged		Plausible College-Aged	
	(1)	(2)	(3)	(4)	(5)	(6)
Mort.Rate	0.0360***	0.0205**	-0.857***	-0.500***	-0.0343***	-0.0245
	(0.00323)	(0.0100)	(0.0852)	(0.0994)	(0.00849)	(0.0287)
Constant	0.557***	0.393***	0.589***	0.409***	0.342***	0.208***
	(0.0321)	(0.0166)	(0.0425)	(0.0314)	(0.0116)	(0.0233)
State FE	Yes	No	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	179923	179923	194468	194468	73539	73539

Standard errors clustered by state and age group.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Regression Table: Log (Income) and High School Completion for groups right around the cut-offs (City-level)

	Dependent variable: Log (Income)			
	(1)	(2)	(3)	(4)
	YoungerSA(2)	Comp. SA(2)	Plaus. SA(2)	Coll.(2)
High School Completion	0.256***	0.277***	0.351***	0.339***
	(0.0152)	(0.0162)	(0.0206)	(0.0236)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	10830	9977	7123	6053

Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.3 Completion of 9th grade

Table A3: Regression Table: High School Completion and specific mortality rate

	Dependent variable: Completion of 9th grade					
	Younger SA		School-Aged		Plausible College-Aged	
	(1)	(2)	(3)	(4)	(5)	(6)
Mort.Rate	0.0434***	0.0299***	-1.191***	-0.728***	-0.0665***	-0.0371
	(0.00316)	(0.00992)	(0.116)	(0.138)	(0.0116)	(0.0481)
Constant	0.509***	0.568***	1.040***	0.633***	0.476***	0.337***
	(0.0367)	(0.0208)	(0.0790)	(0.0430)	(0.0236)	(0.0344)
State FE	Yes	No	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	179127	179127	193281	193281	72926	72926

Standard errors clustered by state and age group.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Regression Table: High School Completion (City-level) 2SLS

	Dependent variable: Completion of 9th grade		
	(1) Baseline	(2) Seriously	(3) Conflict
Days Closed	0.0348***	0.0180***	0.203***
	(0.00320)	(0.00476)	(0.0183)
Days Closed^2	-0.000529***	-0.000278***	-0.00294***
	(0.0000557)	(0.0000802)	(0.000273)
Population Density	0.00000891***	0.00000502***	0.00000464**
	(0.00000156)	(0.00000141)	(0.00000189)
Enrollment rate 1920	0.0387***	0.0424***	0.00615**
	(0.00461)	(0.00420)	(0.00313)
Seriously		-0.0522***	
		(0.0108)	
conflict			0.663***
			(0.0609)
Year FE	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
First-Stage (linear) F	25788.6	28436.2	32028.2
First-Stage (square) F	6392.9	6900.5	6989.2
Observations	67266	67266	65287

Bootstrapped standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Regression Table: IV, First Stage (City-level) 2SLS

	IV 1st stage; Dependent variable: Days closed					
	First Stage I			First Stage II		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Seriously	Conflict	Baseline	Seriously	Conflict
Av.Mort.before School Closure	-1.345***	-0.0815***	-1.963***	-114.7***	-39.78***	-140.8***
	(0.000446)	(0.000806)	(0.000669)	(0.0619)	(0.0858)	(0.0758)
Av.Mort.before School Closure^2	0.0264***	0.0170***	0.0321***	1.948***	1.393***	2.256***
	(0.00000557)	(0.00000911)	(0.00000895)	(0.000852)	(0.000904)	(0.000926)
Population Density	-0.00312***	-0.00371***	-0.00267***	-0.182***	-0.217***	-0.181***
	(0.000000304)	(0.000000488)	(0.000000380)	(0.0000459)	(0.0000463)	(0.0000414)
Enrollment rate 1920	1.676***	-1.363***	1.642***	157.1***	-23.15***	111.2***
	(0.000705)	(0.00134)	(0.000869)	(0.0715)	(0.176)	(0.111)
Seriously		10.59***			628.0***	
		(0.00430)			(0.605)	
conflict			3.419***			460.3***
			(0.00285)			(0.437)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67266	67266	65287	67266	67266	65287

Bootstrapped standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Regression Table: Log (Income) and High School Completion for groups right around the cut-offs (City-level)

	Dependent variable: Log (Income)			
	(1) YoungerSA(2)	(2) Comp. SA(2)	(3) Plaus. SA(2)	(4) Coll.(2)
Completion of 9th grade	0.233***	0.267***	0.288***	0.258***
	(0.0158)	(0.0156)	(0.0186)	(0.0209)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	10804	9954	7104	6032

Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.4 Parents' Circumstances, Additional tables

Table A7: Regression Table: Parents' Occupational Education Score & Occupational Prestige Score with Interactions

	Dependent variable:			
	School Attendance in 1919			
	(1)	(2)	(3)	(4)
	Mother	Father	Mother	Father
Mother's occ. educ. score x Mort.Rate	0.0107***			
	(0.00328)			
Mother's Occ. Educ. Score	-0.000796			
	(0.000969)			
Father's occ. educ. score x Mort.Rate		0.00747***		
		(0.00261)		
Father's Occ. Educ. Score		0.000400		
		(0.000842)		
Mort.Rate	-4.442***	-3.784***	-4.746***	-4.253***
	(0.399)	(0.375)	(0.426)	(0.383)
Mother's occ. prestige score x Mort.Rate			0.0128**	
			(0.00621)	
Mother's Occ. Prestige Score			-0.00171	
			(0.00191)	
Father's occ. prestige score x Mort.Rate				0.0145***
				(0.00264)
Father's Occ. Prestige Score				-0.00117
				(0.000851)
Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	7066	81754	7066	81754

Standard errors clustered by state and age group.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Regression Table: Parents' Occupational Education Score & Occupational Prestige Score with Interactions, TOP 25

	Dependent variable: School Attendance in 1919			
	(1) Mother	(2) Father	(3) Mother	(4) Father
Mother's occ. education score Top 25 x Mort.Rate	1.141***			
	(0.260)			
Mother's Occ. Educ. Score Top 25	-0.153*			
	(0.0776)			
Father's occ. education score Top 25 x Mort.Rate		0.526***		
		(0.161)		
Father's Occ. Educ. Score Top 25		-0.00104		
		(0.0521)		
Mort.Rate	-4.364***	-3.718***	-4.357***	-3.717***
	(0.394)	(0.376)	(0.393)	(0.376)
Mother's occ. prestige score Top 25 x Mort.Rate			-0.0928	
			(1.083)	
Mother's Occ. Prestige Score Top 25			0.178	
			(0.253)	
Father's occ. prestige score Top 25 x Mort.Rate				0.552***
				(0.179)
Father's Occ. Prestige Score Top 25				-0.0127
				(0.0572)
Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	7066	81754	7066	81754

Standard errors clustered by state and age group.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.5 City IV First Stage

Table A9: Regression Table: IV, First Stage (City-level) 2SLS

	IV 1st stage; Dependent variable: Days closed					
	First Stage I			First Stage II		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Seriously	Conflict	Baseline	Seriously	Conflict
Av.Mort.before School Closure	-1.345***	-0.0815***	-1.963***	-114.7***	-39.78***	-140.8***
	(0.000660)	(0.000997)	(0.000568)	(0.0767)	(0.0839)	(0.0637)
Av.Mort.before School Closure^2	0.0264***	0.0170***	0.0321***	1.948***	1.393***	2.256***
	(0.00000782)	(0.0000105)	(0.00000745)	(0.000930)	(0.000957)	(0.000763)
Population Density	-0.00312***	-0.00371***	-0.00267***	-0.182***	-0.217***	-0.181***
	(0.000000340)	(0.000000436)	(0.000000357)	(0.0000391)	(0.0000424)	(0.0000419)
Enrollment rate 1920	1.676***	-1.363***	1.642***	157.1***	-23.15***	111.2***
	(0.000810)	(0.00149)	(0.000783)	(0.103)	(0.151)	(0.106)
Seriously		10.59***			628.0***	
		(0.00513)			(0.544)	
conflict			3.419***			460.3***
			(0.00345)			(0.411)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67266	67266	65287	67266	67266	65287

Bootstrapped standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.6 Data on Local Politicians

Table A10: Keywords for eliciting CHO Pandemic-Handling

	Taking the pandemic lightly	Taking the pandemic seriously
Action	(prematurely) recommend reopening, not adhering to measures (e.g. publicly not wearing mask), blame the public, ignoring recommendations, denial of pandemic (e.g. "no Influenza, only bad colds"), not willing to take charge, downplaying, ignoring facts, contradicting oneself, not strict about measure enforcing, demand for closure from other parties, quickly changing mind	urge, prepare, order quarantine, order closure, banned, detected, postponing cancellation of ban, monitor, take (quick) action, not wait, plan, warn, reform health system, taking precaution, prevent
Description	believe in own achievements/bold statements, sanguine, optimistic, confident, surprised	alarmed, worried, protect, aware, mindful, concerned, praised after crisis, aware
Statement/View towards situation	dismissed, believe pandemic stamped out, thought no/little measures necessary, "not as bad as...", "peak/plateau reached", believe that pandemic short-lived	serious, necessary, gravity, expect more cases
Foundation of Argument	beliefs	data

Table A11: Keywords for eliciting political conflict within city

Conflict	No Conflict
disagree, clash, bitterness, defiant, refused, lax compliance	granted full authority, confirmed right, cooperation, compliance, agree