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

Hamid Noghanibehambari[†]

Jason Fletcher[‡]

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 reverses 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

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[†] Center for Demography of Health and Aging, University of Wisconsin-Madison, 1180 Observatory Drive, Madison, WI 53706
Email: noghanibeham@wisc.edu, Phone: +1-806-620-1812, ORCID ID: https://orcid.org/0000-0001-7868-2900

[‡] La Follette School of Public Affairs, University of Wisconsin-Madison, 1225 Observatory Drive, Madison, WI 53706-1211, USA
Email: jason.fletcher@wisc.edu

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 a 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.

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. 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. 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 (Dioikitopoulos, 2014; Johnson & Jackson, 2019; 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 7.

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 Death Master Files (DMF) extracted from (Goldstein et al., 2021). The DMF data contains male death records of the social security administration linked (at the individual level) to the full-count 1940 census. The data covers death between the years 1975-2005. The advantage of this data 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. 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).

Summary statistics of the final sample are reported in Table 1. The final sample consists of roughly 314,000 observations. The average age-at-death is approximately 911 and 905 months for pre-Notch and Notch cohorts, respectively. About 95 percent of observations are whites, and 5 percent are blacks.

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 ($CA\leq8$), CA9 ($CA=9$), CA10 ($CA\geq10$), CL6 ($CL\leq6$), CL7 ($CL=7$), and CL8 ($CL\geq8$). 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.

The county-level average New Deal spending data is extracted from Fishback & Kantor (2018). County-level indicator of Rosenwald school comes from Aaronson & Mazumder (2011).

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:

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

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 in all regressions. In X, we include individual gender, race, and ethnicity dummies. Finally, ε is an error term.

5. Results

5.1. Early-Life Exposures

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 explore the differences in longevity among those residing in Dust Bowl versus those in non-Dust Bowl counties, conditional on state fixed effects and controls. The results are reported in Table 2. The full model of column 3 suggests a reduction of about 2 months in longevity.

New Deal Programs. 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, 2010). Several studies point to potential short-run and long-run benefits of social spending in general, and specifically New Deal relief spending (Aizer 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

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[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.

data to DMF observations based on their county-of-residence during the 1930s. We then explore the differences between longevity of individuals who live in counties with top-quartile of per capita New Deal spending and bottom-three quartiles. The results are reported in Table 3. We observe that those living in higher spending counties have, on average, 1.3 months higher longevity.

Rosenwald Schools. 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. Therefore, we interact a black dummy with the Rosenwald indicator to examine the longevity differences due to exposure to the school openings among black people. The results are reported in Table 4. The exposure to Rosenwald across the whole population is correlated with an insignificant change in longevity (column 1). However, exposure to Rosenwald among black people is associated with 6.7 months increases in longevity, roughly 49 percent of black versus non-black gap in longevity (column 3).

State-level Compulsory 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).

In Table 5, we explore the differences in longevity of individuals born in states with different compulsory schooling and child labor laws, conditional on region fixed effects, trends, individual, family, and state covariates. We observe a correlation of 1.9 months of additional longevity for those residing in CL8 states (column 1). We also observe significant associations between CA7 and CA8 of about 1.3 and 2.5 months, respectively (column 2). In column 3, we allow the effects of compulsory attendance and child labor laws to compete. We observe that CA8 and CL8 both suggest a correlation of about 1.8 months that is statistically significant.

5.2. Results of Social Security Notch

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 Table 6. 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.

Notch-Longevity Results. We start the main results by depicting a discontinuity in the longevity of cohorts at the Notch in Figure 3. 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 results of Notch-longevity in Table 7. 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 (column 2) and family covariates (column 3). 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. 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. To empirically explore this potential explanation, we employ data from decennial Censuses and American Community Survey for the years 1980-2007 extracted from Ruggles et al. (2020). The limitation of this data source is that it does not report month of birth.[5] Therefore, we can only implement a cross-cohort comparison, conditional on covariates (race/ethnicity/gender dummies). Specifically, we compare the labor market outcomes of those born in 1917 versus 1916 in censuses 1980-1990 (covering Snyder & Evans (2006) period) and 2000-2007. The results are reported in in two panels for two

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[5] However, the 1980 census and ACS for post-2005 years report quarter of birth. Since we need to compare the results across various years, we only use year of birth which is available for all data years.

outcomes: *employed* and *being active in the labor force*. On average and conditional on covariates, post-Notch cohorts are 2.7 and 2.8 percentage-points more likely to be employed and active in the labor force when they are observed in 1980-1990, respectively. However, these effects decrease to 0.1 and 0.07 percentage-points, respectively, when we look at 200-2007 data years. In addition, the coefficients become statistically insignificant. These facts confirm our prior expectation that the positive effects of Notch on labor force activity, that could explain negative income-mortality association of Snyder & Evans (2006), become insignificant for later years, when cohorts reach their 80s and 90s. Hence, we observe the negative impact of the income reduction on 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 10th 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 Appendix Table A-1, 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.

5.3. Early Versus Late-Life Interactions

Dust Bowl. We interact a Dust Bowl county indicator with the Notch and report the results in Table 8. We observe considerably larger effects among Dust Bowl Counties' residents. The double-interaction coefficient in column 2 suggests 2.6 months lower longevity for Notch cohorts who reside in a Dust Bowl county. The Notch effect is about 1.4 months reduction for those outside Dust Bowl counties.

New Deal Spending. To explore the interaction with New Deal spending, we interact with Notch dummy a dummy that indicates the county is at the top quartile of per capita spending over the years 1930-1940. We observe a positive and significant association between top spending county and longevity. The interaction term suggests a quite small and insignificant effect of Notch for those residing in top quartile of spending county. This suggests a degree of substitution between early and late life exposures.

Rosenwald Schools. 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 Table 10. 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. We do not observe a considerable drop in longevity among blacks versus non-blacks due to the Notch (column 1). Among Notch cohorts regardless of race, Rosenwald school is correlated with increases in longevity (column 2). Based on the estimates in column 3, blacks, on average, have 11.1 months lower longevity. Non-blacks who were not exposed to the Rosenwald reveal a Notch discontinuity of about 1.7 months. On average, black people of pre-Notch cohorts who were exposed to Rosenwald schools during K-12 years live 2.9 months longer. Black people of Notch cohorts who were not exposed to the Rosenwald schools live 3 months shorter lives, although the coefficient is statistically insignificant. Finally, the triple-interaction term suggests an increase of roughly 4 months among Notch cohorts who are black and exposed to the Rosenwald schools. These findings suggest that early life improvements in educational quality and school availability could break the income-mortality relationships during old ages.

Compulsory Schooling. 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 11. In column 1, we interact indicators of child labor law with the Notch dummy. The results suggest larger Notch effects for those whose state-of-birth had a child labor law of fewer than 7 years, about 2.1 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, although both coefficients are statistically insignificant. In column 2, we interact the Notch with compulsory attendance laws. Those born in states with a compulsory schooling less than 6 (versus those in Ca7 and CA8 states) 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 reveal an effect size of 3.4 months reduction in longevity. The interaction terms of child labor laws converge to zero. We observe an insulating role of compulsory attendance laws that are almost equal between CA7 and CA8, 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 11 extend this literature and explore its role in insulating other negative exposures even at the retirement age.

6. Discussion

To be written …

7. 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. 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*** (1.19337)	-13.87154*** (1.22418)
Rosenwald	.11297 (1.02396)		-.67913 (.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 – The Effect of Social Security Notch on Longevity

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Discontinuity at Notch	-1.59269** (.67185)	-1.55144** (.67144)	-1.54539** (.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 – Interaction Between Notch and Dust Bowl

	Outcome: Age at Death (Months)	
	(1)	(2)
Dust Bowl County	-1.45692	-1.00664
	(1.43435)	(1.52629)
Discontinuity at Notch	-1.5144**	-1.4152**
	(.62821)	(.65533)
Discontinuity at Notch × Dust Bowl County	-1.92043**	-2.61145**
	(.95581)	(1.30744)
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 9 - Interaction Between Notch and New Deal Spending

	Outcome: Age at Death (Months)	
	(1)	(2)
4th Quartile of Spending County	1.35681*** (.42137)	1.35778*** (.43264)
Discontinuity at Notch	-1.60655** (.60677)	-1.59061** (.59782)
Discontinuity at Notch × 4th Quartile of Spending County	-.14485 (.63747)	-.14553 (.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 10 - Interaction Between Notch and Rosenwald School

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Discontinuity at Notch × Rosenwald × Black			4.03683**
			(1.7857)
Discontinuity at Notch × Rosenwald		2.01289*	1.18462
		(1.14058)	(1.3388)
Discontinuity at Notch × Black	-.44344		-3.0342**
	(.92804)		(1.30858)
Black × Rosenwald			2.91384**
			(1.44635)
Discontinuity at Notch	-1.38007**	-2.12995***	-1.7357**
	(.69326)	(.73028)	(.74419)
Black	-9.76899***		-11.09085***
	(.74013)		(1.04149)
Rosenwald		-.71385	-1.43561
		(1.03649)	(1.16636)
Observations	319819	319819	319819
R-squared	.00549	.00566	.00570
Mean DV	907.545	907.545	907.545
State FE	Yes	Yes	Yes
Birth Date Trend	Yes	Yes	Yes
Individual/Family Controls	Yes	Yes	Yes
State-by-Date Trend	Yes	Yes	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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

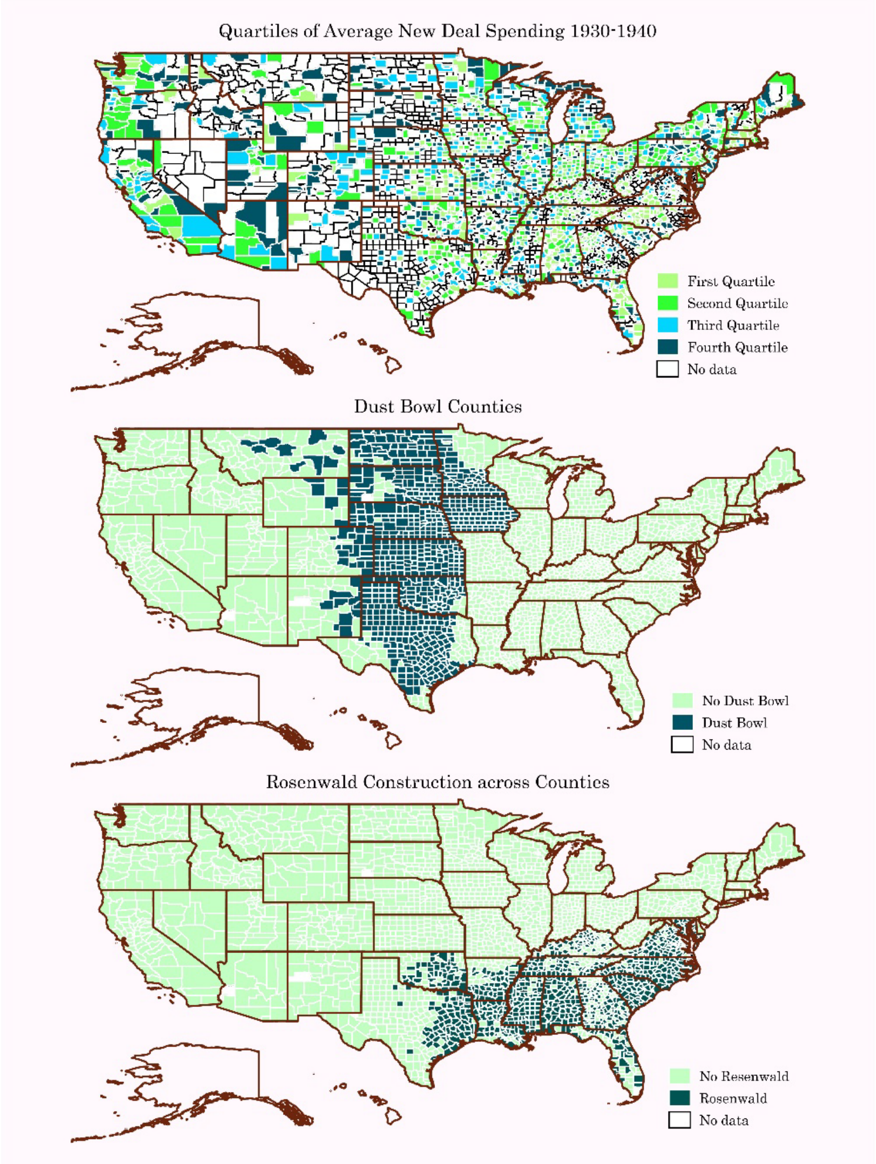
Table 11 - Interaction with Compulsory Schooling and Child Labor in State-of-Birth during Adolescence Years

	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

Figures



Quartiles of Average New Deal Spending 1930-1940

Dust Bowl Counties

Rosenwald Construction across Counties

Figure 1 – Geographic Distributions of Shocks

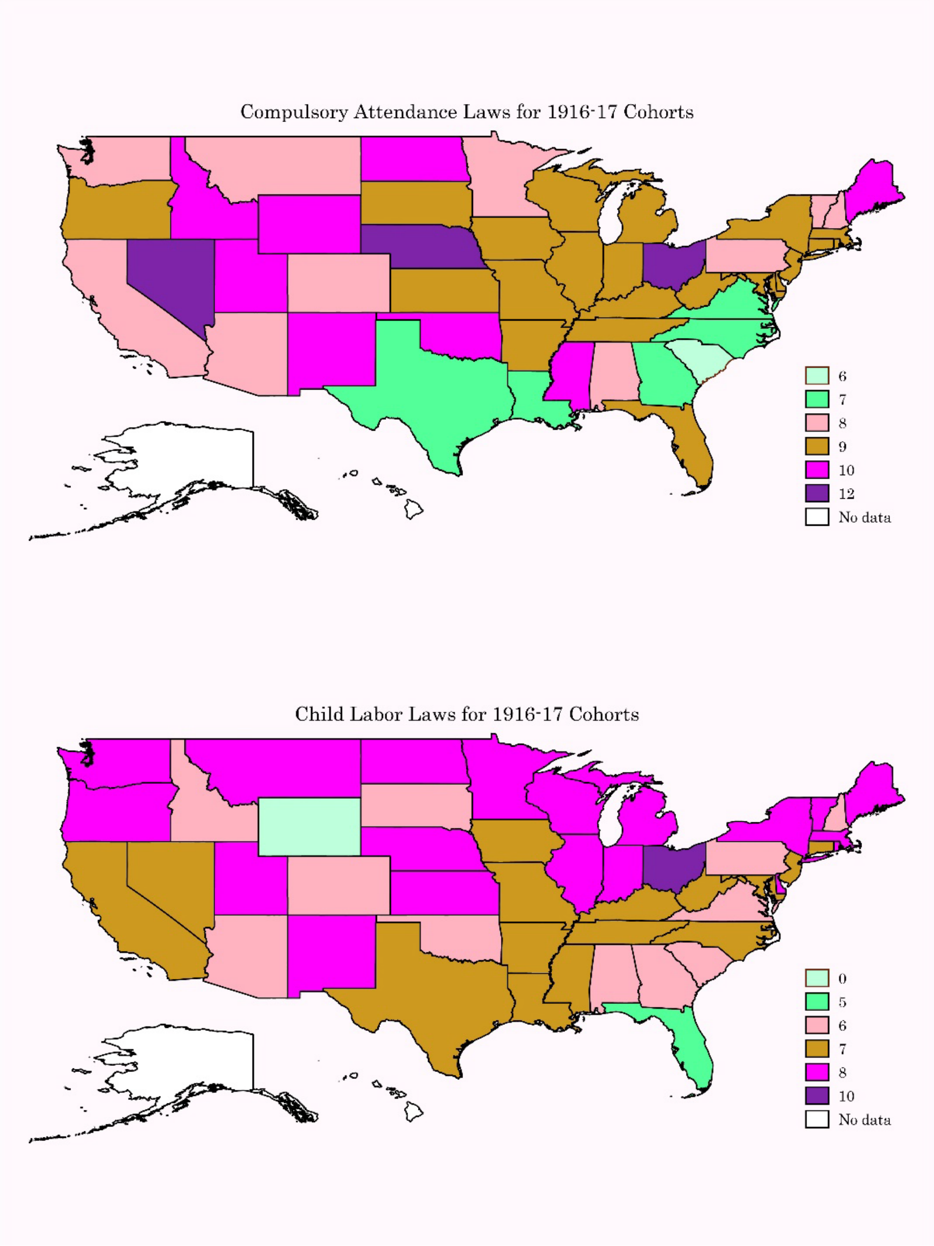


Figure 2 – Geographic Distribution of Compulsory Attendance and Child Labor Laws

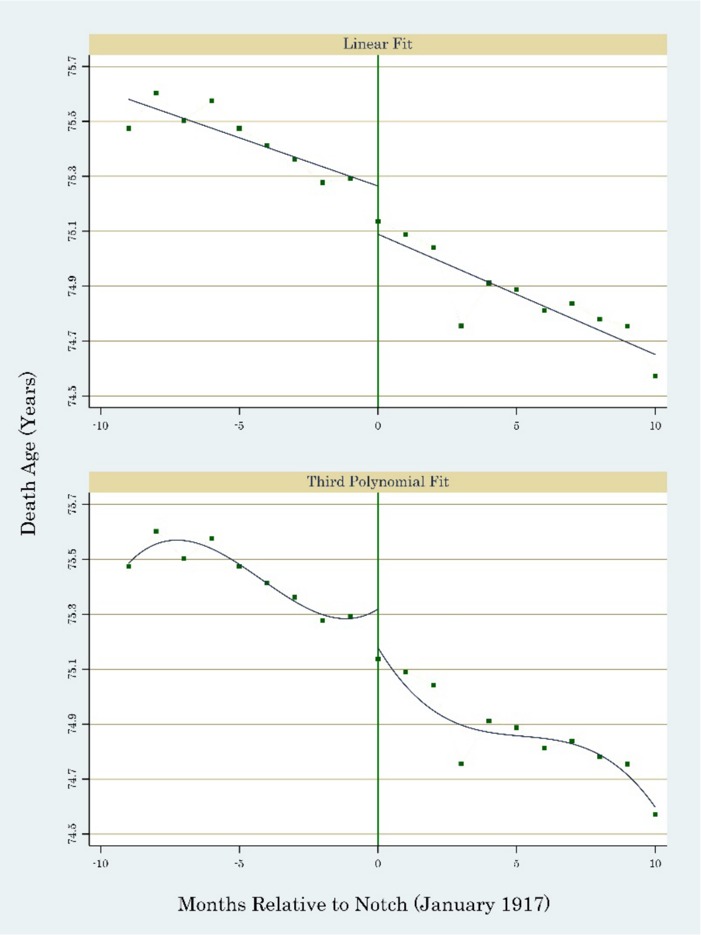


Figure 3 - Plotting Discontinuity at the Notch for Longevity

Appendix A

Appendix Table A-1 - Exploring Changes in Labor Force Outcomes of Elderly Due to the Notch

	Outcomes:					
	Employed			In Labor Force		
	Samples: Census 1980	Samples: Census 1990	Samples: Census 2000, ACS 2001-2007	Samples: Census 1980	Samples: Census 1990	Samples: Census 2000, ACS 2001-2007
	(1)	(2)	(3)	(4)	(5)	(6)
Discontinuity at Notch	.03989*** (.00221)	.0117*** (.00165)	.00112 (.00157)	.04085*** (.00222)	.01188*** (.00168)	.00077 (.00163)
Observations	184641	147004	113495	184641	147004	113495
R-squared	.05444	.01379	.0071	.05818	.0137	.00619
Mean DV	0.392	0.091	0.025	0.408	0.096	0.027
%Change	10.175	12.860	4.483	10.012	12.372	2.852

Notes. Robust standard errors are in parentheses. Regressions include a linear trend in birth date. Individual controls include dummies for race, gender, ethnicity, and missing indicators for their missing values.
*** p<0.01, ** p<0.05, * p<0.1