Dynamic Complementarities between Early- and Late-Life Exposures: Evidence from the Social Security Notch*

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Abstract

An old and debated research examines the income-mortality relationship and finds mixed evidence. In this paper, we extend the literature in two ways. First, we re-evaluate the previous studies using a new dataset and implementing a Notch in social security retirement benefits to overcome selection and endogeneity issues. We find a positive impact of income on longevity among the elderly population with more pronounced effects among blacks, low educated, and people of low socioeconomic status. Second, we examine the dynamic complementarities of the income shock during old ages with several early-life and childhood exposures. We find a protective role of child labor and compulsory schooling laws in the state-of-birth and adolescence years in buffering the effects of an income shock. Among blacks, exposure to early 20th century Rosenwald school construction moderates the negative income effects. Finally, we provide evidence from the Dust Bowl and New Deal relief spending that economic shocks during childhood and early adulthood can exacerbate the negative income shocks in old age, while government social spending mitigates the negative impacts.

Keywords: Social Security, Retirement, Pension, Mortality, Longevity, Historical Data, Education, Compulsory Schooling, Dynamic Complementarities, Dust Bowl, Rosenwald

JEL Codes: H51, H53, H55, H75, I18, J18, J26, N32

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1. Introduction

Life expectancy in the developed world has experienced a dramatic change during the last century since 1800 by roughly 45 years. This so-called *demographic transition* was accompanied by increases in output, rises in income, sharp improvements in public health, and innovations in drugs and medical technology (Eggleston & Fuchs, 2012; Lee & Reher, 2011). Various studies that span several disciplines have explored the potential sources of improvements in longevity (Lichtenberg, 2004).

A strand of this literature assesses the association between public health improvements, sanitations, vaccines, pharmaceutical innovations, and other healthcare technology changes with mortality and longevity (Baird et al., 2016; Bleakley, 2007; Crimmins & Finch, 2006; Rappuoli, 2014). An old and intensely debated body of literature explores the role of income-health gradient and specifically the role of income in mortality outcomes (Altenderfer, 1947; Cutler et al., 2006; Hupfeld, 2011; Lathrop, 1919; Lefèbvre et al., 2018). Cross-country analyses usually find an inverse relationship between measures of income per capita and mortality outcomes (Baird et al., 2011; O'Hare et al., 2013). However, the literature provides mixed results within a country and across individuals. For instance, studies document a within-month mortality cycle where mortality risks are higher at the beginning of a month following income receipt (Evans & Moore, 2012; Phillips et al., 1999). In addition, several studies document the procyclical nature of mortality (Ariizumi & Schirle, 2012; Coile et al., 2014; Edwards, 2008; Miller et al., 2009; Neumayer, 2004; Ruhm, 2000). Studies that directly examine the impact of personal income on longevity usually find a positive and robust link, suggesting a protective effect of income on mortality (Chetty et al., 2016; Gonzalez & Quast, 2013; Kinge et al., 2019; Kitagawa & Hauser, 1973; Lindahl, 2005). These studies find that, in the case of the US, inequality in life expectancy and longevity between

poor and rich individuals rises as income rises. Moreover, this gap has widened over the past several decades (Chetty et al., 2016; Cristia, 2009). In contrast, other studies find zero effects or even a positive impact of income receipt on mortality (Ahammer et al., 2017; Andersson et al., 2015; Dobkin & Puller, 2007; Engelhardt et al., 2022; Evans & Moore, 2011; Snyder & Evans, 2006).

Isolating the effects of income on health and mortality is challenging for two primary reasons. First, health and income could both be the output of other processes subjecting the gradient to bias due to omitted factors. For instance, individuals with higher discount rates are also healthier, invest more in human capital, and thus have a higher income (Fuchs, 1980; Paulden & Claxton, 2012; Takagi et al., 2016; van der Pol, 2011). Another important example of endogeneity is individuals' health status, which affects their income and leads to differential longevity. Second, individuals may observe their expected longevity and make human capital investment decisions accordingly, affecting their lifetime income (Ferreira & Pessôa, 2007; Hoque et al., 2019; Zhang et al., 2003). This heterogenous response to changes in expected life expectancy could also impact parental investment in their children's health and human capital (Fortson, 2011; Jayachandran & Lleras-Muney, 2009). In addition, the parental investment could also differ by children's initial health endowment, which affects their future income and longevity (Almond & Mazumder, 2013; Fan & Porter, 2020; Restrepo, 2016).

In order to tackle these problems, several studies implement policy changes in welfare benefit receipt and social security income receipt as an arguably exogenous source of income change (Arno et al., 2011; Emery et al., 2012; Evans & Moore, 2011; Nelson & Fritzell, 2014; Salm, 2011; Stoian & Fishback, 2010). Their findings are, however, far from conclusive. For instance, Snyder & Evans (2006) use abrupt and large changes in social security retirement benefits

as the source of income shock and show that reductions in benefits reduce mortality rates of the elderly population.

On the other end, there is a growing literature that examines the effects of early-life shocks and experiences of childhood and adolescence years on old-age health, mortality, and longevity (Almond & Currie, 2011; Bailey et al., 2016; Cutler et al., 2006; Cutler et al., 2007; Fletcher, 2015; Fletcher & Noghanibehambari, 2021; Lleras-Muney, 2005; Van Den Berg et al., 2006). In the case of the US, studies have linked various experiences in the early twentieth century to later-life mortality outcomes, including exposure to the Dust Bowl (Atherwood, 2022; Cutler et al., 2007), school spending (Aaronson et al., 2021), child labor and compulsory schooling laws (Fletcher, 2015; Lleras-Muney, 2005; Mazumder, 2008), migration (Black et al., 2015; Deryugina & Molitor, 2021), New Deal social spending (Noghanibehambari & Engelman, 2022), etc. These early-life exposures could interact with the impacts of later-life determinants of health, including income, to influence old-age mortality outcomes (Almond et al., 2018; Cunha & Heckman, 2007; J. J. Heckman, 2007). However, these *dynamic complementarities* between childhood exposures and old-age income in determining longevity have gained very little empirical attention.

In this paper, we employ two datasets from the newly released social security administration death records to make a bridge between the two strands of literature. We use a Notch in the social security retirement benefit payments, which resulted in a sharp and unanticipated reduction in benefits for those born after January 1, 1917, the so-called *Notch cohort*. First, we examine the direct impact of the income shock on longevity. We implement a regression discontinuity design to compare the longevity of individuals born several months after the notch to those born several months before and find reductions in longevity of about 1.5 months. The average income reduction is about $1120, off a mean of $27,330 annually (in 2020 dollars) (Snyder

& Evans, 2006). Therefore, our results suggest that a $1,000 decrease in income (in 2020 dollars) is associated with 0.11 years lower longevity in an elderly population. We show that our results are robust to a wide array of specification checks. We find the effects are more pronounced among females and blacks. Moreover, we replicate the results using alternative data from social security death records linked to the 1940 full-count census. We find almost identical effect sizes. In addition, we find heterogeneous effects that suggest larger impacts among less-educated individuals and those with a low socioeconomic score in 1940.

Moreover, we analyze the dynamic complementarities of the effects by childhood and adolescence exposures. We show that being born in states with stricter child labor laws and compulsory attendance laws has an insulating impact on the income-longevity relationships. The income-longevity association is almost exclusively observed among people whose state-of-birth is the same as their state-of-death, i.e., stayers. Furthermore, we find a stronger income-longevity association among those whose county-of-residence during the 1930s was hit by top-soil erosions and the Dust Bowl. Nonetheless, the effects are imprecisely estimated due to small sample sizes. In addition, we explore the heterogeneity based on cohorts' exposure to government welfare spending under the New Deal program. We find a weaker income-longevity association among those whose county-of-residence during the 1930s experienced higher per capita relief spending and vice versa. Finally, we show that Rosenwald school construction in the early twentieth century had a protective impact on income-longevity links.

This paper contributes to the ongoing literature in two ways. First, we provide new evidence on the income-longevity relationship using new and relatively large data sources. We find effects that contradict similar studies and provide empirical evidence to explain the observed difference. Moreover, since we exploit a Notch in social security payments in our identification

strategy, our results have important policy implications as policymakers constantly revise the schedule and criteria of social security retirement payments. Second, we add to the understudied literature on dynamic complementarities in longevity. While some studies explore the interaction of various exposures for education and other health measures, the literature on longevity is virtually unexplored, specifically for the dynamic complementarities of early-versus-late-life exposures (Bailey et al., 2019; Dioikitopoulos, 2014; Mbiti et al., 2019). This aspect of our study also contains important policy implications and reveals how policies may interact with each other, even with those in effect decades earlier, to affect population health and longevity.

The rest of the paper is organized as follows. Section 2 reviews the related literature. Section 3 gives a background review of the social security policy change that generated the Notch. Section 4 introduces the data sources and econometric method. Section 5 discusses the results. We depart some concluding remarks in section 6.

2. Literature Review

2.1. Income and Mortality

In this subsection, we review the literature examining the association between income and mortality, focusing on old-age mortality.

Several studies document an inverse relationship between income and mortality outcomes. For instance, Arno et al. (2011) explore the link between social security benefits for retirement and elderly mortality. They find significant reductions in mortality rates following the initial implementation of social security in 1940 and the legislation-induced rises in benefits during the 1960s-70s. Salm (2011) examines the mortality effects of two US retirement policy changes in the years 1907 and 1912 that granted old-age pensions to Union Army veterans. He finds substantial reductions in mortality rates of those who received the pension. For instance, he finds that those

exposed to the 1907 reform experience 11.5 percent reductions in age-adjusted mortality, or roughly 0.82 years additional life expectancy, conditional on age 65. Using his estimation of pension payments and converting the dollar values to 2020 dollars, his analysis suggests that a $1,000 increase in income is associated with 1.7 years of higher longevity. Emery et al. (2012) use data from Canada and examine public pension reforms and longevity. They find that a universal public pension program significantly reduces the age-adjusted mortality rate of elderly people. Muennig et al. (2016) examine the impact of tax rebates under the Earned Income Tax Credit program on longevity and find positive effects. They suggest that for each additional life year, the program requires to invest about $7,800. Nelson & Fritzell (2014) use welfare and mortality data from 18 countries over the years 1990-2009 and show that minimum income benefit as the minimum of country-specific social programs is associated with reductions in mortality and gains in life expectancy. Barham & Rowberry (2013) examine the effects of a conditional cash transfer program in Mexico on elderly mortality. They find that the program reduces the old-age mortality rate by about 4 percent. Chetty et al. (2016) employ the universe of tax return data between the years 2001-2014 linked with social security administration death records to examine the income-longevity relationship. They find a strong and robust association. The effects are fairly constant across different income percentiles. For instance, each additional 5 percentile income rise is associated with about 0.8 years higher longevity past age 40. Moreover, they show that the gap in life expectancy between the top and bottom percentiles has increased over time. Kinge et al. (2019) use data from Norway to document a positive income-longevity association. They also find an increasing poor-rich gap in life expectancy between 2005 and 2015.

In contrast to the findings of the discussed papers, other studies suggest zero or positive association between income and mortality. For instance, Snyder & Evans (2006) employ US Vital

Statistics death records from 1979 to 1990 and examine the impact of income on mortality. They use a policy change in 1977 that resulted in a Notch in social security retirement benefit payments. They show that those who experienced a reduction in benefit payments revealed lower mortality rates. They argue that increases in old-age post-retirement labor force participation are likely channels of positive impacts of benefit reductions.

Evans & Moore (2011) examine the income-mortality relationship using US data. They employ several sources of shocks to income, including the 2001 tax relief, social security receipt, and the receipt of Permeant Fund Divided of residents of Alaska. In all cases, they find a consistently positive association. Stoian & Fishback (2010) employ city-level data for the US during the 1930s to examine the potential role of the Old-Age Assistance (OAA) program on elderly mortality. They find improvements in mortality in naïve OLS regressions. However, they find economically small and statistically insignificant effects after implementing instrumental variable strategies to solve the omitted variables bias. (Ahammer et al., 2017) employ an instrumental variable strategy to estimate the effect of wage income on mortality using data from Austria and find zero effects. Engelhardt et al. (2022) explore the impact of social security early retirement age (EEA) on retirement income and mortality. They find that EEA reduces retirement age and increases old-age poverty. The EEA also results in small reductions in age-adjusted mortality rates.

Several studies suggest that unemployment and recessions may benefit survival (Neumayer, 2004; Ruhm, 2000, 2015; Strumpf et al., 2017; Tapia Granados et al., 2017). For instance, Ruhm (2000) documents a procyclical mortality pattern in the US. He argues that economic upturns increase smoking and obesity, reduce physical activity, and result in a less healthy diet. Granados et al. (2017) use a panel of European countries and show that mortality

declined during the Great Recession compared with earlier years. On the other hand, other studies find a negative income-mortality relationship, specifically in the case of older workers. For instance, Coile et al. (2014) show that older workers who experience a recession during their 50s reveal lower longevity. Gordon & Sommers (2016) employ county-level median income and mortality data to document a strong negative association between income and mortality rates.

2.2. Social Security, Retirement, and Elderly Health

Social security retirement benefits can affect a wide range of elderly lives, which may influence their health and longevity. Engelhardt (2008) explores the role of social security benefits on elderly homeownership and finds that a large portion of rises in elderly homeownership during the last decades of the twentieth century is attributable to rises in social security payments. There is narrow evidence of mortality gains of homeownership (Laaksonen et al., 2008). Goda et al. (2011) document that positive income shocks change the utilization of long-term services away from nursing homes toward paid home care services. Tsai (2015) also shows that increases in social security retirement income raise home care utilization. Engelhardt et al. (2005) employ the Notch induced by the social security reform of 1977 (as in the current study) and show that reductions in benefits lead to increases in the likelihood of living with others, altering the living arrangements of elderly people. A small strand of literature examines the impact of different living arrangements on elderly mortality and finds mixed evidence (Feng et al., 2017; Koskinen et al., 2007; Li et al., 2009).

Changes in social security laws can also influence the post-retirement labor supply. Blau & Goodstein (2010) show that changes in social security laws that resulted in the contraction of benefits can explain almost one-half of the post-1990s rise in elderly labor force participation rates. Similarly, Vere (2011) documents that reductions in social security benefits result in more hours

of work among the retired population. This rise in the labor force could influence income, social ties, and social interactions, which in turn affect elderly longevity (Beller & Wagner, 2018; Holwerda et al., 2012; Lleras-Muney, 2022; Rahman, 2010; Steptoe et al., 2013).

Since the current paper's main focus is on the effect of changes in income induced by social security law change on the mortality of elderly population, it is beneficial to review previous research on the health effects of retirement. Generally, the evidence on the health effects of retirement is mixed and inconclusive (Bamia et al., 2008; Bloemen et al., 2017; Bozio et al., 2021; Brockmann et al., 2009; Fitzpatrick & Moore, 2018; Grøtting & Lillebø, 2020; Hagen, 2018; Hernaes et al., 2013; Hult et al., 2010; Stolzenberg, 2011). For instance, Garrouste & Perdrix (2021) reviews the literature on retirement and health. They find that studies usually report improvements in health for early retirement and ambiguous effects on physical health. However, the literature does not find evidence that later retirement affects mortality. Rose (2020) estimates a regression discontinuity design to exploit pension age and find significant effects of retirement on self-reported health. Eibich (2015) uses data from Germany and finds significant improvements in subjective measures of health. He then provides evidence that reductions in work-related stress, increases in sleep duration, and more regular physical exercise are potential mechanisms. Gorry & Slavov (2021) use data from England and employ instruments using the information on eligibility and employer-sponsored pension to instrument retirement decisions. They find mixed effects of retirement on several health biomarkers. For instance, they find insignificant reductions in Diabetes and increases in fibrinogen levels, an indicator of disease risks.

2.3. Early-Life and Childhood Exposures and Later-Life Mortality

There is increasing attention in the literature to linking early-life environment and childhood exposures to later-life outcomes and specifically old-age health and longevity (Almond

et al., 2018; Almond & Currie, 2011b; Caruso & Miller, 2015; Currie & Rossin-Slater, 2015; Goodman-Bacon, 2021; Hayward & Gorman, 2004; Hoynes et al., 2016; Scholte et al., 2015; Steptoe & Zaninotto, 2020). For instance, Bailey et al. (2016) examine the long-run impact of the Mothers' Pension Program, a social program for poor single mothers, on their children's life expectancy later in life. They find that children of beneficiary mothers live almost one year longer lives. Aizer et al. (2020) explore the long-run effects of a large-scale youth training program and find that enrollees are generally taller and reveal higher longevity. Noghanibehambari & Engelman (2022) examine the in-utero and early-life exposure to the social spending under the New Deal program on old-age longevity and find positive and significant effects.

A strand of this literature examines the role of early-life economic conditions on later-life mortality. Duque & Schmitz (2021) and Schmitz & Duque (2022) explore the effect of early-life local economic conditions and later-life mortality and find significant associations. Specifically, those born in states and years with more favorable economic conditions live longer lives in old ages. Cutler et al. (2007) explore the impact of reductions in income during the Dust Bowl in the 1930s on old-age health and find insignificant associations. Atherwood (2022) focus on state-level exposure measures of the Dust Bowl of young adults and explore the effects on their old-age longevity. He finds that those born in areas that were hit harder by the Dust Bowl live slightly longer lives. In more rigorous regressions, he finds effects that are indistinguishable from zero. Noghanibehambari & Fletcher (2022) revisits this question using county-level measures of exposure and document negative and significant effects.

Several studies explore the role of educational policies on later-life health and longevity. Fletcher et al. (2021) employ a sibling comparison strategy and control for the polygenic score to explore the effects of education on old age cognition score. They find economically large and

significant effects of education on old-age cognition. Aaronson et al. (2021) explore the impact of exposure to Rosenwald school construction in the era of Jim Crow laws on later-life mortality and find that exposed cohorts reveal 2-3 months higher longevity. Fletcher & Noghanibehambari (2021) explore the effect of local college expansions on education and mortality. They find that those who attend college due to the increased availability of colleges and new college openings during their adolescence have roughly 1-year higher longevity. Lleras-Muney (2005) examines the association between education and mortality. She exploits compulsory schooling laws as the source of exogenous influence on education and finds significant reductions in mortality.

3. A Short Background on Social Security Notch

Prior to the 1930s, welfare support for old age was primarily by small-scale Old Age Assistance (OAA) program implemented and administered by state and local authorities. After the Great Recession hit the US economy, the federal government intervened through the New Deal relief spending programs, including establishing a social security system in 1935. As a part of this new welfare system, Old Age and Survivors Insurance (OASI) replaced the OAA program and was designed to provide retirement benefits for the elderly. The OASI schedule depended primarily on age at retirement and the pre-retirement nominal wages.

The OASI payments remained constant afterward unless there was a new statutory change adjusting the benefits with new living costs. During the 1970s, Congress used Consumer Price Index (CPI) to index the benefit tables. In the meantime, the schedule was based on unindexed average nominal wages. This so-called double indexation resulted in substantial rises in benefits. In addition, during the 1970s, the pool of workers paying social security taxes expanded as a result of the population booms of the 1950s and 1960s, which generated surpluses in social security. Hence, Congress expanded the benefit schedule even more.

The obviously wrong double indexation coupled with high inflations of the 1970s created a horizon of insolvency for the social security system as soon as the 1980s. Congress enacted in 1977 and replaced nominal wage with an indexed wage method, resulting in reductions in benefits. The new law became effective in 1979. Congress allowed those who retired before this date to remain in the old system. Those who retired after this date are forced to be included in the new system. Hence, a Notch in benefits was created based on which cohorts born after January 1, 1917, receive substantially lower benefits. Krueger & Pischke (1992) suggest that, for a person earning average wages, the Notch results in about 13 percent reductions in benefits. Snyder & Evans (2006) find about 4 percent higher income for pre-Notch generation, or $41 per month (in 1987 dollars).

4. Data and Econometric Method

4.1. Data Sources

Our primary data source is the Berkeley Unified Numident Mortality Database (hereafter BUNMD) extracted from (Goldstein et al., 2021). BUNMD data comprises the most comprehensive individual-level death records released by social security national archives. BUNMD data covers deaths up to 2007 to individuals who filed social security applications. It contains the exact date of birth and death, allowing us to calculate the exact duration of life. It also allows us to measure Notch-cohort individuals precisely. Moreover, it contains information on state-of-birth, which is useful to link with other state-level early-life factors. Another advantage of this data source is that it contains millions of observations, which makes it superior to many other data sources such as the Health and Retirement Study, National Longitudinal Mortality Study, etc. The large sample size also allows for exploring heterogeneity across subsamples while still maintaining statistical power. The only sample restriction is regarding cohort cut-off. For the main analysis of the paper, we restrict the sample to cohorts born up to 10 months before and after the

Notch. Specifically, we avoid including cohorts of 1919 as these cohorts are likely affected by in-utero and early-life exposures of Spanish Flue that may affect their old age health and longevity (Almond, 2006; Fletcher, 2018a, 2018b; Myrskylä et al., 2013). However, in **Error! Reference source not found.**, we show the robustness of the results to alternative bandwidth selection.

As an alternative data source, we also employ Death Master Files (DMF) data that contains male death records of the social security administration linked (at the individual level) to the full-count 1940 census. The DMF data covers death between the years 1975-2005. The advantage of this data over BUNMD is its link to the 1940 census, which allows us to exploit information on family characteristics, individual education and socioeconomic status, and granular county-level geographic information for the years 1935 and 9140. We employ this information to infer county-of-residence during childhood and early adulthood in our analyses of dynamic complementarities.

Summary statistics of BUNMD data are reported in Table 1. The final sample consists of roughly 1,840,000 observations. The average age-at-death is 995 months, or roughly 83 years. Roughly 55 percent of individuals are Notch cohorts. About 90 percent of observations are whites, and 7.8 percent are blacks. The mean age-at-social-security-application is 25.5 years. About 43 percent of observations migrate from their state-of-birth. In **Error! Reference source not found.**, we show the summary statistics of the DMF data and discuss the discrepancies between the average values of the BUNMD sample and the DMF sample.

For the analysis of dynamic interaction with state-of-birth compulsory schooling laws, we employ data on child labor laws and compulsory attendance laws from Acemoglu & Angrist (2000). We follow Acemoglu & Angrist (2000) and Mazumder (2008) to construct separate indices for compulsory attendance and child labor laws as follows. Compulsory Attendance (CA) is a measure of state-imposed mandatory years of schooling and is calculated as the largest of required

years of schooling before dropping out and the difference between minimum school-leaving age and maximum age at enrollment. The child Labor (CL) index measures the enforcement of age limitation for a work permit and is the largest of years of education required for a work permit and the difference between the minimum age for a work permit and the maximum age allowed for school enrollment. We then break these two indices into several dummy variables: CA8 (CA≤8), CA9 (CA=9), CA10 (CA≥10), CL6 (CL≤6), CL7 (CL=7), and CL8 (CL≥8). We merge this data to BUNMD data based on state-of-birth and the year individuals turn 14, an age at which laws could be binding. As reported in Table 1, 43 percent of observations are born in states with a CL8 law, and 13 percent are born in states with a CA10 law.

4.2. Econometric Method

We examine the reduced-form effect of reductions in retirement income due to the Notch generated by the social security policy reform of 1977. The assumption behind our regression is that the discontinuity generated by law is orthogonal to cohort characteristics, and the longevity of cohorts born several months before and after the Notch is unlikely to trend differently except for the effect of the Notch. Moreover, the Notch was unanticipated and could not affect the behavior of the elderly pre-retirement. For instance, it is difficult for those who are in the late stages of their career to have a discernible impact on their wage trend pre-retirement. We exploit the Notch using a regression discontinuity design as follows:

$$y_{id} = \alpha_0 + \alpha_1 Notch_d + \alpha_2 BirthDate_d + \alpha_3 X_i + \varepsilon_{id} \quad (1)$$

Where y is age-at-death of individual i who was born in birth date (month-year) d. The variable $Notch$ is a dummy that equals one if the individual is born after January 1, 1917. We include a linear trend in birth date, while in **Error! Reference source not found.**, we show the

robustness of the main results to including a third-degree polynomial function in birth date. In X, we include individual gender, race, and ethnicity dummies. Finally, ε is an error term.

5. Results

5.1. Balancing Tests

One concern in implementing regression 1 is that there is a systematic difference in Notch cohorts' characteristics versus pre-Notch cohorts and what we observe in estimations of parameter α_1 is a reflection of those characteristics than the Notch effect. For instance, if there are more whites born after January 1, 1917, than those born in the later months of 1916, we may observe coefficients that underestimate (overestimate) the negative (positive) links. The main reason is that white individuals have higher health and longevity for reasons that race dummies in our regression cannot capture. To explore this source of endogeneity concern, we regress observable individual characteristics on $Notch$ dummy controlling for a linear trend in birth date. The results are reported in **Error! Reference source not found.**. The estimated effects suggest very small in magnitude and statistically insignificant changes in the probability of being white, black, Hispanic, and having missing information on race. The percentage change from the outcomes' mean reveals quite small changes. For the outcome of being female, the effect suggests a 25 basis-points rise, off a mean of 0.52. Although the coefficient is significant at 10 percent, the effect suggests only a 0.5 percent change from the outcome mean.

5.2. Placebo Tests

To further validate our regression design, we implement placebo tests in which the Notch is set on January 1, 1916, and the sample includes 10 cohorts (months) before and after the Notch. We use equation 1 to explore the impact of Notch on longevity. The results are reported in column 1 of **Error! Reference source not found.**. The marginal effect suggests an insignificant effect of

0.5 months reductions in longevity. In addition, we define another placebo Notch for the year 1918 and focus on cohorts born 10 months before and after the Notch. Again, the results, reported in column 2 of **Error! Reference source not found.**, suggest an insignificant effect similar to column 1.

5.3. Main Results

We start the main results by depicting a discontinuity in the longevity of cohorts at the Notch in Figure 1. We show the results for cohorts born 10 months pre- and post-Notch. The top panel includes a linear fit in birth date, and the bottom panel implements a third-degree polynomial fit. The outcome is death age in years. Since the death window of BUNMD is limited to deaths between 1977-2007, age-at-death is larger for earlier-born cohorts. This fact does not confound the estimates as the inclusion of birth date trend in regressions accounts for the linear evolution of longevity across cohorts. Moreover, we observe a similar slope in reduction of longevity across cohorts born before and after Notch, suggesting their longevity does not trend differently. At the Notch, we observe a noticeable discontinuity and a sharp drop in the longevity of Notch cohorts.

We report the main results of the paper in **Error! Reference source not found.**. The marginal effect of column 1 suggests a reduction in longevity of about 1.5 months. The effect is virtually unchanged when we include individual covariates in column 2. The results suggest a positive association between income and longevity. This finding is in line with several studies that explore the effect of income benefits, other pension reforms, and personal and family income on health and longevity (Aguila et al., 2015; Chetty et al., 2016; Golberstein, 2015; Kinge et al., 2019; Nelson & Fritzell, 2014; Salm, 2011). However, this effect is in contrast with the findings of Snyder & Evans (2006), who use Vital Statistics death records between the years 1979-1990 and show that the Notch generated mortality gains. To better compare our results with theirs, we split

the sample based on the death window to have a sample that matches their paper and samples outside their paper's death window. Specifically, we start by expanding the sample to cover deaths from 1970-1976. In another sample, we include deaths between 1977 (the start year of our final sample and also the policy reform year) and 1978 (one year before the Snyder & Evans (2006)'s sample). The third sample covers deaths between 1979-1990, similar to Snyder & Evans (2006). In the fourth sample, we include deaths between 1991-2007. We replicate the regression discontinuity figures for each sample and report the results in Figure 2. The top left panel (death years 1970-1976) does not provide a specific pattern in the longevity of cohorts, nor does it reveal a discontinuity at the Notch. While the top right panel (death years 1977-1978) reveals a decreasing trend in longevity, it does not point to any discontinuity at the Notch. For the death years of Snyder & Evans (2006) (bottom left panel), we observe small increases in the longevity of post-Notch cohorts, consistent with their results. However, for death years post-1990 (bottom right panel), we do observe a noticeable drop in the longevity of the Notch cohort. Furthermore, in **Error! Reference source not found.**, we show the regression results for the four subsamples discussed in his section.

One explanation for the observed difference in the effects is that the Notch, as Snyder & Evans (2006) suggest, results in increases in labor force participation which in several ways could bring mortality gains and offset the negative effect of the drop in income. Once these cohorts reach their 80s and 90s and fewer can continue working, the drop in retirement income becomes effective and reduces longevity.

Snyder & Evans (2006) estimate that the Notch resulted in a reduction in benefits of about $1120 per year, off a mean of $27,330 (in 2020 dollars). Therefore, our results suggest that a $1,000 decrease in income is associated with 0.11 years lower longevity in an elderly population

(in 2020 dollars). Chetty et al. (2016) use tax return data and mortality records to estimate the income-longevity relationship. They use income percentile rather than income level and explore its association with expected life expectancy. They find an increase of 5 income percentile is associated with about 0.7-0.9 years increase in life expectancy. For those at the 10^{th} percentile of income, this means an increase in income from \$16,100 to \$23,000 (in 2020 dollars). Therefore, at the lower income levels, they estimate that a decrease of \$1,000 in annual income is associated with 0.12 years lower longevity, almost equal to our estimated effects. However, they find a linear relationship between income percentile increase and longevity increase, suggesting a concave relationship when we look at income levels. Therefore, one would observe smaller associations at the higher income levels.

To understand the magnitude of the observed effect in **Error! Reference source not found.**, we look at similar studies that use similar outcomes and data but explore different shocks. For instance, Halpern-Manners et al. (2020) employ a twin fixed effect strategy to examine the impact of education on mortality. They find that an additional year of schooling is associated with about 0.3 years higher longevity. Therefore, the Notch has an effect of about 0.42 fewer years of education. Fletcher & Noghanibehambari (2021) employ similar data as the current study and explore the impact of college expansions during adolescence years on college education and later-life longevity. They find a treatment-on-treated effect on those who attended college due to a college opening of about 1-year higher longevity. Therefore, the effect of Notch can offset 13 percent of the positive effect of college education on longevity.

5.4. Robustness Checks

We explore the sensitivity of the results to alternative specification checks and functional forms in Table 8. For comparison, we replicate the effect of column 2 of **Error! Reference source**

not found. in column 1. In column 2, we add state-of-birth fixed effects to account for unobserved heterogeneity of birth-state influences in longevity. In column 3, we allow for these place-based unobserved heterogeneities to have a differential effect on longevity based on race and gender by including an interaction between state-of-birth fixed effects and individual race and gender. In column 4, we allow individuals of different races and genders to have a separate trend in longevity. In all cases, the estimated marginal effects are almost identical to that of column 1.

In column 5, we add state-of-death fixed effects to control for time-invariant state-specific contemporaneous factors that affect longevity. The effect drops by about 47 percent, suggesting part of the effects can be offset by state factors such as state-specific welfare codes and social programs. In column 6, we add month-of-death fixed effects to control for seasonality in mortality. The coefficient is similar to that of column 1.

In the next columns, we check for functional form sensitivity. In column 7, we replace the outcome with the log of age-at-death. The effect suggests that Notch cohorts have 0.17 percent lower age-at-death. This effect is almost equal to the percentage change from the outcome mean reported in row 5 of column 1. In columns 8-10, we replace the outcome with a binary variable indicating longevity beyond ages 70, 75, and 80, respectively. The marginal effects and the percentage change from outcomes' mean suggest that the effects are substantially larger for older cohorts. This fact is also consistent with our analysis of various death windows reported in Figure 2.

In the main results, we use robust standard errors. In columns 11-12, we examine the sensitivity of standard errors to clustering at the birth-month and birth-month-by-birth-year, respectively. Standard errors rise in magnitude, but the effects remain significant at 5 percent level.

5.5. Heterogeneity by Race and Gender

Studies point to the inequalities in longevity by sociodemographic background and that income's effect on health could also be heterogenous by race, gender, and socioeconomic status (Eibich, 2015; Etilé & Milcent, 2006; Meraya et al., 2018). In **Error! Reference source not found.**, we explore the heterogeneity of the effects based on race and gender. We observe effects on blacks that are twice the size of effects on whites. We also observe larger effects among females. Figure 3 shows the regressions discontinuity plots for whites and blacks in left and right panels, respectively. There is a visually larger discontinuity among blacks than whites. Moreover, the decrease in longevity for post-Notch cohorts is steeper than pre-Notch cohorts of black individuals. **Error! Reference source not found.** replicates the regression discontinuity results for females and males, respectively. For each cohort, females live longer than males. Also, the drop in age-at-death at Notch is larger among females. Part of this heterogenous effect based on gender could be due to the selection of deaths before the death window of our sample. As reported in column 5 of **Error! Reference source not found.**, we observe more females in the Notch cohort. Although the implied effect is minuscule, this pre-1977 selection could lead to changes in the effects observed among males as male survivors could have been fitter to reach old ages (Bozzoli et al., 2009; Costa, 2012).

5.6. Heterogeneity by Birth-State Schooling Laws

Several studies examine the impact of education on mortality. Although the evidence is not always conclusive, this literature generally suggests the benefits of education for health and mortality (Albouy & Lequien, 2009; Buckles et al., 2016; Fletcher et al., 2021; Fletcher, 2015; Fletcher & Frisvold, 2015; Galama et al., 2018; Heckman et al., 2013; Hong et al., 2020; Lacroix et al., 2019; Meghir et al., 2018; Savelyev, 2020; Savelyev et al., 2022). While part of the

education-health gradient runs through the labor market and income effects, there are other channels through which education may impact the longevity and buffer other health shocks. For instance, education could increase health-related knowledge, lead to healthier diets, and improve the quality of peers, all of which are shown to influence health and mortality (Cohen-Cole & Fletcher, 2008; Fletcher & Marksteiner, 2017; Nanri et al., 2017; Stringhini et al., 2010).

Another strand of research points to the importance of state-level child labor and compulsory attendance laws on educational attainment, income, wage growth, financial investment decisions, retirement decisions, and health (Acemoglu & Angrist, 2000; Cole et al., 2014; Eckstein & Zilcha, 1994; Le Garrec & LHuissier, 2017; Oreopoulos, 2006, 2007). We explore the interaction of social security reform with state-level educational policies during individuals' adolescent years. We discuss the construction of our measures of child labor and compulsory attendance laws in section 4.1. The results are reported in Table 5. In column 1, we interact indicators of child labor law with the Notch dummy. The effects suggest larger effects for those whose state-of-birth had a child labor law of fewer than 7 years, about a 1.8 months drop in longevity. The interaction terms imply a protective role of CL7 (child labor=7 years) and CL8 (child labor≥8) against the income shock, with the largest effect from CL8. In column 2, we interact the Notch with compulsory attendance laws. Those born in states with a CA8 (compulsory schooling≤8) are more affected by the policy change. They reveal a reduction in longevity of about 1.9 months. We observe a positive interaction term suggesting an insulating role of compulsory schooling laws. However, all the interaction terms are imprecisely estimated and limit additional comments. In column 3, we include all interaction terms of columns 1 and 2, allowing the effects to compete with each other. Individuals born in states with the least strict laws (both CA8 and CL6) reveal an effect size of 1.9 months. The interaction terms of child labor laws converge to

zero. We observe an insulating role of compulsory attendance laws that are almost equal between CA9 and CA10, and both are similar to the coefficients of column 2. This line of findings of our paper adds to our understanding of the benefits of education. Previous studies generally focus on short-term and long-term effects on only one aspect of individuals' outcomes. The findings of Table 5 extend this literature and explore its role in insulating other negative exposures even at the retirement age.

5.7. Heterogeneity by Migration Status

The literature points to the relevance of state-of-birth determinants in mortality, and that migration patterns may partly explain geographic gradients in mortality (Black et al., 2015; Finkelstein et al., 2021; Rasulo et al., 2012; Xu et al., 2021; Xu, Engelman, et al., 2020; Xu, Wu, et al., 2020). Therefore, one would expect to observe differences in effects based on the migration status of individuals, i.e., whether their state-of-birth is different than their state-of-residence. In **Error! Reference source not found.**, we replicate the main results for the subsample of movers and stayers in columns 1 and 2, respectively. We observe an 88 percent drop in the coefficient for movers. Among stayers, the effect suggests 1.2 months lower longevity. The effect is only significant for stayers. This result is consistent with studies that suggest larger human and health capital among migrants (Waldorf, 2008).

5.8. Heterogeneity by the Age of Social Security Application

One advantage of BUNMD data is that it contains information on the year individuals apply for a social security number for the first time. Having a social security number helps them to benefit from social relief programs during economic hardships. Although the social security system was established in 1935 when cohorts were reaching their 20s, studies suggest that welfare programs could still have long-term impacts even for those in their adulthood (Kyyrä & Pesola, 2020;

Schmieder & Von Wachter, 2016). In **Error! Reference source not found.**, we replicate the main results across subsamples of those who applied for social security after and before 1940 in columns 1 and 2, respectively. Although our choice of 1940 is arbitrary, it reflects the social benefit resources that could be available during the Great Recession. We observe noticeably larger impacts among those who applied later versus earlier. Those who applied later reveal a reduction of about 2 months if they are born post-Notch, while the effect on post-Notch cohorts who applied earlier is 1.4 months.

5.9. Replicating the Main Results Using DMF Data

The main advantage of using DMF data is its link to the 1940 census, which allows us to explore other aspects of dynamic complementarities. We employ a similar strategy and replicate the results using DMF data. The estimated effects are reported in **Error! Reference source not found.**. Columns 1-2 are similar to columns 1-2 of **Error! Reference source not found.**. In column 3, we use family-level information from the 1940 census to add more covariates to the model. However, the coefficients across models are almost identical, implying 1.5 months lower longevity for the Notch cohorts. This effect is virtually the same as those obtained from BUNMD in **Error! Reference source not found.**.

Moreover, we show the regression discontinuity plots of DMF analysis in **Error! Reference source not found.**. Both the linear fit (top panel) and third-degree polynomial fit plots reveal a discontinuity at the Notch.

5.10. Heterogeneity by Education and Socioeconomic Status

There is an abundance of evidence that documents disparities in health and longevity by sociodemographic features and that exposures and policies could have a heterogenous impact on different subpopulations (Eibich, 2015; Franks et al., 2003; Meraya et al., 2018). We use individual

record information on education and socioeconomic index (SEI) in the 1940 census to explore potential heterogeneity in effects in our DMF data analysis. The results are reported in **Error! Reference source not found.**. We observe substantially larger effects among low-educated people (education less than high school) and those with a low SEI (a score below the sample median). Among low-educated and low-SEI people, Notch results in up to 3- and 2.5-months lower longevity, respectively. The impacts are lower in magnitude and statistically insignificant for high-educated and high-SEI individuals.

To better understand the observed differences, we depict the regression discontinuity plots for the four subsamples (examined in **Error! Reference source not found.**) in **Error! Reference source not found.** for education heterogeneity and **Error! Reference source not found.** for SEI heterogeneity. We observe a sharp drop in the longevity of low-educated people. Moreover, we observe a steeper slop of decrease in longevity for the Notch cohorts in the subsample of low-educated people (left panel of **Error! Reference source not found.**). We also observe a noticeable drop in low-SEI people's longevity after the Notch (left panel of **Error! Reference source not found.**). However, we do not see a similar pattern for high-educated and high-SEI people. For these two subsamples, the figures suggest a more continuous trend in longevity rather than a discontinuity.

5.11. Heterogeneity by Exposure to the Dust Bowl

From the late 19th and early 20th centuries, agricultural production expanded t large scales in the Great Plains. Deep plowing of virgin topsoil and removal of native grassland that used to protect the soil resulted in soil vulnerability to wind and water erosions. Starting from 1930, topsoil in several states had already lost its resistibility. As a result, wind could easily pick up dust from the topsoil and create clouds of dust known as Dust Clouds. These topsoil erosions, coupled with

droughts, resulted in a huge loss in agricultural income and production for affected counties that lasted for years (Hornbeck, 2012). Several studies examine its enduring impacts on those who experienced the so-called Dust Bowl during infancy, childhood, and adulthood (Arthi, 2018; Atherwood, 2022; Cutler et al., 2007; Noghanibehambari & Fletcher, 2022). In this subsection, we explore the potential differences of the effects of the Notch by individuals' exposures to the Dust Bowl.

We use data from Hornbeck (2011) to split counties into Dust Bowl and outside of Dust Bowl counties. We then use the information on county-of-residence in 1940 and county-of-residence in 1935 (as reported in the 1940 census) to merge with the Dust bowl counties database.[4] We replicate the results for a subsample of people who resided in Dust Bowl counties during the 1930s and those who resided outside of Dust Bowl Counties. The results are reported in **Error! Reference source not found.**. We observe considerably larger effects among Dust Bowl Counties' residents. The coefficient suggests 2.4 months lower longevity, although imprecisely estimated due to a much smaller sample size. The effect is about 1.3 months for those outside Dust Bowl counties.

5.12. Heterogeneity by Exposure to New Deal Spending

As the greatest recession in American history, the Great Depression started in 1929, peaked in 1933, and lasted for almost a decade. Hoover administration and later Roosevelt administration engineered the first federal social programs to help the unemployed and boost the economy. They proposed various programs during different stages of the recession with different criteria and purposes under the so-called New Deal relief programs (Fishback et al., 2001, 2006, 2007, 2011,

[4] To infer the county-of-residence during the 1930s, we stat by using the county-of-residence in 1935 as reported by individual in 1940. For those who report no migration since 1935, we use county-of-residence in 1940. For those who report they have migrated but the information on 1935 county is not available, we again use county-of-residence in 1940.

2010). Several studies point to potential short-run and long-run benefits of social spending in general, and specifically New Deal relief spending (Bailey et al., 2016; Hoynes et al., 2016; Modrek et al., 2022; Noghanibehambari & Engelman, 2022). Therefore, one would expect a possible heterogenous income-mortality relationship by exposure to relief spending at younger ages in the presence of dynamic complementarities.

We use county-level data from Fishback & Kantor (2018) to build a cumulative per-capita county-level measure of total relief spending between 1930 and 1940. We then assign the spending data to DMF observations based on their county-of-residence during the 1930s. Next, we split the sample into below and above-median spending and replicate the main results in each group. The results are reported in **Error! Reference source not found.**. The income-mortality relationship is larger in the subsample of below-median spending, suggesting an effect of 1.6 months. In the subsample of above-median spending, we observe a statistically insignificant effect of 1.3 months. In addition, **Error! Reference source not found.** illustrates the regression discontinuity plots for these two subsamples. While both groups reveal similar longevity across cohorts, there is a slightly larger drop in the subsample of below-median spending (left panel). These results provide suggestive evidence of the protective role of social spending even during adolescence and early youth for negative exposures in later life.

5.13. Heterogeneity by Rosenwald School Construction

During the Jim Crow south, schools were racially segregated, and school availability and quality were limited for black children, specifically in southern states. In 1913, Brook Washington, an educator, and Julius Rosenwald, a businessman, initiated a matching grant program to build schools for black children in the rural south. Their initiative resulted in the construction of almost 5,000 schools in the following decades. Several studies suggest that increases in school availability

and rises in school resources and quality due to the so-called *Rosenwald* school constructions were beneficial for black children's education, income, and other later-life health outcomes (Aaronson & Mazumder, 2011; Frisvold & Golberstein, 2011, 2013; Kreisman, 2017). We use data from Aaronson et al. (2021) to calculate the county-year exposure to the Rosenwald schools when individuals are between 5-17 (years of K-12 education). We then define a dummy that equals one if the person was exposed to school construction and zero otherwise. Since these schools were located in southern states where schools were segregated, the effects are expected to have been only on blacks. To explore the interaction effect of the Rosenwald schools with our later-life income-mortality relationship, we include a triple-interaction of the constructed Rosenwald dummy and a black dummy with the Notch dummy. We implement regressions similar to the main results are report the findings in **Error! Reference source not found.**. In column 1, we only include double-interaction of Notch with a black dummy. In column 2, we interact the Notch dummy with the Rosenwald dummy. In column 3, we include the triple interactions. In contrast to the race heterogeneity of BUNMD, we do not observe a considerable drop in longevity among blacks versus non-blacks (column 1). We observe a protective role of Rosenwald exposure for the average individual in the sample, although the estimated interaction term is imprecisely estimated (column 2).

Based on the estimates in column 3, blacks, on average, have 9.8 lower longevity. Non-blacks who were not exposed to the Rosenwald reveal a Notch discontinuity of about 1.5 months. On average, black people of pre-Notch cohorts who were exposed to Rosenwald schools during K-12 years live 1.7 months longer. Black people of Notch cohorts who were not exposed to the Rosenwald schools live 5.8 months shorter lives. Finally, the triple-interaction term suggests an increase of roughly 8 months among Notch cohorts who are black and exposed to the Rosenwald

schools. This effect is about 40 percent higher (in magnitude) than the net effect of the Notch on non-Rosenwald-exposed blacks. These findings suggest that early life improvements in educational quality and school availability could break the income-mortality relationships during old ages.

6. Conclusion

This paper revisited the old question of the role of income in mortality with new data and new perspectives. While the literature on income-mortality is large, it provides mixed evidence and inconclusive findings (Altenderfer, 1947; Barham & Rowberry, 2013; Chetty et al., 2016; Evans & Moore, 2011; Salm, 2011; Snyder & Evans, 2006). To overcome endogeneity issues, we used a change in the social security retirement benefits policy that resulted in substantially lower benefits for cohorts born after January 1, 1917. We implemented regression discontinuity design and found that post-Notch cohorts have 1.5 months lower longevity. We showed that these effects are robust to a wide array of specification and functional form checks. Moreover, we observed considerably larger effects among blacks, low-educated people, people of lower socioeconomic status families, and females.

We explored the dynamic complementarities with a series of early-life exposures. We found discernible interaction between the income shock during old ages and early-life/childhood exposures. We showed that being born in states with stricter child labor or compulsory attendance laws has the potential to insulate individuals from later-life income shocks. Among black people, those exposed to early 20th century Rosenwald school construction reveals higher insulation to income shocks than those not exposed. We found larger impacts of the income shock on longevity among those who experienced the Dust Bowl during childhood and adolescence. In addition, we found that the effects are primarily focused on people who die in their state-of-birth. On the

contrary, we found a small and insignificant effect of income on longevity for cross-state migrants. These aspects of our research provided new empirical evidence for interactions between early-life exposures and later-life shocks in determining mortality outcomes.

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Tables

Table 1 - Summary Statistics of the Final Sample

	Birth Year = 1916		Birth Year = 1917	
Variable	Mean	SD	Mean	SD
Death Age (months)	910.97366	94.88092	904.65915	95.00414
Discontinuity in Birth Date (Birth Date>January 1, 1917)	0	0	1	0
White	.94625	.22553	.9453	.22739
Black	.05041	.2188	.05091	.21981
Race Missing	.00334	.05766	.00379	.06143
Birth Year	1916	0	1917	0
Birth Month	7.99117	2.56411	5.0396	2.61464
Death Year	1992.0445	7.90546	1992.2719	7.91675
Father's SEI 1st Quartile	.116	.32023	.13073	.3371
Father's SEI 2nd Quartile	.10788	.31023	.12178	.32703
Father's SEI 3rd Quartile	.10855	.31108	.1251	.33083
Father's SEI 4th Quartile	.10676	.30881	.1238	.32935
Father's SEI Missing	.60855	.48808	.54905	.49759
Mother's Education < 8 Years	.38184	.48584	.42711	.49466
Mother's Education = [8,12] Years	.0929	.2903	.10716	.30932
Mother's Education > 12 Years	.02026	.1409	.02336	.15103
Mother's Education Missing	.505	.49998	.44237	.49667
Rosenwald School County	.1691	.37484	.16797	.37384
New Deal Spending 1st Quartile	.26935	.44362	.26986	.44389
New Deal Spending 2nd Quartile	.27733	.44768	.27479	.44641
New Deal Spending 3rd Quartile	.25092	.43354	.25276	.4346
New Deal Spending 4th Quartile	.26512	.4414	.26177	.4396
Dust Bowl County	.26935	.44362	.26986	.44389
Child Labor = 7 Years	.11914	.32396	.11874	.32349
Child Labor ≥ 8 Years	.86652	.3401	.86791	.33859
Compulsory Attendance = 7 Years	.30238	.45929	.30198	.45912
Compulsory Attendance ≥ 8 Years	.48231	.49969	.48287	.49971
Share of Homeowners	.48291	.15552	.4843	.15569
Literacy Rate	.9392	.07814	.93992	.07471
Average Occupational Income Score	23.18494	4.0628	23.22276	4.10976
Share of Married	.59153	.0348	.59491	.03419
Observations	156,212		157,860	

Table 2 – Dust Bowl Exposure during Early Adulthood and Old-Age Mortality

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Dust Bowl County	-1.47102*	-1.9864**	-1.99567**
	(.78658)	(.85271)	(.84995)
Observations	319885	319819	319819
R-squared	.00281	.00432	.00448
Mean DV	907.543	907.545	907.545
%Change	-0.204	-0.235	-0.236
State and Birth Year FE	Yes	Yes	Yes
Controls	No	Yes	Yes
State Trend	No	No	Yes

Notes. Robust standard errors, clustered on state, are in parentheses. Regressions include a linear trend in birth date. Individual controls include dummies for race and ethnicity. Family controls include father socioeconomic status dummies, mother education dummies, and missing indicators for their missing values. County controls include share of literate people, share of married people, share of homeowners, and average occupational income score. The regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates. *** p<0.01, ** p<0.05, * p<0.1

Table 3 – New Deal Spending Exposure during Early Adulthood and Old-Age Mortality

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
4th Quartile of Spending	1.19247***	1.27633***	1.27696***
County	(.40328)	(.41552)	(.41574)
Observations	315750	315750	315750
R-squared	.00281	.00519	.00538
Mean DV	907.419	907.419	907.419
%Change	0.131	0.141	0.141
State and Birth Year FE	Yes	Yes	Yes
Controls	No	Yes	Yes
State Trend	No	No	Yes

Notes. Robust standard errors, clustered on state, are in parentheses. Regressions include a linear trend in birth date. Individual controls include dummies for race and ethnicity. Family controls include father socioeconomic status dummies, mother education dummies, and missing indicators for their missing values. County controls include share of literate people, share of married people, share of homeowners, and average occupational income score. The regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates. *** p<0.01, ** p<0.05, * p<0.1

Table 4 – Exposure to Rosenwald Schools in County during K-12 Education and Old-Age Mortality

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Rosenwald × Black			6.7293***
			(1.55501)
Black		-9.70235***	-13.87154***
		(1.19337)	(1.22418)
Rosenwald	.11297		-.67913
	(1.02396)		(.99466)
Observations	319819	319819	319819
R-squared	.00464	.00514	.0052
Mean DV	907.545	907.545	907.545

Notes. Robust standard errors, clustered on state, are in parentheses. Regressions include birth-state and birth-year fixed effects and a state-specific birth-date trend. Individual controls include dummies for race and ethnicity. Family controls include father socioeconomic status dummies, mother education dummies, and missing indicators for their missing values. County controls include share of literate people, share of married people, share of homeowners, and average occupational income score. The regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates.
*** p<0.01, ** p<0.05, * p<0.1

Table 5 – Exposure to Compulsory Schooling and Child Labor in State-of-Birth at Age 14 and Old-Age Mortality

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Child Labor = 7 Years	.73718		.68245
	(.73667)		(.77428)
Child Labor ≥ 8 Years	1.88564***		1.77883***
	(.49393)		(.4389)
Compulsory Attendance = 7 Years		1.25601**	.90204
		(.53865)	(.65503)
Compulsory Attendance ≥ 8 Years		2.4734**	1.88544*
		(.90565)	(.97569)
Observations	314070	314070	314070
R-squared	.00457	.00454	.00457
Mean DV	907.431	907.431	907.431

Notes. Robust standard errors, clustered on state, are in parentheses. Regressions include birth-region and birth-year fixed effects and a region-specific birth-date trend. Individual controls include dummies for race and ethnicity. Family controls include father socioeconomic status dummies, mother education dummies, and missing indicators for their missing values. County controls include share of literate people, share of married people, share of homeowners, and average occupational income score. The regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates.
*** p<0.01, ** p<0.05, * p<0.1

Table 6 - Balancing Tests

	Outcomes:								
	White	Black	Other Race	Father's Years of Schooling	Father's Years of Schooling Missing	Father's SEI	Father's SEI Missing	Mother's Years of Schooling	Mother's Years of Schooling Missing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Discontinuity	-.00198	.00163	.00035	-.04025	.00331	-.03477	.00525	-.00206	.00803**
at Notch	(.0021)	(.00204)	(.00055)	(.04063)	(.00347)	(.23804)	(.00341)	(.03628)	(.00353)
Observations	319885	319885	319885	147551	319885	135347	319885	169074	319885
R-squared	.00001	0	.00001	.00009	.00485	.00002	.00481	.00017	.00531
Mean DV	0.927	0.068	0.005	6.884	0.568	27.766	0.605	6.916	0.499
%Change	-0.214	2.400	7.042	-0.585	0.584	-0.125	0.867	-0.030	1.608

Notes. Robust standard errors are in parentheses. Regressions include a linear trend in birth date. The regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates.

*** p<0.01, ** p<0.05, * p<0.1

Table 7 - Replicating the Results Using DMF Data (1975-2005)

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Discontinuity at Notch	-1.59269**	-1.55144**	-1.54539**
	(.67185)	(.67144)	(.67157)
Observations	319885	319885	319819
R-squared	.00279	.00418	.00435
Mean DV	907.543	907.543	907.545
%Change	-0.175	-0.171	-0.170
Trend in Birth Date	Yes	Yes	Yes
Individual Controls	No	Yes	Yes
Family Controls	No	No	Yes

Notes. Robust standard errors are in parentheses. Regressions include a linear trend in birth date. Individual controls include dummies for race and ethnicity. Family controls include father socioeconomic status dummies, mother education dummies, and missing indicators for their missing values. County controls include share of literate people, share of married people, share of homeowners, and average occupational income score. The regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates.
*** p<0.01, ** p<0.05, * p<0.1

Table 8 - Robustness Checks

	Outcome: Age at Death (Months)					
	Column 3 Table 7	Bandwidth [-12,12]	Bandwidth [-8,8]	Adding a Third-Degree Polynomial in Birth Date	Adding County FE	Adding Individual and Family Covariates Linear Trend
	(1)	(2)	(3)	(4)	(5)	(6)
Discontinuity at	-1.46879**	-1.02902*	-1.49769**	-1.6284**	-1.21625*	-1.46879**
Notch	(.63849)	(.58572)	(.71078)	(.65447)	(.68703)	(.63849)
Observations	356719	425677	283836	356719	356719	356719
R-squared	.00443	.00517	.00407	.00444	.01503	.00443
	Adding Death Month FE	Outcome: Log Age at Death	Outcome: Age at Death > 75 Years	Outcome: Age at Death > 80 Years	Clustering SE at Birth-Month	Unweighted Regressions
	(7)	(8)	(9)	(10)	(11)	(12)
Discontinuity at	-1.46421**	-.00161**	-.00691**	-.00823***	-1.46879**	-1.49839**
Notch	(.63831)	(.00072)	(.00336)	(.00309)	(.6965)	(.63428)
Observations	356719	356719	356719	356719	356719	356719
R-squared	.0051	.0044	.00267	.00267	.00443	.00415

Notes. Robust standard errors are in parentheses. Regressions include a linear trend in birth date. Individual controls include dummies for race and ethnicity. Family controls include father socioeconomic status dummies, mother education dummies, and missing indicators for their missing values. County controls include share of literate people, share of married people, share of homeowners, and average occupational income score. Except for column 12, the regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates.

*** p<0.01, ** p<0.05, * p<0.1

Table 9 - Heterogeneity in DMF Results by Dust Bowl County

	Outcome: Age at Death (Months)	
	(1)	(2)
Dust Bowl County	-1.63383*	-1.41188**
	(.90635)	(.65274)
Discontinuity at Notch	-1.36744*	-1.31471*
	(.6891)	(.73244)
Discontinuity at Notch × Dust	-.60787	-1.01367
Bowl County	(1.34279)	(1.69137)
Observations	319819	319819
R-squared	.0046	.00476
Mean DV	907.545	907.545
State FE	Yes	Yes
Birth Date Trend	Yes	Yes
Individual/Family Controls	Yes	Yes
State-by-Date Trend	No	Yes

Notes. Robust standard errors, clustered on state, are in parentheses. Regressions include a linear trend in birth date. Individual controls include dummies for race and ethnicity. Family controls include father socioeconomic status dummies, mother education dummies, and missing indicators for their missing values. County controls include share of literate people, share of married people, share of homeowners, and average occupational income score. The regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates. *** p<0.01, ** p<0.05, * p<0.1

Table 10 - Heterogeneity in DMF Results by New Deal Spending County

	Outcome: Age at Death (Months)	
	(1)	(2)
4th Quartile of Spending County	1.35681***	1.35778***
	(.42137)	(.43264)
Discontinuity at Notch	-1.60655**	-1.59061**
	(.60677)	(.59782)
Discontinuity at Notch × 4th	-.14485	-.14553
Quartile of Spending County	(.63747)	(.79328)
Observations	315750	315750
R-squared	.0052	.00539
Mean DV	907.419	907.419
State FE	Yes	Yes
Birth Date Trend	Yes	Yes
Individual/Family Controls	Yes	Yes
State-by-Date Trend	No	Yes

Notes. Robust standard errors, clustered on state, are in parentheses. Regressions include a linear trend in birth date. Individual controls include dummies for race and ethnicity. Family controls include father socioeconomic status dummies, mother education dummies, and missing indicators for their missing values. County controls include share of literate people, share of married people, share of homeowners, and average occupational income score. The regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates. *** p<0.01, ** p<0.05, * p<0.1

Table 11 - Heterogeneity in DMF Results by Rosenwald School County

	Outcome: Age at Death (Months)	
	(1)	(2)
Discontinuity at Notch × Rosenwald × Black	7.00662*	6.77836*
	(3.49096)	(3.48465)
Discontinuity at Notch × Rosenwald	.08563	-.42662
	(1.16236)	(1.48715)
Discontinuity at Notch × Black	-4.72419**	-4.65155**
	(2.25441)	(2.2977)
Black × Rosenwald	2.70064	2.84506
	(2.60653)	(2.60432)
Discontinuity at Notch	-1.59242**	-1.4792**
	(.62154)	(.66196)
Black	-11.17422***	-11.23014***
	(1.71764)	(1.746)
Rosenwald	-.68696	-.40546
	(1.41196)	(1.61357)
Observations	319819	319819
R-squared	.00549	.00566
Mean DV	907.545	907.545
State FE	Yes	Yes
Birth Date Trend	Yes	Yes
Individual/Family Controls	Yes	Yes
State-by-Date Trend	No	Yes

Notes. Robust standard errors, clustered on state, are in parentheses. Regressions include a linear trend in birth date. Individual controls include dummies for race and ethnicity. Family controls include father socioeconomic status dummies, mother education dummies, and missing indicators for their missing values. County controls include share of literate people, share of married people, share of homeowners, and average occupational income score. The regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates. *** p<0.01, ** p<0.05, * p<0.1

Table 12 - Interaction with Compulsory Schooling and Child Labor in State-of-Birth during Adolescence Years

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Discontinuity at Notch	-2.10615**	-3.43665**	-3.4347**
	(.8584)	(1.16269)	(1.1669)
Child Labor = 7 Years	.59471		.61195
	(.94145)		(.95577)
Child Labor ≥ 8 Years	1.48615**		1.3928**
	(.63196)		(.56624)
Discontinuity at Notch × Child Labor = 7 Years	.24127		.11649
	(1.07319)		(1.11606)
Discontinuity at Notch × Child Labor ≥ 8 Years	.70881		.68584
	(.70245)		(.83067)
Compulsory Attendance = 7 Years		.23483	-.06908
		(1.17359)	(1.06275)
Compulsory Attendance ≥ 8 Years		1.47535	1.1215
		(1.36097)	(1.44507)
Discontinuity at Notch × Compulsory Attendance = 7 Years		1.78626	1.69917
		(1.22575)	(1.13734)
Discontinuity at Notch × Compulsory Attendance ≥ 8 Years		1.76418	1.35046
		(1.07515)	(1.30292)
Observations	314070	314070	314070
R-squared	.00485	.00482	.00486
Mean DV	907.431	907.431	907.431

Notes. Robust standard errors, clustered on state, are in parentheses. Regressions include region fixed effects and a region-specific birth-date linear trend. Individual controls include dummies for race and ethnicity. Family controls include father socioeconomic status dummies, mother education dummies, and missing indicators for their missing values. County controls include share of literate people, share of married people, share of homeowners, and average occupational income score. The regressions are weighted using inverse probability weighting scheme with the weights extracted from probit regressions of a dummy indicating successful DMD-census linking on observable individual and family covariates.
*** p<0.01, ** p<0.05, * p<0.1

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Figures

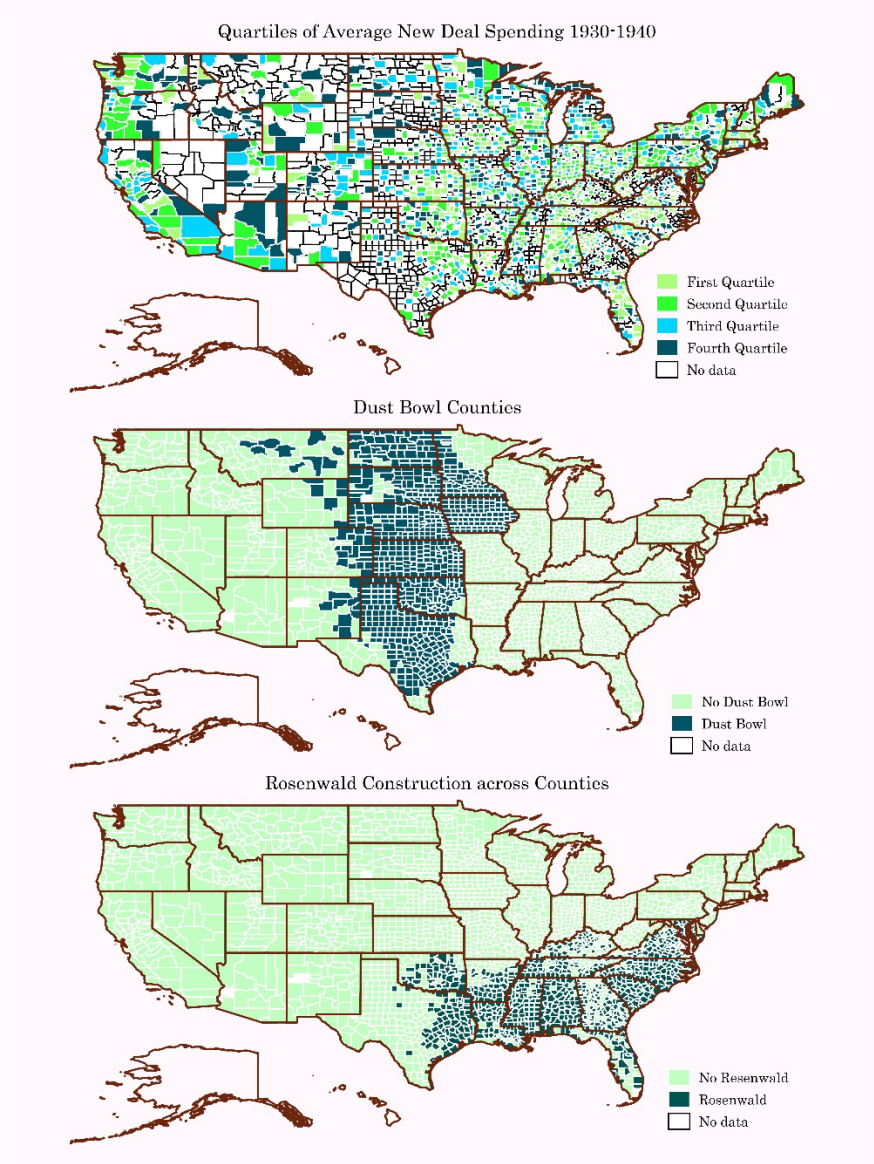


Figure 1 – Geographic Distributions of Shocks

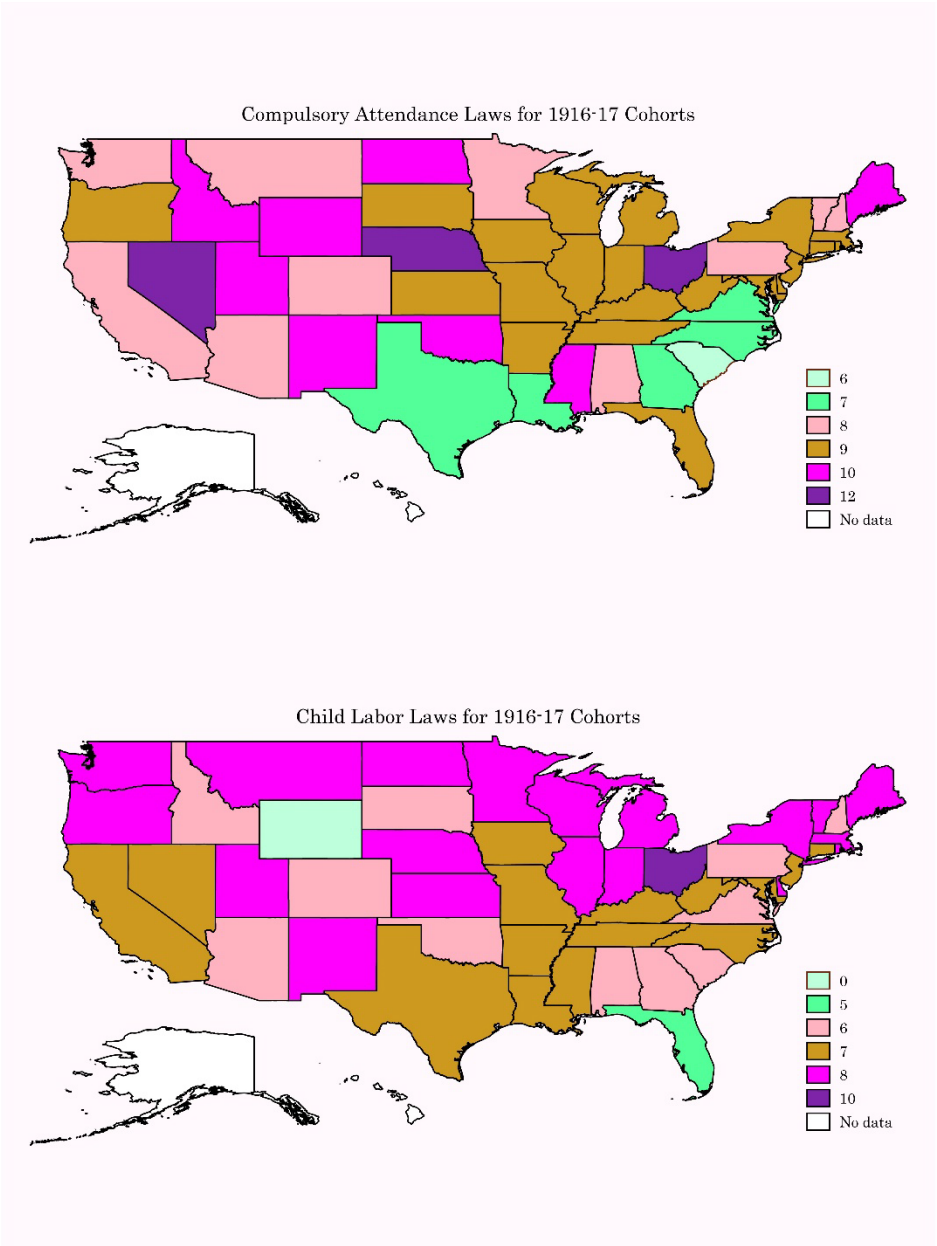


Figure 2 – Geographic Distribution of Compulsory Attendance and Child Labor Laws

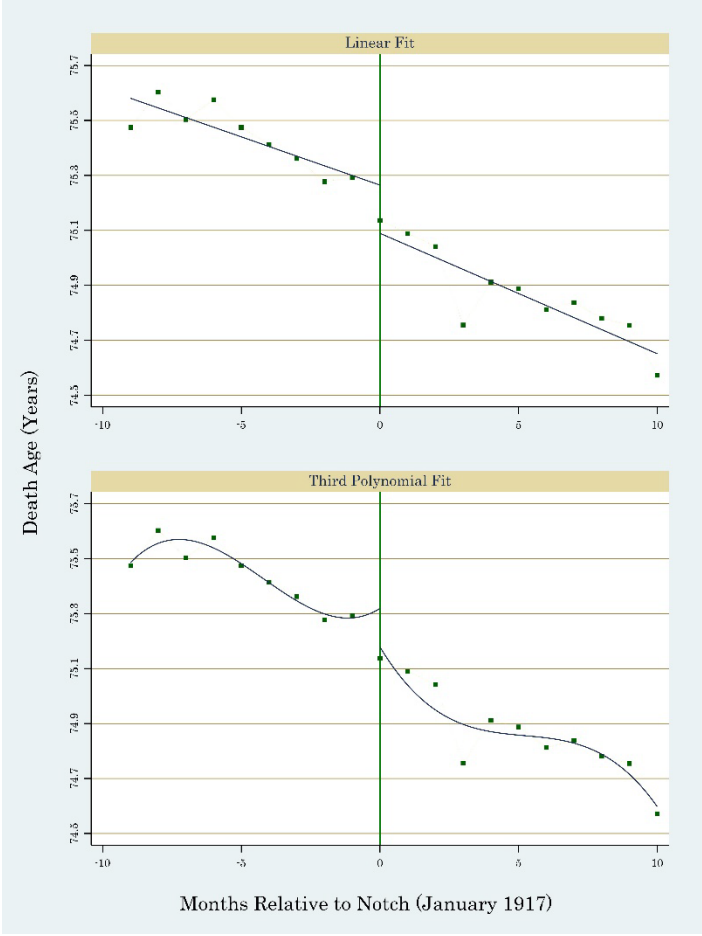


Figure 3 - Plotting Discontinuity at the Notch for Longevity