Early-Life Income Shocks and Old-Age Mortality: Evidence from World War I Veterans' Bonus[*]

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Abstract

In 1936, the US government enacted the later-known Bonus Act, which triggered cash transfers to about 3 million veterans who had served in World War I. The large and unexpected nature of transfers provides an opportunity to examine the impact of family income shocks on children's long-term outcomes. This paper studies the long-run benefits of veterans' bonus receipt on their children's old-age longevity. We employ data from Social Security Administration death records over the years 1975-2005 linked to the full-count 1940 census and implement regressions that compare the longevity of children of veterans versus non-veterans across various ages of exposure to the bonus receipt. We find that those exposed during in-utero and early-life reveal significant improvements in longevity of about 5.6-7.5 months. Our balancing tests fail to provide concerning evidence regarding the endogenous dynamic differences in individual and family characteristics based on veteran-status that vary across cohorts. Further analyses suggest stronger effects among children of low-educated mothers and those with low socioeconomic index fathers. We also show that increases in house values, rise in homeownership, and potential improvements in neighborhood characteristics are candidate mechanisms of impact.

Keywords: Mortality, Longevity, Socioeconomic Status, Income, Cash Transfers, Historical Data

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1. Introduction

The average life expectancy in the US increased substantially over the past century, from about 47 years in 1900 to roughly 77 years in 2000 (Smith & Bradshaw, 2006). However, life expectancy in the US falls below the majority of developed countries, such as members of the Organization for Economic Cooperation and Development (OECD) (Avendano & Kawachi, 2014). Moreover, studies that project future life expectancies in cross-country analyses suggest that the US has one of the lowest projected longevity improvements among its peer countries (Kontis et al., 2017). The longevity disadvantage of the US could be a mirror of life-cycle events, as several studies point to the long-run mortality effects of life-cycle exposures (Almond et al., 2018; Van Den Berg et al., 2015; Gagnon & Mazan, 2009; Goodman-Bacon, 2021; Hayward & Gorman, 2004; Montez & Hayward, 2011; Myrskylä et al., 2013; Schellekens & van Poppel, 2016; Van Den Berg et al., 2006). This literature suggests that, in addition to contemporaneous factors, gains in longevity can also be attained by policies that aim to improve conditions during childhood and early-life.

In 1936, the US Congress enacted the Adjusted Compensation Payment Act, also known as the Bonus Act, which triggered the disbursement of US treasury bonds to about 3 million veterans who had served in World War I (WWI). The payment was part of the federal government plans to support the WWI veterans, which was initiated in 1924 as an insurance policy payable to each veteran based on the time served and adjusted slightly by age (Dickson et al., 2020). However, the bonus was promised to be delivered in 1945 and hence received the infamous title of *tombstone bonus.* The legislative action of the 1936 Bonus Act resulted in a one-time unanticipated and relatively large income shock to veterans' families. The treasury bonds were cashable as early as June 1936 and were equivalent to the average per capita income in 1936 (Quincy, 2022). Veterans immediately cashed about half of their bonus bonds (Telser, 2003). Household consumption surveys suggest that veterans spent a large portion of their bonus (Hausman et al., 2016). Evidence shows that the payment increased veterans' home values and homeownership rates (Quincy, 2022). The unanticipated and large income shock from veterans' bonus can affect a wide array of household aspects, specifically infants and children, which could influence the trajectory of their health capital and be detected in their old-age health and longevity. Even though the bonus provides a clean experiment to analyze the later-life health impacts of temporary cash transfers, virtually no study has touched on this aspect of the bonus act specifically for later-life mortality and longevity. This paper aims to fill this gap in the literature.

We employ death records data from Social Security Administration (SSA) Death Master Files (DMF) linked to the full-count 1940 census. We use cross-census linkage techniques to link fathers in 1940 to their census records in 1930 to exploit the WWI veteran information reported in the 1930 census. Therefore, our final sample covers information on fathers in 1930 and 1940 as well as information on their children's death in DMF files. This dataset is unique in two ways. First, it has a longitudinal aspect that surpasses several decades, much longer than available longitudinal studies in the US. This aspect of data is necessary to examine long-term effects and specifically for exploring childhood exposures and old-age mortality outcomes. Second, it has hundreds of thousands of observations which significantly adds power to our statistical tests. Our empirical strategy compares the longevity of children who were exposed to the bonus package receipt at different ages among veterans versus non-veterans. We find sizeable and significant effects on the longevity of those who were exposed to the bonus payment in-utero and their first year of life. For example, comparing children of veterans versus non-veterans and across ages of exposure, those who were born in 1936 enjoy 7.5 months of additional longevity.

We implement a series of balancing tests to examine whether there is a significant sociodemographic difference across ages of children among veterans versus non-veterans that confound the estimated effects. We do not find any statistically significant across-age and across-veteran-status endogenous differences in the share of whites, blacks, other races, low father education, low mother education, and various quartiles of paternal socioeconomic scores. We carry out a wide range of balancing tests to show the robustness of the results to an extensive set of additional fixed effects and controls, alternative functional forms, and alternative methods of correcting standard errors. Further analyses suggest that the effects are primarily confined among white individuals. In addition, we find larger effects among those raised in smaller families, those with low-educated mothers, and children with low socioeconomic index fathers.

Moreover, to show that bonus receipt improved households' economic situation, we use the cross-census longitudinal aspect of our data and focus on fathers in the 1930 and 1940 censuses. We show that veteran fathers (versus non-veteran fathers) in 1940 (versus 1930) are more likely to be a homeowner. The results suggest that their housing wealth increases by about 12 percent.

This paper makes two important contributions to the literature. First, this is the first study to explore the long-run health impacts of veterans' bonus. Second, this study adds to our understanding of the relevance of economic conditions in early-life to the aging process. In the case of the US, a few studies explore the effects of local area economic conditions or family socioeconomic status on later-life mortality (Atherwood, 2022; Cutler et al., 2007; Hayward & Gorman, 2004; Modin, 2002; Noghanibehambari et al., 2022). Nonetheless, none of these studies have access to a direct measure of income or shock to income and usually rely on intent-to-treat effects. Contrary to these studies, the nature of bonus payment provides an unanticipated and relatively large impact on income of all veterans. Therefore, we have a relatively precise shock to income on the observed treated population.

The rest of the paper is organized as follows. Section 2 provides a brief literature review. Section 3 introduces data sources. Section 4 discusses the econometric method. Section 5 overviews the results. Finally, we conclude the paper in section 6.

2. Literature Review

Economic conditions during in-utero and early-life can change the trajectory of health and human capital accumulation and influence outcomes throughout the life cycle (Almond et al., 2018; Currie, 2009). A strand of literature provides evidence of the relevance of prenatal development and provides pathways through which cash transfers and income shocks may affect infants' health outcomes (Aizer & Currie, 2014; Almond & Currie, 2011b; Bozzoli & Quintana-Domeque, 2014; Brownell et al., 2016; Lindo, 2011; Noghanibehambari & Salari, 2020; Stearns, 2015). For instance, Hoynes et al. (2015) explore the impact of permeant income shocks due to changes in tax rebates and the policies under the Earned Income Tax Credit (EITC) program on birth outcomes. They find that for each additional $1,000 (in 2009 dollars), the probability of low birth weight drops by 2-3 percentage-points. Almond et al. (2011) explore the effects of the introduction of the Food Stamp program as a part of the anti-poverty policies of the 1960s on birth outcomes. They find that, among participants, birth weight increases by about 15-40 grams. Amarante et al. (2016) employ data from Uruguay and exploit policy changes in the social assistance programs that initiated cash transfers to low-income families. They show that exposed pregnant mothers increase their nutrition intake during prenatal development. Their findings suggest sizable reductions in the incidence of low birth weight among treated mothers. Mocan et al. (2015) examine the effects of maternal income and job earnings on birth outcomes using US birth records. They use Current Population Survey data to obtain women's earnings data. They use Bartik instruments and implement a two-sample instrumental variable strategy. They find that doubling mothers' earnings is associated with roughly 100 grams of additional birth weight. They also find positive impacts of income on utilization of prenatal care among low-educated mothers. Clark et al. (2021) use data from the UK and show that a self-reported economic shock during the first half of pregnancy is associated with reductions in birth weight of about 40-70 grams. De Cao et al. (2022) use data from England and find that infants' health outcomes are procyclical and follow the pattern of economic fluctuations in their local areas. However, Kyriopoulos et al. (2019) employ birth records data from Greek and find similar pro-cyclicality in birth outcomes.

These effects on infants' health can be translated into later-life human capital development, labor market outcomes, and health in adulthood (Behrman & Rosenzweig, 2004; Maruyama & Heinesen, 2020; Royer, 2009). For instance, Black et al. (2007) employ data from Norway and explore the effect of birth weight on adult outcomes. They implement family fixed-effect and twin fixed-effect models to account for unobserved heterogeneity in birth outcomes. They find sizeable and significant effects on high school completion, IQ, Body Mass Index (BMI), height, employment, and earnings. Almond et al. (2005) employ twin fixed effects and show that low birth weight is associated with deterioration in postnatal health and increases in infant mortality rates.

Household and local economic shocks can also affect childhood health and human capital, which in turn change the trajectory of outcomes during adulthood (Adhvaryu et al., 2019; Almond et al., 2018; S. E. Black et al., 2016; Currie, 2009; Currie & Rossin-Slater, 2015; Glick et al., 2016; Reinhold & Jürges, 2012). Hoynes et al. (2016) explore the effects of childhood exposure to the introduction of the Food Stamp program on adult outcomes. They find sizeable reductions in metabolic syndrome, decreases in blood pressure, and increases in height. Braga et al. (2020)

explore the effects of childhood exposure to tax rebates under the EITC program on adult outcomes. They find that exposure to higher family income due to higher tax credits increases self-reported health and decreases obesity in adulthood. East (2018) exploits immigrants' Food Stamp eligibility changes and shows that the program's eligibility before age five improves health status and developmental health index among children between ages 6-16.

A growing strand of literature explores the early-life and childhood origins of life-cycle outcomes, specifically old-age mortality (Almond & Currie, 2011; Barker, 1990, 1994; Case & Paxson, 2009; Currie & Rossin-Slater, 2015; Gagnon & Mazan, 2009; Ko & Yeung, 2019; Lazuka, 2019; Lee & Ryff, 2019; Lindeboom et al., 2010; Myrskylä, 2010; Sotomayor, 2013). Banerjee et al. (2010) explore the effects of income shocks to household resources during early-life on adult height and longevity. They exploit the phylloxera pandemic of late nineteenth-century France, which destroyed a large portion of vineyards. They find that those born in regions and years affected by the shock have lower heights during adulthood. However, they do not find any discernable effects on life expectancy. Van Den Berg et al. (2006) exploit fluctuations of business cycles at birth as an aggregate measure of economic conditions to explore the long-term associations with mortality. They find that, after controlling for contemporaneous measures of economic conditions, economic conditions at birth are significantly associated with mortality risks at all ages. Noghanibehambari et al. (2022) ask a similar question and proxy local labor market conditions with county-level fluctuations in bank deposits during the Great Depression. They show that these fluctutations are strongly correlated with other measures of economic conditions and that negative shocks to deposits at birth are associated with sizeable reductions in old-age longevity. Maccini & Yang (2009) use data from Indonesia and explore the impacts of rainfall shocks on long-term outcomes. They posit that rainfall shocks are translated into economic shocks specifically for rural households. They find significant associations between rainfall shocks at birth and adult height and schooling. In a similar study, Yamashita & Trinh (2022) document that in-utero rainfall shocks are associated with changes in cognitive development in ages 5 to 15. Carrillo (2020) uses data from Colombia and documents an association between early-life rainfall shocks and adult mental health and schooling.

Arthi (2018) explores the effects of exposure to the American Dust Bowl, an environmental catastrophe with large effects on agricultural income and revenue, on later-life outcomes and finds negative impacts on disability rates. On the contrary, Cutler et al. (2007) find null effects on disability rates of Dust Bowl exposed cohorts. In a similar study, Atherwood (2022) explores the childhood exposure to Dust Bowl on old-age longevity and fails to find significant effects on the longevity of male individuals. However, Noghanibehambari & Fletcher (2022) find significant longevity effects for those exposed during in-utero. In addition, they show that these effects are primarily driven by reductions in the longevity of females.

3. Data Sources and Sample Selection

The primary source of data for this paper is death records of Social Security Administration (SSA) Death Master Files (DMF) extracted from Censoc Project (Goldstein et al., 2021). The DMF data covers deaths of male individuals over the years 1975-2005. The advantage of the DMF data is that it is linkable to the full-count 1940 census at the individual level. Therefore, for a subset of these cohorts, we have information on parents and place of residence in 1940. The automatic linkage technique between DMF death records and the 1940 census is primarily based on name commonality, age, and place of birth. Therefore, there is little concern about endogeneity in linking as a response to specific economic shocks in their childhood.

While the 1940 census offers a wide range of individual and family covariates, its information on father veteran status is limited and does not expand to all WWI veterans. On the other end, the 1930 census provides detailed information on veteran status, whether the individual was drafted for WWI, and in limited cases, pre-WWI battles veterans participated. To infer the veteran status of fathers in 1940 based on the full-count 1930 records, we use cross-census linking methods provided by Census Linking Project (Abramitzky et al., 2020). The linking of records provides a match rate of about 23 percent. Moreover, we focus on individuals aged 20 and less (born 1920-1940) as they move out of their original households after this age, and the characteristics of non-movers are likely systematically different from others. We also remove those observations for which fathers' information is missing. Quincy (2022) shows that most WWI draftees were white men born between 1892 and 1898. Therefore, we restrict the sample to fathers born between 1890 and 1900.

Summary statistics of the final sample are reported in Table 1 for the subsample of individuals with veteran fathers and those with non-veteran fathers in the left and right panels, respectively. On average, children of non-veteran fathers live about 2.5 months longer lives. In both samples, whites are over-represented, and blacks are under-represented. This is also true in the original DMF data. However, each subgroup represents its respective subpopulation in terms of sociodemographic features (Breen & Osborne, 2022). Age at exposure variables are the primary independent variables of interest and are calculated as 1936 minus the child's birth year. We build dummies to indicate various levels of age at exposure. A value of -4 refers to cohorts of 1940, and a value of 10 points to cohorts of 1926. The distribution of birth cohorts across years is fairly similar in both veteran and non-veteran subsamples, as implied by mean and standard deviations of age at exposure variables. The average father's age is also quite comparable in both subsamples.

However, there are more low-educated fathers and low-educated mothers in the non-veteran subsample. Similarly, house values in 1930 and father's socioeconomic rank in 1930 are higher among veterans.

4. Econometric Method

The econometric method we implement compares the longevity of children who were exposed to the bonus payment at different ages in veteran families versus non-veteran families. Specifically, we employ regressions of the following forms using ordinary least square estimations:

$$DA_{ib} = \alpha_0 + \alpha_2 Veteran_i \times ExposureAge_b + \alpha_3 X_{ib} + \varepsilon_{ib} \tag{1}$$

Where the outcome is age at death of individual i in birth cohort b. Matrix X includes a series of controls and fixed effects to control for potential confounders and account for differences across cohorts and veteran versus non-veteran families. It includes as individual covariates dummies for race, ethnicity, and gender. It also includes father's socioeconomic index dummies, father's education dummies, and maternal education dummies. Moreover, we include father-age-by-veteran-status dummies to account for cross-cohort differences among veteran and non-veteran fathers. We include father-age-by-birth-year dummies to control for differences in father's characteristics across different cohorts that are driven by differences in fathers' age. Finally, we include county-by-birth-year fixed effects to control for all unobserved time-varying county characteristics that may influence the health outcomes of children and appear in long-run longevity outcomes. ε is a disturbance term. We use heteroscedastic robust standard errors.

As discussed in section 3, the DMF-census-linked sample contains different sociodemographic characteristics than the original 1940 population. To address this issue, we apply a weighting scheme that assigns higher values to underrepresented subpopulations and vice versa. In so doing, we treat the sample as longitudinal data with attrition issues and employ the inverse probability weighting method (Hajat et al., 2011; Halpern-Manners et al., 2020; Weuve et al., 2012). Specifically, we start with the full-count 1940 census and impose sample selections discussed in section 3. We then link this selected 1940 original sample with our final sample. Next, we generate a new variable that indicates successful merging between these two datasets. We then regress the successful merging indicator on a series of individual and family controls using probit regressions. We then use the inverse of the predicted value of this regression as weighting scheme in our regressions.

5. Results

5.1. Balancing Tests

Although The Selective Service Act of 1917 made inscription to the Army mandatory, the selection criteria could potentially lead to systematic differences in veterans versus non-veterans in observable characteristics such as physical features or unobservable characteristics. Although we implement an extensive set of fixed effects to control for cohort and veteran differences derived from differences in fathers' cohort, we still expect differences based on unobservables. These systematic veteran-versus-non-veteran differences could bias the estimates of equation 1 if they induce changes in cohort characteristics and such changes vary across cohorts. For instance, if the veteran-versus-non-veteran difference in the share of whites is higher among those with age-at-exposure of zero and one, and this difference varies by the level of age-at-exposure, then the estimates reveal the cross-cohort changes in the observed increases in the share of whites rather than the true effects of the bonus transfer. We explore this potential source of endogeneity by using a series of individual and family characteristics as the outcome variables and implement regressions that control for all other fixed effects introduced in equation 1. The results are reported in Table 2. The estimated coefficients do not provide any significant effects of various exposure ages among children of veteran fathers on several observable individual outcomes such as white, black, and other races. The main balancing test is the F statistics of equality of the interaction terms of exposure zero to exposure 10. We avoid including coefficients of exposure of -1 through -4 as they partly reflect potential endogenous fertility.

The F-statistics and their corresponding p-values are reported in the last two rows of the table. The p-values fail to reject the equality of all the respective interaction coefficients. We observe a similar pattern for fathers' education less than 12 years of schooling, fathers' education missing, fathers' education missing, and fathers' socioeconomic score. Although we observe several statistically significant effects on maternal education and fathers' socioeconomic score, the tests of equality of coefficients reveal insignificant statistics. The only significant test in the table is regarding the equality of coefficients of fathers' socioeconomic status missing (p-value=0.08). Overall, conditional on the implemented fixed effects, we fail to observe significant changes in the difference of veteran-versus-non-veteran at different exposure ages. Therefore, the cross-cohort sociodemographic changes are fairly similar across control and treated groups.

Another concern in interpreting post-bonus-payment coefficients (i.e., age-at-exposure of -4 to -1) is households' potential endogenous fertility decisions. There is evidence that income shocks may affect the future fertility of households, although the literature on income-fertility is inconclusive (Black et al., 2013; Córdoba & Ripoll, 2016; Herzer et al., 2012). To address this concern, we directly test for changes in households' fertility choices across years as a response to the bonus receipt. Specifically, we build a series of dummies to indicate a household has a child in a specific year for several years pre-bonus and all years post-bonus up to 1940. We then regress these indicators on fathers' veteran status dummy conditional on county fixed effects and all other parental covariates in equation 1. The results are reported in Table 3. There is no statistically significant pattern of pre-bonus and post-bonus change in fertility. For instance, for the year 1937, veterans are 5.3 basis-points more likely to have a child compared with non-veterans, equivalent to roughly 1.3 percent change from the mean of the outcome. These results do not offer consistent and discernible evidence for selective fertility issues.

5.2. Main Results

The main results of the paper are reported in Table 3. The first column includes only fixed effects. The second column adds individual covariates, and the third adds family controls. The estimated marginal effects are quite comparable across specifications. Children of veteran fathers born in 1937 and 1936 (i.e., age-at-exposure -1 and 0) live 5.6 and 7.5 months longer lives. We do not find significant impacts across postnatal ages. Although all coefficients are positive, they suggest economically small change and statistically insignificant impacts. The only exception is the coefficient of exposure-at-age 4, which suggest a significant effect of about 4.2 months.

Furthermore, we also do not find significant effects across ages -2 through -4, for those born two years (and more) after the treatment. Although their estimated sizes are relatively larger than those of postnatal ages, we cannot rely on these coefficients as they may reflect the selective fertility of parents. In addition, since we do not have the exact receipt and spending dates, we cannot assign treatment based on in-utero periods. However, the fact that the effects are primarily concentrated among coefficients of year-of-birth and those born a year later suggests that the longevity improvements are driven by in-utero impacts and improvements in prenatal conditions.

Since we include cohorts born between 1920-1940, the reference cohorts are those born between 1920-1925. One concern is that since we observe death records for a limited window (1975-2005), the cross-cohort comparison may mirror longevity differences of older versus younger cohorts. We should note that including cohort fixed effects enable within-cohort comparison and rule out this concern. In addition, the longevity of the 1920-1925 cohorts (reference children) is about 7.3 years higher than the 1926-1940 cohorts. Therefore, if the effects reflect cross-cohort longevity difference due to the limited death window, the effects must have revealed negative coefficients.

We can better understand the magnitude of the effects by comparing them with other studies that explore in-utero and early-life shocks on old-age longevity. For instance, Noghanibehambari et al. (2022) examine the impacts of local labor market conditions during the in-utero period on old-age longevity. They use the local concentration of bank deposits as a proxy for economic conditions. They show a significant association between income and bank deposits and find a sizeable association between in-utero deposits and old-age longevity. They find that reductions in income between the years 1929 and 1933 (the peak to trough of the Great Depression), a change in income roughly equivalent to the bonus payment, are associated with 8.3 months decrease in longevity during old age. This number is surprisingly comparable to those of Table 3. Chetty et al. (2016) explore the income-longevity relationship across income percentiles using all tax records and Social Security Administration death records over the years 1999-2014. They find that an increase of 5 percentile in income is associated with roughly 0.7-0.9 years increases in longevity. This contemporaneous difference in income is roughly the same size as the effect of bonus payment in early-life on longevity.

Halpern-Manners et al. (2020) examine the impact of education on longevity using Social Security Administration death records. They implement twin fixed-effect strategy and find that each additional year of schooling is associated with roughly 4 months. Therefore, the effects of Table 3 (around the birth-year) are equivalent to roughly 1.4-1.9 years of higher education. Fletcher & Noghanibehambari (2021) investigate the effects of college expansion during adolescence years on education and later-life longevity. Their treatment-on-treated back-of-an-envelope calculations suggest that having a college education induced by a new 4-year college opening increases longevity by about 1-1.6 years. The estimated effects of Table 3 for birth-year exposure to the bonus receipt are about 0.4-0.6 times that of college education on mortality. Overall, these comparisons reveal that the estimated early-life exposure effects of Table 3 are relatively large and economically meaningful.

5.3. Heterogeneity Analysis

In this section, we explore the heterogeneity of the results across subsamples. In columns 1 and 2 of Table 4, we replicate the main results for nonwhite and white subsamples, respectively. Veterans of WWI were disproportionately while males (Hausman et al., 2016; Quincy, 2022). This fact is also quite noticeable when we look at summary statistics of the DMF-census-linked sample of panel A of Table 1. Therefore, it is not surprising that the effects are confined to the white subsample and that all the effects on nonwhites are insignificant.

One important potential heterogeneity is regarding the family socioeconomic status. Several studies that explore the health impacts of cash transfers document larger impacts on poorer families and lower-educated parents (Barham, 2011; Chung et al., 2016; Hoynes et al., 2011; Kyriopoulos et al., 2019). Our results also suggest slightly larger impacts among children of families with low-educated mothers and low socioeconomic status fathers (columns 3-4, Table 4).

One interesting difference is the coefficients of the 1938 and 1937 cohorts (age-at-exposure -2 and -1) in column 4. The marginal effects are roughly three times those of the main results. They suggest substantially larger impacts for those who were probably in-utero during the bonus receipt and spending among infants with low socioeconomic index fathers.

Finally, studies suggest that the effects of shocks to socioeconomic status on later-life outcomes could be heterogeneous by sibship size as, all else equal, more resources are allocated to each child of smaller families (Baranowska-Rataj et al., 2017; Smith et al., 2009, 2014). Column 5 of Table 4 replicates the main results for the subsample of people with at most one sibling in 1940. We observe small and insignificant effects for postnatal ages and exposures before the year of birth. For 1936 cohorts, we observe relatively larger effects than the main results suggesting improvements in longevity of about 8 months.

5.4. Robustness Checks

In Table 5, we explore the robustness of the main findings across various alternative specifications. To have a benchmark comparison, we replicate the full specification of column 3 of Table 3 in the first column. We allow counties' time-invariant characteristics to have differential effects on longevity based on individual race, maternal education, and paternal socioeconomic status by adding county-by-individual-family-covariates fixed effects into the regression. The results, reported in column 2, reveal quite similar and comparable coefficients to those in column 1.

Another concern is the seasonality in birth, which could be correlated with months of bonus payments and also with longevity (Buckles & Hungerman, 2013). There is also evidence for seasonality in death and that vulnerability in specific seasons could be the result of a dynamic complementarity impact with early-life exposures. We account for these two potential confounders by adding to the full-model a series of birth-month and death-month fixed effects. The results are reported in column 7. We observe a very similar pattern across coefficients. The effect on 1936 cohorts (age-at-exposure 0) is only slightly smaller and remains statistically significant.

In column 5, we explore the sensitivity of the functional form by replacing the outcome with the log of age-at-death. The effect of age-at-exposure of -1 and 0 suggest a 0.8 and 1.1 percent increase in longevity, respectively. The coefficients of age-at-exposure of -1 and 0 in column 1 imply a 0.7 and 1 percent change from the mean of age-at-death. These effects are quite similar to the percent changes retrieved from the semi-log regression suggesting that the results are not sensitive to the functional form of the outcome. We further probe this issue by replacing the outcome with a dummy variable indicating longevity beyond 55 years. The results, reported in column 6, suggest a quite similar pattern as column 1. The effect of age-at-exposure of 0 implies an increase in the probability of living beyond 55 years by about 2.9 percentage-points, equivalent to a 12.5 percent rise from the mean of the outcome.

In the main results, we use Huber-White heteroscedastic-robust standard errors. In column 7, we use raw uncorrected standard errors. In column 8, we employ two-way robust standard errors clustered at the county and birth-year levels. While the coefficients of age-at-exposure of -1 and 0 remain statistically significant (with smaller standard errors), the effect of age-at-exposure -2 also becomes significant for two-way clustering.

Finally, while in the main results, we use inverse probability weights to make the results representative of the original population, we show the unweighted regressions in column 9. The effects suggest smaller effects for age-at-exposure of 0 and -1. However, similar to the main results, the effects are larger (and statistically significant) for in-utero and early-life exposures.

5.5. Potential Mechanisms

Cash transfers and positive income shocks can improve infants' and children's health outcomes in various ways. Transfers may increase access to materials that directly influence health outcomes, such as food security (Haeck & Lefebvre, 2016; Leete & Bania, 2010). For instance, they could lower financial distress and improve adults' mental health, which has spillovers in birth outcomes and children's cognitive development (Carney, 2021; Herring et al., 2006; Neece, 2014; Vänskä et al., 2017). Transfers may also impact early-life development and health outcomes through indirect channels. For instance, income shocks could induce moving to better neighborhoods and potentially a healthier environment (Katz et al., 2001; Raj Chetty et al., 2016). Moreover, income rises may increase access to medical care and increase prenatal doctor visits, which in turn influence birth outcomes (Carney, 2021; Hoynes et al., 2015; Noghanibehambari, 2022; Thompson, 2017).

In this section, we explore some candidate mechanisms based on available information. We use information in 1940 to examine the change in veteran fathers' economic conditions relative to 1930. As explained in section 3, to infer veteran status, we use cross-census linking techniques to link fathers in 1940 to their 1930 records. Therefore, our final sample has fathers' characteristics in 1930. For the analysis of this section, we focus on fathers and hence we need to change the structure of the data. We construct a longitudinal panel in which each record is a father (whose children are in our final sample) that is observed in 1930 and 1940. We then implement difference-in-difference equations to compare the outcomes of veterans in 1940 versus 1930, conditional on county fixed effects and covariates. We control for spousal education dummies and race/ethnicity dummies in the regressions. The outcomes that we study include house value, log house value, and a dummy indicating homeownership. The results are reported in Table 6. The main effects of year dummies suggest that, relative to 1930, house values and homeownership drop considerably likely caused by the Great Depression (Balcilar et al., 2014). The main effects of veteran dummy imply that veterans have, on average, higher house values and homeownership rates. The interaction terns suggests substantial improvements in veterans' house values and homeownerships. Relative to 1930, veterans are 7.5 percentage-points more likely to be homeowners, off a mean of 0.4. Their houses are valued about $15K (in 2020 dollars) higher, equivalent to 12 percent rise from the mean. These results suggest general improvements in wealth and well-being of veteran families. The rise in their housing consumption may also signify rises in consumption of other goods that could directly or indirectly affect health and human capital of infants and children. In addition, moving to better neighborhood could be translated into better access to health-related services as well as less polluted environment. These pathways could lead to improved health capital and be detected in old-age longevity effects (Chyn, 2018).

6. Conclusion

Cash transfers and social spending are costly, and their benefits may have spillover effects for outcomes that are not immediately observed. Evaluating their long-term effects adds to the usually unobserved benefits of the programs and leads to more optimal designs in social and public policies. This paper provided new insights into the long-run effects of early-life exposure to transfers on old-age longevity. We exploit the unexpected policy change that resulted in bonus payments to veterans who had served in WWI. The bonus was a one-time payment to veterans in 1936 and was roughly equivalent to 1936 per capita income. We show positive effects on the old-age longevity of children of veterans. Our results suggest that infants who were likely in-utero or in the first year of life benefited most. The effect sizes point to improvements of about 5.6 and 7.5 months additional months of life for 1937 and 1936 cohorts of children of veteran fathers, respectively. However, while the effects are positive across various ages of postnatal and pre-prenatal exposure, they are mostly small in magnitude and statistically insignificant.

We implement a series of balancing tests to explore the potential endogeneity caused by cross-cohort and cross-veteran-status changes in the share of individuals with different sociodemographic characteristics. Our empirical tests fail to provide concerning evidence regarding the endogenous dynamic difference in characteristics based on veteran-status that vary across cohorts. We implement a battery of sensitivity analyses and show that the results are robust to adding an extensive set of additional fixed effects and controls. We also show the robustness of the results to functional form and alternative standard error correction techniques. Furthermore, we implement heterogeneity analyses and find slightly larger impacts on people with low-educated mothers and low socioeconomic status fathers. Finally, we provide evidence that housing values of veteran fathers reveal substantial and significant improvements from 1930 to 1940 versus non-veterans. We argue that these improvements in housing and possibly neighborhood conditions could lead to better health outcomes through various channels that could also be detected in old-age mortality outcomes.

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Tables

Table 1 - Summary Statistics

	Veterans			Non-Veterans		
	Observations	Mean	SD	Observations	Mean	SD
Death Age (Months)	124757	812.365	109.399	264351	814.9	109.519
White	124757	.971	.168	264351	.964	.186
Black	124757	.028	.164	264351	.033	.179
Other	124757	.001	.038	264351	.003	.053
Birth Year	124757	1926.299	4.551	264351	1926.029	4.618
Death Year	124757	1993.986	8.358	264351	1993.922	8.328
Age at Exposure: -4	124757	.008	.088	264351	.008	.087
Age at Exposure: -3	124757	.01	.099	264351	.009	.096
Age at Exposure: -2	124757	.012	.11	264351	.012	.107
Age at Exposure: -1	124757	.031	.173	264351	.029	.169
Age at Exposure: 0	124757	.022	.148	264351	.022	.146
Age at Exposure: 1	124757	.027	.161	264351	.025	.156
Age at Exposure: 2	124757	.033	.178	264351	.032	.175
Age at Exposure: 3	124757	.037	.189	264351	.036	.187
Age at Exposure: 4	124757	.047	.212	264351	.045	.208
Age at Exposure: 5	124757	.054	.226	264351	.05	.218
Age at Exposure: 6	124757	.063	.242	264351	.058	.235
Age at Exposure: 7	124757	.072	.258	264351	.068	.252
Age at Exposure: 8	124757	.079	.27	264351	.074	.262
Age at Exposure: 9	124757	.085	.279	264351	.082	.274
Age at Exposure: 10	124757	.092	.289	264351	.088	.283
Father Age	124757	44.783	2.613	264351	44.804	3.249
House Value in 1940	32011	70625.611	801737.31	55042	58575.708	68187.5
House Value in 1930	32011	94741.636	646413.8	55042	87245.926	220109.94
House Owner in 1940	123864	.542	.498	262585	.506	.5
House Owner in 1930	123864	.447	.497	262585	.448	.497
Father Education<12	124757	.866	.341	264351	.918	.274
Father Education Missing	124757	.019	.137	264351	.021	.142
Mother Education<12	124757	.905	.293	264351	.95	.218
Mother Education Missing	124757	.04	.196	264351	.044	.205
Father's 1930 SEI Score	120311	33.366	22.338	252512	29.584	20.471
Father's 1930 SEI Score Missing	124757	.036	.185	264351	.045	.207

Notes. Dollar values are converted into 2020 dollars.

	Observations	Mean	SD	Observations	Mean	SD
Death Age (Months)	132902	812.278	109.421	316070	813.471	109.682
White	132902	.972	.166	316070	.954	.209
Black	132902	.027	.162	316070	.043	.202
Other	132902	.001	.039	316070	.003	.056
Birth Year	132902	1926.312	4.559	316070	1926.12	4.656
Death Year	132902	1993.993	8.36	316070	1993.894	8.334
Age at Exposure: -4	132902	.008	.089	316070	.008	.089
Age at Exposure: -3	132902	.01	.1	316070	.01	.099
Age at Exposure: -2	132902	.012	.11	316070	.012	.11
Age at Exposure: -1	132902	.031	.173	316070	.031	.173
Age at Exposure: 0	132902	.023	.149	316070	.023	.149
Age at Exposure: 1	132902	.027	.161	316070	.026	.159
Age at Exposure: 2	132902	.033	.178	316070	.032	.176
Age at Exposure: 3	132902	.037	.19	316070	.037	.189
Age at Exposure: 4	132902	.047	.211	316070	.046	.209
Age at Exposure: 5	132902	.054	.226	316070	.051	.219
Age at Exposure: 6	132902	.063	.242	316070	.059	.236
Age at Exposure: 7	132902	.072	.258	316070	.068	.252
Age at Exposure: 8	132902	.079	.27	316070	.074	.261
Age at Exposure: 9	132902	.085	.28	316070	.081	.273
Age at Exposure: 10	132902	.092	.289	316070	.087	.282
Father Age	132902	44.785	2.623	316070	44.769	3.252
House Value in 1940	34856	73561.967	769035.57	64383	54883.634	65027.124
House Value in 1930	34856	99075.208	635595.6	64383	81764.272	208120.36
House Owner in 1940	131707	.545	.498	313776	.493	.5
House Owner in 1930	131707	.451	.498	313776	.43	.495
Father Education<12	132902	.849	.358	316070	.926	.262
Father Education Missing	132902	.019	.137	316070	.02	.141
Mother Education<12	132902	.897	.304	316070	.955	.206
Mother Education Missing	132902	.04	.196	316070	.045	.208
Father’s 1930 SEI Score 1st Quartile	132902	.308	.462	316070	.459	.498
Father’s 1930 SEI Score 2nd Quartile	132902	.121	.326	316070	.125	.331
Father’s 1930 SEI Score 3rd Quartile	132902	.203	.402	316070	.174	.379
Father’s 1930 SEI Score 4th Quartile	132902	.324	.468	316070	.205	.404
Father’s 1930 SEI Score Missing	132902	.044	.204	316070	.037	.19

Notes. Dollar values are converted into 2020 dollars.

Table 2 - Balancing Tests

	Outcomes:								
	White	Black	Other Races	Father Education<12	Father Education Missing	Mother Education<12	Mother Education Missing	Father SEI Score	Father SEI Score Missing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Father Veteran × Age at Exposure=-4	.0194	-.0159	-.0035	-.02306	.04045*	.00375	.04428*	-1.77524	.05679**
	(.02049)	(.02029)	(.00241)	(.02446)	(.0235)	(.01277)	(.0232)	(1.16582)	(.02212)
Father Veteran × Age at Exposure=-3	-.00243	.00596	-.00354	.03697**	-.02431	.01938*	-.00818	-.70156	.01558
	(.01235)	(.01195)	(.00308)	(.01832)	(.01557)	(.01128)	(.01774)	(.94283)	(.01463)
Father Veteran × Age at Exposure=-2	.00411	-.00876	.00465	-.00645	.00867	-.00992	.02262	-.36544	.01055
	(.00961)	(.00907)	(.00337)	(.01798)	(.0154)	(.01088)	(.01532)	(.94197)	(.01013)
Father Veteran × Age at Exposure=-1	.00454	-.00188	-.00266*	-.00654	-.00639	-.01635**	-.00506	.85267	.01615***
	(.00598)	(.00578)	(.00157)	(.01059)	(.0087)	(.0068)	(.00829)	(.61779)	(.0055)
Father Veteran × Age at Exposure=0	-.00191	.00339	-.00148	.00493	-.00555	.00226	.00599	.4695	.01273*
	(.00827)	(.00788)	(.00254)	(.01357)	(.01237)	(.00715)	(.01158)	(.70609)	(.00662)
Father Veteran × Age at Exposure=1	-.00331	.00385	-.00054	.02021	-.0156	-.00862	-.01521	-.45079	.01292**
	(.00726)	(.00694)	(.00241)	(.01324)	(.01232)	(.00672)	(.01163)	(.65335)	(.00635)
Father Veteran × Age at Exposure=2	-.00274	.00374	-.001	-.00399	.00844	-.00252	-.00523	.5737	.01684***
	(.00644)	(.00633)	(.00118)	(.01206)	(.01136)	(.00581)	(.01006)	(.58707)	(.00526)
Father Veteran × Age at Exposure=3	-.00532	.00734	-.00201	-.01098	.00175	-.01778***	.00365	.33984	.01848***
	(.00545)	(.00526)	(.00159)	(.01128)	(.01058)	(.00574)	(.00998)	(.53897)	(.00513)
Father Veteran × Age at Exposure=4	.00208	-.0004	-.00168	.00528	-.00999	-.00676	-.00955	.68065	.00526
	(.00495)	(.0048)	(.00126)	(.00993)	(.0092)	(.00514)	(.0088)	(.48173)	(.0043)
Father Veteran × Age at Exposure=5	-.00512	.00531	-.0002	-.00514	-.00643	-.01133**	.00421	.62957	-.00122
	(.00496)	(.00487)	(.00098)	(.00933)	(.00873)	(.00494)	(.00818)	(.45785)	(.00486)
Father Veteran × Age at Exposure=6	-.0013	.00089	.00041	.00291	-.00603	-.01007**	-.01491**	.65315	.0074*
	(.00467)	(.00461)	(.00078)	(.0087)	(.00807)	(.00452)	(.00706)	(.44818)	(.00412)
Father Veteran × Age at Exposure=7	-.00368	.00381	-.00014	-.01265	.00761	-.00929**	.009	.91262**	.00432
	(.00431)	(.00421)	(.00096)	(.00875)	(.00833)	(.00418)	(.00775)	(.4155)	(.00364)
Father Veteran × Age at Exposure=8	.00467	-.00395	-.00072	-.00155	-.00307	-.00814**	-.01507**	.98574**	.00355
	(.00375)	(.00364)	(.00088)	(.00815)	(.00763)	(.00403)	(.00676)	(.39567)	(.0035)
Father Veteran × Age at Exposure=9	-.00088	.0022	-.00132	.00015	-.00644	-.01182***	-.00187	.43038	.00906***
	(.00339)	(.00329)	(.00089)	(.00782)	(.00737)	(.00407)	(.00693)	(.38055)	(.00344)
Father Veteran × Age at Exposure=10	.00302	-.00173	-.00128	-.00525	.0052	-.00979**	-.00332	.68228*	.0066*
	(.00325)	(.00313)	(.00087)	(.0074)	(.00695)	(.00383)	(.00645)	(.36105)	(.00366)
Observations	377371	377371	377371	377371	377371	377371	377371	360890	377371
R-squared	.44353	.44946	.3876	.3569	.51341	.15899	.45712	.23141	.18732
Mean DV	0.938	0.058	0.004	0.814	0.116	0.941	0.117	29.370	0.047
P-Value of Equality of Coefficients of Exposure 0-10	0.717	0.710	0.812	0.694	0.684	0.648	0.174	0.868	0.083
F-Stat of Equality of Coefficients of Exposure 0-10	0.709	0.716	0.604	0.733	0.743	0.780	1.398	0.533	1.662

Notes. Robust standard errors are in parentheses. All regressions include father's age by father veteran status dummies, father's age by birth year dummies, and county by birth year fixed effects. The regressions are weighted using the inverse probability weights where weights are extracted from probit regressions of successful merging between DMF and 1940-census on individual and parental covariates.

Table 3 - Testing for Endogenous Fertility

	Outcomes:						
	Child Birth Year=1927	Child Birth Year=1928	Child Birth Year=1929	Child Birth Year=1930	Child Birth Year=1931	Child Birth Year=1932	Child Birth Year=1933
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Father Veteran	.00051	.00102	.00094	.00045	-.0016	.00075	.00248*
	(.00109)	(.00116)	(.00119)	(.0013)	(.00134)	(.00147)	(.00149)
Observations	389084	389084	389084	389084	389084	389084	389084
R-squared	.02448	.02919	.03182	.03572	.03538	.04053	.0398
Mean DV	0.060	0.060	0.058	0.061	0.057	0.057	0.052
	Child Birth Year=1934	Child Birth Year=1935	Child Birth Year=1936	Child Birth Year=1937	Child Birth Year=1938	Child Birth Year=1939	Child Birth Year=1940
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Father Veteran	.00013	.00041	.00195	.00053	.00164	.00094	.00015
	(.00149)	(.00153)	(.00151)	(.00159)	(.0015)	(.00138)	(.00077)
Observations	389084	389084	389084	389084	389084	389084	389084
R-squared	.04426	.04639	.0446	.05951	.05201	.05852	.05013
Mean DV	0.051	0.047	0.041	0.038	0.034	0.030	0.006

Notes. Robust standard errors are in parentheses. All regressions include county fixed effects. Individual covariates include race dummies. Family controls include father education and socioeconomic score dummies and mother education dummies. The regressions are weighted using the inverse probability weights where weights are extracted from probit regressions of successful merging between DMF and 1940-census on individual and parental covariates.

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

Table 4 - Main Results

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Father Veteran × Age at Exposure=-4	1.5649	1.32311	1.29611
	(5.30723)	(5.33342)	(5.31628)
Father Veteran × Age at Exposure=-3	3.96201	3.97634	4.14576
	(5.2271)	(5.22429)	(5.20649)
Father Veteran × Age at Exposure=-2	4.94348	4.91385	4.8877
	(5.11396)	(5.1091)	(5.10014)
Father Veteran × Age at Exposure=-1	5.66765*	5.60372*	5.59577*
	(2.98247)	(2.98186)	(2.9821)
Father Veteran × Age at Exposure=0	7.45616**	7.47267**	7.45506**
	(3.10139)	(3.09348)	(3.09293)
Father Veteran × Age at Exposure=1	3.58429	3.62098	3.64666
	(3.01413)	(3.01164)	(3.01437)
Father Veteran × Age at Exposure=2	-.27362	-.24548	-.23635
	(2.70311)	(2.69945)	(2.69894)
Father Veteran × Age at Exposure=3	.50648	.56094	.53842
	(2.53397)	(2.53168)	(2.53046)
Father Veteran × Age at Exposure=4	4.24008*	4.2089*	4.20581*
	(2.2514)	(2.25031)	(2.25076)
Father Veteran × Age at Exposure=5	.35626	.41556	.39564
	(2.14264)	(2.14537)	(2.14374)
Father Veteran × Age at Exposure=6	1.30218	1.31909	1.3288
	(2.07306)	(2.07346)	(2.07337)
Father Veteran × Age at Exposure=7	.92821	.97084	.92655
	(1.91408)	(1.91593)	(1.91558)
Father Veteran × Age at Exposure=8	2.09807	2.04038	1.98543
	(1.83367)	(1.83404)	(1.8345)
Father Veteran × Age at Exposure=9	.54272	.54777	.52619
	(1.78758)	(1.78766)	(1.78791)
Father Veteran × Age at Exposure=10	.24182	.2013	.17107
	(1.69253)	(1.69115)	(1.6911)
Observations	377371	377371	377360
R-squared	.33496	.33534	.33551
Mean DV	786.831	786.831	786.830
Fixed Effects	✓	✓	✓
Individual Covariates		✓	✓
Family Controls			✓

Notes. Robust standard errors are in parentheses. All regressions include father's age by father veteran status dummies, father's age by birth year dummies, and county by birth year fixed effects. Individual covariates include race dummies. Family controls include father education and socioeconomic score dummies and mother education dummies. The regressions are weighted using the inverse probability weights where weights are extracted from probit regressions of successful merging between DMF and 1940-census on individual and parental covariates.

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

Table 5 - Heterogeneity across Subsamples

	Outcome: Age at Death (Months), Subsamples:				
	Nonwhites	Whites	Mother Education<12	Father SEI below Median	1-2 Child Families
	(1)	(2)	(3)	(4)	(5)
Father Veteran × Age at Exposure=-4	131.30783***	.25135	2.31253	-.37452	.2028
	(37.37646)	(5.40311)	(5.57319)	(8.68092)	(6.91926)
Father Veteran × Age at Exposure=-3	-43.95395*	4.64638	4.67723	2.95232	2.66774
	(23.72347)	(5.36878)	(5.54203)	(8.28631)	(5.96484)
Father Veteran × Age at Exposure=-2	-74.84019**	5.76066	6.15044	18.32696**	4.03839
	(31.18188)	(5.32906)	(5.52107)	(7.7787)	(6.40623)
Father Veteran × Age at Exposure=-1	31.88914	4.0622	4.9389	17.15213***	3.20488
	(20.96096)	(3.05771)	(3.192)	(4.48609)	(3.61481)
Father Veteran × Age at Exposure=0	-21.52911	9.64456***	8.6949***	11.25913**	8.03072**
	(22.37953)	(3.18303)	(3.25546)	(4.6321)	(3.66249)
Father Veteran × Age at Exposure=1	23.90135	3.33474	4.30311	10.76876**	2.14105
	(19.4433)	(3.0923)	(3.19318)	(4.81175)	(3.63664)
Father Veteran × Age at Exposure=2	10.94703	-1.75344	-.22987	-1.85981	-1.73275
	(22.12866)	(2.75946)	(2.85259)	(4.16505)	(3.11901)
Father Veteran × Age at Exposure=3	-.56566	.96929	1.41944	1.61363	.81643
	(22.40154)	(2.573)	(2.69227)	(3.8323)	(3.06903)
Father Veteran × Age at Exposure=4	10.15538	3.75294	4.72378**	.41911	3.15752
	(18.10746)	(2.30416)	(2.3734)	(3.48876)	(2.80783)
Father Veteran × Age at Exposure=5	6.23776	-.89697	.56423	8.01614**	-1.40297
	(15.86763)	(2.17003)	(2.27473)	(3.30152)	(2.72014)
Father Veteran × Age at Exposure=6	3.77249	.88084	2.13043	5.65633*	.87402
	(17.55729)	(2.10797)	(2.18915)	(3.05862)	(2.49266)
Father Veteran × Age at Exposure=7	13.67282	-.07695	1.35207	4.20012	-.6538
	(14.93004)	(1.94112)	(2.0264)	(2.86636)	(2.38103)
Father Veteran × Age at Exposure=8	-.26409	1.56154	2.22734	4.42987	2.04353
	(16.14408)	(1.87124)	(1.93144)	(2.74627)	(2.34007)
Father Veteran × Age at Exposure=9	2.07013	.26853	1.17411	2.68488	.18868
	(14.85089)	(1.81709)	(1.88349)	(2.6881)	(2.29555)
Father Veteran × Age at Exposure=10	4.17653	-.1565	.48664	-.39436	-1.08759
	(14.12904)	(1.71019)	(1.77577)	(2.60473)	(2.19429)
Observations	7699	364035	352068	176330	252897
R-squared	.56924	.32782	.33929	.40761	.36236
Mean DV	763.395	788.876	787.119	785.727	783.841

Notes. Robust standard errors are in parentheses. All regressions include father's age by father veteran status dummies, father's age by birth year dummies, and county by birth year fixed effects. Individual covariates include race dummies. Family controls include father education and socioeconomic score dummies and mother education dummies. The regressions are weighted using the inverse probability weights where weights are extracted from probit regressions of successful merging between DMF and 1940-census on individual and parental covariates.

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

Table 6 - Robustness Checks

	Column 3 Table 3	County-by-Individual-Family-Covariates FE	Veteran-by-Individual-Family-Covariates Dummies	Birth-Month and Death-Month FE	Outcome: Log Age at Death	Outcome: Age at Death>55	SEs not corrected	SE Clustered at County-Birth-Year Level	Unweighted Regressions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Father Veteran × Age at Exposure=-4	1.29611	1.65283	1.3285	1.01998	.00189	-.00323	1.29611	1.29611	-.62
	(5.31628)	(5.35335)	(5.30932)	(5.28733)	(.00843)	(.02596)	(7.37096)	(2.78853)	(5.08243)
Father Veteran × Age at Exposure=-3	4.14576	4.64667	4.1598	3.5725	.00508	.02104	4.14576	4.14576	5.96714
	(5.20649)	(5.31598)	(5.20457)	(5.21106)	(.00814)	(.02434)	(3.19342)	(2.55652)	(4.35042)
Father Veteran × Age at Exposure=-2	4.8877	4.6257	4.96595	4.86925	.00764	.00968	4.8877*	4.8877**	-.498
	(5.10014)	(5.16456)	(5.10045)	(5.0715)	(.00799)	(.02188)	(2.58248)	(2.28428)	(3.75675)
Father Veteran × Age at Exposure=-1	5.59577*	5.07004*	5.65732*	4.92876*	.00805*	.01916	5.59577***	5.59577***	5.37036**
	(2.9821)	(3.02438)	(2.98576)	(2.97561)	(.0045)	(.01268)	(1.64643)	(1.65941)	(2.39392)
Father Veteran × Age at Exposure=0	7.45506**	7.19546**	7.50922**	7.07852**	.01103**	.02847**	7.45506***	7.45506***	5.36582*
	(3.09293)	(3.10854)	(3.09377)	(3.09688)	(.00447)	(.01296)	(1.35935)	(1.97754)	(2.73986)
Father Veteran × Age at Exposure=1	3.64666	3.34703	3.68941	3.55962	.00571	.00858	3.64666**	3.64666*	-.28999
	(3.01437)	(3.03895)	(3.01489)	(3.01668)	(.0043)	(.01273)	(1.62015)	(1.91005)	(2.53481)
Father Veteran × Age at Exposure=2	-.23635	-1.09316	-.18315	-.61227	-.00126	-.00502	-.23635	-.23635	-2.24879
	(2.69894)	(2.73243)	(2.69835)	(2.69261)	(.00379)	(.01106)	(1.67043)	(1.8098)	(2.26533)
Father Veteran × Age at Exposure=3	.53842	.10934	.59636	.07229	.00018	-.01319	.53842	.53842	-2.11432
	(2.53046)	(2.55664)	(2.53096)	(2.52735)	(.0035)	(.0102)	(1.29954)	(1.79976)	(2.13746)
Father Veteran × Age at Exposure=4	4.20581*	3.40668	4.25378*	3.86119*	.00528*	-.00184	4.20581***	4.20581**	2.09947
	(2.25076)	(2.27777)	(2.25125)	(2.24165)	(.00305)	(.0086)	(1.1179)	(1.72566)	(1.88873)
Father Veteran × Age at Exposure=5	.39564	.72465	.45736	.13417	.00037	-.0022	.39564	.39564	.50072
	(2.14374)	(2.16406)	(2.14462)	(2.14159)	(.00286)	(.00787)	(.94613)	(1.72842)	(1.78225)
Father Veteran × Age at Exposure=6	1.3288	1.23207	1.37313	1.06656	.00182	.00509	1.3288	1.3288	-1.1006
	(2.07337)	(2.09229)	(2.07394)	(2.06362)	(.00275)	(.00704)	(1.18678)	(1.70181)	(1.66314)
Father Veteran × Age at Exposure=7	.92655	.66864	.96487	.61996	.00107	-.00118	.92655	.92655	1.55624
	(1.91558)	(1.93648)	(1.91636)	(1.91268)	(.00249)	(.00659)	(1.17519)	(1.68205)	(1.56651)
Father Veteran × Age at Exposure=8	1.98543	1.92394	2.0166	1.7769	.00249	.00281	1.98543	1.98543	.64635
	(1.8345)	(1.86824)	(1.83508)	(1.83235)	(.00236)	(.00585)	(1.21753)	(1.70371)	(1.51464)
Father Veteran × Age at Exposure=9	.52619	.34397	.54945	.34632	.00082	.00375	.52619	.52619	-.54588
	(1.78791)	(1.8243)	(1.78787)	(1.7874)	(.00227)	(.00536)	(.95858)	(1.71738)	(1.45705)
Father Veteran × Age at Exposure=10	.17107	-.20131	.20393	.00728	.00019	-.00542	.17107	.17107	-.64476
	(1.6911)	(1.71717)	(1.69088)	(1.69046)	(.00212)	(.00474)	(1.02225)	(1.72636)	(1.40938)
Observations	377360	376189	377360	377360	377360	377360	377360	377360	377360
R-squared	.33551	.35166	.33552	.33659	.33417	.22733	.33551	.33551	.24517

Notes. Robust standard errors are in parentheses. All regressions include father's age by father veteran status dummies, father's age by birth year dummies, and county by birth year fixed effects. Individual covariates include race dummies. Family controls include father education and socioeconomic score dummies and mother education dummies. The regressions are weighted using the inverse probability weights where weights are extracted from probit regressions of successful merging between DMF and 1940-census on individual and parental covariates.
*** p<0.01, ** p<0.05, * p<0.1

Table 7 - The Results of Home Ownership and House Value

	Outcomes:		
	House Value (in 2020 dollars)	Log House Value	House Owner
	(1)	(2)	(3)
Veteran×I(Year=1940)	15413.734***	.08609***	.07508***
	(4097.2692)	(.00419)	(.00119)
Veteran	-8303.0602***	-.04982***	-.04222***
	(2007.1327)	(.00293)	(.00085)
I(Year=1940)	-33237.367***	-.23847***	.01446***
	(481.95691)	(.00178)	(.00043)
Observations	1227760	1227760	5973586
R-squared	.02401	.32502	.05548
Mean DV	130990.707	11.298	0.488
County FE	✓	✓	✓
Individual-Family Controls	✓	✓	✓

Notes. Robust standard errors are in parentheses. Family controls include mother education dummies.
*** p<0.01, ** p<0.05, * p<0.1

Figures

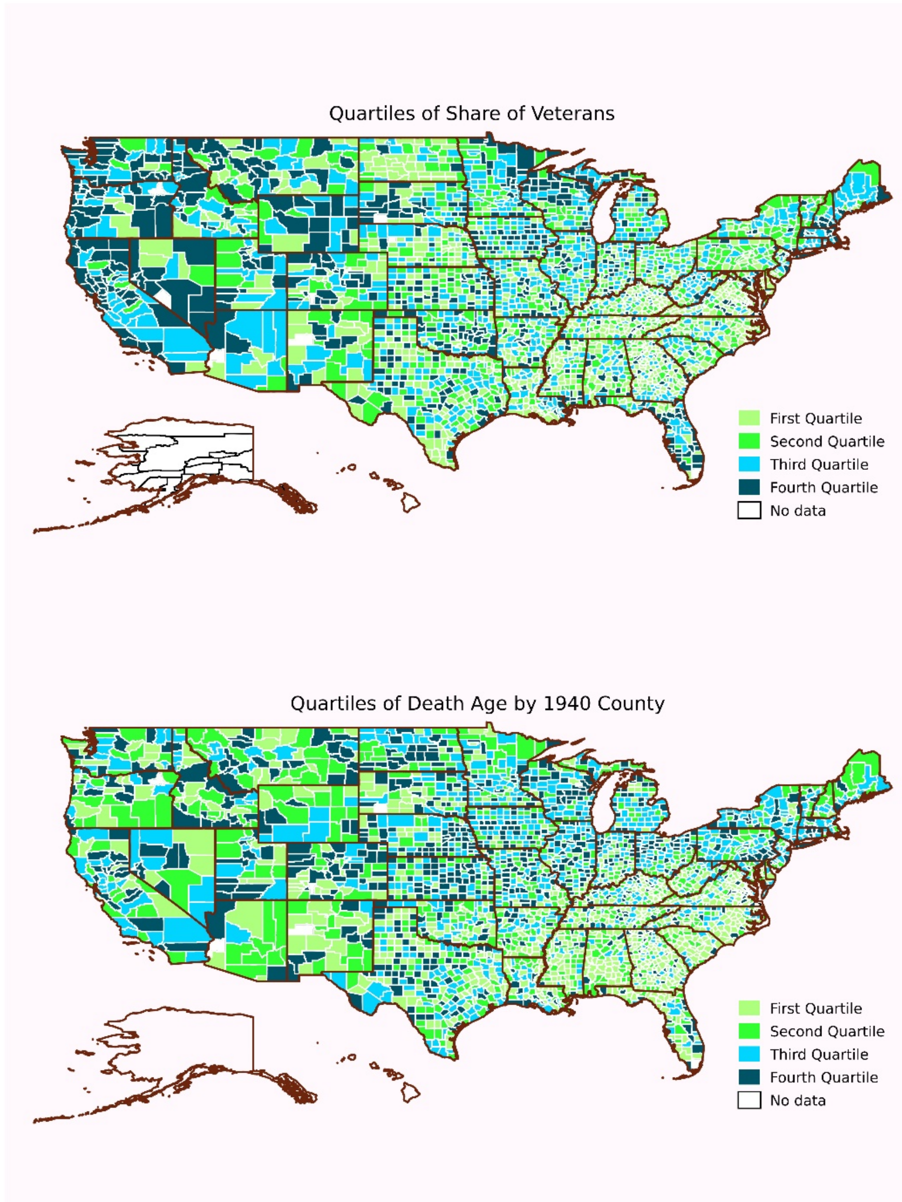

Figure 1 - Distribution of Veterans and Longevity across counties