Lockdowns Echo: Exploring the Impact on Later-Life Longevity*

Hoa Vu† William Duncan‡ Hamid Noghanibehambari§

March 2, 2024

Abstract

Non-Pharmaceutical Interventions (NPIs) remain a subject of intense debate during major pandemics and endemics, with studies highlighting varied benefits and costs. Yet, little is known about the long-term effects of NPIs, particularly among those exposed during early life and childhood. This study examines the long-term effects of early-life and childhood exposure to NPIs implemented during the 1918-1919 influenza pandemic on later-life longevity. Utilizing Social Security Administration death records linked to the 1940 census, we investigate the differences in longevity of cohorts exposed to the pandemic during early childhood compared to those born post-pandemic, in cities with stricter NPIs to those with less stringent measures. The findings suggest a reduction in longevity of approximately 2.8 months for those exposed at ages 7-10. We attribute these effects to school closures and disruptions in children's socioemotional and cognitive development and provide empirical evidence that their later-life reductions in education and socioeconomic status as potential pathways.

Keywords: Mortality, Longevity, Life Expectancy, Lockdowns, Pandemic

JEL Classification: H75, I18, J18, N32, N92

---

*We thank seminar participants at the Southern Economic Association Annual Meeting 2023 for helpful comments and discussions. All errors are our own. The authors claim that they have no conflict of interest to report.

†School of Education and Social Policy, Northwestern University. Email: hoa.vu@northwestern.edu.

‡Kansas Data Science Consortium, University of Kansas. Email: w295d127@ku.edu.

§College of Business, Austin Peay State University. Email: Noghanih@apsu.edu

1 Introduction

The first documented case of the infamous Spanish Flu in the United States of America (US) in the spring of 1918. The influenza peaked in the fall of that year and saw a successive wave occur in early 1919. Roughly one-third of the population contracted the disease and the death toll in the country reached about 650,000 (Patterson and Pyle, 1991; Gagnon et al., 2013). During this period, US GDP and personal consumption dropped by about 1.5 and 2 percent, respectively (Barro et al., 2020).[1] In response to the pandemic, local and state public health authorities implemented various measures to curb its deadly spread. Critical to these efforts were Non-Pharmaceutical Interventions (NPIs) such as social distancing, mask-wearing mandates, travel restrictions, public awareness campaigns, school closures, and lockdowns (Tomes, 2010). Recent studies suggest that these NPIs were successful in reducing death rates (Hatchett et al., 2007; Markel et al., 2007).

These NPI measures were focused on reducing short-term adverse health effects of the pandemic. There is also research that looks at the 1918 pandemic's long-lasting impacts for the survivors, particularly those who experienced the pandemic during early life (Beach et al., 2022). Research indicates that exposure to the pandemic in-utero and early-life is associated with increased later-life disability (Almond, 2006), worse self-reported health (Almond and Mazumder, 2005), and worse outcomes related to old-age mortality (Mazumder et al., 2010; Myrskylä et al., 2013; Fletcher, 2018, 2019). However, very few studies have examined the long-run effects of NPIs on later-life outcomes. This paper aims to fill this gap in the literature by examining the effects of early-life and childhood exposure to the NPIs implemented during the 1918 pandemic on old-age longevity in later life.

We employ Social Security Administration death records linked to the full-count 1940 census. The linked data allows us to infer the city of childhood in addition to many individual

[1] However, estimates suggest that areas that were hit harder by the pandemic-induced recession recovered faster and experienced larger wage growth gains (Brainerd and Siegler, 2003).

and family characteristics, vital information in our setting. We then gather city-level data on the severity of the implemented NPIs and examine the long-term effects on longevity. Specifically, we use a two-way fixed effect model to compare the longevity of cohorts who experienced the pandemic during early life and childhood versus those who were born post-pandemic, in cities with stricter NPIs versus cities with less strict NPIs. We find significant reductions in longevity of cohorts who experienced strict NPIs between ages 7-10, but we fail to find any significant impacts for earlier ages, including children who were in-utero during the implementation of NPIs. We argue that the observed effect for children ages 7-10 is due to school closures and distortions in children's socioemotional and cognitive outcomes, as the critical age of these developmental outcomes starts around age seven when children start going to school. Further, we implement a series of balancing tests and show that exposure to these NPIs is not associated with a significant and consistent pattern of change in the sociodemographic and socioeconomic composition of individuals in the final sample. These tests partly rule out the concerns regarding endogenous survival into adulthood that may confound our findings. In addition, we provide empirical evidence that children who experience the NPIs reveal reductions in schooling outcomes and socioeconomic status measures. These pathways further lend credibility that school closures and disruptions in social developments may have played a role in the long-term links between exposure as children and reduced longevity later in life.

This study makes two contributions to the literature. First, to our knowledge, this study is the first to examine later-life impacts of the NPIs during the 1918 pandemic. Further, there is little research done to examine later-life impacts of NPIs across other pandemics. This is a timely and important question to understand given the breadth of policy debates over the costs and benefits of NPIs during the Covid-19 pandemic (Lai et al., 2020; Mendez-Brito et al., 2021). Understanding the usually unobserved long-term effects of NPIs directly addresses these policy and public debates. Second, we add to the ongoing research and growing literature that examines the role of early-life and childhood exposures and conditions on later-life mortality (Hayward and Gorman, 2004; Van den Berg et al., 2006; Almond et al., 2018; Schmitz and Duque, 2022). Our study also contributes to a more focused research area that examines the role of early-life diseases environment and later-life health outcomes and the potential mitigating influence of policy interventions (Bozzoli et al., 2009; Case and Paxson, 2009; Noghanibehambari and Fletcher, 2023a,b).

2 Background and Conceptual Framework

The 1918 influenza pandemic, commonly known as the Spanish flu, had a profound impact on the United States. The virus, which emerged during the final months of World War I, quickly spread across the nation, leading to widespread illness and mortality. In response to the escalating crisis, various NPIs were implemented on a state and local level. Cities and states adopted measures such as the closure of schools, theaters, and public gatherings, as well as the enforcement of isolation and quarantine protocols.

While these NPIs were implemented with the aim of curbing the spread of the virus and protecting public health, they also had several negative consequences, which we summarize below. First, lockdowns resulted in the closure of businesses, loss of jobs, and economic downturn. Many industries, such as hospitality, travel, and retail, were severely affected, leading to financial hardships for individuals and businesses alike (Garrett et al., 2007). Small businesses, in particular, faced significant challenges and closures, impacting livelihoods and exacerbating income inequality. Worsening local economic conditions and reductions in parental income may have long-lasting impacts, specifically if experienced early in a child's life (Montez and Hayward, 2011; Aizer et al., 2016). For instance, Schmitz and Duque (2022) examine the effects of early-life exposure to the Great Depression on later-life health and find that exposed individuals reveal faster biological aging decades later in their lives.

A second negative consequence, and an important channel relevant to the current study, comes through the disruption of education. School closures may pose challenges for students, parents, and educators. Empirical research for other epidemics of the 20th century points to the negative impacts of school closures on children's education and health outcomes (Villegas et al., 2021). The school closures in 1918 induced by lockdown mandates may in turn affect schooling outcomes, which in turn impact later-life mortality outcomes (Lleras-Muney, 2005; Fletcher, 2015; Meghir et al., 2018; Halpern-Manners et al., 2020). This was particularly true in 1918 and 1919 when compulsory schooling laws did not require students to attend school for as long into their childhood or for as many months each year (Katz, 1976).

A third adverse consequence of lockdowns is the delay in addressing non-pandemic-related diseases and health issues. Delayed diagnoses and treatments may have resulted in worsened health outcomes for some children. There is evidence that link childhood disease contraction and physical health to later-life outcomes (Bozzoli et al., 2009; Peracchi and Arcaleni, 2011).

While these negative consequences discussed above are frequently weighed against the short-run public health benefits of NPIs, a priori, we cannot be certain about the direction or magnitude of the long-run effects of NPIs on health and well-being such as later-life mortality. Therefore, the role of NPIs on later-life health and mortality remains an empirical question. We should also note that the severity and duration of these negative effects varied across regions and depended on the specific measures implemented. Policy-makers and health authorities aimed to strike a balance between protecting public health and minimizing the negative consequences of lockdown policies, but it was a challenging task with no one-size-fits-all solution.

3 Data Sources

The primary data sources utilized in this study are the Death Master Files (DMF) and the Numerical Identification (Numident) records of the Social Security Administration obtained from the CenSoc Project (Goldstein et al., 2021). Both datasets contain records of deceased individuals. The Numident data covers deaths to both men and women between the years 1988-2005 while DMF data covers deaths that occurred to male individuals who died between 1975-2005.

One significant advantage of using DMF-Numident data is its linkage to the complete 1940 census, enabling the identification of individuals' city of birth. Considering our research's emphasis on the long-term effects of local NPI policies, it is crucial for our analysis to consider birthplaces at the local level. Another advantage of utilizing the DMF data is the availability of millions of observations prior to any sample selection. This allows us to narrow down our sample to specific cohorts and narrower geographic regions (cities that implemented NPI policies at some point in 1918 and 1919), while still maintaining sufficient sample size and statistical power. A third advantage of the 1940-census-DMF linked sample is the inclusion of family characteristics and socioeconomic outcomes for individuals in 1940. This additional information enables us to explore potential endogeneity in exposure and investigate mechanism channels in subsequent analyses. More importantly, recent studies on the later-life effects of the 1918 influenza point to the changes in sociodemographic characteristics of births before and after the pandemic, which makes it essential to control for family covariates (Beach et al., 2022).[2]

We compiled a city-month panel on NPIs by using three primary sources: Markel et al. (2007), Berkes et al. (2023), and Correia et al. (2022). These sources provide comprehensive information on NPIs implemented in 54 major cities across the United States. We then

[2]Relatedly, Beach et al. (2022) show that the later-life disability and educational reduction impacts reported by Almond (2006) become smaller after accounting for family characteristics.

expanded the database to include four more cities using information from a variety of news articles.[3] The aggregate duration of NPIs is defined as the cumulative count of days encompassing three major categories: school closures, cancellation of public gatherings, and isolation and quarantine. To merge this data with the DMF-Numident, we match it based on the individual's city of birth.

In our regression analysis, we also incorporate city controls as covariates. These covariates are derived from the full-count decennial censuses 1910-1930 and linearly interpolated for the inter-decennial years (Ruggles et al., 2022). They include literacy rate, average occupational income score, the proportion of immigrants, the proportion of females, the proportion of families with children below the age of five, and the proportion of people in different age groups.

We limited the sample to cohorts that were born between 1910 and 1924 to have three distinct groups exposed to NPIs at varying ages: ages 0-2 (birth years 1918-1920), ages 3-6 (birth years 1914-1917), and ages 7-10 (birth years 1910-1913). Additionally, we included one cohort born between 1921 and 1924 that was not exposed to NPIs, serving as a control group. The final sample includes 1,388,715 individuals. Table 1 reports summary statistics of the final sample. The average age at death in the final sample is 928 months (77.3 years). Approximately 19 percent of individuals reside in long-NPI cities where NPIs have a duration of more than 90 days.

4 Empirical Strategy

Our identification strategy is a difference-in-differences model, in which we compare the difference in life expectancy between individuals in cities with longer NPIs and individuals in cities with shorter NPIs, relative to that difference of those born between 1921 and 1924,

[3]These cities include Charlotte, NC; Houston, TX; Tulsa, OK, and Wichita, KS, which have extended the sample to include vibrant locations in the South and lower Midwest.

the years in our sample after all NPIs had been rescinded. Specifically, we estimated models of the following form:

$$\begin{aligned} Y_{ict} = \alpha &+ \beta_1 1[\text{NPIs length} > 90 \text{ days}]_{ict} \times 1[\text{Birth Year} = 1910 - 1913]_{ict} \\ &+ \beta_2 1[\text{NPIs length} > 90 \text{ days}]_{ict} \times 1[\text{Birth Year} = 1914 - 1917]_{ict} \\ &+ \beta_3 1[\text{NPIs length} > 90 \text{ days}]_{ict} \times 1[\text{Birth Year} = 1918 - 1920]_{ict} \\ &+ \beta_4 X_{ict} + \beta_5 Z_{ct} + \xi_c + \zeta_t + \varepsilon_{ict} \end{aligned} \tag{1}$$

Where Y_{ict} is age-at-death (longevity) of person i who was born in city c during the month and year t. Following Berkes et al. (2023), 1[NPIs length > 90 is a dummy variable that equals one if the length of NPI policies is greater than 90 days and equals zero otherwise.[4] The coefficients of interest are β_1, β_2, and β_3 which capture the impacts on cohorts born between 1910-1913, 1914-1917, and 1918-1920, respectively, relative to the cohorts born between 1920 and 1924 (the omitted cohorts). In particular, these coefficients measure the differences in outcomes observed among these cohorts residing in cities with longer NPIs durations (> 90 days) compared to the same cohorts in cities with shorter NPIs durations (first difference), relative to the same differences among the omitted cohorts born between 1920 and 1924 when all NPIs were removed (second difference).

In Figure 1, following Berkes et al. (2023) we explore the robustness of our findings when using different cutoffs to categorize cities as having longer or shorter NPIs. We conduct a series of analyses employing various thresholds for the duration of NPIs, ranging from as low as 33 days to as high as 156 days (representing the 10th and 90th percentiles in NPI duration distribution, respectively). Our findings reveal that when the threshold is set at 53 days or more, the resulting estimates closely resemble those from our baseline results and

[4] Berkes et al. (2023) noted a significant absence of NPI lengths around 90 days, with no cities having NPI durations between 82 and 99 days. They interpreted this as a natural gap in the distribution and established a binary definition of treatment based on NPIs lasting around 90 days.

are statistically significant.

The matrix X_{ict} comprises individual race dummies (non-Hispanic Black, non-Hispanic white) and controls for parental education and father's occupation scores. City-level controls, denoted by Z_{ct}, encompass various factors such as the proportion of the population belonging to different age groups (11-18, 19-25, 26-55, and >55), the percentage of females and Black population, immigrants, literacy rate, average occupational score, and the proportion of families with children below the age of five. City fixed effects, represented by ξ_c, account for both observable and unobservable characteristics of each city that remain constant over time. Birth year-month fixed effects, denoted by ζ_t, are included to capture time-invariant unobserved heterogeneity that might influence birth cohorts. We cluster the standard errors at the city level to account for serial correlation in error terms.

5 Results

5.1 Balancing Tests

Our empirical strategy hinges on the fundamental assumption that there are no systematic disparities in the selection of individuals between the treatment and control groups that could be linked to their longevity later in life. This means that any variations in survival rates during adulthood due to exposure to NPI policies among children from different socioeconomic backgrounds would introduce bias into our final sample, resulting in estimations that reflect, to some extent, the influence of endogenous survival rather than solely the effects of exposure to NPI policies. Table 2 presents our assessment of the credibility of this identifying assumption by investigating any differences in observable characteristics between the treatment and control groups. Specifically, we show results from the regression models in equation 1 with maternal and paternal characteristics as dependent variables and omitting vector X_{ict}.

We observe predominantly small and statistically insignificant coefficients across most of the outcomes in this exercise. However, a noteworthy trend stands out: a lower proportion of mothers had education beyond high school for the 1910-1913, 1914-1917, and 1918-1920 cohorts (column 4). This suggests that the estimates from equation 1 may overestimate the true effects, as previous literature has documented positive associations between parental education and old-age health and longevity. We also notice a higher proportion of missing mothers' education information for the 1910-1913 and 1914-1917 cohorts (column 5) and a higher proportion of individuals with missing fathers' occupation score information for these same cohorts (column 10). These findings suggest that there may be specific factors or circumstances related to the time periods of 1910-1913 and 1914-1917 that contributed to the increased likelihood of missing information regarding mothers' education and fathers' occupation scores. Another speculation is that older cohorts (1910-1913 and 1914-1917) in high-NPI cities are more likely to have left the household than other cohorts and the significant coefficients of columns 5, 8, and 10 on missing information represent this fact. For instance, as we argue in section 5.4, the 1910-1913 cohorts in high-NPI cities reveal lower education due to exposure to school closures in this period. Therefore, it is not surprising that they leave households earlier, and, as we observe parental information in 1940, they constitute a higher share of missing parental information.

However, it is important to emphasize that these results are not consistently replicated across various measures, indicating a lack of a uniform and statistically significant pattern in the estimated coefficients. Consequently, the absence of pronounced differences in observable characteristics between the treatment and control groups implies that we should not anticipate establishing an association based on unobservable factors, as argued in previous research (Altonji et al., 2005).

5.2 Main Results

The primary results of the regressions presented in equation 1 can be found in Table 3. In the first column, we present results with city fixed effects and birth-year-month fixed effects. Subsequently, we introduce parental controls and city-level controls in columns 2 and 3, respectively. According to the fully parameterized model in column 3, cohorts aged 7-10 resided in longer NPI cities exhibit a reduction in lifespan by approximately 2.8 months.

The differing impacts of NPIs on various age cohorts can be attributed to several factors. Cohorts aged 7-10, might be more susceptible to the effects of prolonged NPIs due to their developmental stage and social interactions. First, children in the 7-10 age group typically attend school regularly. Extended NPIs, such as a school closure, would disrupt their educational and social routines, leading to potential stress and learning gaps. On the other hand, younger children (0-2 and 3-6) are less likely to have established school routines and social networks, which could make them less vulnerable to the negative effects of extended NPIs. Second, children aged 7-10 are in a critical phase of social development. Prolonged periods of isolation or limited social interactions due to NPIs could have adverse effects on their emotional and social well-being, possibly impacting their long-term health outcomes.

To understand the magnitude of these intent-to-treat effects, it is useful to compare them with documented effects of other early-life exposures on lifespan as reported in existing literature. For instance, a study by Vu et al. (2023) examines the impact of in-utero exposure to lynching incidences on old-age longevity. Their findings reveal an effect of 3.7 months among Black males who were exposed to lynching in utero. In contrast, our findings indicate that NPIs had no discernible impact on those in utero, but exposure to NPIs during the critical ages of 7-10, a pivotal phase of social development, results in similar declines in longevity for children exposed to historical racialized violence. This underscores the substantial influence of NPIs during childhood on overall well-being.

In another context, Aizer et al. (2016) analyze the Mother's Pension program in the early

20th century and find that male children lived an average of 11.6 months longer than similar children whose mothers were not in the program. This cash assistance amounted to about 30-40% of the mothers' income before they received it. The study suggests that the impact of childhood (aged 7-10) exposure to the NPIs on life expectancy is roughly 25 percent (in magnitude) of a substantial and long-lasting cash transfer to poor single mothers.

5.3 Mechanisms

The effects of NPIs on young children's later-life longevity may operate through distortions in their social, emotional, and cognitive development as there is evidence of an interconnected link between these outcomes and lockdowns (Fernández Cruz et al., 2020; Sancho et al., 2021; Martín-Requejo and Santiago-Ramajo, 2021). These short-run negative impacts can then translate into lower educational outcomes, affecting individuals' measures of socioeconomic status. There is empirical evidence that both education and socioeconomic status may influence later-life mortality outcomes (Lleras-Muney, 2005; Salm, 2011; Fletcher, 2015; Chetty et al., 2016). We examine these pathways using available census data. In so doing, we focus on census data over a similar period as the main analysis sample. Specifically, we use Census data for the decennial years 1980-2000 combined with the 2005 American Community Survey.[5] We restrict this sample to the same birth cohorts as before using individuals born between 1910-1924. We further restrict the sample to individuals whose state of residence in the 1980-2000 censuses as well as the 2005 American Community Survey is the same as their state-of-birth to mitigate migration issues.

We implement similar sample merging and empirical approaches as in sections 3 and 4. We examine the effects on socioeconomic measures and educational outcomes. These results are reported in Table 4. We observe reductions in the socioeconomic index for the 1910-1913

[5]This sample selection has a similar coverage as the DMF years 1975-2005. Moreover, we are unable to use ACS 2001-2004 as they do not report city codes.

and 1914-1917 cohorts, though the coefficients are insignificant (column 1). We also find small, positive, and significant increases in the socioeconomic index of 1918-1920 cohorts, suggesting very small benefits of NPIs for those probably in-utero and their early-life.

However, we find significant reductions in the occupational educational score and occupational income score of 1910-1913 and 1914-1917 cohorts (columns 2-3). For instance, we observe a reduction of 1.5 and 0.6 units for the occupational educational score and occupational income score of the 1910-1913 cohort, respectively. This represents a 3.5 and 1.2 percent change with respect to the outcome mean. Moreover, we observe significant increases in the likelihood of less than five years of schooling (column 4). We find significant increases in educational attainment of 0-4 years of about 20 and 14 basis points for the 1910-1913 and 1914-1917 cohorts, respectively, with an outcome mean of 0.0048. These estimates indicate that the effects may be partly attributed to decreases in educational attainment, likely stemming from school closures, as well as reductions in social interactions and the development of social skills.[6] This fact is more pronounced specifically among those at the lower tail of education distribution as we do not find significant changes for other educational groups (columns 5-7).

Our long-term negative findings regarding education align with Li and Malmendier (2022) documentation of a significant adverse effect of the pandemic and pandemic-induced school closures on school attendance post-reopening, as well as on the high-school graduation rates of affected cohorts. These results also resonate with a broader body of literature that identifies a negative long-term effect of the pandemics on the educational outcomes of exposed cohorts (Almond, 2006; Meyers and Thomasson, 2021; Beach et al., 2022). However, Ager et al. (2023) reported null short-term effects of school closures on school attendance. As mentioned above, we posit that the enduring effects on education might stem from distortions in social, emotional, and cognitive development. This finds support in evidence linking

[6] Based on 1920 Census data, about 20 percent of those at ages 3-6 attend school while this number rises to 88 percent for those at ages 7-10 (Ruggles et al., 2022).

these outcomes with lockdowns (Fernández Cruz et al., 2020; Idoiaga Mondragon et al., 2021; Martín-Requejo and Santiago-Ramajo, 2021).

5.4 Robustness Checks

In Table 5, we show that our results are robust to alternative specifications and functional forms. Serving as our benchmark, Model 1 replicates the model in Column 3 of Table 3. Model 2 incorporates seasonality in mortality by including death-month fixed effects, while Model 3 accounts for cross-state migration by comparing migrants and non-migrants, incorporating birth-state by state-of-residence fixed effects. The coefficients for Models 2 and 3 are very similar to our baseline findings. Model 4 shows results from a specification including census-region-of-birth by birth-year fixed effects. These models account for cross-region convergence in longevity across cohorts. The coefficients drop by about 40 percent. Further, Model 5 demonstrates the robustness of our results when clustering standard errors by state rather than by city.

To explore functional form sensitivity, Model 6 transforms the outcome by adopting the log of age-at-death. The resulting effect of 0.28% aligns with the implied percentage change in Model 1 with respect to the mean of age-at-death (2.8 off a mean of 928). Therefore, there is little concern regarding nonlinearity issues. Finally, to further address potential nonlinearity in the effects, Model 7 adopts an alternative outcome, indicating longevity beyond age 70 ($0 =$ age at death ≤ 70; $1 =$ age at death > 70). The estimated coefficient suggests that exposure to lockdown measures is associated with a 1.16 percentage point reduction in the probability of living beyond age 70, based on a mean of 0.82.

In Table A.1, we also show that our primary results hold when considering school closure length rather than the overall length of NPIs. In comparison to measures such as public gathering bans or isolation and quarantine, school closures seem more likely to impede interactions crucial for children entering school age. To validate these insights, we replicate the analysis presented in Table 3, this time focusing on school closure duration instead of the total length of NPIs.[7] Our findings reveal a slight reduction compared to the primary results based on the total length of NPIs. This difference indicates that factors related to social, emotional, and cognitive development, affected by isolation and quarantine, may play a significant role.

6 Conclusion

During major pandemics, the implementation of pharmaceutical interventions tends to be sluggish, particularly in the face of a novel pathogen. Consequently, Non-Pharmaceutical Interventions often emerge as the initial and arguably the most immediately effective policy response (Hatchett et al., 2007; Mendez-Brito et al., 2021). However, the social-distancing measures and lockdowns associated with these NPI initiatives come at the expense of job losses and other societal costs, sparking controversy and passionate debates in both public discourse and policy arenas. From the policymakers' standpoint, it is critical to understand the whole range of NPIs' costs and benefits. This paper directly addresses these debates by providing evidence of the long-term costs incurred by exposed young children.

Using linked data from Social Security Administration death records and the 1940 census, we examined the long-term effects of NPIs on longevity. We compared the longevity of cohorts who experienced pandemics at various childhood ages to those who were born post-pandemic, in cities with longer implemented NPIs versus those with shorter NPIs. Our findings imply a significant reduction of approximately 2.8 months in longevity for cohorts exposed to pandemics between the ages 7-10. We attribute this to school closures and disruptions in children's socioemotional and cognitive development, as these developmental

[7] In Figure A.1, we conduct a comparable analysis to Figure 1, evaluating the results using different cutoffs for school closure duration. Our findings reveal that the resulting estimates, closely resembling those from Table A.1, are statistically significant.

outcomes become critical around age 7 when children start attending school. Furthermore, empirical evidence suggests that exposed children experienced reductions in schooling outcomes and socioeconomic measures, further supporting the role of school closures and disruptions in social development. The original population of 1910-1913 cohorts in long-NPI cities observed in the 1940 full-count census count to 75,876 individuals. Based on the results of Table 3, the intent-to-treat effect is a 2.8-month reduction in longevity. Therefore, we calculate roughly 17,072 life-years are lost due to childhood exposure to NPIs among these cohorts. Further, we use estimates of the Value of Statistical Life (VSL) to put these numbers into perspective.

One way to put the magnitudes into perspective is to employ VSL estimates. The average age-at-death in the final sample is 76.2 years. The average remaining life expectancy of individuals in the US conditional on survival to 76 is about 11 years (Arias, 2014). Assuming a VSL of \$10M in \$2017 (Viscusi, 2018; Kniesner and Viscusi, 2019; Colmer, 2020) and a discount rate of three percent, we can calculate a Value of Statistical Life Year of roughly \$1.1M. Using the life-years lost mentioned above, we can reach a back-of-an-envelope estimate of \$18.8B lost due to longevity reductions as a result of NPIs.

References

Ager, P., Eriksson, K., Karger, E., Nencka, P., and Thomasson, M. A. (2023). School closures during the 1918 flu pandemic. *Review of Economics and Statistics*, pages 1–11.

Aizer, A., Eli, S., Ferrie, J., and Lleras-Muney, A. (2016). The long-run impact of cash transfers to poor families. *American Economic Review*, 106(4):935–971.

Almond, D. (2006). Is the 1918 influenza pandemic over? long-term effects of in utero influenza exposure in the post-1940 us population. *Journal of political Economy*, 114(4):672–712.

Almond, D., Currie, J., and Duque, V. (2018). Childhood circumstances and adult outcomes: Act ii. *Journal of Economic Literature*, 56(4):1360–1446.

Almond, D. and Mazumder, B. (2005). The 1918 influenza pandemic and subsequent health outcomes: an analysis of sipp data. *American Economic Review*, 95(2):258–262.

Altonji, J. G., Elder, T. E., and Taber, C. R. (2005). Selection on observed and unobserved variables: Assessing the effectiveness of Catholic schools. *Journal of political economy*, 113(1):151–184.

Arias, E. (2014). United States life tables, 2009.

Barro, R., Ursua, J., and Weng, J. (2020). Coronavirus meets the great influenza pandemic. *VoxEU. org*, 20.

Beach, B., Clay, K., and Saavedra, M. (2022). The 1918 influenza pandemic and its lessons for covid-19. *Journal of Economic Literature*, 60(1):41–84.

Berkes, E., Deschenes, O., Gaetani, R., Lin, J., and Severen, C. (2023). Lockdowns and innovation: Evidence from the 1918 flu pandemic. *Review of Economics and Statistics*, pages 1–30.

Bozzoli, C., Deaton, A., and Quintana-Domeque, C. (2009). Adult height and childhood disease. *Demography*, 46(4):647–669.

Brainerd, E. and Siegler, M. V. (2003). The economic effects of the 1918 influenza epidemic. *Available at SSRN 394606*.

Case, A. and Paxson, C. (2009). Early life health and cognitive function in old age. *American Economic Review*, 99(2):104–109.

Chetty, R., Hendren, N., and Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. *American Economic Review*, 106(4):855–902.

Colmer, J. (2020). What is the meaning of (statistical) life? Benefit–cost analysis in the time of COVID-19. *Oxford Review of Economic Policy*, 36(Supplement_1):S56–S63.

Correia, S., Luck, S., and Verner, E. (2022). Pandemics depress the economy, public health interventions do not: Evidence from the 1918 flu. *The Journal of Economic History*, 82(4):917–957.

Fernández Cruz, M., Álvarez Rodríguez, J., Ávalos Ruiz, I., Cuevas López, M., de Barros Camargo, C., Díaz Rosas, F., González Castellón, E., González González, D., Hernández Fernández, A., Ibáñez Cubillas, P., et al. (2020). Evaluation of the emotional and cognitive regulation of young people in a lockdown situation due to the Covid-19 pandemic. *Frontiers in psychology*, 11:565503.

Fletcher, J. M. (2015). New evidence of the effects of education on health in the us: Compulsory schooling laws revisited. *Social science & medicine*, 127:101–107.

Fletcher, J. M. (2018). New evidence on the impacts of early exposure to the 1918 influenza pandemic on old-age mortality. *Biodemography and social biology*, 64(2):123–126.

Fletcher, J. M. (2019). Examining the long-term mortality effects of early health shocks. *Applied Economics Letters*, 26(11):902–908.

Gagnon, A., Miller, M. S., Hallman, S. A., Bourbeau, R., Herring, D. A., Earn, D. J., and Madrenas, J. (2013). Age-specific mortality during the 1918 influenza pandemic: unravelling the mystery of high young adult mortality. *PloS one*, 8(8):e69586.

Garrett, T. A. et al. (2007). Bird flu pandemic: history warns of economic pain, though some might gain. *The Regional Economist*, (Oct):10–11.

Goldstein, J., Alexander, M., Breen, C., Miranda-González, A., Menares, F., Osborne, M., and Yildirim, U. (2021). CenSoc Mortality File: Version 2.0. [dataset].

Halpern-Manners, A., Helgertz, J., Warren, J. R., and Roberts, E. (2020). The effects of education on mortality: Evidence from linked us census and administrative mortality data. *Demography*, 57(4):1513–1541.

Hatchett, R. J., Mecher, C. E., and Lipsitch, M. (2007). Public health interventions and epidemic intensity during the 1918 influenza pandemic. *Proceedings of the National Academy of Sciences*, 104(18):7582–7587.

Hayward, M. D. and Gorman, B. K. (2004). The long arm of childhood: The influence of early-life social conditions on men's mortality. *Demography*, 41(1):87–107.

Idoiaga Mondragon, N., Berasategi Sancho, N., Dosil Santamaria, M., and Eiguren Munitis, A. (2021). Struggling to breathe: a qualitative study of children's wellbeing during lockdown in Spain. *Psychology & Health*, 36(2):179–194.

Katz, M. S. (1976). A History of Compulsory Education Laws. Fastback Series, No. 75. Bicentennial Series.

Kniesner, T. J. and Viscusi, W. K. (2019). The value of a statistical life. *Forthcoming, Oxford Research Encyclopedia of Economics and Finance, Vanderbilt Law Research Paper*, (19-15).

Lai, C.-C., Shih, T.-P., Ko, W.-C., Tang, H.-J., and Hsueh, P.-R. (2020). Severe acute respiratory syndrome coronavirus 2 (sars-cov-2) and coronavirus disease-2019 (covid-19): The epidemic and the challenges. *International journal of antimicrobial agents*, 55(3):105924.

Li, K. and Malmendier, U. (2022). School Closures, Inequality, and Politics–Evidence from the 1918 Spanish Flu. Working paper.

Lleras-Muney, A. (2005). The relationship between education and adult mortality in the united states. *The Review of Economic Studies*, 72(1):189–221.

Markel, H., Lipman, H. B., Navarro, J. A., Sloan, A., Michalsen, J. R., Stern, A. M., and Cetron, M. S. (2007). Nonpharmaceutical interventions implemented by us cities during the 1918-1919 influenza pandemic. *Jama*, 298(6):644–654.

Martín-Requejo, K. and Santiago-Ramajo, S. (2021). Reduced emotional intelligence in children aged 9–10 caused by the Covid-19 pandemic lockdown. *Mind, Brain, and Education*, 15(4):269–272.

Mazumder, B., Almond, D., Park, K., Crimmins, E. M., and Finch, C. E. (2010). Lingering prenatal effects of the 1918 influenza pandemic on cardiovascular disease. *Journal of developmental origins of health and disease*, 1(1):26–34.

Meghir, C., Palme, M., and Simeonova, E. (2018). Education and mortality: Evidence from a social experiment. *American Economic Journal: Applied Economics*, 10(2):234–256.

Mendez-Brito, A., El Bcheraoui, C., and Pozo-Martin, F. (2021). Systematic review of empirical studies comparing the effectiveness of non-pharmaceutical interventions against COVID-19. *Journal of Infection*, 83(3):281–293.

Meyers, K. and Thomasson, M. A. (2021). Can pandemics affect educational attainment? Evidence from the polio epidemic of 1916. *Cliometrica*, 15(2):231–265.

Montez, J. K. and Hayward, M. D. (2011). Early life conditions and later life mortality. In *International handbook of adult mortality*, pages 187–206. Springer.

Myrskylä, M., Mehta, N. K., and Chang, V. W. (2013). Early life exposure to the 1918 influenza pandemic and old-age mortality by cause of death. *American journal of public health*, 103(7):e83–e90.

Noghanibehambari, H. and Fletcher, J. (2023a). Childhood exposure to birth registration laws and old-age mortality. *Health economics*, 32(3):735–743.

Noghanibehambari, H. and Fletcher, J. (2023b). Long-term health benefits of occupational licensing: Evidence from midwifery laws. *Journal of Health Economics*, 92:102807.

Patterson, K. D. and Pyle, G. F. (1991). The geography and mortality of the 1918 influenza pandemic. *Bulletin of the History of Medicine*, 65(1):4–21.

Peracchi, F. and Arcaleni, E. (2011). Early-life environment, height and BMI of young men in Italy. *Economics & Human Biology*, 9(3):251–264.

Ruggles, S., Flood, S., Goeken, R., Schouweiler, M., and Sobek, M. (2022). IPUMS USA: Version 12.0 [dataset].

Salm, M. (2011). The effect of pensions on longevity: Evidence from union army veterans. *The Economic Journal*, 121(552):595–619.

Sancho, N. B., Mondragon, N. I., Santamaria, M. D., and Munitis, A. E. (2021). The well-being of children in lock-down: Physical, emotional, social and academic impact. *Children and Youth Services Review*, 127:106085.

Schmitz, L. L. and Duque, V. (2022). In utero exposure to the great depression is reflected in late-life epigenetic aging signatures. *Proceedings of the National Academy of Sciences*, 119(46):e2208530119.

Tomes, N. (2010). "destroyer and teacher": Managing the masses during the 1918–1919 influenza pandemic. *Public Health Reports*, 125(3_suppl):48–62.

Van den Berg, G. J., Lindeboom, M., and Portrait, F. (2006). Economic conditions early in life and individual mortality. *American Economic Review*, 96(1):290–302.

Villegas, C. C., Peirolo, S., Rocca, M., Ipince, A., and Bakrania, S. (2021). Impacts of health-related school closures on child protection outcomes: a review of evidence from past pandemics and epidemics and lessons learned for covid-19. *International journal of educational development*, 84:102431.

Viscusi, W. (2018). *Pricing lives: Guideposts for a safer society*. Princeton University Press.

Vu, H., Noghanibehambari, H., Fletcher, J., and Green, T. (2023). Prenatal Exposure to Racial Violence and Later-Life Mortality among Males: Evidence from Lynching. Working paper.

7 Tables

Table 1: Summary Statistics

	Mean	SD	Min	Max
Age at death (months)	928.429	91.97	601	1151
Log age at death	4.344	0.103	3.932	4.554
Age at death > 70 Years	0.815	0.388	0	1
Year of birth	1917.569	3.994	1910	1924
Year of death	1994.944	7.207	1975	2005
[NPIs length > 90 days]×[Birth Year=1910-1913]	0.04	0.196	0	1
[NPIs length > 90 days]×[Birth Year=1914-1917]	0.064	0.244	0	1
[NPIs length > 90 days]×[Birth Year=1918-1920]	0.036	0.187	0	1
[NPIs length > 90 days]	0.05	0.219	0	1
Non-Hispanic Black	0.042	0.201	0	1
Non-Hispanic white	0.947	0.224	0	1
Mother's education < high school	0.484	0.5	0	1
Mother's education = high school	0.119	0.323	0	1
Mother's education > high school	0.013	0.112	0	1
Mother's education missing	0.377	0.485	0	1
Father's education < high school	0.417	0.493	0	1
Father's education = high school	0.089	0.284	0	1
Father's education > high school	0.014	0.115	0	1
Father's education missing	0.014	0.116	0	1
Observations		1,388,715		

Table 2: Childhood Exposure to NPIs and Observable Characteristics

	Outcomes:									
	Non-Hispanic Black	Non-Hispanic white	Mother's education less than HS	Mother's education more than HS	Mother's education missing	Father's education less than HS	Father's education more than HS	Father's education missing	Father's occupational income score	Father's occupational income score missing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
[NPIs length $>$ 90 days] $\times$ [Birth Year $= 1910 - 1913$]	-0.0009	-0.0001	0.0029	-0.0151*	0.0577***	-0.0041	-0.0129	0.0067**	-0.3166	0.0446***
	(0.0045)	(0.0047)	(0.0278)	(0.0081)	(0.0111)	(0.0245)	(0.0086)	(0.0032)	(0.2673)	(0.0089)
[NPIs length $>$ 90 days] $\times$ [Birth Year $= 1914 - 1917$]	0.0017	-0.0050	-0.0128	-0.0110*	0.0595***	-0.0159	-0.0091	0.0029	0.1411	0.0507***
	(0.0050)	(0.0053)	(0.0127)	(0.0061)	(0.0206)	(0.0109)	(0.0063)	(0.0025)	(0.2189)	(0.0165)
[NPIs length $>$ 90 days] $\times$ [Birth Year $= 1918 - 1920$]	0.0008	-0.0027	-0.0019	-0.0061*	0.0283*	-0.0085	-0.0051	0.0007	0.0302	0.0257*
	(0.0037)	(0.0039)	(0.0085)	(0.0031)	(0.0161)	(0.0074)	(0.0035)	(0.0010)	(0.1362)	(0.0135)
Observations	1,388,715	1,388,715	1,388,715	1,388,715	1,388,715	1,388,715	1,388,715	1,388,715	766,484	1,388,715
R-squared	0.0834	0.0967	0.1442	0.0108	0.2273	0.1294	0.0102	0.0178	0.0139	0.2081
Birth-Year-Month fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
City fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Standard errors, clustered on city, are in parentheses. Parental controls include dummies for maternal education, paternal education, and paternal occupation score. City covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, share of homeowners, share of households with children under 5, and literacy rate. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3: Childhood Exposure to NPIs and Later-Life Longevity

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
[NPIs length $>$ 90 days] $\times$ [Birth Year $= 1910 - 1913$]	-4.7646***	-4.5161***	-2.7546***
	(1.4569)	(1.4748)	(0.6826)
[NPIs length $>$ 90 days] $\times$ [Birth Year $= 1914 - 1917$]	-1.9188**	-1.7849*	-0.8427
	(0.9424)	(0.9352)	(0.6092)
[NPIs length $>$ 90 days] $\times$ [Birth Year $= 1918 - 1920$]	0.2586	0.3219	0.5692
	(0.5245)	(0.5482)	(0.7685)
Observations	1,388,715	1,388,715	1,388,715
R-squared	0.1435	0.1442	0.1444
Birth-Year-Month fixed effects	✓	✓	✓
City fixed effects	✓	✓	✓
Parental controls		✓	✓
City-level controls			✓

Notes: Standard errors, clustered on city, are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 4: Exploring Mechanisms Using 1980-2000 Census and 2005 American Community Survey

	Outcomes:						
	Socioeconomic Index (1)	Occupational Education Score (2)	Occupational Income Score (3)	Education 0-4 Years (4)	Education 5-8 Years (5)	Education 9-12 Years (6)	Education >12 Years (7)
[NPIs length $>$ 90 days] $\times$ [Birth Year $= 1910 - 1913$]	-0.7559	-1.5328***	-0.6494**	0.0020***	-0.0035	0.0111	-0.0096
	(0.5575)	(0.5035)	(0.2542)	(0.0006)	(0.0034)	(0.0195)	(0.0180)
[NPIs length $>$ 90 days] $\times$ [Birth Year $= 1914 - 1917$]	-0.5377	-1.1776**	-0.4740**	0.0014**	-0.0006	0.0090	-0.0098
	(0.5119)	(0.5827)	(0.2010)	(0.0005)	(0.0034)	(0.0227)	(0.0209)
[NPIs length $>$ 90 days] $\times$ [Birth Year $= 1918 - 1920$]	0.6361*	0.2292	-0.0283	0.0002	-0.0004	-0.0206	0.0208
	(0.3365)	(0.4410)	(0.1343)	(0.0006)	(0.0028)	(0.0214)	(0.0202)
Observations	515,605	513,758	515,605	515,605	515,605	515,605	515,605
R-squared	0.0948	0.1157	0.1201	0.0012	0.0101	0.1353	0.1477
Birth-Year fixed effects	✓	✓	✓	✓	✓	✓	✓
City fixed effects	✓	✓	✓	✓	✓	✓	✓
City-level controls	✓	✓	✓	✓	✓	✓	✓

Notes: Standard errors, clustered on city, are in parentheses. City covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, share of homeowners, share of households with children under 5, and literacy rate. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

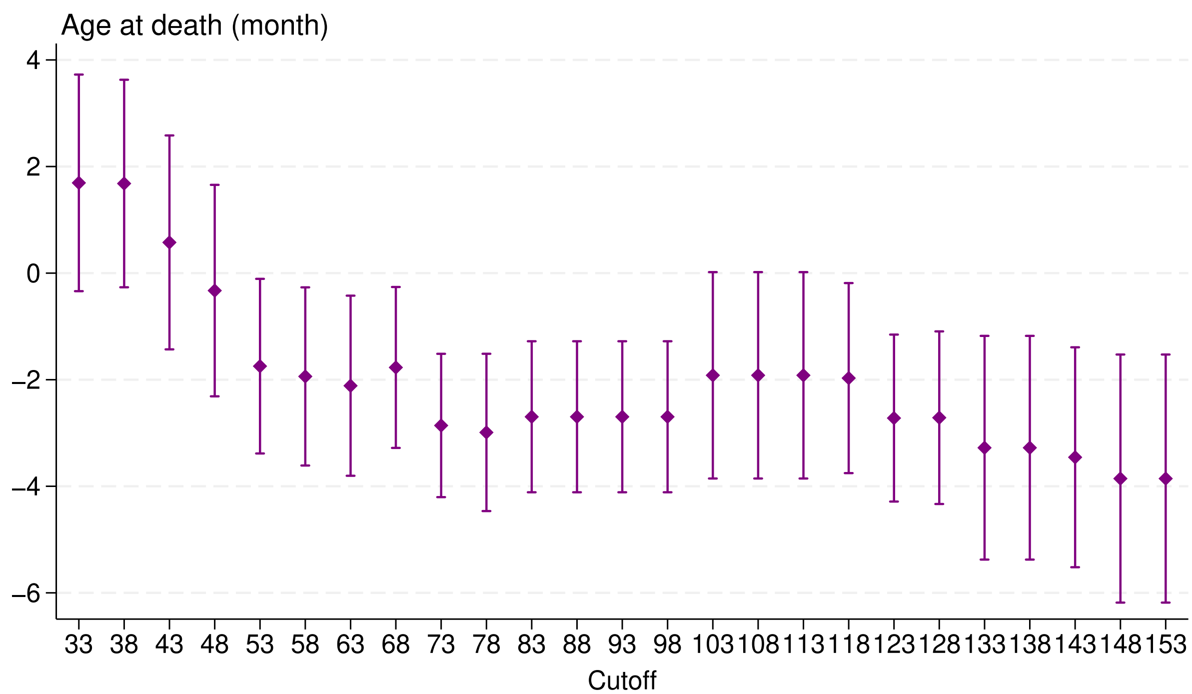
Table 5: Robustness Checks

	Outcomes:						
	Baseline (column 3 of table 3) (1)	Adding death-month FE (2)	Adding birth-state by 1940-state FE (3)	Adding region-by-birth-year FE (4)	Clustering std. err. on state (5)	Outcome: Log age at death (6)	Outcome: Age at death > 70 years (7)
[NPIs length > 90 days] $\times$ [Birth Year $= 1910 - 1913$]	-2.7546***	-2.7261***	-2.6690***	-1.8583**	-2.7546***	-0.0029***	-0.0122***
	(0.6826)	(0.6766)	(0.7097)	(0.7539)	(0.5965)	(0.0007)	(0.0026)
[NPIs length > 90 days] $\times$ [Birth Year $= 1914 - 1917$]	-0.8427	-0.8197	-0.8361	-0.6994	-0.8427	-0.0009	-0.0053**
	(0.6092)	(0.6092)	(0.6398)	(0.5435)	(0.6713)	(0.0007)	(0.0026)
[NPIs length > 90 days] $\times$ [Birth Year $= 1918 - 1920$]	0.5692	0.5732	0.5801	0.5304	0.5692	0.0006	-0.0019
	(0.7685)	(0.7612)	(0.7354)	(0.8545)	(0.5305)	(0.0008)	(0.0031)
Observations	1,388,715	1,388,715	1,388,715	1,388,715	1,388,715	1,388,715	1,388,715
R-squared	0.1444	0.1450	0.1452	0.1454	0.1444	0.1365	0.0463
Birth-Year fixed effects	✓	✓	✓	✓	✓	✓	✓
City fixed effects	✓	✓	✓	✓	✓	✓	✓
City-level controls	✓	✓	✓	✓	✓	✓	✓

Notes: Standard errors, clustered on city, are in parentheses. City covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, share of homeowners, share of households with children under 5, and literacy rate. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

8 Figures

Figure 1: Robustness to Different Cutoffs



Notes: This figure explores the robustness of our findings when using different cutoffs to categorize cities as having longer or shorter NPIs. We conduct a series of analyses employing various thresholds for the duration of NPIs, ranging from as low as 33 days to as high as 156 days (representing the 10th and 90th percentiles in the NPIs duration distribution, respectively). This figure reports the estimates of β_1 from Equation 1. The vertical bars present the 95% confidence intervals. Standard errors are clustered at the city-level.

A Appendix

Table A.1: Robustness to Using School Closure Length

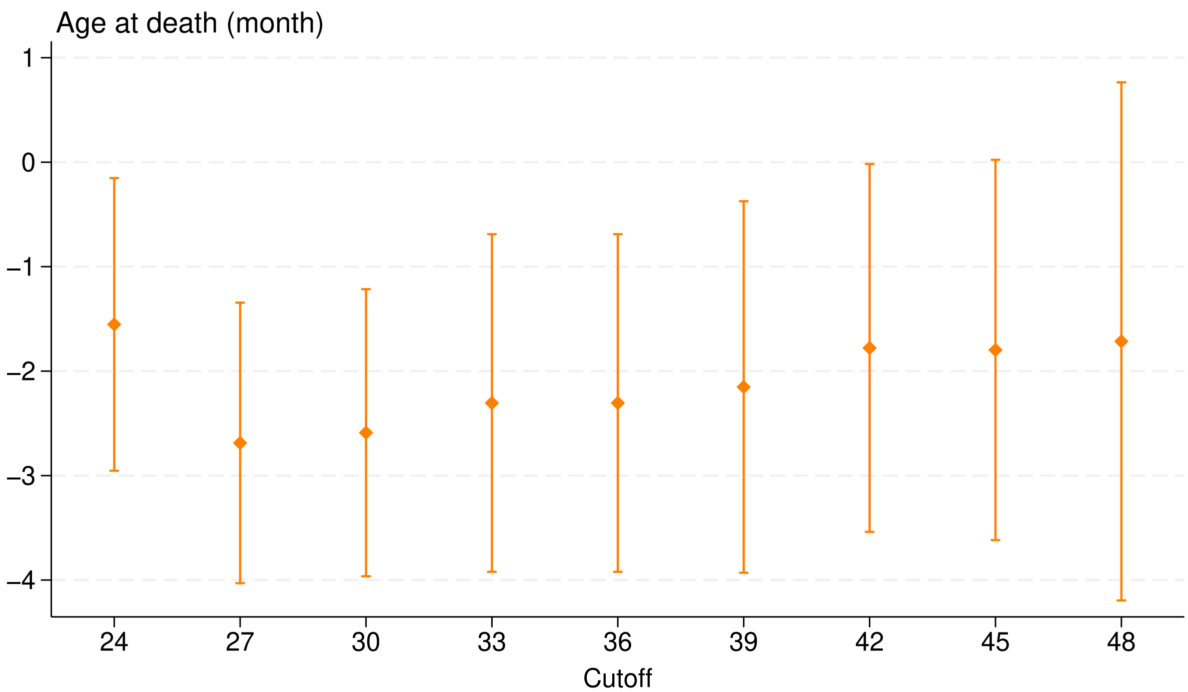
	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
[School closures $>$ 30 days] $\times$ [Birth Year $= 1910 - 1913$]	-3.9347**	-3.6773**	-2.5895***
	(1.5463)	(1.5452)	(0.6854)
[School closures $>$ 30 days] $\times$ [Birth Year $= 1914 - 1917$]	-1.5562	-1.4698	-0.8188
	(0.9625)	(0.9527)	(0.5690)
[School closures $>$ 30 days] $\times$ [Birth Year $= 1918 - 1920$]	-0.0140	-0.0157	0.4839
	(0.4833)	(0.4946)	(0.6421)
Observations	1,388,715	1,388,715	1,388,715
R-squared	0.1435	0.1442	0.1444
Birth-Year-Month fixed effects	✓	✓	✓
City fixed effects	✓	✓	✓
Parental controls		✓	✓
City-level controls			✓

Notes: Standard errors, clustered on city, are in parentheses. City covariates include share of population in different age groups, share of different occupations, share of females, share of Blacks, share of immigrants, share of homeowners, share of households with children under 5, and literacy rate. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.2: Non-Pharmaceutical Interventions Across 57 Cities

City	State	Days of NPIs	Source
Albany	NY	47	Markel et al.
Atlanta	GA	46	Correia et al.
Baltimore	MD	43	Markel et al.
Birmingham	AL	48	Markel et al.
Boston	MA	50	Markel et al.
Buffalo	NY	49	Markel et al.
Cambridge	MA	49	Markel et al.
Charleston	SC	69	Berkes et al.
Charlotte	**NC**	**114**	**Authors**
Chicago	IL	68	Markel et al.
Cincinnati	OH	123	Markel et al.
Cleveland	OH	99	Markel et al.
Columbus	OH	147	Markel et al.
Dallas	TX	41	Berkes et al.
Dayton	OH	156	Markel et al.
Denver	CO	151	Markel et al.
Des Moines	IA	56	Berkes et al.
Detroit	MI	29	Berkes et al.
Fall River	MA	29	Markel et al.
Grand Rapids	MI	60	Markel et al.
Houston	**TX**	**51**	**Authors**
Indianapolis	IN	62	Markel et al.
Jersey City	NJ	82	Correia et al.
Kansas City	MO	170	Markel et al.
Los Angeles	CA	154	Markel et al.
Louisville	KY	145	Markel et al.
Lowell	MA	59	Markel et al.
Memphis	TN	33	Correia et al.
Milwaukee	WI	132	Markel et al.
Minneapolis	MN	116	Markel et al.
Nashville	TN	55	Markel et al.
New Haven	CT	39	Markel et al.
New Orleans	LA	78	Markel et al.
New York City	NY	73	Markel et al.
Newark	NJ	33	Markel et al.
Oakland	CA	127	Markel et al.
Omaha	NE	140	Markel et al.
Paterson	NJ	172	Correia et al.
Philadelphia	PA	51	Markel et al.
Pittsburgh	PA	53	Markel et al.
Portland	OR	162	Markel et al.
Providence	RI	42	Markel et al.
Richmond	VA	60	Markel et al.
Rochester	NY	54	Markel et al.
Saint Louis	MO	143	Markel et al.
Saint Paul	MN	28	Markel et al.
Salt Lake City	UT	141	Berkes et al.
San Antonio	TX	81	Correia et al.
San Francisco	CA	67	Markel et al.
Scranton	PA	69	Correia et al.
Seattle	WA	168	Markel et al.
Spokane	WA	164	Markel et al.
Syracuse	NY	39	Markel et al.
Toledo	OH	102	Markel et al.
Tulsa	**OK**	**84**	**Authors**
Washington	DC	64	Markel et al.
Worcester	MA	44	Markel et al.
Wichita	**KS**	**153**	**Authors**

Figure A.1: Robustness to Different School Closure Cutoffs



Notes: This figure explores the robustness of our findings when using different cutoffs to categorize cities as having longer or shorter school closures. We conduct a series of analyses employing various thresholds for the duration of school closures, ranging from as low as 24 days to as high as 48 days (representing the 25th and 75th percentiles in the school closure duration distribution, respectively). The vertical bars present the 95% confidence intervals. Standard errors are clustered at the city-level.