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

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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 that stricter NPIs reduced longevity of approximately 2.8 months for individuals exposed between ages 7 and 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, Life Expectancy, Lockdowns, Flu Pandemic, Covid-19

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

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1 Introduction

The first documented case of the infamous Spanish Flu in the United States emerged in the spring of 1918. The pandemic peaked in the fall of that year and saw a subsequent wave in early 1919. Approximately one-third of the U.S. population contracted the virus, resulting in an estimated 650,000 deaths (Patterson and Pyle, 1991; Gagnon et al., 2013).[1] In response to the pandemic, local and state public health authorities implemented a range of measures to curb its spread. Central to these efforts were Non-Pharmaceutical Interventions (NPIs), including social distancing, mask mandates, travel restrictions, public awareness campaigns, school closures, and lockdowns (Tomes, 2010). Recent studies suggest that these interventions were successful in reducing mortality rates (Hatchett et al., 2007; Markel et al., 2007).

Existing research has largely focused on the lasting impacts of the pandemic itself, particularly for those exposed during early life (Beach et al., 2022). Studies suggest that in-utero and early-life exposure to the pandemic is associated with higher rates of disability in later life (Almond, 2006), poorer self-reported health (Almond and Mazumder, 2005), and increased old-age mortality (Mazumder et al., 2010; Myrskylä et al., 2013; Fletcher, 2018, 2019). Despite this growing body of evidence, few studies have examined the long-term effects of NPIs on later-life outcomes. This paper seeks to address this gap by investigating how early-life and childhood exposure to NPIs during the 1918 pandemic influenced longevity in old age.

We utilize Social Security Administration death records linked to the full-count 1940 census, which provides key individual and family characteristics as well as inferred childhood city of residence—critical information for our analysis. We then compile city-level data on NPIs stringency to assess their long-term effects on longevity. Using a difference-in-differences approach, we compare the longevity of cohorts exposed to stricter NPIs during

[1] During this period, U.S. GDP and personal consumption dropped by about 1.5 and 2 percent, respectively (Barro et al., 2020). 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).

early life and childhood to those in less restrictive environments, relative to the difference among cohorts born after the pandemic, when no NPIs were in place.

Our findings indicate a significant reduction in longevity for cohorts exposed to strict NPIs—defined as total NPIs duration exceeding 90 days—between ages 7 and 10, while we detect no significant effects for younger children, including those in utero during NPIs implementation. A plausible explanation for this longevity decline is the disruption of socioemotional and cognitive development due to school closures, particularly as formal education typically begins around age seven. However, other mechanisms may also contribute. Increased household stress from parental illness or death, economic hardship, and broader indirect effects of NPIs could have negatively impacted children's long-term health and socioeconomic trajectories.

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 partial role in the long-term links between exposure as children and reduced longevity later in life.

Our study makes two contributions to the literature. First, to our knowledge, this study is the first to examine the long-term effects of NPIs implemented during the 1918 pandemic. More broadly, research on the later-life impacts of NPIs across historical pandemics remains scarce. Given the ongoing policy debates surrounding the costs and benefits of NPIs during the COVID-19 pandemic (Lai et al., 2020; Mendez-Brito et al., 2021), understanding these typically unobserved long-term effects is both timely and critical. Our findings provide valuable insights that directly inform these discussions. Second, we contribute to the growing body of research on how early-life and childhood exposures shape long-term mortality (Hayward and Gorman, 2004; Van den Berg et al., 2006; Almond et al., 2018; Schmitz and Duque, 2022). More specifically, we add to a focused research area examining the long-term health consequences of early-life disease environments and the potential mitigating role 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

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 NPIs 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 NPIs 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-NPIs 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 NPIs 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).

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

[4]Berkes et al. (2023) noted a significant absence of NPIs lengths around 90 days, with no cities having NPIs 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.

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 Identification Assumptions

5.1.1 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 NPIs 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 NPIs 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-NPIs 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-NPIs 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).

5.1.2 Parallel Trend Assumption

Our empirical strategy also relies on the standard parallel trends assumption: mortality outcomes in short-NPIs cities provide a valid counterfactual for mortality outcomes in long-NPIs cities. Therefore, as is common with difference-in-differences designs, we test this identifying assumption by presenting the longevity of each single-year cohort plotted in one graph, splitting the time series by more and less stringent locations (essentially a raw version of an event study), as shown in Figure 1. We would expect to see a fixed difference in longevity for cohorts born after the pandemic and a separation of longevity trends for cohorts exposed to the pandemic.

Figure 1 shows that, while there is a noticeable divergence in longevity for cohorts born between 1910 and 1913—who were directly impacted by the NPIs during their school years—such divergence is not observed for the earlier and later cohorts, including those born between 1921 and 1924. The longevity trends for these later cohorts remain consistent across cities, regardless of NPIs stringency, indicating that these cohorts were less influenced by the Spanish Flu NPIs. This evidence suggests that the chosen control cohorts may provide a plausible and stable comparison group, with less likelihood of being significantly affected by pandemic-related disruptions.

Figure 1 also addresses a potential concern regarding the control cohorts (those born between 1921 and 1924). Specifically, some children in these cohorts were born in 1920–1921 and were 7–10 years old during the Great Depression. These cohorts could have been exposed to various place- and time-specific economic shocks that might affect their longevity. Furthermore, the geographic distribution of these economic shocks could correlate with the patterns of NPIs stringency during the Spanish Flu. However, the data show no significant differences in the longevity of the 1921–1924 cohorts between short- and long-NPIs cities. This finding suggests that the economic shocks of the Great Depression were not systematically correlated with the geographic patterns of NPIs stringency during the Spanish Flu.

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 NPIs 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.

Parental death. One potential threat to our results is the impact of parental death. The Spanish flu caused significant mortality, likely leaving some children orphaned—a factor that could have lifelong consequences and partly explain our findings. However, evidence suggests that longer durations of NPIs during the 1918-1919 Influenza Pandemic were associated with a reduced overall mortality burden (Markel et al., 2007). Therefore, parental death rates in cities with longer NPIs may have been lower, which would likely bias our estimates toward zero.

5.3 Mechanisms

The effects of NPIs on the longevity of young children later in life may operate through several pathways. First, isolation and lockdowns can disrupt the social, emotional, and cognitive development of children. There is evidence suggesting that lockdowns and other NPIs lead to increases in stress, anxiety, and isolation, which can negatively affect children's socioemotional outcomes and measures of aptitude abilities (Fernández Cruz et al., 2020; Sancho et al., 2021; Martín-Requejo and Santiago-Ramajo, 2021). Such disruptions can be amplified by school closures and reductions in cognitive stimulations in specific critical periods with consequences for overall human capital development. Moreover, NPIs also associated with business closures and a downturn in economic activities that could result in parental job loss, reductions in household income, and financial distress. Several strands of research examining the link between general economic conditions and children's developmental outcomes (Mörk et al., 2020). Further, lower financial resources may lead to lower investments in children's health and education with potential long-run impacts (Fletcher and Wolfe, 2016).

Furthermore, NPIs could indirectly impact children's health and development through increased household stress or family disruptions caused by parental illness or death. Such disruptions, especially resulting from parental death, have long-lasting consequences for children's health and mortality outcomes, with the effects concentrated on exposure during ages

7-17 (Smith et al., 2014; Berg et al., 2014; Case and Ardington, 2006).

These early-life adversities can then translate into lower educational attainment and reduced socioeconomic status later in life. Empirical evidence supports the idea that both education and socioeconomic status are influential determinants of later-life mortality outcomes (Lleras-Muney, 2005; Salm, 2011; Fletcher, 2015; Chetty et al., 2016). Building on this, we examine how exposure to NPIs impacted the educational attainment and socioeconomic status of the exposed children.

In so doing, we focus on census data over a period similar to that of 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. For the socioeconomic index, we observe reductions for the 1910-1913 and 1914-1917 cohorts, though the coefficients are insignificant (column 1). We also find small, positive, and significant increases in the socioeconomic index of the 1918-1920 cohorts, suggesting very small benefits of NPIs for those probably in-utero and their early-life.

Turning to occupational outcomes, we find significant reductions in both the occupational educational score and occupational income score of the 1910-1913 and 1914-1917 cohorts (columns 2-3). For example, we observe a reduction of 1.5 and 0.6 units for the occupational educational score and the 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.

[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.

Educational attainment also shows notable effects. The likelihood of completing fewer than five years of schooling increases significantly for the 1910–1913 and 1914–1917 cohorts (Column 4). Specifically, 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 findings suggest that NPIs may have contributed to lower educational attainment.[6] Importantly, these effects are concentrated among individuals in the lower tail of the education distribution, as no significant changes are observed for other educational groups (Columns 5–7).

Our findings of negative impacts on education are consistent with Li and Malmendier (2022), which documents significant adverse effects 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 these outcomes with lockdowns (Fernández Cruz et al., 2020; Idoiaga Mondragon et al., 2021; Martín-Requejo and Santiago-Ramajo, 2021).

Taken together, our findings suggest that the long-term effects of NPIs on longevity for children aged 7 to 10 may operate through mid-term outcomes such as education and income. A likely explanation for this is the impact of school closures, which disrupted socioemotional and cognitive development during a critical period when formal education typically begins. However, other factors, such as increased household stress, economic hardship, or instability caused by parental death or illness, may also contribute. Importantly, as shown in Table

[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).

A.1, our primary results remain robust when focusing specifically on the duration of school closures rather than the overall length of NPIs. This further supports the hypothesis that school closures may play a significant role in the observed effects, though they may not be the only contributing mechanism.

5.4 Robustness Checks

One potential concern is that NPIs are not randomly assigned across cities and are more likely implemented in areas where the epidemic is more severe. To address this, we conduct an exercise to mitigate this issue.While the duration of NPIs is not random, categorizing cities as having long or short NPIs based on predefined thresholds may approximate random assignment of treatment status, particularly for cities with NPIs durations close to the cutoff. Below, we demonstrate that our results remain robust across a series of analyses using various thresholds for NPIs duration.

In Figure 2, 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 NPIs 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 are statistically significant.

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.

[7] In Figure A.1, we conduct a comparable analysis to Figure 2, 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.

6 Conclusion

During major pandemics, the implementation of pharmaceutical interventions are often slow to develop, especially when dealing with 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 NPIs 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 outcomes become critical around age 7 when children start attending school. Empirical evidence further suggests declines in educational attainment and socioeconomic status among exposed cohorts, reinforcing the role of school closures and social development disruptions. However, other factors, such as household stress, economic hardship, and parental illness or death, may also contribute.

The original population of 1910-1913 cohorts in long-NPIs cities observed in the 1940 full-count census consists of 75,876 individuals. Based on the results in Table 3, the intent-to-treat effect shows a 2.8-month reduction in longevity. Consequently, we estimate that approximately 17,704 life-years are lost due to childhood exposure to NPIs within these cohorts. To put these losses into context, we apply estimates of the Value of Statistical Life (VSL). With an average age-at-death of 76.2 years for the final sample, and assuming a VSL of $10 million in 2017 dollars (Viscusi, 2018; Kniesner and Viscusi, 2019; Colmer, 2020), we calculate the Value of Statistical Life for one year to be $131,000 ($10 million/76.2). This results in a back-of-the-envelope estimate of $2.3 billion (in 2017 dollars) in economic losses attributable to NPIs-related reductions in longevity.

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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: Balancing Test: 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: The reference cohort consists of individuals born between 1921 and 1924. Standard errors, clustered on city, are in parentheses. Parental controls include dummies for maternal education, paternal education, and paternal occupation score. City-level controls 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: The reference cohort consists of individuals born between 1921 and 1924. Standard errors, clustered on city, are in parentheses. Parental controls include dummies for maternal education, paternal education, and paternal occupation score. City-level controls 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 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-level controls 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. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

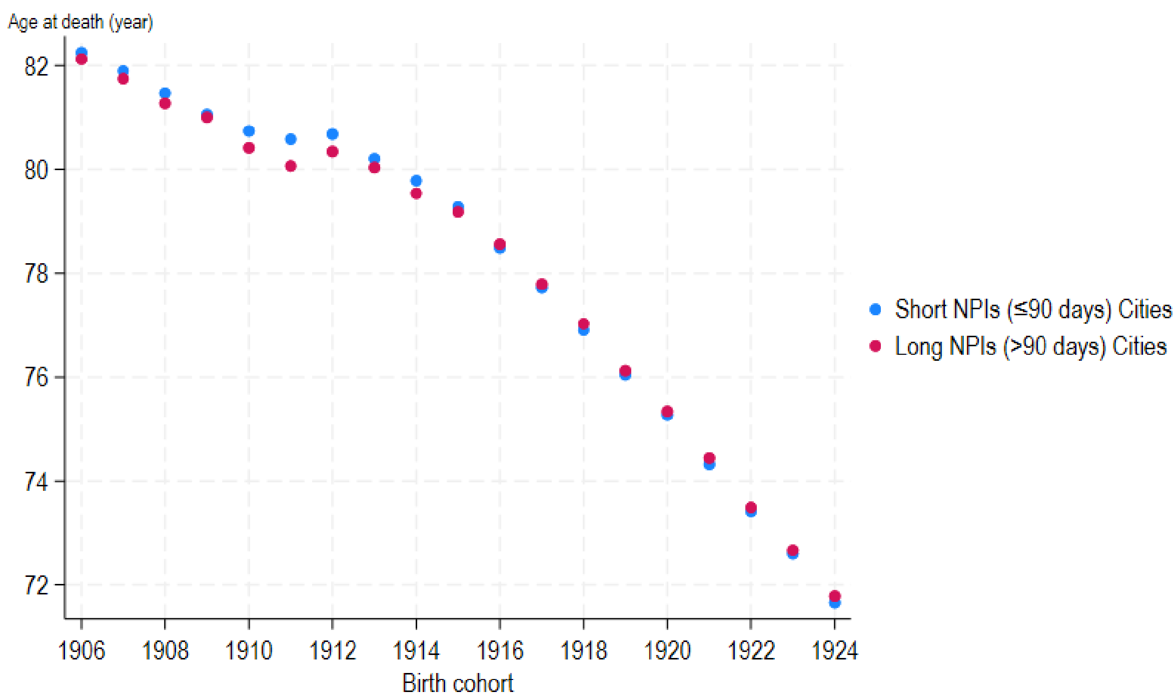
Table 5: Robustness Checks

	Outcomes:						
	Baseline (column 3 of table 3)	Adding death-month FE	Adding birth-state by 1940-state FE	Adding region-by-birth-year FE	Clustering std. err. on state	Outcome: Log age at death	Outcome: Age at death > 70 years
	(1)	(2)	(3)	(4)	(5)	(6)	(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: The reference cohort consists of individuals born between 1921 and 1924. Standard errors, clustered on city, are in parentheses. City-level controls 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. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

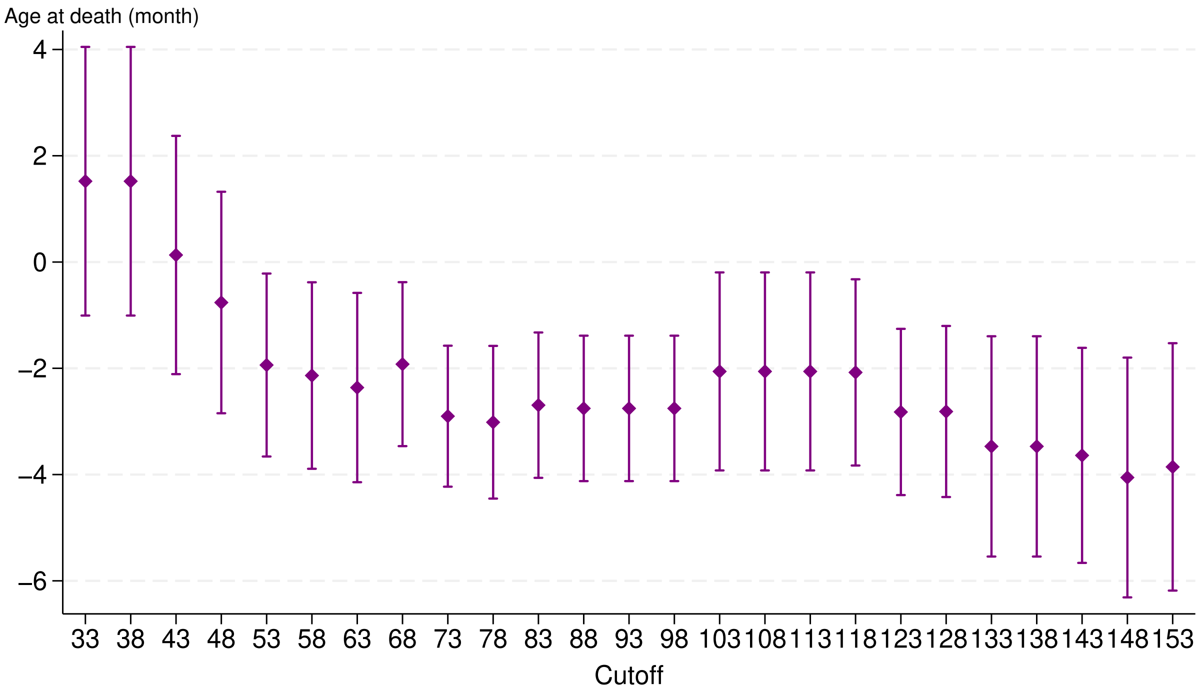
8 Figures

Figure 1: Longevity by Year of Birth and NPIs Status



Notes: This figure compares the longevity of different birth cohorts across cities with short ($\leq$ 90 days) and long ($>$ 90 days) of NPIs. A noticeable divergence in longevity is observed for cohorts born between 1910 and 1913, who were directly impacted by NPIs during their school years. However, no such divergence is seen for later cohorts, including those born between 1921 and 1924 (the reference cohorts in our main specification). It is also important to note that the longevity of younger cohorts appears lower because their full lifespan was not observed, as many were still alive between 1975 and 2005.

Figure 2: 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

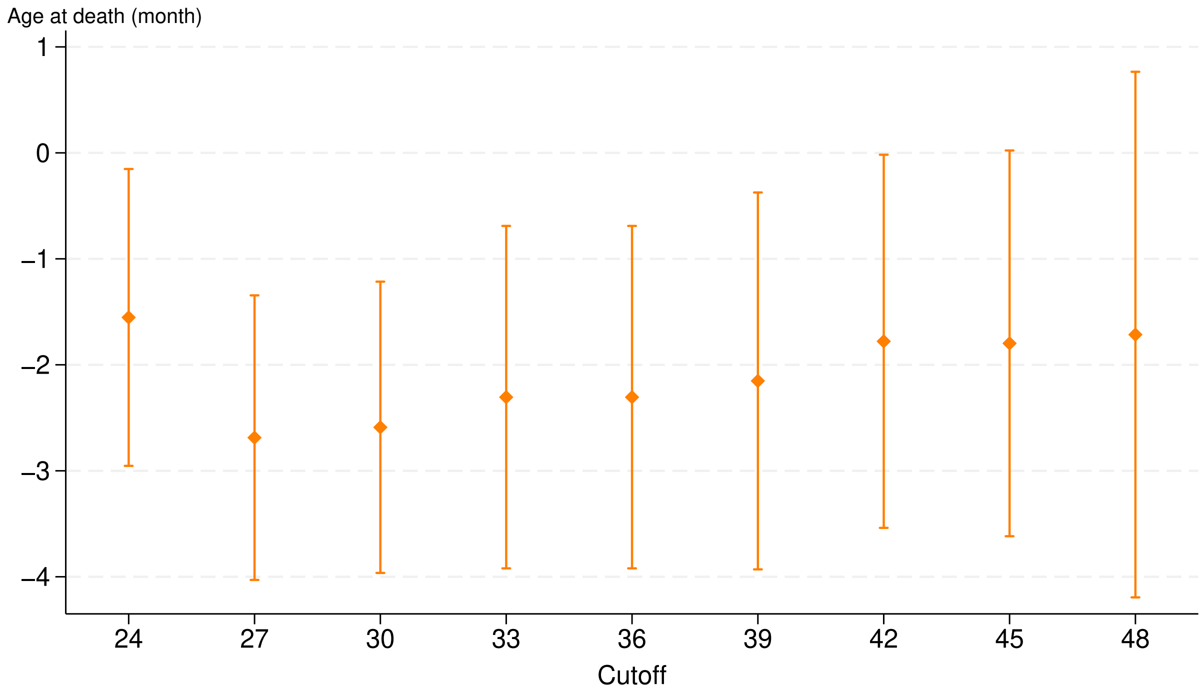
	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
[School closures $>$ 30 days] $\times$ [Birth Year $= 1910 - 1913$]	-3.9347**	-3.6773**	-2.5895***
	(1.5463)	(1.5452)	(0.6854)
[School closures $>$ 30 days] $\times$ [Birth Year $= 1914 - 1917$]	-1.5562	-1.4698	-0.8188
	(0.9625)	(0.9527)	(0.5690)
[School 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: The reference cohort consists of individuals born between 1921 and 1924. Standard errors, clustered on city, are in parentheses. Parental controls include dummies for maternal education, paternal education, and paternal occupation score. City-level controls 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 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.