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 a reduction in longevity of approximately 2.7 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 of their later-life reductions in education and measures of socioeconomic as potential pathways.

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

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

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1. Introduction

The infamous Spanish Flu arrived in the US in spring of 1918 and continued through 1919. Roughly one-third of the population contracted the disease and 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).[5] In response to the pandemic, public health authorities implemented various measures to curb its deadly spread. Despite limitations in available technology and knowledge hindering vaccine or drug development, the focus shifted to crucial strategies such as social distancing, mask-wearing mandates, travel restrictions, public awareness campaigns, school closures, and lockdowns (Tomes, 2010). Recent studies suggest that these Non-Pharmaceutical Interventions (NPI) were successful in reducing death rates (Hatchett et al., 2007; Markel et al., 2007).

The 1918 pandemic resulted in long-lasting impacts for the survivors, specifically those that 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), family formation (J. M. Fletcher, 2018c), and outcomes related to old-age mortality outcomes (Mazumder et al., 2010; Myrskylä et al., 2013; J. M. Fletcher, 2018b, 2018a). However, very few studies have examined the long-run effects of NPIs for 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.

[5] 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).

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 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 longevity of cohorts who experienced 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 pandemic between ages 7-10, but we fail to find any significant impacts for earlier ages and specifically in-utero exposure. We argue that the observed effects are due to school closures and distortions in children's socioemotional and cognitive outcomes, as the critical age of these developmental outcomes starts around age 7, when children start going to schools. 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 sociodemographic and socioeconomic composition of individuals in the final sample. These tests partly rule out the concerns regarding the endogenous survival into adulthood that may confound our findings. In addition, we provided empirical evidence that exposed children reveal reductions in schooling outcomes and socioeconomic measures. These pathways further lend credibility that school closures and disruptions in social developments may have played a role for the long-term links.

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 width 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; Fletcher, 2018b; Schmitz and Duque, 2022). Our study also adds to a narrower research that examine 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, 2023b).

The rest of the paper is organized as follows.

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 national 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 policies 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, 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-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 life.

Second, extended periods of lockdown and social isolation may take a toll on people's mental health. Feelings of loneliness, anxiety, depression, and increased stress levels might become prevalent, as it was the case for the recent pandemic (Adams-Prassl et al., 2022; Viner et al., 2022). The lack of social interaction and limited access to support systems contributed to a decline in overall well-being. There is evidence that maternal stress during pregnancy and more generally parental mental health and well-being affect infants' and children's developmental outcomes, which in turn affect their later-life health and mortality (Goodman et al., 2011; Carlson, 2015; Pierce et al., 2020; Spinelli et al., 2020). Further, the mental pressure can also translate into increased domestic violence and abuse. Stay-at-home mandates change human condition and the pathways of social and physical relationships. This may induce additional burden and exacerbate mental health outcomes of marginally ill individuals leading to the infamous outbreak of domestic violence, the so-called "pandemic within a pandemic" (Evans et al., 2020; Bonomi et al., 2021). Studies suggest that childhood exposure to violence is associated with several measures of social and mental adversities later in life (Greenfield and Marks, 2009; Thoresen et al., 2018).

Another important channel and relevant to the current study are through education disruption. 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 for children's education and health outcomes (Chavez Villegas et al., 2021). These school closures induced by lockdown mandates may in turn affect schooling outcomes, which in turn impact later-life mortality outcomes (Lleras-Muney, 2005; J. M. Fletcher, 2015; Meghir et al., 2018; Halpern-Manners et al., 2020).

Another 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; Almond et al., 2012).

Overall, a priori, we cannot determine the direction of the effects of lockdowns and NPIs on health and well-being. Therefore, the role of NPIs on later-life health and mortality remain 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 source utilized in this study is 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 years 1988-2005 while DMF data covers deaths 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 NPIs policies), 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).[6]

We compile 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 expand the database to include four more cities using information from a variety of news articles.[7] The aggregate duration of nonpharmaceutical interventions is defined as the cumulative count of days encompassing three major categories: school closure, 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 and month-year 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

[6] 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.
[7] 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.

inter-decennial years (Ruggles et al., 2020). 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 cohorts who exposed to NPIs at different ages (age 0-2 (birth years 1918-1920), age 3-6 (birth years 1914-1917), and age 7-10 (birth years 1910-1913) and one cohort born after 1920 who did not expose to NPIs to serve as a control group. The final sample include 1,388,715 individuals. Table Appendix A-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 Method

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 after 1920 when all NPIs were eliminated. Specifically, we estimated models of the following form:

$$\begin{aligned} Y_{ict} = \alpha &+ \beta_1 1[NPIs\_length > 90\ days]_c \times 1[BirthYear = 1910 - 1913]_{ict} \\ &+ \beta_2 1[NPIs\_length > 90\ days]_c \times 1[BirthYear = 1914 - 1917]_{ict} \\ &+ \beta_3 1[NPIs\_length > 90\ days]_c \times 1[BirthYear = 1918 - 1920]_{ict} \\ &+ \beta_4 X_{ict} + \beta_5 Z_{ct} + \xi_c + \zeta_t + \varepsilon_{ict} \end{aligned} \quad (1)$$

Where Y_{ict} is age-at-death (longevity) of person *i* who was born in city *c* during the month and year *t*. $1[NPIs\_length > 90\ days]$ is a dummy variable equals one if the length of NPI policies is greater than 90 days and equals zero otherwise. 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 after 1920 (the omitted cohorts). In particular, these coefficients measure the differences in outcomes observed among these cohorts residing in cities with longer NPI durations (> 90 days) compared to the same cohorts in cities with shorter NPI durations (first difference), relative to the same differences among the omitted cohorts born after 1920 when all NPIs were removed (second difference).

In Appendix Figure A.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 the 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 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 >56), 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 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 1 - Summary Statistics

	Mean	SD	Min	Max
Age at death (months)	928.429	91.97	601	1151
Log age at death	4.344	.103	3.932	4.554
Age at death > 70 Years	.815	.388	0	1
Year of birth	1917.569	3.994	1910	1924
Year of death	1994.944	7.207	1975	2005
$[NPIs\_length > 90\ days] \times [BirthYear = 1910 - 1913]$	.04	.196	0	1
$[NPIs\_length > 90\ days] \times [BirthYear = 1914 - 1917]$	.064	.244	0	1
$[NPIs\_length > 90\ days] \times [BirthYear = 1918 - 1920]$	.036	.187	0	1
$[NPIs\_length > 90\ days]$	.05	.219	0	1
Non-Hispanic Black	.042	.201	0	1
Non-Hispanic white	.947	.224	0	1
Mother’s education < high school	.484	.5	0	1
Mother’s education = high school	.119	.323	0	1
Mother’s education > high school	.013	.112	0	1
Mother’s education missing	.377	.485	0	1
Father’s education < high school	.417	.493	0	1
Father’s education = high school	.089	.284	0	1
Father’s education > high school	.014	.115	0	1
Father’s education missing	.014	.116	0	1
Observations		1,388,715		

Table 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 mother's education more than 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 (Huebener, 2019, 2020). 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 5.3, 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; Fletcher et al., 2021).

5.2. Main Results

The primary results of the regressions presented in equation 1 can be found in Table . 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 parametrized model in column 3, cohorts aged 7-10 resided in longer NPI cities exhibit a reduction in lifespan by approximately 2.7 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 school closure, could 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) examine 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 an approximately 73 percent reduction in longevity compared to the documented impact of historical racialized violence. This underscores the substantial influence of NPIs during childhood on overall well-being.

Aizer et al. (2016) find that children whose mothers received cash assistance under the Mother's Pension (MP) program in the early 20th century lived an average of 11.6 months longer than those who did not. 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 (in magnitude) is roughly 25 percent 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; Berasategi 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 American

Community Survey (ACS) 2005.[8] We restrict the sample to individuals born between 1910-1924 We further restrict the sample to those whose state of residence in the census is the same as state-of-birth to mitigate migration issues.

We implement similar sample merging and empirical approach as sections 3 and 4. We examine the effects on socioeconomic measures and educational outcomes. These results are reported in Table . We observe reductions in socioeconomic index for 1910-1913 and 1914-1917 cohorts, though the coefficients are insignificant (column 1). We also find small, positive, and significant increase in 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 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 occupational educational score and occupational income score of 1910-1913 cohorts, 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 5-years of schooling (column 4). For instance, we find significant increases in education 0-4 years of about 20 and 14 basis-points for 1910-1913 and 1914-1917 cohorts, respectively, off a mean of 0.0048. These estimates indicate that the effects may be partly attributed to decreases in education, likely stemming from school closures, as well as reductions in social interactions and the development of social skills.[9] This fact is more pronounced specifically among those at the

[8] 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.
[9] Based on census 1920, 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., 2020).

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's (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, 2020; and Beach et al, 2018). 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; Berasategi Sancho et al., 2021; Martín-Requejo and Santiago-Ramajo, 2021).

5.4. Robustness Checks

In Table XX, 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 2. 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 is 0.28% aligns with the implied percentage change in Model 1 with respect to the mean of age-at-death (2.7 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.

6. Conclusion

During major pandemics, the implementation of pharmaceutical interventions tend 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 2.7 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. 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 , the intent-to-treat effect is 2.7 months 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 Value of Statistical Life (VSL) to put these numbers into perspective.

One way to put the magnitudes into perspective is to employ Value of Statistical Life (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 (Viscusi, 2018; Kniesner and Viscusi, 2019; Colmer, 2020) and a discount rate of 3 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

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	.103	3.932	4.554
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Father's education missing	.014	.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)
$[NPIs\_length > 90\ days] \times [BirthYear = 1910 - 1913]$	-.0009 (.0045)	-.0001 (.0047)	.0029 (.0278)	-.0151* (.0081)	.0577*** (.0111)	.0041 (.0245)	-.0129 (.0086)	.0067** (.0032)	-.3166 (.2673)	.0446*** (.0089)
$[NPIs\_length > 90\ days] \times [BirthYear = 1914 - 1917]$	.0017 (.005)	-.005 (.0053)	-.0128 (.0127)	-.011* (.0061)	.0595** (.0206)	-.0159 (.0109)	-.0091 (.0063)	.0029 (.0025)	.1411 (.2189)	.0507*** (.0165)
$[NPIs\_length > 90\ days] \times [BirthYear = 1918 - 1920]$	.0008 (.00)	-.0027 (.0039)	-.0019 (.0085)	-.0061* (.0031)	.0283* (.0161)	-.0085 (.0074)	-.0051 (.0035)	-.0007 (.001)	.0302 (.1362)	.0257* (.0135)
Observations	1388715	1388715	1388715	1388715	1388715	1388715	1388715	1388715	766484	1388715
R-squared	.0834	.0967	.1442	.0108	.2273	.1294	.0102	.0178	.0139	.2081
Birth-Year-Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

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

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

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
$[NPIs\_length > 90\ days] \times [BirthYear = 1910 - 1913]$	-4.7753***	-4.5453***	-2.6957***
	(1.4608)	(1.4822)	(.7068)
$[NPIs\_length > 90\ days] \times [BirthYear = 1914 - 1917]$	-1.8121*	-1.6835*	-.7128
	(.9435)	(.9404)	(.6158)
$[NPIs\_length > 90\ days] \times [BirthYear = 1918 - 1920]$	.3585	.428	.6362
	(.5341)	(.5595)	(.7921)
Observations	1388715	1388715	1388715
R-squared	.1435	.1442	.1444
Birth-Year-Month FE	✓	✓	✓
City FE	✓	✓	✓
Parental Controls		✓	✓
City-level Controls			✓

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

	Outcomes:						
	Socioeconomic Index	Occupational Education Score	Occupational Income Score	Education 0-4 Years	Education 5-8 Years	Education 9-12 Years	Education >12 Years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$[NPIs\_length > 90\ days] \times [BirthYear = 1910 - 1913]$	-.7559 (.5575)	-1.5328*** (.5035)	-.6494** (.2542)	.002*** (.0006)	-.0035 (.0034)	.0111 (.0195)	-.0096 (.018)
$[NPIs\_length > 90\ days] \times [BirthYear = 1914 - 1917]$	-.5377 (.5119)	-1.1776** (.5827)	-.474** (.201)	.0014** (.0005)	-.0006 (.0034)	.009 (.0227)	-.0098 (.0209)
$[NPIs\_length > 90\ days] \times [BirthYear = 1918 - 1920]$	.6361* (.3365)	.2292 (.441)	-.0283 (.1343)	.0002 (.0006)	-.0004 (.0028)	-.0206 (.0214)	.0208 (.0202)
Mean of dependent variable	43.1031	48.0528	26.7276	0.0048	0.0162	0.4845	0.4944
Observations	515605	513758	515605	515605	515605	515605	515605
R-squared	.0948	.1157	.1201	.0012	.0101	.1353	.1477
Birth-Year-Month FE	✓	✓	✓	✓	✓	✓	✓
City FE	✓	✓	✓	✓	✓	✓	✓
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

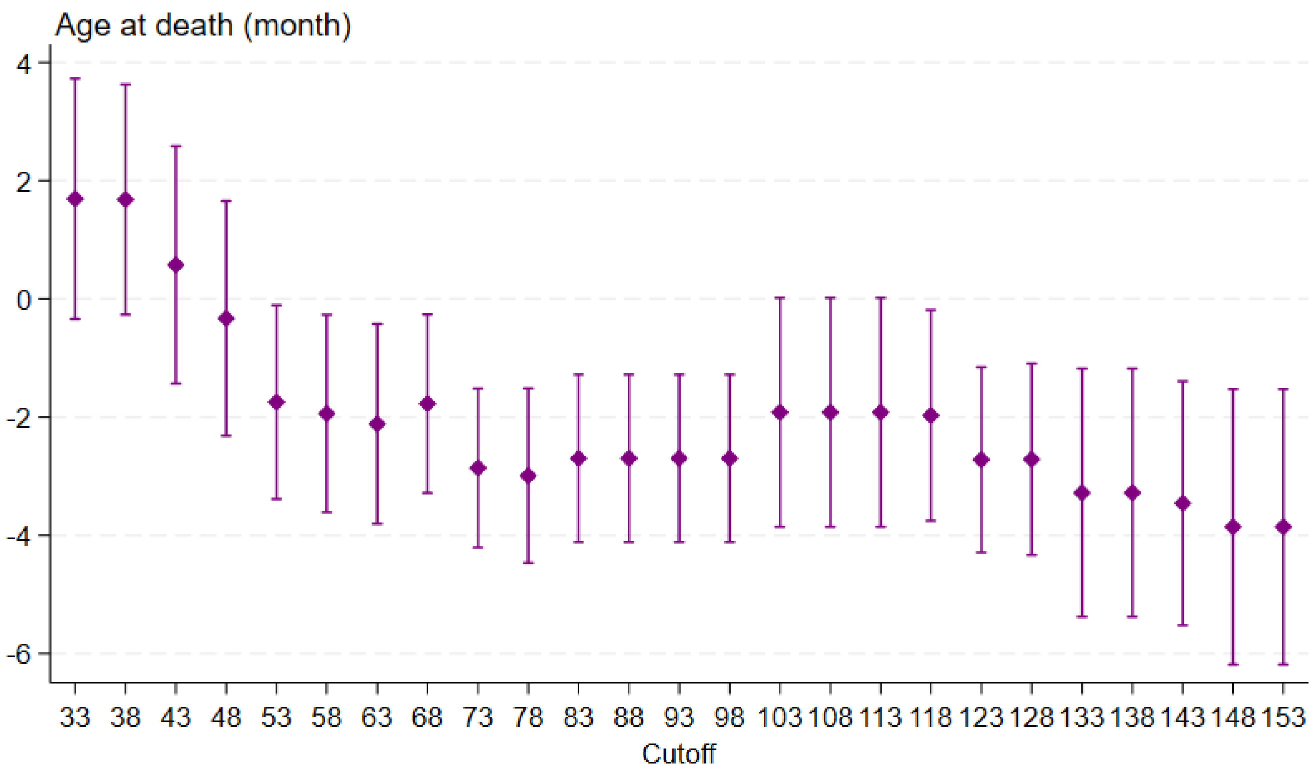
	Outcome: Age at death (months)						
	Baseline (column 3 of table 3)	Adding death-month FE	Adding birth-state by 1940-state FE	Adding region-by-birth-year FE	Clustering std. err. on state	Outcome: Log age at death	Outcome: Age at death > 70 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$[NPIs\_length > 90\ days] \times [BirthYear = 1910 - 1913]$	-2.6957*** (.7068)	-2.6622*** (.7013)	-2.5528*** (.741)	-1.4865* (.8088)	-2.6957*** (.626)	-.0028*** (.0007)	-.0116*** (.0027)
$[NPIs\_length > 90\ days] \times [BirthYear = 1914 - 1917]$	-.7128 (.6158)	-.6917 (.6162)	-.6639 (.6415)	-.3862 (.5366)	-.7128 (.6761)	-.0007 (.0007)	-.0045* (.0025)
$[NPIs\_length > 90\ days] \times [BirthYear = 1918 - 1920]$	.6362 (.7921)	.6413 (.7843)	.6669 (.756)	.6264 (.8643)	.6362 (.5326)	.0007 (.0009)	-.0012 (.0032)
Observations	1388715	1388715	1388715	1388715	1388715	1388715	1388715
R-squared	.1444	.145	.1452	.1454	.1444	.1365	.0463
Birth-Year-Month FE	✓	✓	✓	✓	✓	✓	✓
City FE	✓	✓	✓	✓	✓	✓	✓
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

Online Appendix

Appendix A

Appendix Figure A.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 NPI 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.