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IN MONEY, WE SURVIVE:
THE EFFECTS OF SOCIAL SECURITY RETIREMENT INCOME ON LONGEVITY

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ABSTRACT

An old and debated line of research examines the income-mortality relationship and finds mixed evidence. In this paper, we re-evaluate previous studies using a new dataset and implementing a difference-in-difference model based on a Notch in Social Security retirement benefits to overcome selection and endogeneity issues. We employ Social Security Administration death records and find a positive income-longevity relationship. Moreover, we find more pronounced effects among low-educated individuals and people from low socioeconomic status families. Analyses using census data suggest that part of the reductions in retirement income are offset by wage income due to post-retirement labor force participation. Past age 80, the net adverse effects of the policy on both income and longevity become more pronounced.

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1. Introduction

Life expectancy in the developed world has experienced a dramatic change 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). Various studies that span several disciplines have explored the potential sources of improvements in longevity (Lichtenberg, 2004). An old and intensely debated body of literature explores the role of the income-health gradient and specifically the role of income in mortality outcomes (Altenderfer, 1947; Chetty et al., 2016; Cutler et al., 2006). Cross-country analyses usually find an inverse relationship between measures of income per capita and mortality outcomes (Baird et al., 2011). 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). In addition, several studies document the procyclical nature of mortality (Miller et al., 2009; 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 (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 (Andersson et al., 2015; Dobkin & Puller, 2007; Engelhardt et al., 2022; Evans & Moore, 2011). For instance, Snyder & Evans (2006) exploit variation in retirement income induced by a Social Security policy change and find that reductions in retirement income *improved* mortality outcomes.

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; Takagi et al., 2016). Another important example of endogeneity is individuals' health status, which affects their income and leads to differential longevity. In addition, parental investment in their children's human capital could also differ by children's initial health endowment, which affects their future income and longevity (Almond & Mazumder, 2013; Fan & Porter, 2020; Restrepo, 2016). 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).

In order to tackle these problems, several studies leverage policy changes in welfare benefit receipt and Social Security income receipt as an arguably exogenous source of income change (Evans & Moore, 2011; Nelson & Fritzell, 2014; Salm, 2011; Stoian & Fishback, 2010). Income receipt may affect mortality outcomes through several channels, with effects that operate in opposite directions. First, income receipt increases the consumption of quality food that insulates people from diseases and mortality, specifically among the elderly population (Reedy et al., 2014). However, studies from the medical literature show that increases in caloric intake and consumption of specific foods could raise the probability of a heart attack and death (Lv et al., 2015). Second, income receipts may reduce labor force participation. Hence, it reduces work-related stress and the possible adverse effects of physical activities on health (Nilsen et al., 2016; Witte et al., 2005). In contrast, labor force activity can enhance the engagement of older adults in the community. A strand of research supports the potential benefits of elderly labor force activity and links social isolation to increases in mortality (Holt-Lunstad et al., 2015).

The literature that directly examines the effect of income receipt on mortality is far from conclusive. Table 1 provides a summary of studies that examine the income-mortality relationship. For instance, Salm (2011) exploits pension reforms in the early 20th century that granted pension benefits to Union Army veterans. He finds that benefit payments reduced age-adjusted mortality by about 11-29 percent. In contrast, Evans & Moore (2011) and Evans & Moore (2012) analyze how predictable income payments (such as Social Security checks, Supplemental Security Income, military wages, tax rebates, and Alaska Permanent Fund dividends) affect short-term mortality rates. They find that mortality increases immediately after these income payments. These studies suggest that the influx of income may lead to increased economic activity and risky behaviors, such as substance use or increased travel, which in turn elevate mortality risks. Berman (2021) exploits variation in Social Security benefits caused by anomalies in the benefit calculation formulas based on wage indexation, benefit indexation, and individuals' exact dates of birth. He reports that for each $100 increase in benefits, there is a $38 reduction in Medicare expenditures. Furthermore, he finds reductions in mortality rates post-age 75 as a result of increases in benefit payments.

In this paper, we employ newly released Social Security Administration death records to re-evaluate the income-mortality relationship. 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*. Snyder & Evans (2006) (hereafter S&E) employ the Social Security Notch as a quasi-experimental source of income variation, use death records from 1979 to 1990, and document reductions in mortality following decreases in retirement income payments. In contrast, our study leverages a longer observation window (1979–2005 and 1979-2015), a substantially larger sample, and newly linked administrative data to reassess this policy episode. Our findings challenge S&E's conclusions. We find that the reduction in retirement benefits due to the *Notch* decreased longevity, particularly among low-SES and low-educated individuals.

We implement a difference-in-difference estimation strategy that compares the difference in longevity of the Notch cohort (1917) and the immediate pre-Notch cohort (1916) with the same difference for the adjacent cohorts (i.e., 1915 versus 1914 cohort). We find a reduction in longevity of about one month. Using census data, we observe that the average retirement income reduction is about $1141, off a mean of $19,467 annually (in 2020 dollars). Therefore, our results suggest that a $1,000 decrease in income (in 2020 dollars) is associated with 0.9 months lower longevity in an elderly population, in contrast with findings of S&E. We further employ census data and confirm the previous findings that notch cohorts respond to the reduction in retirement income by increasing labor force participation, which results in increases in wage income. However, we show that the effects on total personal income are negative, and this net negative effect becomes substantially larger as individuals become older, especially past age 80. A series of heterogeneity analyses reveal considerably larger impacts among individuals who come from lower socioeconomic status families and those individuals who are low educated.

While our study builds on the foundational work of Snyder and Evans (2006), it introduces several key innovations that allow for a more comprehensive assessment of the Social Security Notch's effects on longevity. First, we leverage newly released administrative death records that extend the observation window through 2005, with additional data extending to 2015—adding 15 to 25 years beyond S&E's sample, which ends in 1990. Second, our sample includes individuals who died both before and after age 65, in contrast to S&E, who focus exclusively on post-65 mortality. Third, we link individual-level death records to rich demographic data from the 1940

Census, enabling subgroup analyses by race, education, and family background. Finally, while S&E limit their analysis to a narrow one-quarter bandwidth around the cutoff, we show that expanding the bandwidth meaningfully alters the magnitude of the estimated difference-in-difference effects. Importantly, whereas S&E conclude that reduced retirement income improved mortality outcomes, we find the opposite: reduced benefits lowered longevity, particularly among socioeconomically disadvantaged groups. Our analysis suggests that the length of the follow-up period is critical in explaining the contrasting findings. By replicating the S&E approach using a comparable five-year mortality rate, we obtain similarly small and statistically insignificant effects on mortality, consistent with their original conclusions. However, when we extend the observation window to cover longer-term mortality (up to 10 and 20 years after age 65), the estimated effects reverse in sign and become both larger and statistically significant. Thus, our findings highlight that the shorter death window in S&E's analysis is a key reason for the difference in results, and that the long-term negative consequences of benefit reductions only become apparent with extended mortality coverage.

This paper contributes to the ongoing literature by providing 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.

The Social Security Administration (SSA) trust funds are projected to become insolvent by 2034, which means that the program will only be able to pay out about 76% of scheduled benefits at that time (SSA, 2022). The SSA is contemplating specific policies, which could involve discussing the necessity of making benefit reductions in the coming years. The findings of this paper have the potential to demonstrate the impact of such changes on health and mortality. Furthermore, it evaluates the potential heterogeneity in the effects of policy changes on mortality by socioeconomic status and lifetime experiences. Thus, its implications suggest that it may be beneficial to consider making adjustments that vary based on an individual's history and socioeconomic status, in order to minimize the negative consequences of any potential benefit reductions.

The rest of the paper is organized as follows. Section 2 provides a review of the background of the Social Security policy change. Section 3 discusses the data source and econometric method. Section 4 reviews the results. We conclude the paper in the section 5.

2. A Short Background on the Social Security Notch

Prior to the 1930s, welfare support for old age was primarily provided by the small-scale Old Age Assistance (OAA) program implemented and administered by state and local authorities. After the Great Depression hit the US economy, the federal government intervened through 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 older adults. The OASI schedule depended primarily on age at retirement and the pre-retirement nominal wages.

The OASI payments remained constant over time unless Congress enacted statutory changes to adjust benefits. During the 1970s, Congress introduced automatic indexing using the Consumer Price Index (CPI) to adjust benefits for inflation. Prior to this change, benefits were based on unindexed average nominal wages, which meant they implicitly rose with increases in earnings levels. This so-called double indexation—whereby benefits effectively increased based on both average nominal wages (used in the benefit formula) and the CPI (used for post-entitlement adjustments)—resulted in substantial increases in retirement benefits. Additionally, the Social Security trust fund experienced temporary surpluses during the 1970s due to the large cohort of workers from the postwar baby boom entering the labor force. These conditions prompted Congress to expand the benefit schedule further.

The double indexation coupled with high inflation of the 1970s created a threat of insolvency for the Social Security system as early as the 1980s. In 1977, Congress replaced the nominal wage method 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 were forced to be included in the new system. Hence, a Notch in benefits was created based on cohorts born after January 1, 1917, who received substantially lower benefits. Krueger & Pischke (1992) suggest that, for a person earning average wages, the Notch resulted in about a 13 percent reduction in benefits. S&E find about 4 percent higher income for the pre-Notch generation, or $41 per month (in 1987 dollars).

3. Data and Econometric Method

3.1. Data Sources

Our primary data source is the Death Master Files (DMF) extracted from the Censoc project (Goldstein et al., 2021). This dataset contains individual-level records of male deaths occurring between 1975 and 2005 and is linked to the full-count 1940 U.S. Census. Compared to other commonly used sources—such as the Health and Retirement Study (HRS) or the Vital Statistics death records used in S&E—the DMF offers several key advantages. First, the linkage to the 1940 Census provides detailed information on individuals' early-life socioeconomic and demographic characteristics, enabling heterogeneity analyses that are not possible with the data used in S&E. Second, the DMF includes exact dates of birth and death, allowing us to calculate precise age at death and accurately identify Notch-affected cohorts—again, absent in S&E. Third, the DMF's large sample size—millions of observations—offers substantial statistical power for estimating effects across narrow birth cohorts and implementing flexible regression designs. In contrast, using HRS linked to the 1940 Census yields a substantially smaller sample—mainly when focusing on just four birth cohorts as in our setting—resulting in limited statistical power for detecting effects.

We implement two sample restrictions. First, we restrict the sample to cohorts born between 1916-1917 (Notch and Pre-Notch cohorts) and 1914-1915 (used as a comparison group). Specifically, we avoid including cohorts of 1918-1919 as these cohorts are likely affected by in-utero and early-life exposures to the Spanish Flu that may affect their old age health and longevity (Almond, 2006; Fletcher, 2018a, 2018b). Second, since we work with a truncated death window and the primary variation comes from longevity of different cohorts, it is essential to have a balanced death window. With a fixed death window, the earlier cohorts of each two-cohort pair (e.g., the 1916-1917 group) will have older decedents than the later cohorts. To overcome this unbalanced death window, we force a balanced age at death for each two-cohort pair. For instance, given the death window of 1979-2005, the 1916 cohort dies between ages 63-89 and the 1917 cohort between ages 62-88. For this pair of cohorts, we restrict age at death to be between 63-88, the typical age at death for both 1916 and 1917 cohorts. Similarly, for the 1914-1915 pair, we restrict age at death to be between 65-90.

Summary statistics of the final sample are reported in Table 2. The final sample consists of roughly 488,580 observations. The average age-at-death is 928.4 months, or approximately 77.4

years. Approximately 24.3 percent of individuals are Notch cohorts. About 95.7 percent of observations are whites, and 3.8 percent are blacks.

3.2. 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. In our primary method, we implement a difference-in-difference model to compare the longevity of the immediate Notch cohort (i.e., born in 1917) versus the immediate pre-Notch cohort (i.e., born in 1916) in comparison with the same difference for earlier cohorts (i.e., the difference between the 1915 cohort and the 1914 cohort). While the first difference reveals a cross-cohort comparison between Notch and pre-Notch cohorts, it could be the case that such a difference picks up the overall changes in longevity across cohorts, with the seasonal changes in longevity for these cohorts that cannot be accounted for by the usual fixed effects. The comparison with similar adjacent cohorts (1914-1915) could absorb such cross-cohort and seasonal patterns and leave the Notch effect. We provide several balancing tests and placebo tests to support this argument. We implement this difference-in-difference method using ordinary least squares regressions of the following form:

$$y_i = \alpha_0 + \alpha_1 F \times N + \alpha_2 F + \alpha_3 N + \alpha_4 X_i + \varepsilon_i \tag{1}$$

Where y is age-at-death of individual i. We should highlight that the sample covers birth cohorts of 1914-1917. The parameter F represents belonging to forward cohorts, i.e., born in 1915 or 1917. The parameter N represents the combination of Notch and immediate pre-Notch cohorts, i.e., born in 1916 or 1917. Therefore, $F \times N$ indicates the Notch cohort. The parameter α_1 captures the change in longevity of the notch cohort versus pre-notch differencing out the changes in longevity of 1915-versus-1914 cohorts. In X, we include birth month fixed effects to control the influence of birth seasonality in longevity, 1940-county fixed effects, individual race and ethnicity dummies, paternal socioeconomic status dummies, and maternal education dummies. We also include missing indicators for missing values of these covariates.

To complement our difference in difference analysis, we also implement a regression discontinuity to detect the effect of the notch on longevity after accounting for a secular linear trend. The assumption behind our regression is that the discontinuity generated by the 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:

$$y_i = \alpha_0 + \alpha_1 T_i + \alpha_2 (L_i - C) + \alpha_3 T_i (L_i - C) + \alpha_4 X_i + \varepsilon_i \quad (2)$$

Where y is age-at-death of individual i. The variable T is a dummy that equals one if the individual is born after January 1, 1917. The variable L represents running variable (i.e., date of birth). C represents cutt-off date (January 1, 1917). The parameter X is defined in Equation (1). Finally, ε is an error term. We cluster standard errors at the birth-month level. Regression discontinuity results are reported for a wide range of bandwidths around the cutoff date.

4. Results

4.1. Difference-in-Difference Results

Table 3 reports the main results of Equation (1). In column 1, we restrict the sample to cohorts of 1916-1917. Therefore, the reported coefficient documents the notch versus the pre-notch difference in longevity after partialling out covariates and fixed effects. Since we observe age at death through a truncated death window (1975-2005), the earlier cohorts reveal higher age at death in our data. Therefore, it is not surprising to see a negative coefficient in the sample.[4] In column 2, we repeat this analysis for the sample of 1914-1915 cohorts and observe a similarly negative coefficient. In column 3, we report the main difference-in-difference results. The estimated α_1 (DD coefficient) is reported in the first row. This DD coefficient suggests a reduction of a 1.1-month in longevity for the Notch cohort.

The second panel of Table 3, replicates the first panel but replaces the 1914-1915 cohorts as the comparison group in the final sample with 1912-1913 cohorts. The DD coefficient of column 6 implies a 0.8-months reduction in longevity. Although this coefficient is a statistically insignificant, its magnitude is comparable to that of column 3.

We implement a placebo analysis by assigning the notch status to the cohort of 1915 and using cohorts of 1912-1913 as the comparison group. These results are reported in the third panel of Table 3. In columns 7 and 8, we observe similar coefficients for the difference between 1915-versus-1914 and 1913-versus-1912, respectively. The DD coefficient of column 9 suggests a small, economically meaningless, and statistically insignificant coefficient. These results suggest that our design accounts for the overall change in cross-cohort longevity and seasonality patterns and that the effect of the notch on longevity, contrary to prior literature, is negative and significant.

[4] The truncated death window (1979–2005) imposes cohort-specific upper limits on observable age at death. This truncation mechanically allows earlier cohorts (e.g., 1914) to reach higher ages than later ones (e.g., 1915) in the death data. Such right-censoring can create artificial differences in mean longevity across adjacent cohorts, even in the absence of true underlying mortality variation. One way to demonstrate that these observed differences are mechanical artifacts rather than genuine cohort effects is to adopt a stricter sample selection in which all cohorts are similarly represented in the death data. To do so, we restrict the sample to deaths occurring between ages 70 and 85 and estimate the discontinuity for each set of two-year adjacent cohorts, going back to the 1904–1905 cohorts. These results are reported in Appendix Figure A-6. While the strict sample selection leads to a somewhat larger estimated notch discontinuity than our main results, comparisons with other placebo notch dates suggest that there is no meaningful mortality variation between adjacent cohorts when observed within a comparable death window.

The effects of Table 3 are especially in contrast with the findings of S&E, who use Vital Statistics death records between the years 1979-1990 and show that the Notch generated mortality gains. However, there are slight differences between our analysis sample and theirs. In column 1 of Table 4, we build a sample similar to their study: focusing on death years of 1979-1990, post-65 age at death, and restricting the sample to one-quarter post and pre-notch. The DD coefficient implies a positive and noisy effect. The fact that the notch has a positive effect, if anything, on longevity is in line with S&E's study. In column 2, we increase the bandwidth to cover one year post and pre-notch. Although the coefficient remains positive, it is quite small in magnitude and still insignificant. In column 3, we include all death ages in our final sample, i.e., adding death ages of 61-65 to column 2. We observe a negative and significant coefficient, implying a roughly 0.3 months decrease in longevity. In column 4, we increase the death window to cover post-1990 death years. This column replicates the main results of column 3 of Table 3, pointing to a one-month reduction in longevity. Although all these sample selections make a difference between S&E's results and those of the current study, the largest change appears in expanding the death window that covers deaths up to 2005.

To further examine the discrepancy between our findings and those of Snyder and Evans (2006), we replicate their mortality regressions more closely by constructing a comparable outcome: five-year mortality rates beginning at age 65. Because the DMF data lacks universal coverage and contains a truncated window of death records, we are unable to calculate mortality rates using total population denominators, as S&E did. Instead, we follow an approach similar to Goldstein et al. (2023), computing mortality rates using only DMF death records.[5] Specifically,

[5] For the analysis in this section, we also include unique male death records from the Numident data—specifically, records not found in the DMF—also extracted from the CenSoc project (Goldstein et al., 2021). In addition, we do not impose the cohort or age restrictions applied in the sample selection described in Section 2.

we define the 5-year mortality rate at age 65 as the number of individuals who died between ages 65 and 70 (numerator), divided by the total number of individuals who died after age 65 (denominator). Similarly, the 10-year and 20-year mortality rates are calculated using deaths between ages 65–75 and 65–80, respectively, relative to the same denominator.[6]

Table 5 presents difference-in-difference estimates using mortality rates as the outcome. In line with S&E, we focus on the 65-70 mortality rate (i.e., 5-year mortality rate at age 65).[7] The estimated effect is 0.3%, which closely matches the magnitude and sign of S&E's findings. Although the coefficient is statistically insignificant in our sample, it remains quite comparable in size and sign. Thus, despite data limitations in calculating mortality rates, we are able to reasonably replicate the results of S&E.

Next, we examine effects on 65-75 mortality rate and 65-85 mortality rates. These results are reported in Columns 2 and 3 of Table 5. We find that the point estimates change sign and become statistically significant in the longer mortality coverage, with Column 3 indicating a 0.4% increase in mortality. In Panel B, we replicate the same analysis using a 1-year bandwidth. Although the coefficients are larger than those in Panel A, we observe a similar pattern: the S&E death window suggests a reduction in mortality rates, while extending mortality coverage produces coefficients of the opposite sign, suggesting increases in mortality. Overall, the effects tell a different story when using extended mortality coverage, suggesting that the long-term adverse impacts of the policy change are obscured in shorter observation windows.

[6] In the table, we label these outcomes as "65–70," "65–75," and "65–85" mortality rates to more accurately reflect that we are calculating age-specific mortality rates.

[7] We should note that all the regressions in Table 5 use 1979-2005 death years. Since we use death records for the denominator, it is essential to include a wider death window follow up than that of S&E (i.e., 1979-2005) to reduce denominator bias in the construction of mortality rates. In contrast, restricting the death window to 1979–1990, as in S&E, may undercount deaths after 1990, leading to biased estimates of mortality rates. Relatedly, the average mortality rate in Column 1 is nearly identical to that of S&E.

In section 4.7, we use census data and show that the Notch resulted in a reduction in retirement income of about $1,141 (in 2020 dollars).[8] Therefore, our results suggest that a $1,000 decrease in income is associated with 0.9 months 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 percentiles 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 1.4 months lower longevity, about 40% larger than 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 Table 3, 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 0.3 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

[8] This first-stage effect is almost identical to that of S&E which estimates that the Notch resulted in a reduction in benefits of about $1,120 per year, off a mean of $27,330 (in 2020 dollars)

1-year higher longevity. Therefore, the effect of Notch can offset 5.6 percent of the positive effect of college education on longevity.

4.2. Regression Discontinuity Results

The top panel of Figure 1 shows the regression discontinuity estimates. We observe a clear break in longevity trend for notch cohorts.[9] The longevity of pre-notch cohorts reveals a stagnant trend while it points to the declining trend for notch cohorts with a clear break at the notch. The bottom panel of this figure depicts the same regression discontinuity estimates using the subsample of S&E, i.e., death years of 1979-1990. The sample does not provide a break in trends for notch cohorts.

The top panel of Appendix Figure A-1 reports regression discontinuity estimates using various bandwidth selections. The figure shows that the negative discontinuity in longevity at the Notch threshold is consistent across a range of bandwidths around the January 1, 1917 cutoff. The middle panel reports similar estimates assuming a placebo cutoff date of January 1, 1915 (comparing the 1915 and 1914 cohorts). The fact that these placebo point estimates are small and statistically insignificant supports the validity of the RD design and suggests that the effects observed in the top panel are unlikely to be driven by seasonality or cohort-specific trends. The bottom panel of this figure displays the difference between the estimates in the top and middle panels, following our preferred DiD-RD specification.

Appendix Figure A-2 replicates the same exercise using the S&E sample restrictions—i.e., limiting the death window to 1979–1990 and restricting to death ages above 65. In this subsample, the RD estimates suggest small and statistically insignificant *positive* effects on longevity,

[9] Appendix Figure A-4 howes the robustness of regression discontinuity results of Figure 1 to using a second-degree polynomial specification.

consistent with the original findings of S&E that reduced benefits may have improved mortality outcomes. A comparison of Appendix Figure A-1 and Appendix Figure A-2 highlights the importance of using a longer observation window, as in our study, to detect the long-term adverse effects of the Notch on longevity.

As an additional robustness check, Appendix Figure A-3 presents DiD-RD estimates without including a linear time trend in birth date (i.e., assuming $\alpha_2 = 0$ and $\alpha_3 = 0$ in Equation (2)). While this more flexible specification may capture nonlinearities and seasonal variation in birth cohorts, the estimated discontinuity at the Notch threshold remains negative and sizable. This further supports the robustness of our main RD-based findings.

The overall 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).

4.3. Heterogeneity Analysis

Several studies point to the influence of education and family socioeconomic status on longevity and mortality outcomes and the potential interaction between these factors and other policy exposures in shaping mortality trends (Barham & Rowberry, 2013; Engelhardt et al., 2022; Johnson & Jackson, 2019; Lleras-Muney, 2005; Noghanibehambari & Fletcher, 2023). To explore this potential heterogeneity, we use information from the 1940 census to examine the differential impacts based on education and socioeconomic score.

About 40% of individuals in the final sample still resided in their original household in 1940. Using the paternal socioeconomic index reported in the 1940 census, we split the sample into individuals with low and high socioeconomic status families and replicate the main difference-in-difference results. These estimates are reported in columns 1 and 2 of Appendix Table A-1. The DD coefficient of the low socioeconomic status subsample is about 1.6 times larger than that of the high socioeconomic status subsample. This pattern can be partly explained by the intergenerational transmission process through which lower socioeconomic status during childhood is translated to lower socioeconomic status during adulthood and higher exposure to the adverse effects of the reductions in retirement income.

In columns 3 and 4, we examine the effects among individuals with lower and higher than 12 years of education, respectively. We observe a DD coefficient among low-educated individuals of roughly 6.6 times that of high-educated individuals. The higher income and wealth of higher-educated people may insulate them against negative shocks to their stream of Social Security retirement income.

In columns 5 and 6, we replicate the results among whites and Blacks, respectively. We find that the effects are larger among Blacks although the point estimate is a statistically insignificant, probably due to a considerably smaller sample size.

4.4. Robustness Checks

In Appendix Table A-3, we examine the sensitivity of the results to alternative specifications. In column 2, we allow for fixed effects of birth month to have differential impacts based on race and parental characteristics. Specifically, we interact these fixed effects with dummy variables for race, ethnicity, paternal socioeconomic status, and maternal education. We observe identical coefficients to the main results. In column 3, we replace the outcome with the log of age at death. The DD coefficient suggests a reduction of about 0.12% in the outcome, comparable to the 0.11% change implied by the DD coefficient of Table 3 compared with the mean of age at death. In columns 4-5, we examine the effects on longevity beyond ages 75 and 80, respectively.

We observe a much larger DD coefficient for longevity beyond age 80, suggesting that the impacts might have been delayed until later ages at death. To complement this analysis, Appendix Figure A-5 reports the DD estimates for the probability of surviving beyond specific ages, where the outcomes are binary indicators for survival past ages 65 through 85. The figure shows that the estimated effects become more negative as the survival threshold increases. This indicates that the adverse impact of reduced retirement income due to the Notch grows stronger at more advanced ages. In other words, the negative effects on longevity are more pronounced at older ages, suggesting that income reductions may have cumulative or delayed effects on longevity.[10]

In column 6, we use the Heckman two-step estimate (Heckman, 1979). In so doing, we use the universe of individuals born between 1914 and 1917 observed in the full count 1940 census. We merge these records with our final sample and generate a successful merging dummy. In the first step, the model predicts the successful merging based on observable characteristics. Based on this selection equation, the model creates an Inverse Mills Ratio (IMR) which is then used as an additional variable in the longevity equation in the second step. Therefore, it accounts for potential selection bias due to data merging. Nonetheless, the estimated DD coefficient of column 6 points to an almost identical coefficient compared with that of the main results. We further examine this selection bias concern by using the successful merging dummy as the outcome of Equation (1). We report these results in Appendix Table A-5. The DD coefficient suggests a change in the

[10] One argument is that we might expect more pronounced effects among earlier deaths, particularly among lower-income individuals who are more sensitive to benefit reductions. However, the observed coefficients of the appendix suggest delayed effects. There are several potential explanations for this timing pattern. First, Social Security retirement benefits are more consequential for survival at older ages, when individuals' other resources—such as savings, private pensions, and informal support—may become depleted. By age 75 and beyond, Social Security tends to become the dominant or sole source of income for many elderly individuals, especially those without private savings. Second, the longevity effects of income shocks may operate through slow-moving channels, such as cumulative stress, deferred medical care, or accelerated aging processes. These mechanisms may not manifest immediately after retirement but rather gradually, becoming more apparent in later years (Markon et al., 2024).

probability of successful merging that is indistinguishable from zero, both statistically and economically.

While in the paper, we cluster standard errors at the birth-month level, in column 7 of Appendix Table A-3, we show that we attain the same statistical significance if we simply use robust standard errors to account for heteroscedasticity in error terms.

One concern in interpreting the main difference-in-difference results is the endogenous changes in cohorts' sociodemographic and socioeconomic composition. This can be the result of changes in the survival of 1916-1917 cohorts versus the same differential survival of 1914-1915 cohorts. Such selective survivals could be problematic for our estimates if the surviving individuals possess characteristics that correlate with their health and longevity. We empirically implement a balancing test of our sample by regressing observable characteristics on the main independent variables of Equation (1). We report these results in Appendix Table A-4. The DD coefficient provides insignificant and very small associations with the probability of being white and black (columns 1-2). Although we observe some associations with the father's schooling, the point estimate is very small in magnitude compared to the mean of the outcome (column 3). Further, we observe very small and insignificant associations with the mother's years of schooling and individuals' own years of schooling (columns 6-7). On the other end, the significant DD coefficients for missing values of the father's and mother's years of schooling simply reflect the fact that the 1917 cohort (versus the 1916 cohort) leaves their original household at a faster rate than the 1915 cohort (versus the 1914 cohort). The big picture of this table rules out the concerns regarding endogenous compositional change and survival into adulthood.

4.5. Additional Analysis Using NCHS Data

To further complement the analysis based on the DMF data and evaluate the robustness of our results across a wider death window, we employ the restricted-access National Center for Health Statistics death records, extracted from (NCHS, 2020). This dataset covers the universe of deaths in the U.S. and includes information on age at death, from which we infer individuals' birth cohorts. However, the data has certain limitations: it does not report birth month, and the birth year is estimated based on age at death and year of death, which may introduce measurement error in cohort assignment.

Using a similar estimation strategy as specified in Equation (1), we replicate the DD estimates and report the results in Appendix Table A-6. The top panel presents DD coefficients comparing the 1916–1917 cohorts to the 1914–1915 cohorts, while the bottom panel compares the 1916–1917 cohorts to the 1912–1913 cohorts. We begin by examining effects for deaths occurring between 1979 and 1985 (column 1), and gradually extend the death window in subsequent columns. The DD coefficients remain small and statistically insignificant through death years 1995. However, as we extend the window to include deaths up to 2010, the estimates grow in magnitude and become statistically significant, indicating reductions in longevity between 0.8 and 1.3 months, quite comparable with our main results based on the DMF data. Notably, the coefficients decline in magnitude when we further extend the window to include deaths through 2015. This attenuation likely reflects survivor bias, as individuals reaching 2010–2015 would have to survive to extremely old ages (around 95), and the estimates may be influenced by outliers with unusually long lifespans.

Additionally, Appendix Table A-7 investigates the effect of the Notch using death records from 1959–1979.[11] This earlier window serves as a falsification test, as most individuals in the 1916–1917 birth cohorts had not yet reached retirement age by 1979 and therefore should not have been affected by the Notch during this period. Moreover, because the Social Security policy change was unanticipated, it could not have induced behavioral changes that would result in differential mortality across these cohorts prior to 1979. The difference-in-difference estimate (−0.1 months, s.e. = 0.2) is statistically insignificant and close to zero, confirming that no mortality effect is detected in this early death window. This finding supports the identifying assumption that cohort differences in longevity prior to the Notch's implementation were minimal and that the observed effects in later periods are likely attributable to the policy change.

4.6. Alternative Treatment/Control Groups

The results so far have primarily relied on the residual difference in longevity between Notch cohorts and pre-Notch cohorts, while partialling out seasonalities observed in the 1915 versus 1914 cohorts. In Appendix Table A-8, we extend the analysis by expanding the treatment cohorts to include those born in 1917–1918 (Panel A) and 1917–1919 (Panel B), using the 1911–1914 cohorts as the control group for the second difference. Although the DD coefficients remain negative and statistically significant, their magnitudes are substantially larger than those in the main results, suggesting that the adverse effects on longevity may be more pronounced for later Notch cohorts.

S&E use female mortality outcomes as a falsification test, since women in the 1916–1917 birth cohorts typically received Social Security retirement income based on their husbands'

[11] In these regressions, we include birth state fixed effects, birth month fixed effects, and individual race and gender dummies. The data covers individuals who died between the ages of 42 – 65.

earnings. To replicate this exercise in our setting, we use the Numident data extracted from the Censoc project (Breen et al., 2023; Goldstein et al., 2021). Although the Numident data include both male and female deaths, the death window is limited to 1988–2005. Like the DMF, the Numident data are linked at the individual level to the 1940 Census. We combine the Numident and DMF data, removing duplicate records, and apply the same sample restrictions described in Section 2. We then implement the same estimation strategy as outlined in Equation (1).

Column 1 of Appendix Table A-9 replicates the DD estimates for male individuals using the combined DMF/Numident data. We find a reduction in longevity of approximately 2.7 months. The larger magnitude of this estimate is likely due to the fact that Numident death records are more heavily concentrated in the post-1990 period. As shown in Figure 1 and Appendix Table A-8, the negative effects of the Notch policy are more pronounced in later death years, when individuals reach more advanced ages.

Column 2 reports the corresponding results for females. The estimated coefficient is economically and statistically indistinguishable from zero. In Column 3, we report the difference in DD coefficients between males and females. The triple-difference estimate in the first row indicates a significant reduction in male longevity of approximately 2.4 months. Meanwhile, the DD coefficient for females remains both statistically and economically insignificant.

4.7. Mechanisms

S&E's study employs the Current Population Survey data around 1980 and suggests increases in employment and labor force participation as mechanisms of improvement in health and longevity. In this section, we focus on the 1990-2000 censuses (extracted from Ruggles et al. (2020)) to revisit the policy effects on income and labor force outcomes. Specifically, we implement regressions similar to Equation (1), conditional on individual race and ethnicity, birth state, and census year dummies. These results are reported in panel A of Table 6. The DD coefficient suggests a reduction of about 5.9% in retirement income and an increase in wage income of about 2.1% (columns 1-2). Rises in wage income are due to increases in labor force participation and employment of the elderly population post-retirement ages (columns 3-4). Despite increases in wage income, the total personal income of the notch cohort reveals a reduction of 0.4%. Although the point estimate of total personal income is noisy, its negative sign suggests that the benefits of increases in employment and labor force participation were not significant enough to offset the adverse impacts of reductions in retirement income.

In Appendix Table A-3, we show that the effects become considerably larger when we look at longevity beyond age 80. This is also evident when we compare the point estimate of column 5 versus 4 (of Appendix Table A-3) for longevity beyond age 80 and 75, respectively. Therefore, one would expect to observe a similar pattern for the mechanism analysis. In panel B of Table 6, we restrict the sample to individuals past age 80. We observe larger reductions in retirement income. However, increases in labor force participation do not translate into significantly higher wage income. We observe an insignificant increase in wage income of roughly 1.4%. On the other hand, the net negative effect on total personal income becomes considerably larger compared with panel A. The DD coefficient of column 5 suggests a reduction in total personal income of about 3.8%. Further, we observe small but statistically significant increases in work disability. The DD coefficient points to a 38 basis-point increase in the probability of work disability, off a mean of 0.26.

Appendix Table A-2 examines heterogeneity in the impact of the Social Security Notch on log retirement income, using the same data sources and estimation strategy as in Table 6. Among White individuals, the Notch led to a 6.5% decline in retirement income, while the reduction was substantially larger among Black individuals, at 19.2%, suggesting greater vulnerability to benefit cuts in this group. The effect was also more pronounced among individuals with less than 12 years of education (a 6.6% reduction) compared to those with at least 12 years of education (a 3.0% reduction). These findings indicate that the adverse income effects of the Notch disproportionately affected socioeconomically disadvantaged groups. Importantly, these results mirror the heterogeneity patterns observed in Appendix Table A-1 (i.e., larger effects on longevity of black individuals and low-educated individuals), suggesting that the differences in longevity outcomes are likely driven by the differential changes in retirement income resulting from the policy change.

5. Conclusion

This paper revisited the old question of the role of income on mortality with new data and new perspectives. While the literature on income-mortality is extensive, it provides mixed evidence and inconclusive findings (Altenderfer, 1947; Chetty et al., 2016; Evans & Moore, 2011; Salm, 2011; Snyder & Evans, 2006). To overcome endogeneity issues, we follow a subset of this literature and use a change in the Social Security retirement benefits policy that resulted in substantially lower benefits for cohorts born after January 1, 1917. We employed death records from the Social Security Administration linked to the full-count 1940 census. We implemented difference-in-difference models and found that the reductions in retirement income as a result of the policy change were associated with about one month lower longevity. Our analysis shows that estimates based on short-term mortality closely replicate previous findings of this literature. Nonetheless, as we extend the mortality death window, the effects reverse in sign and become statistically significant. This indicates that the long-term negative impacts of retirement income reductions only become apparent with extended mortality coverage, which shorter observation windows may obscure.

The results suggest considerable heterogeneity. The effects appeared larger among people from lower socioeconomic status families and low-educated individuals. We implemented a wide range of robustness checks and functional form checks. Further, we showed that these results are not driven by endogenous changes in the sociodemographic and socioeconomic composition of the final sample or other selection biases due to data linking.

Using census data (1990-2000), we found significant reductions in retirement income as a result of the policy change and increases in labor force participation and employment, which resulted in rises in wage income. However, the net effect on total personal income is negative and significant. Further, these negative impacts (on both income and longevity) grow in size at older ages, especially past age 80. This evidence suggests a positive income-longevity relationship among the elderly population.

One way to understand the magnitude of the estimated effect on longevity is to use Value of Statistical Life (VSL) calculations. Studies suggest VSL estimates of roughly $10 million in the case of the US (Colmer, 2020). Using the average longevity in our sample, we estimate a per-person loss of about $10.8K. In our final sample, 123,168 individuals belong to the notch cohort. A simple back-of-the-envelope calculation suggests a loss of $1.3 billion due to life years lost as a result of the Social Security policy change.

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Tables

Table 1 - Brief Literature Summary of Income-Mortality Relationship

Study	*Income Type*	*Identification*	*Sample*	*Outcome*	*Summary of Results*
(Altenderfer, 1947)	Per capita income across large U.S. cities	Cross-sectional comparison (cities grouped by income level)	92 U.S. cities (≥100k population); data from 1939–40	Mortality rates (total, infant, maternal)	Higher city income associated with lower mortality rates. Cities in the highest income group had ~10% lower age-adjusted death rates than the lowest.
(Kitagawa & Hauser, 1973)	Family income	Descriptive correlations	Linked Census-death record; U.S. adults aged 25+, 1960	Mortality rates	Higher income and education is associated with lower mortality at all ages- Early evidence of SES gradient
(Lindahl, 2005)	lottery winning prizes	IV and reduced-form regressions using lottery winnings as exogenous variation in income	Survey data from Sweden; aged 18-76; years 1968-1981	5-year and 10-year mortality rates	A 10% income increase reduces 5- and 10-year mortality by 6%-13%
(Cutler et al., 2006)	Broad socioeconomic status (education, income, occupational rank)	Literature synthesis	Multiple countries and time periods	Mortality rates/Life expectancy	While mortality strongly correlates with income, the causal impact of income on mortality is limited, with education and public health interventions playing more central roles in explaining variations in mortality
(Snyder & Evans, 2006)	Social Security income variation	Quasi-experimental design using DiD and RD based on birth date cutoff induced by the 1977 legislative reform	U.S. elderly men born 1915–1918; mortality tracked post-age 65 during 1980s	5-year mortality rates post-retirement	Cohorts with higher Social Security benefits (born before the notch) had higher mortality.
(Dobkin & Puller, 2007)	Government transfer timing (e.g., SSI)	Exploits sharp monthly cycles of benefit disbursement	U.S. transfer recipients, 1994-2000, adults aged 20-57	Drug use, hospitalization, mortality	22% increase in mortality following SSI receipt
(Cristia, 2009)	Lifetime earnings	Descriptive correlations	U.S. adults aged 35–75, 1983–2003	1-year mortality; life expectancy between ages 35–76	Strong income–mortality gradient; life expectancy rose for top earners but stagnated or declined for bottom earners
(Evans & Moore, 2011)	Wage/welfare Income (SSI payments, military wages, tax rebates, Alaska Permanent Fund dividends) receipt timing	DiD and event-study	U.S. population (adults and elderly); 1973–2006	Daily mortality rates	Mortality spikes immediately after income receipt
(Baird et al., 2011)	Aggregate income measured as per capita GDP	Panel data analysis with fixed effects	59 developing countries, 1975–2004	Infant mortality/maternal mortality	A 1% fall in per capita GDP increases infant mortality by 0.24–0.40 per 1,000 births
(Salm, 2011)	Union Army pensions	Quasi-natural experiment using changes under the 1907 and 1912 U.S. pension laws	Union Army veterans; born 1810-1851	Longevity	Higher pensions led to longer lives, particularly among low-income veterans.
(Evans & Moore, 2012)	Wage, tax rebate, SSI	Event Study exploiting variations in payment timing	U.S. elderly, 1973-2005	Daily mortality	Deaths increase by ~1% after income receipt
(Andersson et al., 2015)	Monthly salary receipt	Exploits within-month and within-week timing of paychecks	Swedish public-sector employees; aged 18-66, years 1995-2000	Daily mortality	Mortality increases 23% on payday, driven by low-income and young workers
(Chetty et al., 2016)	Household income percentile	Descriptive correlations	All U.S. adults aged 40–76; 2001–2014	Life expectancy at age 40	Life expectancy increases steadily with income across the entire distribution
(Kinge et al., 2019)	Household income in Norway	Descriptive correlations	All Norwegian adults aged 40–76; 2001–2014	Life expectancy at age 40	Large income gaps in life expectancy persisted.
(Berman, 2021)	Social Security retirement benefits	RD using changes in wage indexation policies	US men born 1925-1937; years 2009-2017	8-year mortality	An income-mortality elasticity of -0.88

Notes. SSI: Supplemental Security Income; IV: Instrumental Variable; Did: Difference-in-Difference; RD: Regression Discontinuity

Table 2 - Summary Statistics

Variable	Mean	SD	Min	Max
DMF Data:				
Age at Death (months)	928.44	85.81	745	1103
Born 1914	.26	.44	0	1
Born 1915	.25	.43	0	1
Born 1916	.25	.43	0	1
Born 1917	.24	.43	0	1
White	.96	.2	0	1
Black	.04	.19	0	1
Birth Year	1915.48	1.12	1914	1917
Birth Month	6.47	3.44	1	12
Death Year	1992.85	7.14	1979	2005
Death Month	6.43	3.53	1	12
Father's SEI 1st Quartile	.11	.31	0	1
Father's SEI 2nd Quartile	.1	.3	0	1
Father's SEI 3rd Quartile	.1	.3	0	1
Father's SEI 4th Quartile	.1	.3	0	1
Father's SEI Missing	.64	.48	0	1
Mother Education < HS	.36	.48	0	1
Mother Education = HS	.08	.27	0	1
Mother Education > HS	.02	.13	0	1
Mother Education Missing	.54	.5	0	1
Observations		488,580		
Census Data:				
Retirement Income ($1,000)	19.47	26.01	0	342.68
Log (Retirement Income+1)	4.17	4.69	0	12.74
Wage and Salary Income ($1,000)	4.22	23.14	0	532.05
Log (Wage and Salary Income+1)	1.25	3.24	0	13.18
Total Personal Income ($1,000)	42.07	50.59	-23	1082.74
Log Total Personal Income	10.06	1.72	0	13.9
Labor Force Participation	.11	.31	0	1
Employed	.1	.3	0	1
Work Disability	.28	.45	0	1
Born 1914	.21	.41	0	1
Born 1915	.24	.43	0	1
Born 1916	.26	.44	0	1
Born 1917	.28	.45	0	1
White	.92	.27	0	1
Black	.07	.25	0	1
Census Year	1993.13	4.64	1990	2000
Observations		168,145		

Table 3 - Difference-in-Difference Results of Notch on Male Longevity

	Outcome: Age at Death (Months), Sample:		
	Born 1916-1917	Born 1914-1915	Column (1) – Column (2)
	(1)	(2)	(3)
Born 1917 (DD)			-1.07**
			(.4)
Later Cohort	-3.9***	-2.73***	-2.77***
	(.44)	(.41)	(.42)
Born 1916-1917			-17.04***
			(.36)
Observations	241272	247274	488577
R-squared	.02	.02	.02
Mean DV	919.791	936.882	928.442
	Born 1916-1917	Born 1912-1913	Column (4) – Column (5)
	(4)	(5)	(6)
Born 1917 (DD)			-.84
			(.5)
Later Cohort	-3.9***	-2.88***	-2.96***
	(.44)	(.43)	(.42)
Born 1916-1917			-33.19***
			(.29)
Observations	241272	241002	482299
R-squared	.02	.02	.05
Mean DV	919.791	952.671	936.221
	Born 1914-1915	Born 1912-1913	Column (7) – Column (8)
	(7)	(8)	(9)
Born 1915 (DD)			.18
			(.42)
Later Cohort	-2.73***	-2.88***	-2.9***
	(.41)	(.43)	(.44)
Born 1914-1915			16***
			(.2)
Observations	247274	241002	488295
R-squared	.02	.02	.02
Mean DV	936.882	952.671	944.675

Notes. Robust standard errors are in parentheses. All regressions include birth-month and 1940-county fixed effects. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies. "*Later Cohort*" refers to the later cohort of each two-year cohort pair, e.g., for 1916-1917 cohorts, it refers to the 1917 cohort.

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

Table 4 - Comparing the Results with the Snyder-Evans Sample

	Outcome: Age at Death (Months), Sample:			
	Death Years 1979-1990; Death Age 65-92; 1-Quarter Bandwidth	Death Years 1979-1990; Death Age 65-92; 1-Year Bandwidth	Death Years 1979-1990; Death Age 61-92; 1-Year Bandwidth	Death Years 1979-2005; Death Age 61-92; 1-Year Bandwidth
	(1)	(2)	(3)	(4)
Born 1917 (DD)	.4	.02	-.3	-1.07**
	(.52)	(.37)	(.4)	(.4)
Later Cohort	-1.46***	-6.45***	-6.8***	-2.77***
	(.33)	(.2)	(.2)	(.42)
Born 1916-1917	-12.91***	-12.85***	-23***	-17.04***
	(.55)	(.25)	(.3)	(.36)
Observations	43180	175066	195866	488577
R-squared	.09	.06	.1	.02
Mean DV	843.798	844.065	835.725	928.442

Notes. Robust standard errors are in parentheses. All regressions include birth-month and 1940-county fixed effects. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies. The sample includes birth cohorts of 1914-1917.
*** p<0.01, ** p<0.05, * p<0.1

Table 5 – Replicating and Extending Snyder-Evans: 5, 10 and 20 Year Mortality Rates

	Outcome: 65-70 Mortality Rate	*Outcome: 65-75 Mortality Rate*	*Outcome: 65-85 Mortality Rate*
	(1)	(2)	(3)
Panel A. 1-Quarter Bandwidth			
Born 1917 (DD)	-.0028	.0048***	.0055***
	(.0057)	(.0015)	(.0008)
Observations	179167	179167	179167
R-squared	.022	.0142	.014
Mean DV	0.147	0.333	0.782
Panel B. 1-Year Bandwidth			
Born 1917 (DD)	-.0072***	.013***	.0064***
	(.002)	(.0019)	(.0014)
Observations	719961	719961	719961
R-squared	.006	.006	.007
Mean DV	0.148	0.334	0.784

Notes. Robust standard errors are in parentheses. All regressions include birth-month, birth year, and 1940-county fixed effects. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies. All regressions are weighted using the number of deaths after 65 (the denominator) in each cell.

The data covers birth cohorts of 1914-1917 and death years of 1979-2005.

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

Table 6 - Exploring Changes in Income and Labor Force Outcomes of Elderly Male Individuals Due to the Notch

	Outcomes:					
	Log Retirement Income	Log Wage Income	Labor Force Participation	Employed	Log Total Personal Income	Work Disability
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Age 73-86						
Born 1917 (DD)	-.059***	.021***	.006***	.006***	-.004	.002
	(.011)	(.007)	(.001)	(.001)	(.004)	(.001)
Later Cohort	.119***	.081***	.006***	.007***	.056***	-.017***
	(.008)	(.005)	(.001)	(0)	(.003)	(.001)
Born 1916-1917	.252***	.178***	.013***	.013***	.068***	-.029***
	(.008)	(.005)	(0)	(0)	(.003)	(.001)
Observations	3100711	3100711	3100711	3100711	3099547	3100711
R-squared	.017	.018	.012	.013	.022	.014
Mean DV	4.287	1.248	0.107	0.101	10.067	0.275
Panel B. Age 80-86						
Born 1917 (DD)	-.065***	.014	.007***	.006***	-.038***	.004**
	(.02)	(.01)	(.001)	(.001)	(.008)	(.002)
Later Cohort	.107***	.044***	.003***	.003***	.086***	-.02***
	(.015)	(.007)	(.001)	(.001)	(.006)	(.001)
Born 1916-1917	.211***	.091***	.004***	.006***	.073***	-.029***
	(.014)	(.007)	(.001)	(.001)	(.006)	(.001)
Observations	967713	967713	967713	967713	967324	967713
R-squared	.012	.003	.002	.002	.017	.008
Mean DV	4.406	0.655	0.063	0.056	10.046	0.264

Notes. Robust standard errors are in parentheses. All regressions include birth-state and census year fixed effects. All regressions also include individual controls (dummies for race and ethnicity). Regressions are weighted using IPUMS weights. The data comes from 1990 and 2000 censuses. The sample includes birth cohorts of 1914-1917.
*** p<0.01, ** p<0.05, * p<0.1

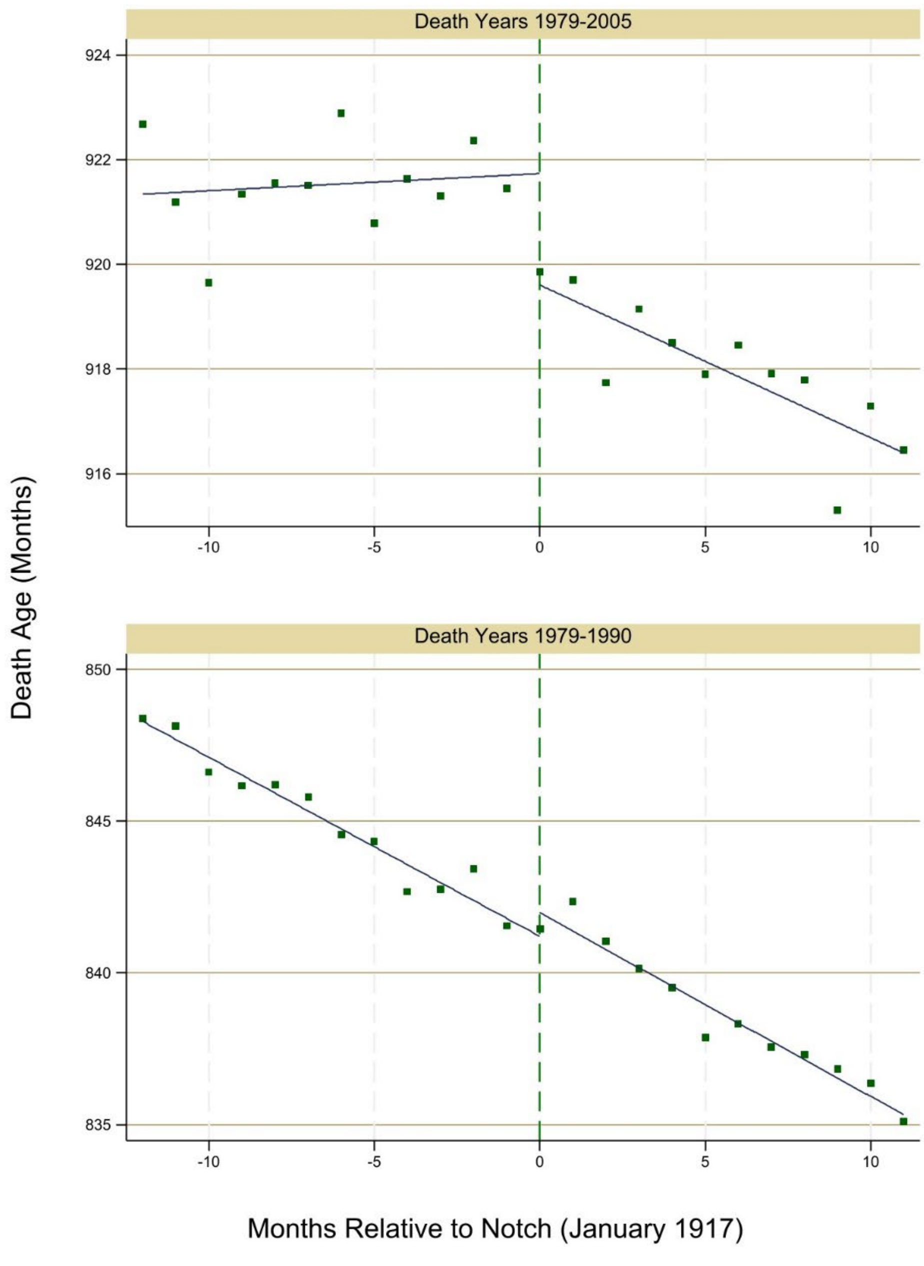
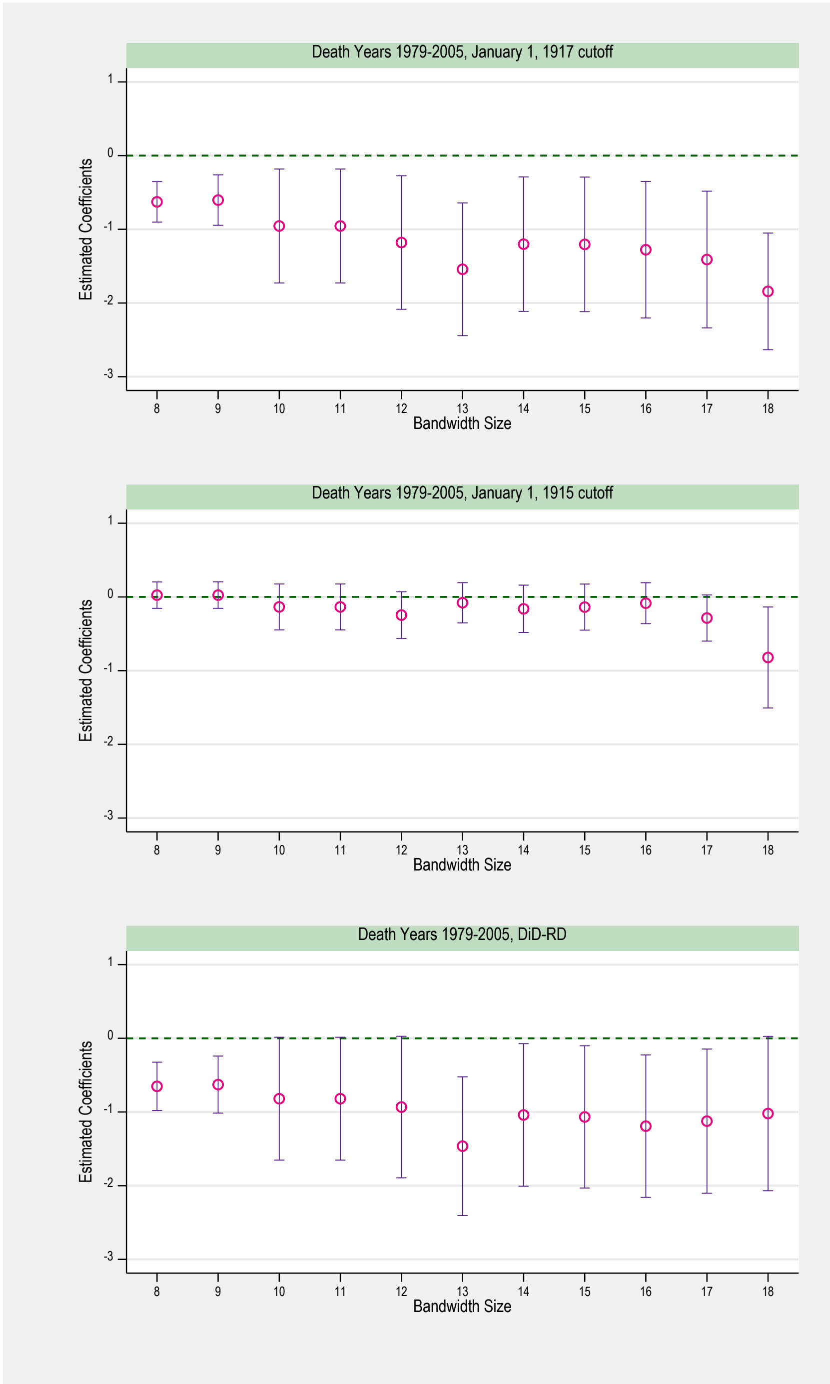


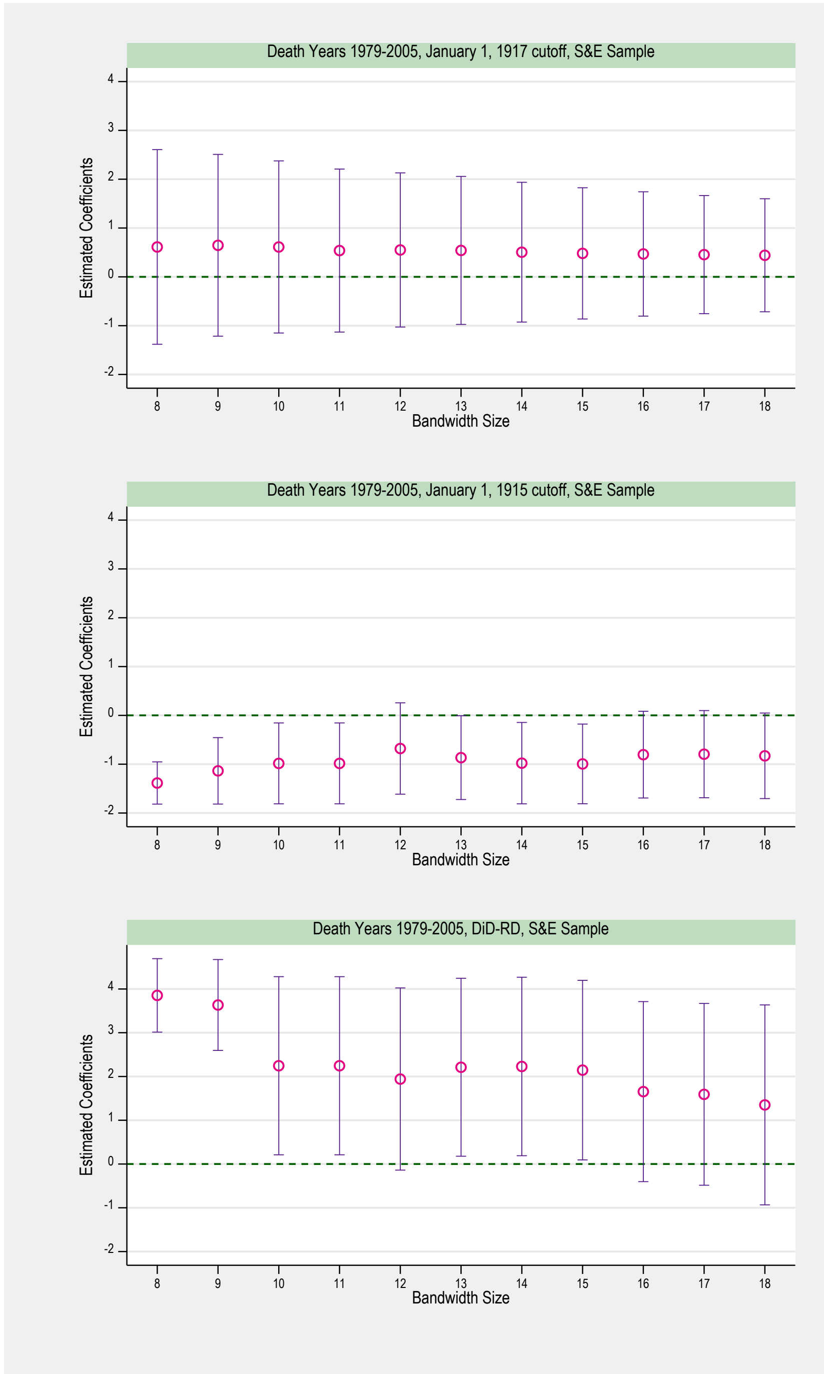
Figure 1 - Regression Discontinuity Graph

Appendix A



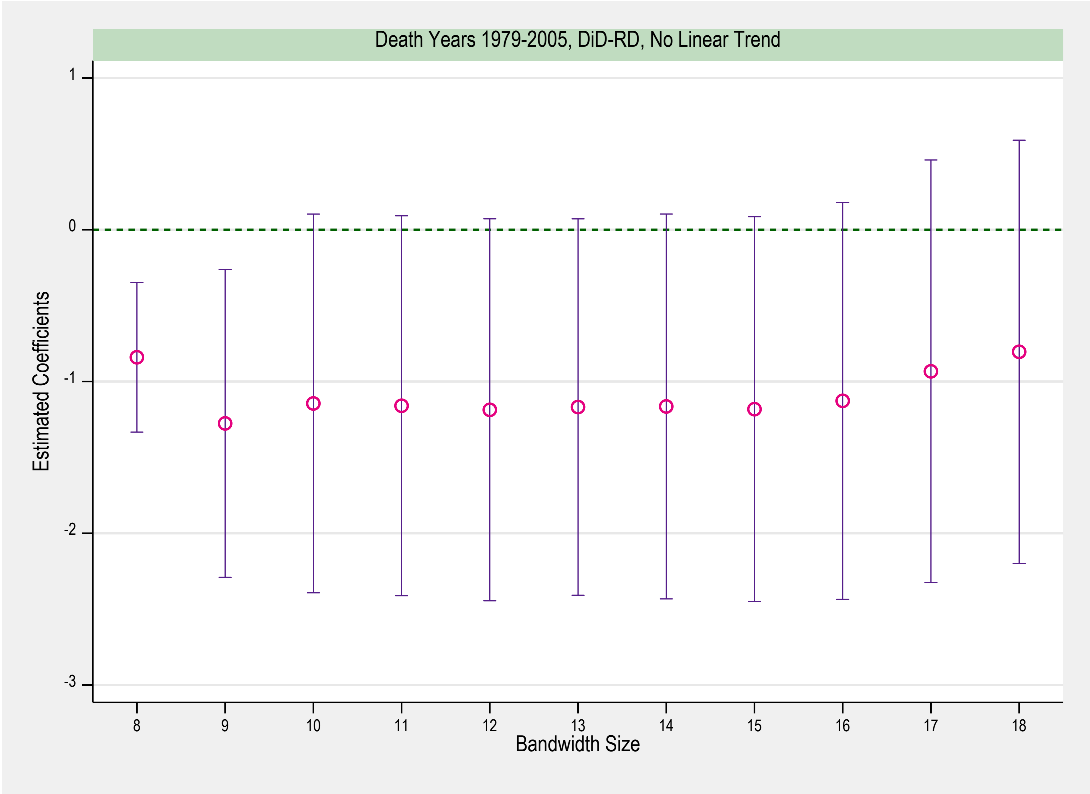
Appendix Figure A-1 - Regression Discontinuity Estimates across Various Bandwidths

This figure plots regression discontinuity estimates from Equation (2), varying the bandwidth around the January 1, 1917 cutoff. The x-axis indicates the bandwidth used in each specification (in months). All models include birth-month and county fixed effects, as well as individual and family controls.



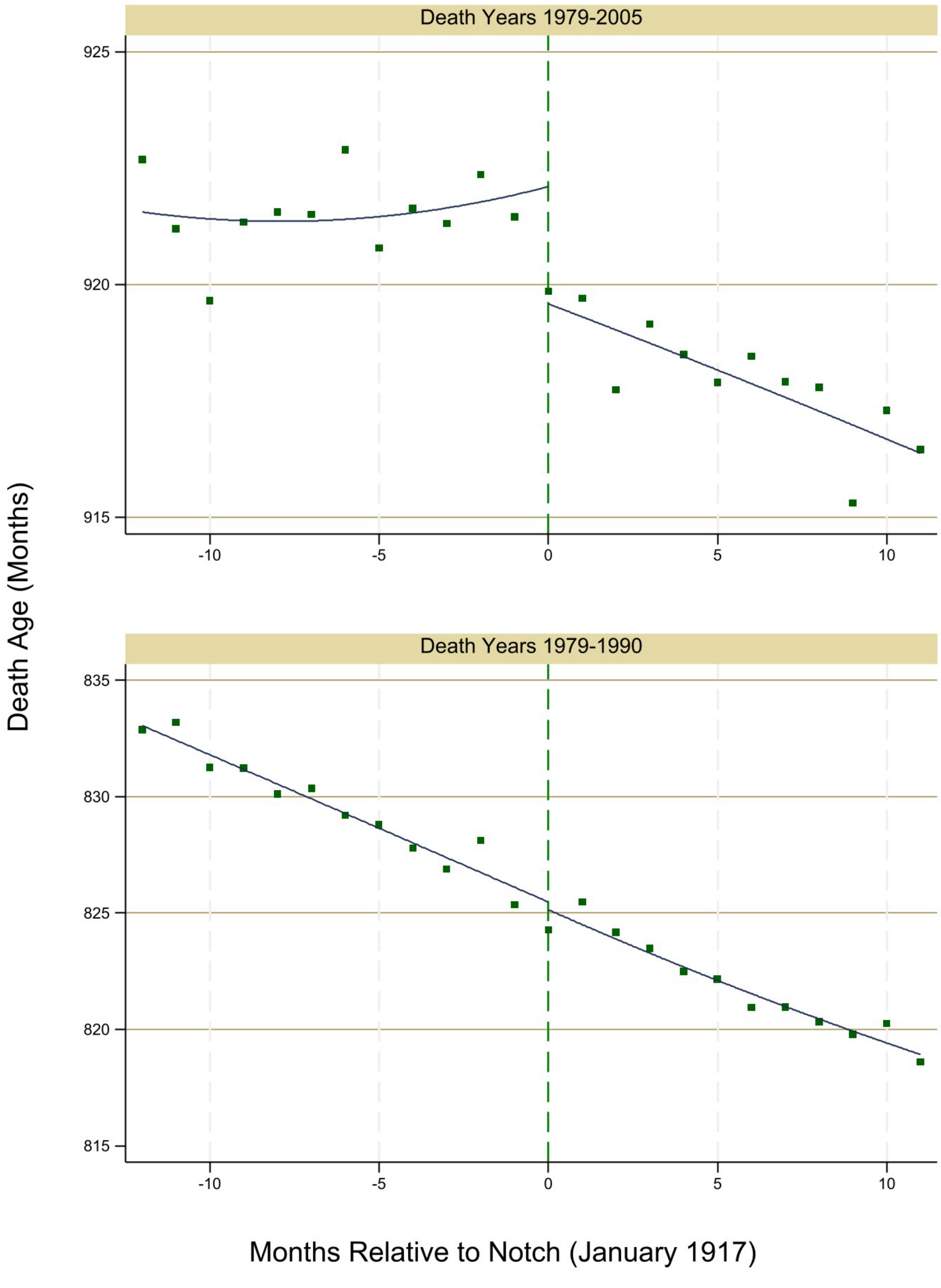
Appendix Figure A-2 - Regression Discontinuity Estimates across Various Bandwidths Using S&E Sample

This figure plots regression discontinuity estimates from Equation (2), varying the bandwidth around the January 1, 1917 cutoff. The x-axis indicates the bandwidth used in each specification (in months). All models include birth-month and county fixed effects, as well as individual and family controls.



Appendix Figure A-3 – Difference-in-Difference Regression Discontinuity Estimates without Trend

This figure plots regression discontinuity estimates from Equation (2) (excluding the linear trend), varying the bandwidth around the January 1, 1917 cutoff. The x-axis indicates the bandwidth used in each specification (in months). All models include birth-month and county fixed effects, as well as individual and family controls.



Appendix Figure A-4 - Robustness of Regression Discontinuity Results to 2nd Degree Local Polynomial to Construct Point Estimates

Appendix Table A-1 - Heterogeneity Analysis

	Outcome: Age at Death (Months), Subsamples:					
	Father Socioeconomic < Median	Father Socioeconomic ≥ Median	Education < 12 Years	Education ≥ 12 Years	White	Black
	(1)	(2)	(3)	(4)	(5)	(6)
Born 1917 (DD)	-2.33**	-1.49	-1.46*	-.22	-1.04**	-1.61
	(.99)	(1.11)	(.76)	(.87)	(.45)	(2.74)
Later Cohort	-1.6*	-1.64*	-2.89***	-3.03***	-2.82***	-2.14
	(.86)	(.79)	(.6)	(.49)	(.41)	(2)
Born 1916-1917	-15.37***	-15.16***	-16.9***	-17.43***	-16.94***	-19.38***
	(.69)	(.93)	(.53)	(.46)	(.34)	(1.65)
Observations	103242	97397	270315	210340	467645	18510
R-squared	.04	.04	.03	.03	.02	.08
Mean DV	928.264	929.697	922.742	935.735	928.899	916.332

Notes. Robust standard errors are in parentheses. All regressions include birth-month and 1940-county fixed effects. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table A-2 - Examining Heterogeneity in the Effects on Retirement Income

	Outcome: Log Retirement Income, Subsamples:			
	White	Black	Education < 12 Years	Education ≥ 12 Years
	(1)	(2)	(3)	(4)
Born 1917 (DD)	-.07***	-.19***	-.07***	-.03**
	(.01)	(.04)	(.02)	(.01)
Later Cohort	.13***	.05*	.10***	.08***
	(.01)	(.03)	(.01)	(.01)
Born 1916-1917	.28***	.13***	.19***	.20***
	(.01)	(.03)	(.01)	(.01)
Observations	2836014	219115	1237914	1862797
R-squared	.01	.02	.03	.005
Mean DV	4.39	3.15	3.37	4.89

Notes. Robust standard errors are in parentheses. All regressions include birth-state and census year fixed effects. All regressions also include individual controls (dummies for race and ethnicity). Regressions are weighted using IPUMS weights. The data comes from 1990 and 2000 censuses. The sample includes birth cohorts of 1914-1917.

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

Appendix Table A-3 - Robustness Checks

	Outcome: Age at Death (Months)						
	Column 3 Panel A	Adding Covariates by Birth-Month FE	Outcome: Log Age-at-Death	Outcome: Age-at-Death ≥ 75	Outcome: Age-at-Death ≥ 80	Heckman (1979) Estimate	Robust SE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Born 1917	-1.07**	-1.07**	-.001**	-.004*	-.007**	-1.07**	-1.07**
(DD)	(.40)	(.40)	(.0004)	(.002)	(.003)	(.49)	(.48)
Later Cohort	-2.77***	-2.77***	-.002***	-.007***	-.01***	-2.77***	-2.77***
	(.42)	(.41)	(.0004)	(.002)	(.002)	(.34)	(.34)
Born 1916-	-17.04***	-17.04***	-.019***	-.052***	-.054***	-17.11***	-17.04***
1917	(.36)	(.36)	(.0004)	(.002)	(.002)	(.34)	(.35)
Observations	488577	488577	488577	488577	488577	9134212	488577
R-squared	.02	.02	.02	.012	.013	---	.020
Mean DV	928.44	928.44	6.829	0.572	0.355	928.44	928.44

Notes. Robust standard errors are in parentheses. All regressions include birth-month and 1940-county fixed effects. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table A-4 - Balancing Tests

	Outcomes:						
	White	Black	Father's SEI	Father's SEI Missing	Mother's Years of Schooling	Mother's Years of Schooling Missing	Own Years of Schooling
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Born 1917 (DD)	-.0001	-.0005	-.211	-.024***	.023	-.025***	-.033
	(.0012)	(.001)	(.12)	(.003)	(.027)	(.004)	(.019)
Later Cohort	-.001	.001	.416***	-.057***	.068***	-.062***	.08***
	(.001)	(.001)	(.126)	(.002)	(.02)	(.002)	(.015)
Born 1916-1917	-.001*	.001*	.489***	-.123***	.145***	-.134***	.101***
	(.001)	(.001)	(.077)	(.002)	(.021)	(.002)	(.012)
Observations	488577	488577	178141	488577	226514	488577	480712
R-squared	.182	.197	.093	.058	.107	.072	.139
Mean DV	0.957	0.039	27.617	0.635	6.906	0.536	10.274

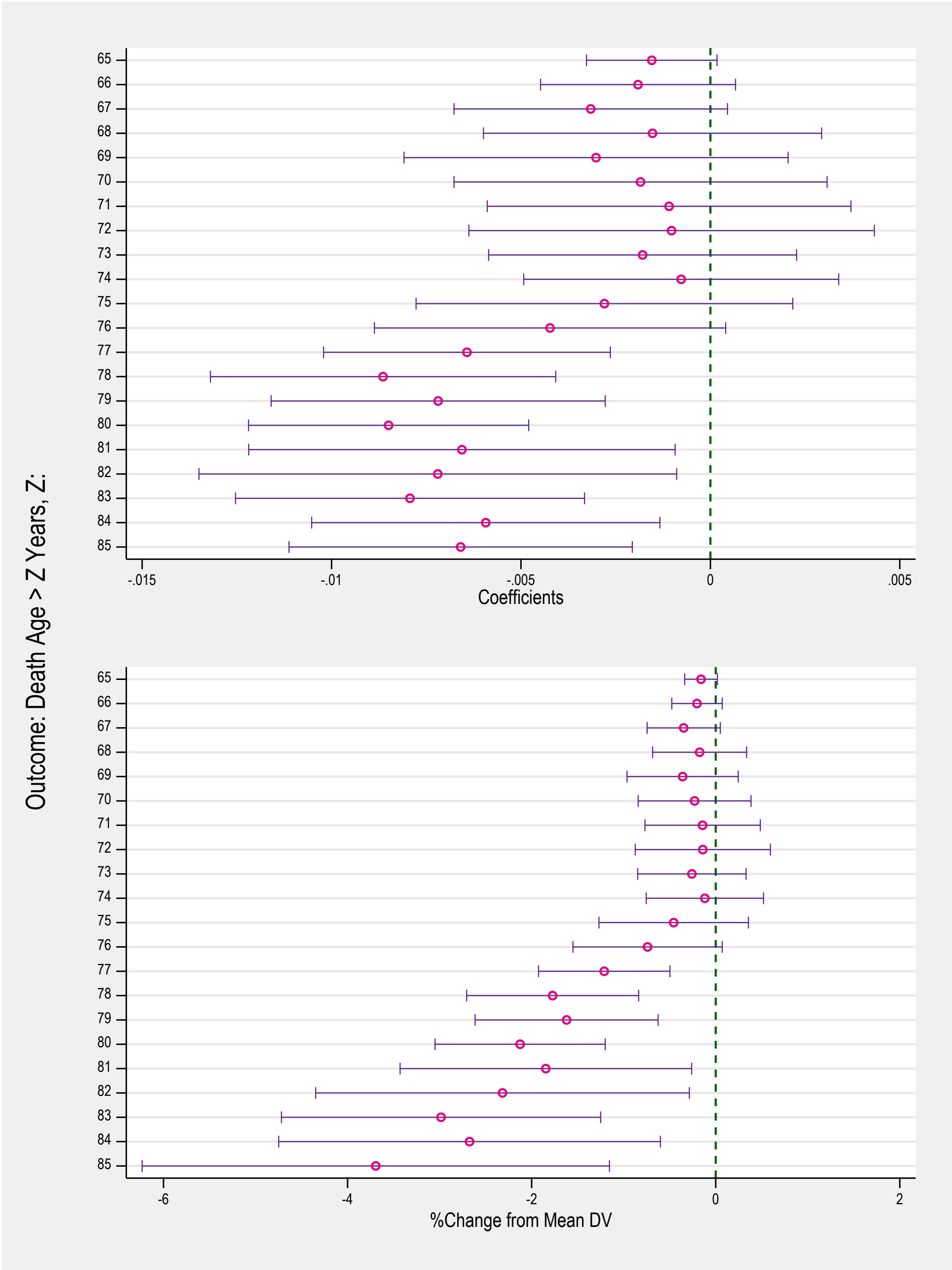
Notes. Robust standard errors are in parentheses. All regressions include birth-month and 1940-county fixed effects. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies.

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

Appendix Table A-5 - Endogenous Merging

	Outcome: Successful DMF-1940-Census Merging		
	Born 1916-1917	Born 1914-1915	Column (1) – Column (2)
	(1)	(2)	(3)
Born 1917 (DD)			.00008
			(.00009)
Later Cohort	-.00027***	-.00031***	-.00033***
	(.00006)	(.00006)	(.00006)
Born 1916-1917			-.00054***
			(.00006)
Observations	4568672	4565540	9134212
R-squared	.91629	.91479	.9155
Mean DV	0.053	0.054	0.053

Notes. Robust standard errors are in parentheses. All regressions include birth-month and 1940-county fixed effects. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies.
*** p<0.01, ** p<0.05, * p<0.1



Appendix Figure A-5 - Examining The Effects across Various Survival Ages

Notes. 95% confidence intervals are reported. Robust standard errors are clustered on birth month. All regressions include birth-month and 1940-county fixed effects. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies. The sample includes birth cohorts of 1914-1917.

Appendix Table A-6 - Replicating the Difference-in-Difference Results across Various Death Windows Using NCHS Death Records

	Outcome: Age at Death (Months), Sample:						
	Death Years 1979-1985	Death Years 1979-1990	Death Years 1979-1995	Death Years 1979-2000	Death Years 1979-2005	Death Years 1979-2010	Death Years 1979-2015
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. 1914-1915 and 1916-1917 Cohorts							
Born 1917 (DD)	-.002	.05	.087	-.108	-.538***	-.83***	-.414***
	(.085)	(.109)	(.124)	(.135)	(.144)	(.152)	(.159)
Later Cohort	-11.915***	-11.773***	-11.331***	-10.481***	-8.841***	-6.392***	-4.108***
	(.058)	(.075)	(.086)	(.094)	(.101)	(.107)	(.111)
Born 1916-1917	-23.807***	-23.503***	-22.637***	-21.076***	-17.994***	-13.253***	-8.531***
	(.059)	(.076)	(.087)	(.095)	(.101)	(.107)	(.112)
Observations	930547	1911606	3041560	4324603	5596347	6524063	6982136
R-squared	.298	.111	.059	.043	.043	.049	.055
Mean DV	794.432	827.792	861.732	896.396	928.281	951.806	964.788
Panel B. 1912-1913 and 1916-1917 Cohorts							
Born 1917 (DD)	-.006	-.07	-.161	-.392***	-1.049***	-1.273***	-.224
	(.084)	(.108)	(.123)	(.135)	(.144)	(.152)	(.159)
Later Cohort	-11.912***	-11.652***	-11.079***	-10.19***	-8.323***	-5.948***	-4.302***
	(.057)	(.073)	(.085)	(.094)	(.101)	(.107)	(.111)
Born 1916-1917	-47.745***	-46.947***	-45.099***	-41.765***	-35.065***	-25.55***	-17.171***
	(.059)	(.075)	(.087)	(.095)	(.102)	(.108)	(.112)
Observations	955661	1952725	3082270	4334826	5517964	6325479	6701616
R-squared	.59	.295	.163	.104	.076	.064	.061
Mean DV	807.597	840.569	873.816	907.337	937.088	957.887	968.802

Notes. Robust standard errors are in parentheses. All regressions include birth state fixed effects. All regressions also include individual race and gender dummies.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Table A-7 - Examining the Effects Using NCHS Data in Death Years 1959-1979

	Outcome: Age at Death (Months)		
	Born 1916-1917	Born 1914-1915	Column (1) – Column (2)
	(1)	(2)	(3)
Born 1917 (DD)			-.1
			(.206)
Later Cohort	-11.413***	-11.318***	-11.318***
	(.151)	(.141)	(.141)
Born 1916-1917			-22.848***
			(.143)
Observations	821366	953775	1775141
R-squared	.014	.014	.04
Mean DV	657.118	680.051	669.440

Notes. Robust standard errors are in parentheses. All regressions include birth state fixed effects. All regressions also include individual race and gender dummies.

In these regressions, we include birth state fixed effects, birth month fixed effects, and individual race and gender dummies. The data covers individuals who died between the ages of 42 – 65.

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

Appendix Table A-8 – Examining Robustness to Including 1918-1919 Cohorts

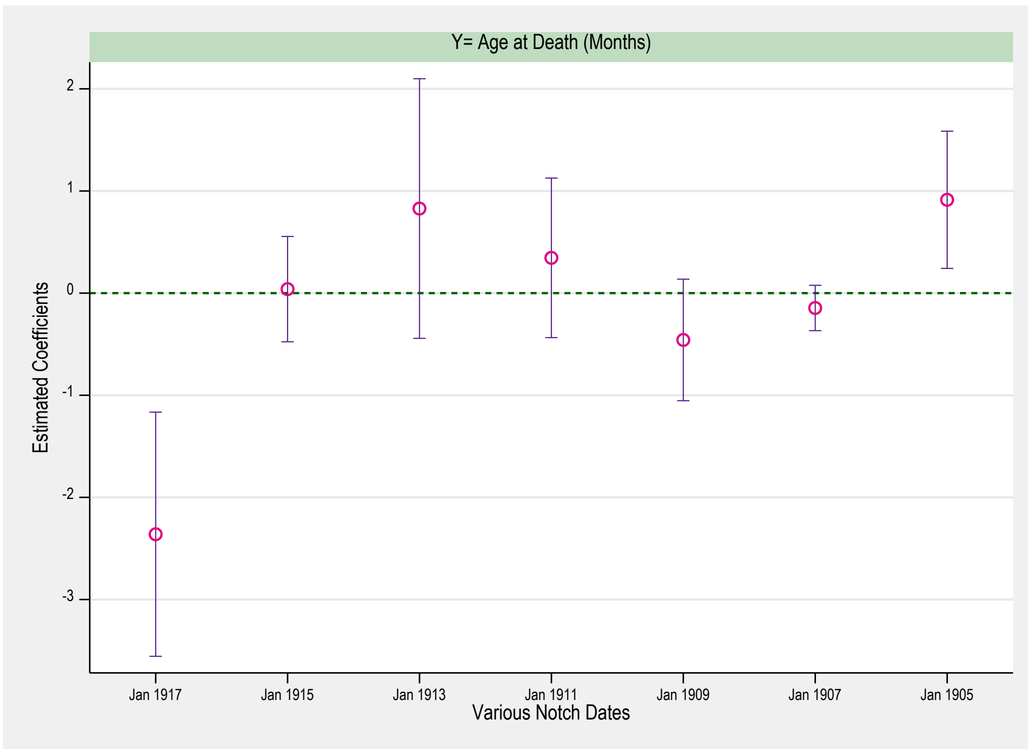
	Outcome: Age at Death (Months), Sample:		
	Born 1915-1918	Born 1911-1914	Column (1) – Column (2)
	(1)	(2)	(3)
Born 1917-1918 (DD)			-3.41***
			(.48)
Later Cohort	-17.37***	-13.53***	-13.72***
	(.39)	(.22)	(.21)
Born 1916-1917			-29.92***
			(.27)
Observations	486133	481519	967661
R-squared	.02	.02	.05
Mean DV	920.192	951.239	935.641
	Born 1915-1919	Born 1911-1914	Column (4) – Column (5)
	(4)	(5)	(6)
Born 1917-1919 (DD)			-8.34***
			(.51)
Later Cohort	-22.2***	2.73***	-13.68***
	(.42)	(.41)	(.21)
Born 1916-1917			-29.85***
			(.26)
Observations	601441	247274	1082967
R-squared	.02	.02	.06
Mean DV	915.628	936.882	931.461

Notes. Robust standard errors are in parentheses. All regressions include birth-month and 1940-county fixed effects. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies. "*Later Cohort*" refers to the later cohort of each two-year cohort pair, e.g., for 1916-1917 cohorts, it refers to the 1917 cohort.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table A-9 - Replicating the Main Results Using Females as Control Group

	Outcome: Age at Death (Months), Sample:		
	Male	Female	Column (1) – Column (2)
	(1)	(2)	(3)
Born 1917-1918 (DD) × Male			-2.4***
			(.4)
Later Cohort × Male			5.3***
			(.4)
Born 1916-1917 × Male			5.4***
			(.4)
Born 1917-1918 (DD)	-2.71***	-.15	-.3
	(.31)	(.26)	(.3)
Later Cohort	-5.04***	-10.41***	-10.4***
	(.35)	(.25)	(.2)
Born 1916-1917	-15.96***	-21.35***	-21.3***
	(.4)	(.17)	(.2)
Male			-37.3***
			(.7)
Observations	1240366	821655	2062026
R-squared	.02	.04	.1
Mean DV	953.376	985.040	965.993

Notes. Robust standard errors are in parentheses. All regressions include birth-month and 1940-county fixed effects. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies. "*Later Cohort*" refers to the later cohort of each two-year cohort pair, e.g., for 1916-1917 cohorts, it refers to the 1917 cohort.
*** p<0.01, ** p<0.05, * p<0.1



Appendix Figure A-6 – Regression Discontinuity Estimates to Examine The Effects across Various Placebo Notch Dates

Notes. 95% confidence intervals are reported. Robust standard errors are clustered on birth month. All regressions also include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include father's socioeconomic status dummies and mother's education dummies. Bandwidths are selected using rdrobust command in Stata. The sample is restricted to death ages 70-85.