In Money, We Survive: The Effects of Social Security Retirement Income on Longevity*

Hamid Noghanibehambari†

Jason Fletcher‡

Abstract

An old and debated 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 postretirement labor force participation. Past age 80, the net negative effects of the policy on both income and longevity become more pronounced. We further discuss these findings in the context of potential changes to Social Security policies, considering the predicted future insolvency of Social Security funds.

Keywords: Social Security, Retirement, Pension, Mortality, Longevity, Historical Data

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

---

* The authors claim that they have no conflict of interest to report. The authors would like to acknowledge financial support from NIA grants (R01AG060109, R01AG076830) and the Center for Demography of Health and Aging (CDHA) at the University of Wisconsin-Madison under NIA core grant P30 AG17266. The authors thank participants at the NBER Cohort Studies Spring 2023 meeting for helpful comments.

† College of Business, Austin Peay State University, Marion St, Clarksville, TN 37040, USA
Email: noghanih@apsu.edu, Phone: +1-806-620-1812

‡ 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 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).

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 food could raise the probability of 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 negative effects of physical activities on health (Nilsen et al., 2016; Witte et al., 2005). In contrast, labor force activity can enhance the engagement of the elderly 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. 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, Snyder & Evans (2006) use abrupt and large changes in social security retirement benefits as an income shock and show that reductions in benefits reduce mortality rates of the elderly population. However, these findings focus on short-term effects.

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*. First, First, 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. 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 0.9 months lower longevity in an elderly population. The estimated effect is in contrast with the previous research on mortality effects of social security Notch (Snyder & Evans, 2006). 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.

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 certain policies, which could involve discussing the necessity of making benefit reductions in the coming years. The findings of this paper has 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 life-time 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 Social Security policy change. Section 3 discusses the data source and econometric method. Section 4 reviews the results. We conclude the paper in section 5.

2. A Short Background on Social Security Notch

Prior to the 1930s, welfare support for old age was primarily 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 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 based on cost of living adjustments. During the 1970s, Congress used the Consumer Price Index (CPI) to index the benefit tables. Prior to this change, 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 double indexation coupled with high inflations of the 1970s created a threat of insolvency for the social security system as soon 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. Snyder & Evans (2006) find about 4 percent higher income for pre-Notch generation, or $41 per month (in 1987 dollars).

3. Data and Econometric Method

3.1. Data Sources

Our primary data source is Death Master Files (DMF) data extracted from the Censoc project (Goldstein et al., 2021). The DMF data covers deaths that occurred in male individuals between 1975-2005. The advantage of DMF is that it is linked with the full-count 1940 census.

Therefore, we can observe individuals' socioeconomic and sociodemographic characteristics in 1940 and implement additional heterogeneity analyses. Moreover, it contains the exact date of birth and death, allowing us to calculate the exact duration of life. It also allows us to measure Notch-cohort individuals precisely. 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.

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 important to have a balanced death window. With a fixed death window, the earlier cohorts of each two-cohort pairs (e.g., 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 common age at death for both 1916 and 1917 cohorts. Similarly, for 1914-1915 pair, we restrict age at death to be between 65-90.

Summary statistics of the final sample are reported in Table 1. The final sample consists of roughly 487,087 observations. The average age-at-death is 928.6 months, or roughly 77.4 years.

Roughly 24.3 percent of individuals are Notch cohorts. About 95.7 percent of observations are whites, and 3.9 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 1915 cohort and 1914 cohorts). While the first difference reveals a cross-cohort comparison between Notch and pre-Notch cohorts, it could be the case that such 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 square 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 forwarding 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 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:

$$y_{id} = \alpha_0 + \alpha_1 Notch_d + \alpha_2 BirthDate_d + \alpha_3 X_i + \varepsilon_{id} \quad (2)$$

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 the birth date. The parameter X is defined in equation 1. Finally, ε is an error term. We cluster standard errors at the birth-month level. We use the method developed by Cattaneo et al. (2020) to determine an optimal bandwidth of 12-months, which we use for the regression discontinuity analysis.

4. Results

4.1. Difference-in-Difference Results

Table 2 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. 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 one month in longevity for the Notch cohort.

The second panel of Table 2, 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 2. 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.

The effects of Table 2 are especially 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. However, there are slight differences between our analysis sample and theirs. In column 1 of Table 3, 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 Snyder & Evans (2006) 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 0.4 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 2, pointing to a one-month reduction in longevity. Although all these sample selections make a difference between Snyder & Evans (2006) results and those of the current study, the largest change appears in expanding the death window that covers deaths up to 2005.

Snyder & Evans (2006) estimate that the Notch resulted in a reduction in benefits of about $1,120 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 0.9 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 2, 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 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. 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 Snyder & Evans (2006), i.e., death years of 1979-1990. The sample does not provide a break in trends for notch cohorts.

We report the regression discontinuity estimates in Appendix Table A-1. The point estimate from the regression discontinuity suggests an almost identical effect to the difference-in-difference results. In this setting, when we look at the change in longevity of 1914-1915 cohorts (assigning a placebo notch for the 1915 cohorts), we observe a positive and statistically significant change. Therefore, the seasonality and cross-cohort trends could only bias the regression discontinuity results downward. For instance, the difference between column 1 and column 2 of Appendix Table A-1 implies a reduction in longevity of about 1.7 months.

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-2. The DD coefficient of the low socioeconomic status subsample is about 1.8 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 5 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 2 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.

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 Miller 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 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, 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. Mechanisms

Snyder & Evans (2006) 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 4. 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 big 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 4, 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 38 basis-points increase in the probability of work disability, off a mean of 0.26.

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 large, 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 sub-set of the 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. The results suggest considerable heterogeneity. The effects appeared larger among people of 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 bias 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.

References

Aguila, E., Kapteyn, A., & Smith, J. P. (2015). Effects of income supplementation on health of the poor elderly: The case of Mexico. *Proceedings of the National Academy of Sciences of the United States of America*, *112*(1), 70–75. https://doi.org/10.1073/PNAS.1414453112/SUPPL_FILE/PNAS.201414453SI.PDF

Almond, D. (2006). Is the 1918 influenza pandemic over? Long-term effects of in utero influenza exposure in the post-1940 US population. *Journal of Political Economy*, *114*(4), 672–712. https://doi.org/10.1086/507154

Almond, D., & Mazumder, B. (2013). Fetal Origins and Parental Responses. *Annual Review of Economics*, *5*, 37–56. https://doi.org/10.1146/ANNUREV-ECONOMICS-082912-110145

Altenderfer, M. (1947). Relationship between per capita income and mortality in the cities of 100,000 or more population. *Public Health Reports*, *62*(48), 1681–1691.

Andersson, E., Lundborg, P., & Vikström, J. (2015). Income receipt and mortality — Evidence from Swedish public sector employees. *Journal of Public Economics*, *131*, 21–32. https://doi.org/10.1016/J.JPUBECO.2015.08.006

Baird, S., Friedman, J., & Schady, N. (2011). Aggregate income shocks and infant mortality in the developing world. *Review of Economics and Statistics*, *93*(3), 847–856. https://doi.org/10.1162/REST_a_00084

Barham, T., & Rowberry, J. (2013). Living longer: The effect of the Mexican conditional cash transfer program on elderly mortality. *Journal of Development Economics*, *105*, 226–236. https://doi.org/10.1016/J.JDEVECO.2013.08.002

Cattaneo, M. D., Jansson, M., & Ma, X. (2020). Simple Local Polynomial Density Estimators. *Journal of the American Statistical Association*, *115*(531), 1449–1455. https://doi.org/10.1080/01621459.2019.1635480

Chetty, R., Stepner, M., Abraham, S., Lin, S., Scuderi, B., Turner, N., Bergeron, A., & Cutler, D. (2016). The Association Between Income and Life Expectancy in the United States, 2001-2014. *JAMA*, *315*(16), 1750–1766. https://doi.org/10.1001/JAMA.2016.4226

Colmer, J. (2020). What is the meaning of (statistical) life? Benefit–cost analysis in the time of COVID-19. *Oxford Review of Economic Policy*, *36*(Supplement_1), S56–S63. https://doi.org/10.1093/OXREP/GRAA022

Cristia, J. P. (2009). Rising mortality and life expectancy differentials by lifetime earnings in the United States. *Journal of Health Economics*, *28*(5), 984–995. https://doi.org/10.1016/j.jhealeco.2009.06.003

Cutler, D., Deaton, A., & Lleras-Muney, A. (2006). The Determinants of Mortality. *Journal of Economic Perspectives*, *20*(3), 97–120. https://doi.org/10.1257/JEP.20.3.97

Dobkin, C., & Puller, S. L. (2007). The effects of government transfers on monthly cycles in drug abuse, hospitalization and mortality. *Journal of Public Economics*, *91*(11–12), 2137–2157. https://doi.org/10.1016/J.JPUBECO.2007.04.007

Eggleston, K. N., & Fuchs, V. R. (2012). The New Demographic Transition: Most Gains in Life Expectancy Now Realized Late in Life. *Journal of Economic Perspectives*, *26*(3), 137–156.

https://doi.org/10.1257/JEP.26.3.137

Engelhardt, G. V., Gruber, J., & Kumar, A. (2022). Early Social Security Claiming and Old-Age Poverty. *Journal of Human Resources*, *57*(4), 1079–1106. https://doi.org/10.3368/JHR.57.4.0119-9973R1

Evans, W. N., & Moore, T. J. (2011). The short-term mortality consequences of income receipt. *Journal of Public Economics*, *95*(11–12), 1410–1424.

Evans, W. N., & Moore, T. J. (2012). Liquidity, Economic Activity, and Mortality. *The Review of Economics and Statistics*, *94*(2), 400–418. https://doi.org/10.1162/REST_A_00184

Fan, W., & Porter, C. (2020). Reinforcement or compensation? Parental responses to children's revealed human capital levels. *Journal of Population Economics*, *33*(1), 233–270. https://doi.org/10.1007/s00148-019-00752-7

Ferreira, P. C., & Pessôa, S. de A. (2007). The effects of longevity and distortions on education and retirement. *Review of Economic Dynamics*, *10*(3), 472–493. https://doi.org/10.1016/J.RED.2007.01.003

Fletcher, J. M. (2018a). New evidence on the impacts of early exposure to the 1918 influenza pandemic on old-age mortality. *Biodemography and Social Biology*, *64*(2), 123–126. https://doi.org/10.1080/19485565.2018.1501267

Fletcher, J. M. (2018b). Examining the long-term mortality effects of early health shocks. *Applied Economics Letters*, *26*(11), 902–908. https://doi.org/10.1080/13504851.2018.1520960

Fletcher, J. M., & Noghanibehambari, H. (2021). *The Effects of Education on Mortality: Evidence Using College Expansions*. https://doi.org/10.3386/W29423

Fuchs, V. R. (1980). *Time Preference and Health: An Exploratory Study*. https://doi.org/10.3386/W0539

Golberstein, E. (2015). The Effects of Income on Mental Health: Evidence from the Social Security Notch. *The Journal of Mental Health Policy and Economics*, *18*(1), 27. /pmc/articles/PMC4494112/

Goldstein, J. R., Alexander, M., Breen, C., Miranda González, A., Menares, F., Osborne, M., Snyder, M., & Yildirim, U. (2021). Censoc Project. In *CenSoc Mortality File: Version 2.0. Berkeley: University of California*. https://censoc.berkeley.edu/data/

Halpern-Manners, A., Helgertz, J., Warren, J. R., & Roberts, E. (2020). The Effects of Education on Mortality: Evidence From Linked U.S. Census and Administrative Mortality Data. *Demography*, *57*(4), 1513–1541. https://doi.org/10.1007/S13524-020-00892-6

Heckman, J. J. (1979). Sample selection bias as a specification error. *Econometrica*, *47*(1), 153–161. https://doi.org/10.2307/1912352

Holt-Lunstad, J., Smith, T. B., Baker, M., Harris, T., & Stephenson, D. (2015). Loneliness and Social Isolation as Risk Factors for Mortality: A Meta-Analytic Review. *Perspectives on Psychological Science*, *10*(2), 227–237. https://doi.org/10.1177/1745691614568352

Hoque, M. M., King, E. M., Montenegro, C. E., & Orazem, P. F. (2019). Revisiting the relationship between longevity and lifetime education: global evidence from 919 surveys. *Journal of Population Economics*, *32*(2), 551–589. https://doi.org/10.1007/S00148-018-

0717-9/TABLES/17

Johnson, R., & Jackson, K. (2019). Reducing Inequality through Dynamic Complementarity: Evidence from Head Start and Public School Spending. *American Economic Journal: Economic Policy*, *11*(4), 310–349. https://doi.org/10.1257/POL.20180510

Kinge, J. M., Modalsli, J. H., Øverland, S., Gjessing, H. K., Tollånes, M. C., Knudsen, A. K., Skirbekk, V., Strand, B. H., Håberg, S. E., & Vollset, S. E. (2019). Association of Household Income With Life Expectancy and Cause-Specific Mortality in Norway, 2005-2015. *JAMA*, *321*(19), 1916–1925. https://doi.org/10.1001/JAMA.2019.4329

Kitagawa, E. M., & Hauser, P. M. (1973). Differential mortality in the United States. In *Differential Mortality in the United States*. Harvard University Press.

Krueger, A. B., & Pischke, J.-S. (1992). The Effect of Social Security on Labor Supply: A Cohort Analysis of the Notch Generation. *Journal of Labor Economics*, *10*(4), 412–437. https://doi.org/10.1086/298294

Lichtenberg, F. R. (2004). Sources of U.S. longevity increase, 1960–2001. *The Quarterly Review of Economics and Finance*, *44*(3), 369–389. https://doi.org/10.1016/J.QREF.2004.05.005

Lindahl, M. (2005). Estimating the Effect of Income on Health and Mortality Using Lottery Prizes as an Exogenous Source of Variation in Income. *Journal of Human Resources*, *XL*(1), 144–168. https://doi.org/10.3368/JHR.XL.1.144

Lleras-Muney, A. (2005). The Relationship Between Education and Adult Mortality in the United States. *The Review of Economic Studies*, *72*(1), 189–221. https://doi.org/10.1111/0034-6527.00329

Lv, J., Qi, L., Yu, C., Yang, L., Guo, Y., Chen, Y., Bian, Z., Sun, D., Du, J., Ge, P., Tang, Z., Hou, W., Li, Y., Chen, J., Chen, Z., & Li, L. (2015). Consumption of spicy foods and total and cause specific mortality: population based cohort study. *BMJ*, *351*. https://doi.org/10.1136/BMJ.H3942

Miller, D. L., Page, M. E., Stevens, A. H., & Filipski, M. (2009). Why are recessions good for your health? *American Economic Review*, *99*(2), 122–127.

Nelson, K., & Fritzell, J. (2014). Welfare states and population health: The role of minimum income benefits for mortality. *Social Science & Medicine*, *112*, 63–71. https://doi.org/10.1016/J.SOCSCIMED.2014.04.029

Nilsen, C., Andel, R., Fritzell, J., & Kåreholt, I. (2016). Work-related stress in midlife and all-cause mortality: can sense of coherence modify this association? *European Journal of Public Health*, *26*(6), 1055–1061. https://doi.org/10.1093/EURPUB/CKW086

Noghanibehambari, H., & Fletcher, J. M. (2023). *Dynamic Complementarities between Early- and Late-Life Exposures: Evidence from the Social Security Notch*.

Reedy, J., Krebs-Smith, S. M., Miller, P. E., Liese, A. D., Kahle, L. L., Park, Y., & Subar, A. F. (2014). Higher Diet Quality Is Associated with Decreased Risk of All-Cause, Cardiovascular Disease, and Cancer Mortality among Older Adults. *The Journal of Nutrition*, *144*(6), 881–889. https://doi.org/10.3945/JN.113.189407

Restrepo, B. J. (2016). Parental investment responses to a low birth weight outcome: Who compensates and who reinforces? *Journal of Population Economics*, *29*(4), 969–989.

Ruggles, S., Flood, S., Goeken, R., Grover, J., & Meyer, E. (2020). IPUMS USA: Version 10.0 [dataset]. *Minneapolis, MN: IPUMS*. https://doi.org/10.18128/D010.V10.0

Ruhm, C. J. (2000). Are recessions good for your health? *The Quarterly Journal of Economics*, *115*(2), 617–650.

Salm, M. (2011). The Effect of Pensions on Longevity: Evidence from Union Army Veterans. *The Economic Journal*, *121*(552), 595–619. https://doi.org/10.1111/J.1468-0297.2011.02427.X

Snyder, S. E., & Evans, W. N. (2006). The effect of income on mortality: Evidence from the social security Notch. *Review of Economics and Statistics*, *88*(3), 482–495. https://doi.org/10.1162/rest.88.3.482

Stoian, A., & Fishback, P. (2010). Welfare spending and mortality rates for the elderly before the Social Security era. *Explorations in Economic History*, *47*(1), 1–27. https://doi.org/10.1016/j.eeh.2009.05.005

Takagi, D., Kondo, N., Takada, M., & Hashimoto, H. (2016). Educational attainment, time preference, and health-related behaviors: A mediation analysis from the J-SHINE survey. *Social Science & Medicine*, *153*, 116–122. https://doi.org/10.1016/J.SOCSCIMED.2016.01.054

Witte, D. R., Grobbee, D. E., Bots, M. L., & Hoes, A. W. (2005). A Meta-analysis of excess cardiac mortality on Monday. *European Journal of Epidemiology 2005 20:5*, *20*(5), 401–406. https://doi.org/10.1007/S10654-004-8783-6

Tables

Table 1 - Summary Statistics

Variable	Mean	SD	Min	Max
Age at Death (months)	928.62828	85.46885	756	1092
Born 1914	.25661	.43676	0	1
Born 1915	.24954	.43275	0	1
Born 1916	.25047	.43328	0	1
Born 1917	.24339	.42913	0	1
White	.95715	.20251	0	1
Black	.03857	.19256	0	1
Other	.00428	.06529	0	1
Birth Year	1915.4806	1.11786	1914	1917
Birth Month	6.47308	3.43579	1	12
Death Year	1992.8698	7.11421	1979	2005
Death Month	6.43188	3.53482	1	12
Father's SEI 1^{st} Quartile	.11064	.31369	0	1
Father's SEI 2^{nd} Quartile	.10079	.30105	0	1
Father's SEI 3^{rd} Quartile	.10241	.30319	0	1
Father's SEI 4^{th} Quartile	.09759	.29676	0	1
Father's SEI Missing	.63533	.48134	0	1
Mother Education < HS	.36446	.48128	0	1
Mother Education = HS	.08148	.27358	0	1
Mother Education > HS	.01769	.13181	0	1
Mother Education Missing	.53637	.49868	0	1
Observations		487,087		

Table 2 - 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)			-1.06547**
			(.39256)
Later Cohort	-2.91147***	-1.74219***	-1.786***
	(.48778)	(.46011)	(.46827)
Born 1916-1917			-17.12459***
			(.35422)
Observations	240533	246520	487084
R-squared	.01749	.01591	.02046
Mean DV	919.936	937.110	928.629
	Born 1916-1917	Born 1912-1913	Column (4) – Column (5)
	(4)	(5)	(6)
Born 1917 (DD)			-.80837
			(.46981)
Later Cohort	-2.91147***	-1.91609***	-2.00272***
	(.48778)	(.4378)	(.42946)
Born 1916-1917			-33.34367***
			(.26867)
Observations	240533	240260	480818
R-squared	.01749	.01616	.04691
Mean DV	919.936	952.962	936.438
	Born 1914-1915	Born 1912-1913	Column (7) – Column (8)
	(7)	(8)	(9)
Born 1915 (DD)			.20239
			(.38938)
Later Cohort	-1.74219***	-1.91609***	-1.93363***
	(.46011)	(.4378)	(.44975)
Born 1914-1915			16.07228***
			(.20944)
Observations	246520	240260	486799
R-squared	.01591	.01616	.01857
Mean DV	937.110	952.962	944.934

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 3 - 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)	.40312	.0152	-.39675	-1.06547**
	(.52159)	(.37435)	(.30693)	(.39256)
Later Cohort	-1.45667***	-6.45157***	-6.48559***	-1.786***
	(.32938)	(.19919)	(.20372)	(.46827)
Born 1916-1917	-12.90723***	-12.84791***	-23.00682***	-17.12459***
	(.55335)	(.25441)	(.25878)	(.35422)
Observations	43180	175066	188722	487084
R-squared	.09323	.05945	.10997	.02046
Mean DV	843.798	844.065	838.611	928.629

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 4 - 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)	-.05859***	.02073***	.00633***	.00593***	-.00417	.00151
	(.01068)	(.00732)	(.0007)	(.00068)	(.00399)	(.00102)
Later Cohort	.11917***	.08137***	.00637***	.00667***	.0556***	-.01733***
	(.0079)	(.00529)	(.0005)	(.00049)	(.00299)	(.00077)
Born 1916-1917	.25206***	.17758***	.01315***	.01341***	.06796***	-.02884***
	(.00772)	(.00523)	(.0005)	(.00048)	(.00293)	(.00075)
Observations	3100711	3100711	3100711	3100711	3099547	3100711
R-squared	.01707	.01794	.01195	.01299	.02221	.01413
Mean DV	4.287	1.248	0.107	0.101	10.067	0.275
Panel B. Age 80-86						
Born 1917 (DD)	-.06474***	.01417	.00736***	.00629***	-.03764***	.00381**
	(.01967)	(.00999)	(.00099)	(.00094)	(.0083)	(.00182)
Later Cohort	.10712***	.0444***	.00279***	.00282***	.08592***	-.01978***
	(.01483)	(.00731)	(.00073)	(.00069)	(.00632)	(.00139)
Born 1916-1917	.21073***	.09112***	.00444***	.00555***	.07256***	-.02923***
	(.0144)	(.00721)	(.00071)	(.00067)	(.00625)	(.00135)
Observations	967713	967713	967713	967713	967324	967713
R-squared	.01242	.00291	.00244	.00245	.01654	.00754
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

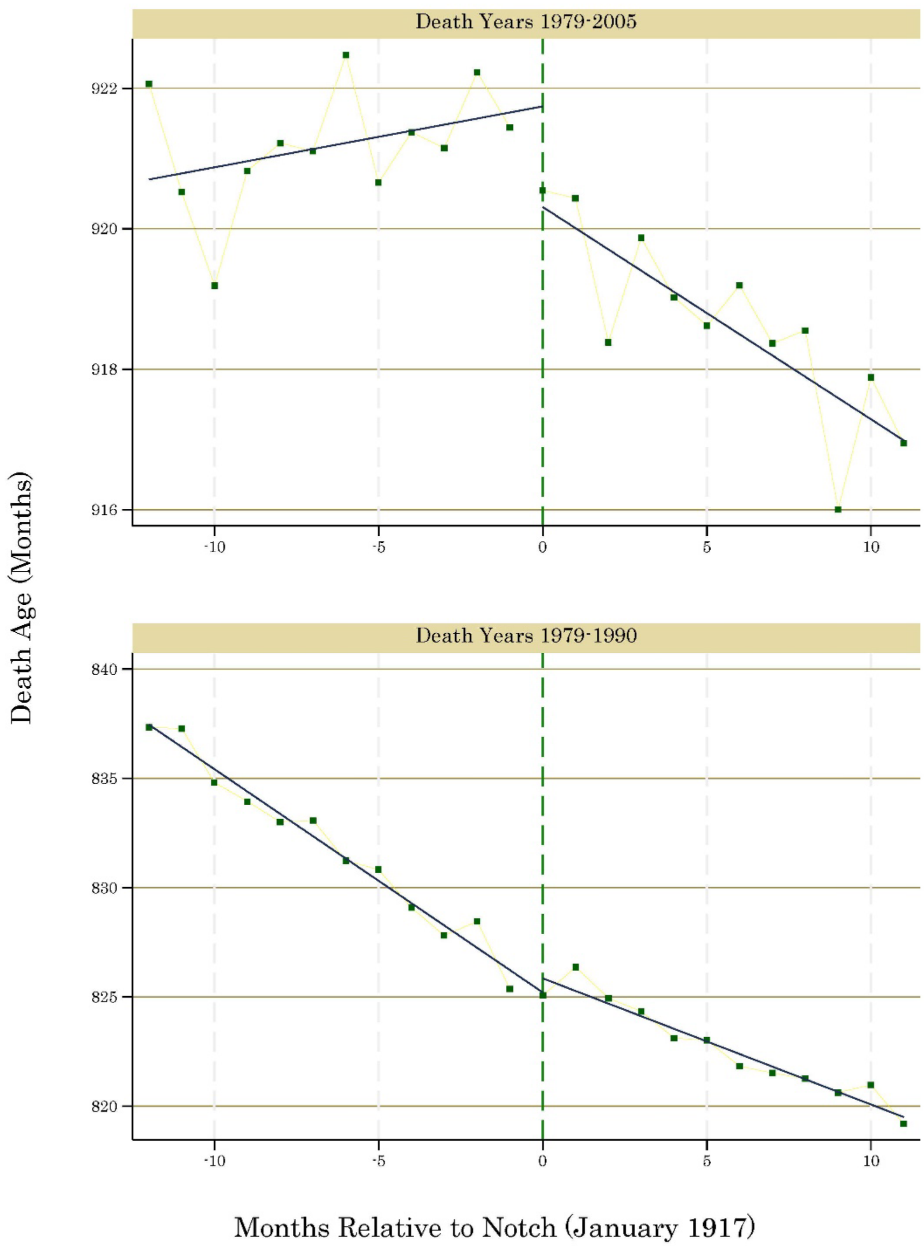

Figure 1 - Regression Discontinuity Graph

Appendix A

Appendix Table A-1 - Regression Discontinuity Estimates

	Outcome: Age-at-Death (Months)		
	1916-1917 Cohorts	1914-1915 Cohorts (Placebo 1915 Notch)	Column (1) – Column (2)
	(1)	(2)	(3)
Discontinuity at Notch	-1.04365**	.63283***	-1.67863**
	(.42048)	(.09196)	(.47066)
Observations	240552	246535	496944

Notes. Robust standard errors are in parentheses. All regressions include a linear trend in birth date. All regressions also include individual and family controls (dummies for race and ethnicity, dummies for maternal education, dummies for paternal socioeconomic index).
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table A-2 - 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)	-2.47126**	-1.38997	-1.4016*	-.27573	-1.02911**	-1.64757
	(.98103)	(1.10162)	(.76218)	(.87534)	(.43953)	(2.72817)
Later Cohort	-.52887	-.72529	-1.92304**	-2.02376***	-1.83017***	-1.21138
	(.88591)	(.80549)	(.6398)	(.53462)	(.46379)	(1.98059)
Born 1916-1917	-15.37115***	-15.33685***	-17.02886***	-17.46541***	-17.03545***	-19.28434***
	(.7068)	(.92798)	(.52868)	(.45408)	(.33728)	(1.75466)
Observations	102915	97090	269478	209706	466214	18459
R-squared	.04404	.03704	.02581	.02666	.02002	.08133
Mean DV	928.509	929.948	922.942	935.904	929.085	916.547

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-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 (DD)	-1.09082**	-1.09865**	-.0012**	-.00447*	-.00649**	-1.06755**	-1.09082**
	(.38269)	(.38232)	(.00042)	(.0021)	(.00258)	(.48709)	(.49394)
Later Cohort	-1.783***	-1.77869***	-.0018***	-.00349*	-.00743**	-1.78687***	-1.783***
	(.48351)	(.48457)	(.0005)	(.0018)	(.00251)	(.34267)	(.34727)
Born 1916-1917	-17.13151***	-17.13338***	-.01869***	-.05205***	-.05464***	-17.20313***	-17.13151***
	(.35936)	(.3577)	(.00039)	(.0018)	(.00231)	(.34384)	(.35115)
Observations	487084	487084	487084	487084	487084	9141310	487084
R-squared	.02103	.02111	.02104	.01276	.01334	---	.02103
Mean DV	929.141	929.141	6.830	0.573	0.357	929.141	929.141

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	-.00005	-.00054	-.21954	-.02465***	.02193	-.02454***	-.03383
(DD)	(.00122)	(.0012)	(.12483)	(.00302)	(.0275)	(.00372)	(.01898)
Later Cohort	-.00064	.00073	.42336***	-.05705***	.06835***	-.06182***	.08186***
	(.00065)	(.00056)	(.12967)	(.00201)	(.02)	(.00217)	(.01493)
Born 1916-	-.00128*	.00116*	.48146***	-.12347***	.1468***	-.13383***	.10205***
1917	(.00064)	(.00058)	(.07739)	(.00205)	(.02143)	(.00236)	(.01253)
Observations	487084	487084	177580	487084	225799	487084	479242
R-squared	.18208	.19681	.09298	.05829	.10658	.07185	.13888
Mean DV	0.957	0.039	27.619	0.635	6.906	0.536	10.275

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)			.00007 (.00009)
Later Cohort	-.00028*** (.00006)	-.00031*** (.00006)	-.00033*** (.00006)
Born 1916-1917			-.00053*** (.00006)
Observations	4573298	4568012	9141310
R-squared	.91634	.91481	.91554
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