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. This paper studies the long-run benefits of veterans' bonus receipt on their sons' 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 7.6 months.

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

JEL Codes: H75, I12, I18, J18

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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; Goodman-Bacon, 2021; Hayward & Gorman, 2004; 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's plans to support 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 veteran bonus program offered unconditional and unanticipated cash transfers. Individuals may plan marriage, fertility, change their residential location, change their labor supply, and borrow against the future and increase current consumption in anticipation of future cash transfers. This fact makes it difficult to interpret the transfer effects, as many of the prior changes in the prediction of transfers correlate with outcomes of interest. Therefore, the unanticipated nature of the veteran bonus facilitates a clearer interpretation of our identification strategy. The unanticipated and large income shock from veterans' bonuses 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 sons' deaths 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 7.6 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 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 to 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 homeowners. The results suggest that their housing wealth increases by about 5 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 (Aizer et al., 2016; Atherwood, 2022; Cutler et al., 2007; Hayward & Gorman, 2004; Modin, 2002). Contrary to these studies, the nature of the bonus payment provides an unanticipated shock on the income of all veterans. More importantly, although the payments depended on age and length of service, they had very low variations across veterans, suggesting they were a nearly-flat universal payment (Hausman et al., 2016; Quincy, 2022). Therefore, we have a relatively precise shock to income on the observed treated population. In addition, the bonus payment is much larger in magnitude when we compare it with other welfare payments. For instance, payments under the Food Stamp Program (FSP) and Aid to Families with Dependent Children (AFDC) are estimated to peak around 25-37 percent of per capita income during the 1970s and 1980s, while the veterans' bonus was roughly 100 percent of the 1936 per capita income (Crouse, 1995).

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). [4] For instance, Hoynes et al. (2015) explore the impact of permeant income shocks due to

[4] Similar studies that examine other policy-initiated income shocks also find positive impacts for infants' health. 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. 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.

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. Chung et al. (2016) investigate the effects of cash transfers under the Alaska Permanent Fund Dividend (APFD) program on birth outcomes and find that a transfer of about $2,500 (in 2020 dollars) is associated with about 14% lower incidence of low birth weight.[5]

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 &

[5] These effects on infants' health can be translated into later-life human capital development, labor market outcomes, and health in adulthood (Behrman & Rosenzweig, 2004; 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.

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). For instance, Aizer et al. (2016) examine the effects of cash transfers under the Mothers' Pension (MP) program in the US over the years 1911-1935 on schooling and longevity of children. They show that children of mothers whose applications were accepted to receive benefits live about 1 year longer lives. Barr et al. (2022) examine the long-term effects of early childhood cash transfers on adulthood labor market outcomes. They exploit the January 1 cut-off of payments under the Earned Income Tax Credit, which results in substantially higher benefits for those born just before the new year. They find that adults whose families were eligible for an additional $1,000 in EITC payments in their year of birth reveal 1-3 percent higher income. 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. (2024) 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 fluctuations are strongly correlated with other measures of economic conditions and that negative shocks to deposits at birth are associated with reductions in old-age longevity. Their findings suggest that in-utero exposures to reductions in income between 1929 and 1933 (peak to trough of the Great Depression) is associated with about 8.3 months shorter lives during old ages.[6]

Despite the positive impacts documented in these studies, several other studies suggest that the influence of cash transfers and income shocks on children's outcomes could be limited. For instance, Cesarini et al. (2016) investigate the effects of lottery prizes on adult mortality and children's developmental outcomes in Sweden. Although they find that winning the lottery is associated with increases in children's health care utilization, they do not observe any positive impacts on an array of children's developmental and health outcomes. Bleakley & Ferrie (2016) study the impacts of Georgia's Cherokee land lottery of 1832. They find that children of lottery winners and non-winners have similar wealth, income, and literacy during adulthood. Further, the observed similar levels of outcomes for grandchildren of winners and non-winners.

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 the 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

[6] A narrow literature also focuses on in-utero and childhood exposure to the Dust Bowl on later-life outcomes. For instance, 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.

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 the Census Linking Project (Abramitzky et al., 2020). The linking of records provides a match rate of about 23 percent.[7] 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.

The top panel of Figure 1 illustrates the geographic distribution of the share of veterans in the final sample. The bottom panel of this figure depicts the geographic distribution of age-at-death based on the 1940 county of residence. 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 0.8 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, two aspects of the DMF-census-linked data mitigate the concern over endogenous merging. First, the link between death records

[7] To assure accuracy of the match, we employ the conservative version of the ABE-NYIIS links.

and the 1940 census is primarily based on name commonalities and information on place of birth and age. This linking rule does not depend on the individuals' birth year and veteran status of their fathers. Second, 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.

In Appendix G, we show the summary statistics of selected variables across consecutive sample selections. We start with the original population in the 1940 census and restrict the sample based on gender, age, parental age, and presence of fathers in 1940 census records. We then show the summary statistics for the sample merged with the 1930 census and finally the DMF death records.

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:

$$DA_{ib} = \alpha_1 + \alpha_2 Veteran_i \times ExposureAge_b + \alpha_3 Veteran_i \times X_{ib} + \varepsilon_{ib} \qquad (1)$$

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. We include birth year fixed effects to account for temporal changes in longevity that arise from secular differences in cohorts' longevity. Further, we include county fixed effects to absorb time-invariant features of local conditions that influence longevity and a county-specific linear trend in birth year to control for all unobserved features of a county that evolves linearly across cohorts. Moreover, regressions include birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. Veteran is a dummy that indicates children of veteran fathers. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status dummy to allow for the effects of these covariates to be different for veterans and nonveterans. $ExposureAge$ is a binary indicating age of children in 1936. We do not have a reference point since all veterans' children are potentially treated and benefited from the cash transfers. To enable comparison across cohorts, however, we eliminate coefficients of 1920-1925 cohorts (age at exposure of 11-16) so that the estimated effects can be interpreted with respect to the longevity of these cohorts. Finally, ε is a disturbance term. We cluster standard errors at the county level to account for serial correlations in error terms.

As discussed in section 3 and Appendix G, the DMF-census-linked sample contains different sociodemographic characteristics than the original 1940 population. One concern that may arise from this selective data linking is that selected individuals in the final sample possess characteristics that might be correlated with the exposure measures as well as their health and longevity, i.e., confounding the long-run estimates of equation 1. In Appendix E, we empirically test this concern by examining the Association between exposure measures and successful merging. In so doing, we start with the 1940 census and implement similar sample selections as in the final analysis sample of the paper. We then merge this data with our final sample and generate a dummy variable indicating successful merging. Next, we regress this successful merging on the exposure measures of equation 1, conditional on covariates and fixed effects. The estimates imply very small and statistically insignificant associations, which rules out the concerns regarding endogenous data merging.

To further 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 a weighting scheme in our regressions.

5. Results

5.1. Balancing Tests

Although The Selective Service Act of 1917 made conscription 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 the 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 various panels of Figure 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.

The F-statistics and their corresponding p-values are reported in the upper section of each panel. The p-values fail to reject the equality of all the respective interaction coefficients. Specifically, we do not observe significant changes between veterans and nonveterans across different birth years for race outcomes (top panels of Figure 2). The P-values of equality of these coefficients are not rejected at conventional levels. Looking at several parental characteristics' outcomes reveals the same story. We observe mostly small and insignificant interaction coefficients across years, especially for father education (bottom panels of Figure 2), father homeownership in 1930 (bottom right panel of Figure 3), and father literacy and employment in 1930 (bottom panels of Figure 4). For other outcomes that we do observe significant coefficients, F-statistics suggest that we cannot reject the hypothesis that the coefficients across years are statistically different. However, there are exceptions such as maternal education being less than 12 years (top left panel of Figure 3). These are not concerning for two reasons. First, they are not consistent across different outcomes. Second, the point estimates of different years suggest quite small changes with respect to the mean. Overall, conditional on the implemented fixed effects, we fail to observe consistently significant changes in the difference between veteran-versus-non-veteran at different exposure ages. Therefore, the cross-cohort sociodemographic changes are fairly similar across control and treated groups. The estimated coefficients of these balancing test figures are reported in Appendix D. As a further analysis, we group coefficients for age at exposure of [1,10] and [-4,-1] and replicate these balancing tests. We test whether the coefficient of age at exposure of 0 is different from the grouped dummy variable for pre-1936 coefficients as well as the grouped dummy variable indicating post-1936 coefficients. For almost all outcomes in our balancing tests, these post-estimation tests fail to reject the null hypothesis of equality of coefficients. This fact suggests that there are little concerns that selective and endogenous changes (e.g., sociodemographic and socioeconomic changes) for age at exposure of 0 versus other ages at exposure confound the estimates.

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 the top panel of Figure 5.[8] The outcomes are shown in the vertical axis. The horizontal axis reports the coefficient of veteran status. There is no statistically significant pattern of pre-bonus and post-bonus change in fertility. For instance, for the year 1936, veterans are 5.2 basis-points more likely to have a child compared with non-veterans, equivalent to roughly 0.6 percent change from the mean of the outcome. These results do not offer consistent and discernible evidence for selective fertility issues. In the bottom panel of this figure, we show the results of selective fertility for a sample constructed from the 1940 census and linked to the 1930 census to extract father's veteran status. We implement similar sample selections based on age, father's age, and mother's age. The sample includes roughly 5.2 million observations. We observe a very similar pattern in the coefficients as those in the final sample. Although the larger sample size results in several statistically significant coefficients the overall pattern is similar and does not point to the selective fertility around the year 1936.

One remaining concern relates to the potential influence of cash transfers on fetal mortality and infant mortality. This association has been documented in the literature, especially for developing countries (Barham, 2011; Galofré Vilà, 2020). For instance, Galofré Vilà (2020) shows that conditional cash transfers under the Aid to Dependent Children (ADC) as a part of the 1935 Social Security act resulted in significant reductions in infant mortality. The current data limits our

[8] Appendix F reports the regression results of Figure 5.

ability to track children of the trends from 1936 to 1940 and examine changes in fetal, infant, and child death. However, to the extent that the transfers help the survival of frailer children of veterans to 1940, they might attenuate the true effects of these transfers as these children experience shorter lives due to their lower initial health capital.

5.2. Main Results

The main results of the paper are reported in two panels of Figure 6. In the top panel, we implement regressions similar to equation 1 for veterans and non-veterans, separately. In these regressions, we exclude birth year fixed effects. Across cohorts, we observe similar longevity for veterans' and non-veterans' children, suggesting that the payment did not have a differential impact on children's longevity of various age groups. The only noticeable difference is for those born in 1936, who were likely in-utero or experiencing the payment very early in life (age at exposure = zero). Looking at the trend of longevity of non-veterans, we do not observe a discernible change for these children relative to neighboring cohorts, i.e., those aged 1 (born in 1935) and aged -1 (born in 1937). However, among veterans' children, the longevity of 1936-born cohorts shows an observable jump. The magnitude of veterans' 1936-born children is larger than any other age groups. Moreover, the estimated effect of non-veterans' 1936-born children is virtually zero in magnitude, although with confidence intervals that overlap with the lower bound confidence intervals of the estimated effect of veterans' 1936-born children. However, to infer statistical inference of these visual differences, we prefer the difference-in-difference estimations.

The difference-in-difference results of equation 1 are reported in the bottom panel of Figure 6.[9] The only significant coefficient is that of the 1936-born cohort. It suggests that children of

[9] We report the regression results in Appendix A.

veteran fathers born in 1936 (i.e., age-at-exposure 0) live 7.6 months longer lives.[10] We do not find significant impacts across postnatal ages. Although most coefficients are positive, they suggest economically small and statistically insignificant impacts.

Furthermore, we also do not find significant effects across ages -1 through -4, for those born two years (and more) after the treatment. These small and insignificant coefficients reflect a combination of income and selective fertility of parents, further supporting the fertility results of Figure 5. 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 suggests that longevity improvements are driven by in-utero impacts and improvements in prenatal conditions.

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 enables within-cohort comparison and rules 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, the possible bias due to cross-cohort longevity difference due to the limited death window would likely underestimate the observed positive effects.

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. (2024) 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

[10] We should emphasize that for comparison purposes we eliminated the coefficients of those with age at exposure of [11,16]. Therefore, these age groups are considered the contrast group and all coefficients should be interpreted with respect to the longevity of these age groups.

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 an 8.3-month decrease in longevity during old age. This number is quite comparable to our main findings.

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 0.8 years increases in longevity (averaging men and women). For a household in the median of the sample, this means an increase of about $40K (in 2020 dollars). The average veteran payment is roughly $10,300 (in 2020 dollars) (Quincy, 2022). Therefore, the impact of in-utero and early-life income is about 3.1 times that of contemporaneous effects of income during adulthood.[11]

Aizer et al. (2016) investigate the impacts of the Mothers' Pension (MP) program, a government-sponsored cash transfer to single mothers operated between 1911-1935, on later-life longevity. The MP program transferred about 29-39 percent of maternal income and usually lasted for three years. The authors compare children's outcomes of accepted versus rejected mothers and find improvements in children's old-age longevity of about 11.6 months. The transfers under the bonus act were roughly equal to the average family income in 1936. However, we find an effect size that is about 66 percent of those found by Aizer et al. (2016). We speculate three reasons for the observed differences. First, MP was designed for single mothers who were very poor. As we

[11] We calculate per dollar cost of change in longevity for contemporaneous income change using Chetty et al. (2016)'s figures: 0.8/40K=0.00002. We replicate this with our estimates: 0.53/10.3K=0.000052. The latter is almost 2.6 times the former number.

can see from the comparison of veteran and non-veteran fathers' socioeconomic scores in 1930 in Table 1, veterans are relatively richer and have higher socioeconomic status. Consistent with the heterogeneity analysis of Aizer et al. (2016) and the results of heterogeneity analyses in Appendix Table B-1 suggesting larger benefits of cash transfers for poorer families, one would expect higher effects for the MP program. Second, the veterans' bonus was a one-time payment, while the MP program usually lasted for several years, and its effects could accrue over time. Third, the years around 1936 were followed by huge increases in welfare spending under the New Deal programs (Fishback, 2017). The additional benefits of the bonus transfer may have been lower, given the cumulative benefits of other welfare programs. Prior to the Social Security Act of 1935 and the rise of US welfare systems, the MP was one of the very few that could benefit mothers.

Halpern-Manners et al. (2020) examine the impact of education on longevity using Social Security Administration death records. They implement a twin fixed-effect strategy and find that each additional year of schooling is associated with roughly 4 months. Therefore, the effects of Figure 6 (around the birth-year) are equivalent to roughly 1.9 years of higher education. Fletcher & Noghanibehambari (2023) 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 Figure 6 for birth-year exposure to the bonus receipt are about 0.47-0.71 times that of college education on mortality. Overall, these comparisons reveal that the estimated early-life exposure effects are relatively large and economically meaningful.

5.3. Additional Analyses

We explore the potential heterogeneity of the results in Appendix B. We find larger effects among individuals with lower pre-transfer parental socioeconomic status and those with low-educated mothers. We further check for the robustness of our results in Appendix C. We implement additional specifications that include an extensive set of interactions between covariates and fixed effects to flexibly allow for place and veteran characteristics to vary by sociodemographic features. We also control for seasonality in birth and death. The results provide quite similar patterns and magnitudes as the main findings. Moreover, we show that the effects are not sensitive to alternative functional forms, unweighted regressions, and alternative corrections of standard errors.

5.4. 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 from 1940 to examine the change in veteran fathers' economic conditions relative to 1930. As explained in section 3 of the main text, 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 was 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 parental age dummies. We further 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 the top panel of Table 2. The main effects of year dummies suggest that, relative to 1930, house values and homeownership dropped considerably likely caused by the Great Depression (Balcilar et al., 2014). The main effects of the veteran dummy imply that veterans have, on average, higher house values and homeownership rates. The interaction terms suggest substantial improvements in veterans' house values and homeownerships. Relative to 1930, veterans' houses are valued at about 4.9 percent higher than non-veterans. Moreover, they are 3.2 percentage-points more likely to be homeowners, off a mean of 0.48. These results suggest general improvements in the 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 a better neighborhood could be translated into better access to health-related services as well as a less polluted environment. These pathways could lead to improved health capital and be detected in old-age longevity effects (Chyn, 2018).

Cash transfers also have a crowding-out effect on labor supply. Individuals may substitute these transfers for their labor market earnings, as suggested by several empirical studies (Bibler et al., 2023; Del Boca et al., 2021). In column 3 of Table 2, we observe a reduction in the labor supply of veterans versus nonveterans post-transfer. The magnitude implies a 1.2% reduction in labor force participation. In column 4, we observe a small increase in the socioeconomic index, equivalent to a 0.8% change with respect to the mean of the outcome. Despite the positive impacts of the transfer on consumption and wealth, the negative effects on labor supply might have mitigated the net effect of the transfer on long-run health and longevity.

In panel B of Table 2, we replicate the results of panel A for a sample that is constructed from the full count 1940 census (not linked with the DMF records). We implement a similar sample selection and include a similar set of covariates and fixed effects. We observe comparable coefficients to those of panel A. For instance, we observe a 3.4% increase in housing value versus 4.7% observed in panel A. This suggests that individuals in our final sample invested slightly more in their housing wealth. We also observe a reduction in labor force participation equivalent to a 1% change, a slight change compared to the 1.2% reported in panel A. It appears that veterans in the final sample reduced their labor force participation slightly more than the average veterans in the 1940 census.

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 the most. The effect sizes point to improvements of about 7.6 months of life for 1936 cohorts of children of veteran fathers. 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 the 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	
	Mean	SD	Mean	SD
Death Age (Months)	812.628	109.355	813.419	109.564
White	.975	.156	.96	.196
Black	.023	.151	.037	.188
Other	.002	.042	.003	.058
Birth Year	1926.315	4.546	1926.153	4.664
Death Year	1994.026	8.356	1993.923	8.324
Age at Exposure: -4	.008	.088	.008	.089
Age at Exposure: -3	.01	.101	.01	.098
Age at Exposure: -2	.012	.11	.013	.112
Age at Exposure: -1	.03	.172	.032	.175
Age at Exposure: 0	.022	.148	.023	.149
Age at Exposure: 1	.026	.16	.026	.16
Age at Exposure: 2	.033	.179	.033	.177
Age at Exposure: 3	.037	.19	.038	.191
Age at Exposure: 4	.048	.213	.046	.21
Age at Exposure: 5	.054	.226	.051	.22
Age at Exposure: 6	.064	.244	.059	.236
Age at Exposure: 7	.072	.258	.068	.251
Age at Exposure: 8	.08	.271	.074	.262
Age at Exposure: 9	.085	.28	.081	.273
Age at Exposure: 10	.092	.289	.086	.281
Father Age	44.72	2.602	44.641	3.227
House Value in 1940	76661.381	886627.88	55424.029	65774.364
House Value in 1930	99292.986	712302	80067.368	122367.44
House Owner in 1940	.555	.497	.5	.5
House Owner in 1930	.454	.498	.434	.496
Father Education<12	.84	.366	.924	.265
Father Education Missing	.018	.134	.02	.138
Mother Education<12	.887	.316	.952	.214
Mother Education Missing	.016	.124	.017	.129
Father’s 1930 SEI Score	34.938	23.719	26.004	20.499
Father’s 1930 SEI Score Missing	.075	.264	.036	.187
Observations	96,053		219,745	

Notes. Dollar values are converted into 2020 dollars.

Table 2 - Exploring Mechanisms Using Information in 1940 on Housing Wealth, Socioeconomic Status, and Labor Force Participation

	Outcomes:			
	Log House Value	House Owner	Labor Force Participation	Socioeconomic Index
	(1)	(2)	(3)	(4)
Panel A. Final Sample				
Veteran×I(Year=1940)	.04964***	.03166***	-.0117***	.21866*
	(.00925)	(.00272)	(.00085)	(.11794)
Veteran	.0907***	.00612***	.00087**	4.29596***
	(.00648)	(.00196)	(.00037)	(.08521)
I(Year=1940)	-.29592***	-.10698***	-.02003***	.08433
	(.01664)	(.00434)	(.00111)	(.17794)
Observations	143968	627778	631264	618281
R-squared	.37492	.08708	.02187	.21888
Mean DV	13.596	0.478	0.978	28.238
Panel B. 1940 Census				
Veteran×I(Year=1940)	.03479***	.03118***	-.00982***	-.26369***
	(.00212)	(.00057)	(.0002)	(.0256)
Veteran	.092***	-.00411***	-.00115***	4.49061***
	(.00146)	(.0004)	(.0001)	(.01822)
I(Year=1940)	-.39284***	-.08511***	-.01833***	-.05376***
	(.00087)	(.0002)	(.00007)	(.00822)
Observations	6106966	31823311	32029178	31041753
R-squared	.35072	.11297	.36055	.22258
Mean DV	10.838	0.460	0.932	25.675
Fixed Effects	✓	✓	✓	✓
Individual-Family Controls	✓	✓	✓	✓

Notes. Robust standard errors are in parentheses. Regressions include county fixed effects, father's age, and mother's age dummies. Individual controls include dummies for race and ethnicity. Family controls include mother education dummies. The Panel A Final Sample are those linked between 1930, 1940 and DMF files. Panel B 1940 Census sample do not need to be linked to 1930 or DMF files.

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

Figures

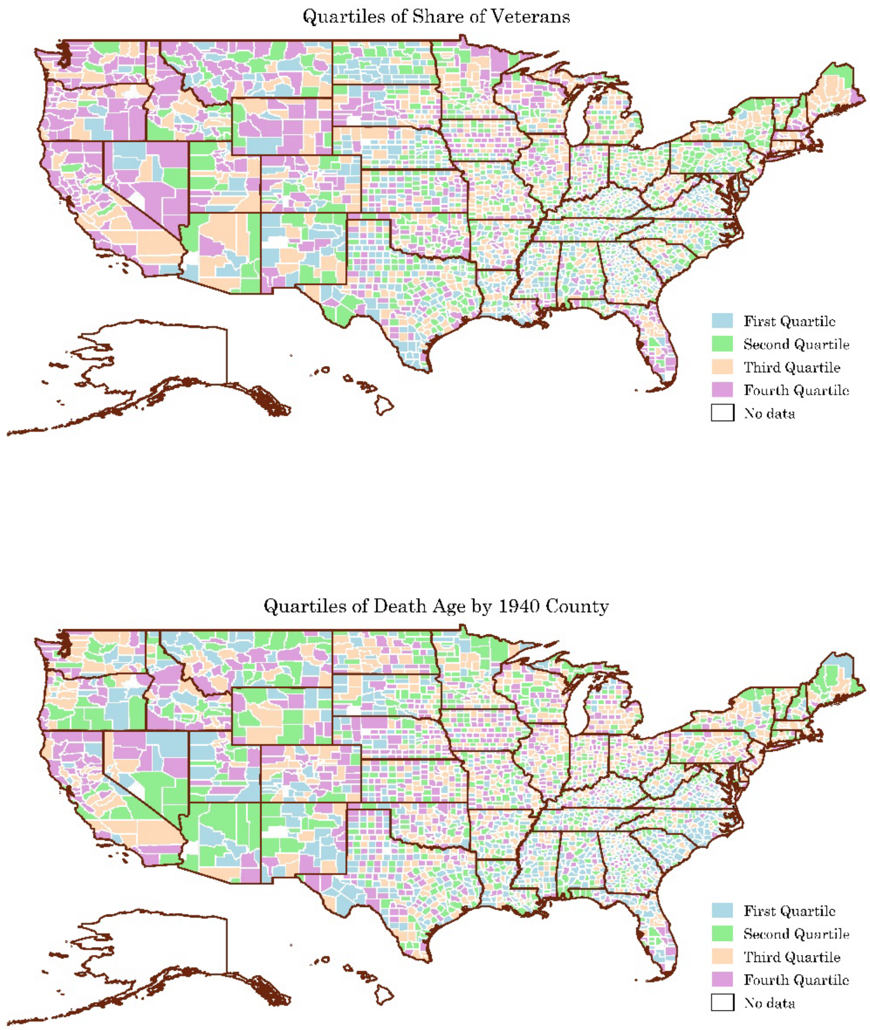

Figure 1 - Distribution of Veterans and Longevity across counties

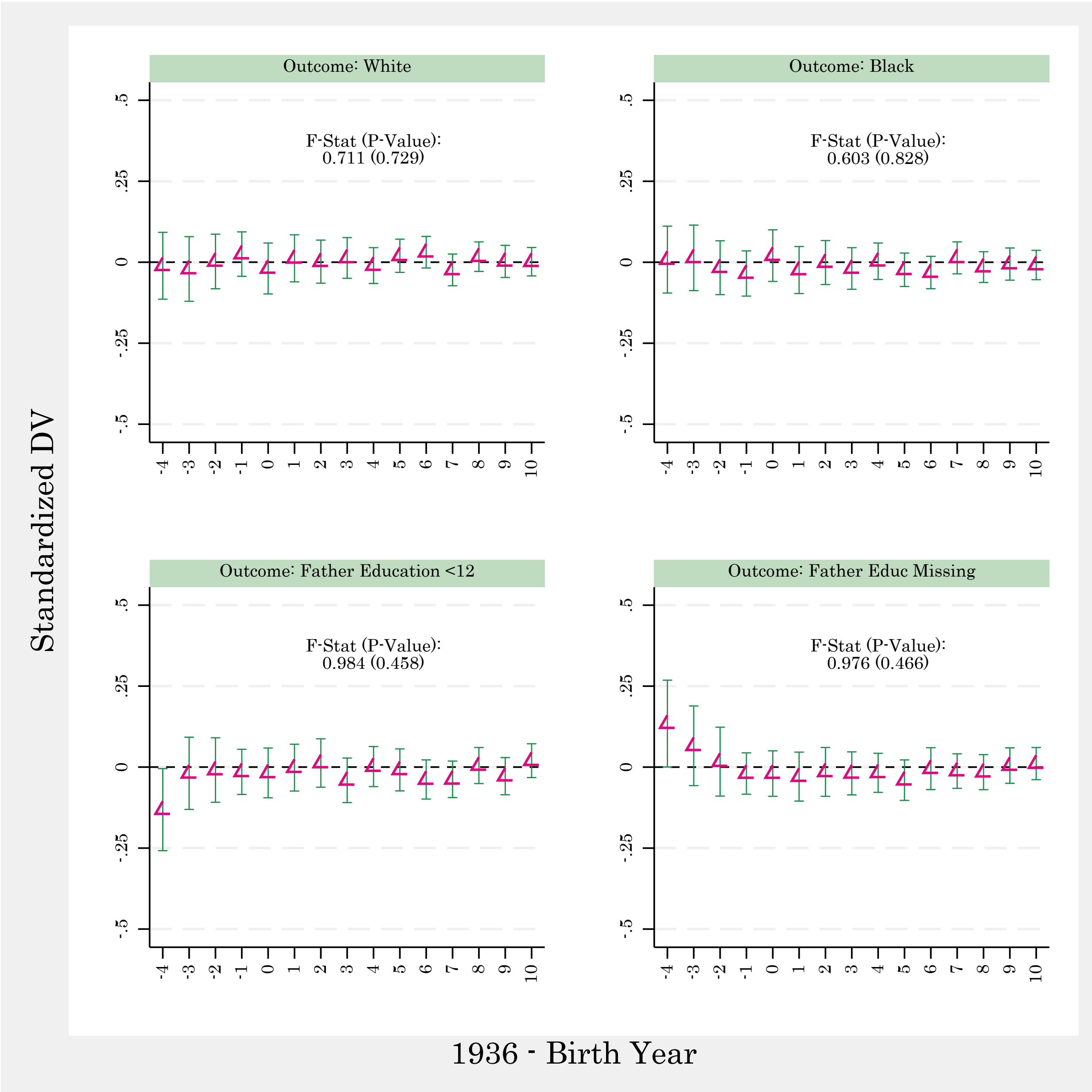

Figure 2 - Balancing Tests of Changes in Sociodemographic Characteristics among Veterans versus Non-Veterans in Years Relative to the Bonus Payment Year

Notes. Point estimates and 95 percent confidence intervals are depicted. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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.

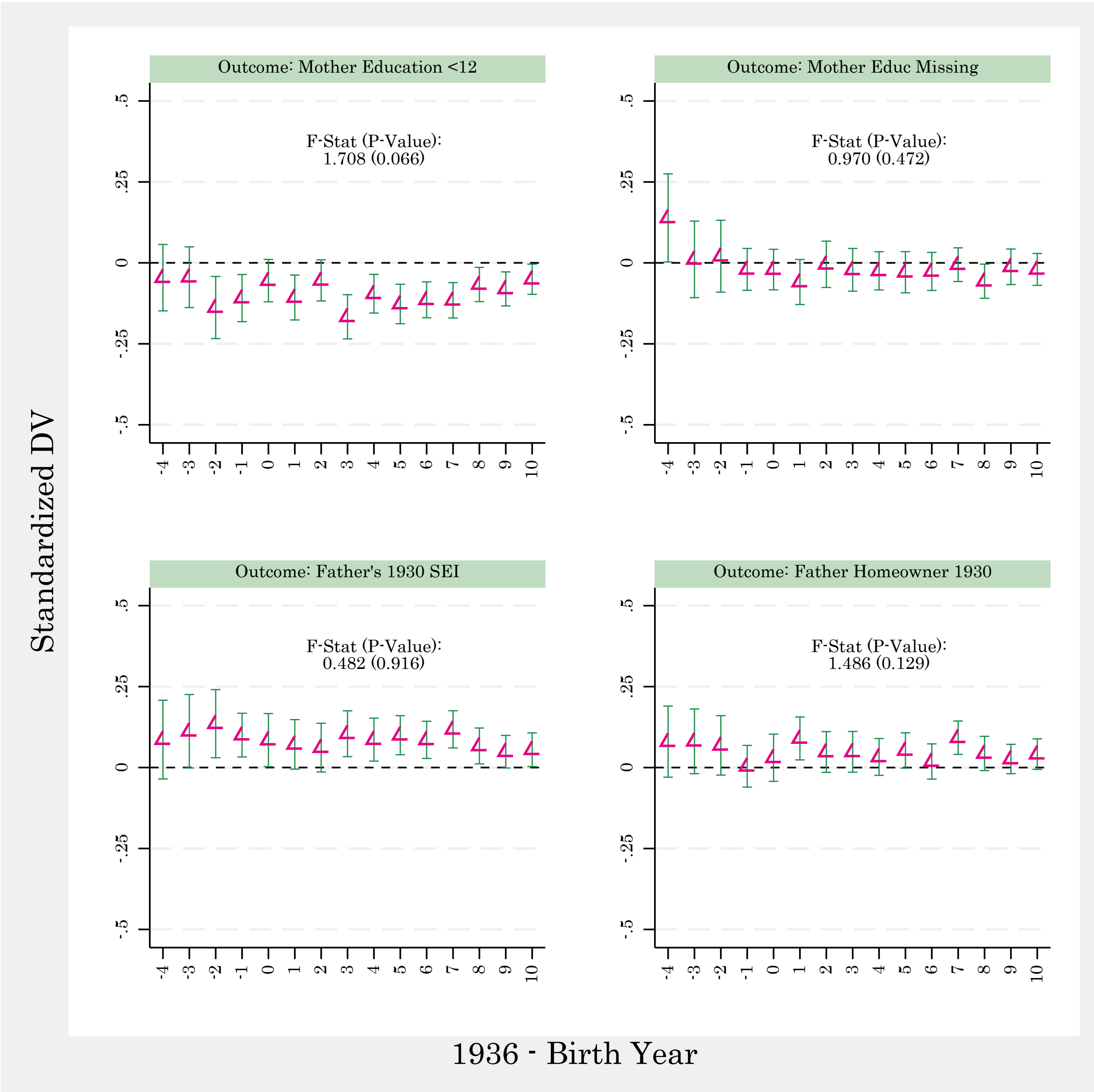

Figure 3 - Balancing Tests of Changes in Sociodemographic Characteristics among Veterans versus Non-Veterans in Years Relative to the Bonus Payment Year

Notes. Point estimates and 95 percent confidence intervals are depicted. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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.

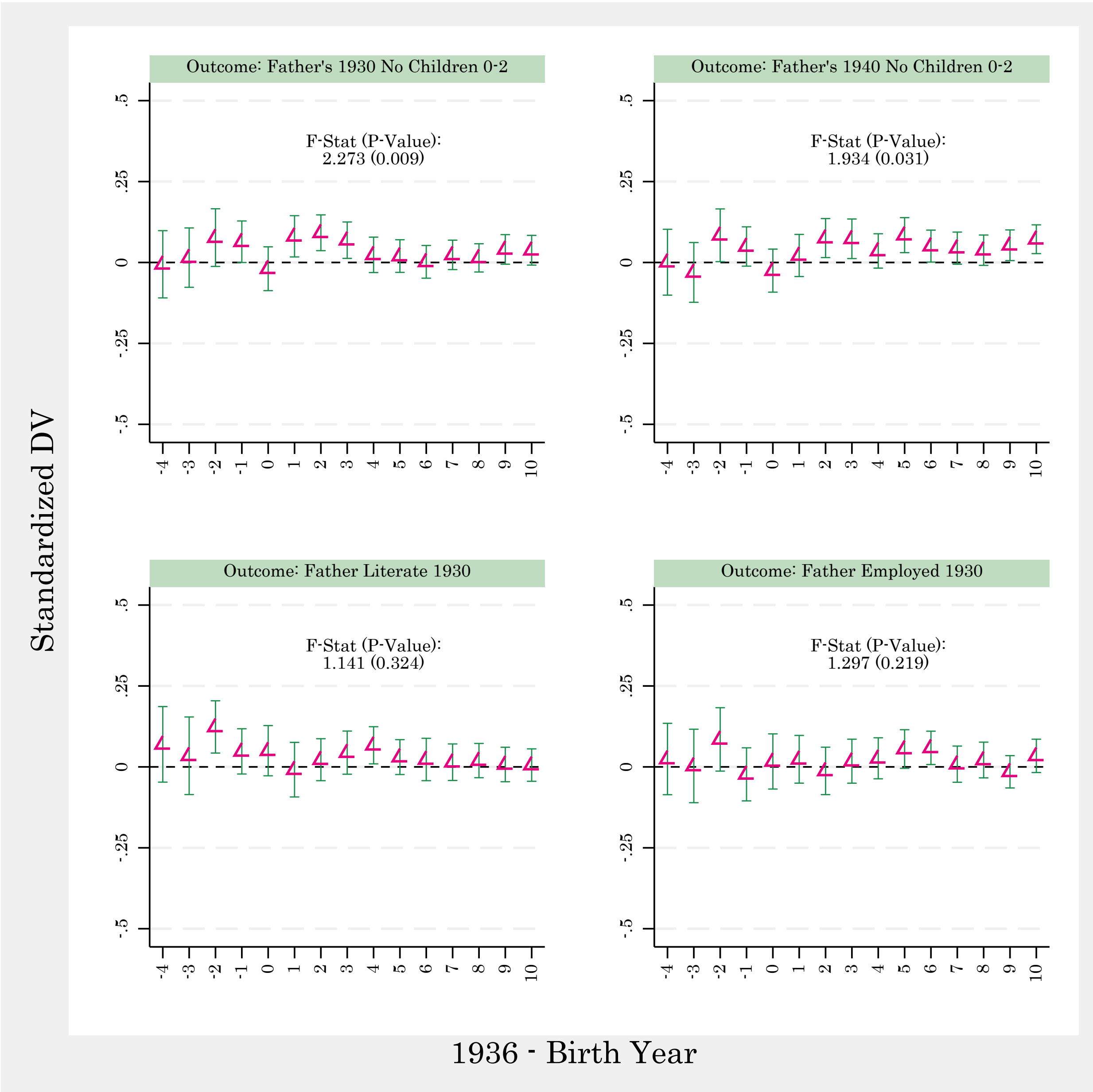

Figure 4 - Balancing Tests of Changes in Sociodemographic Characteristics among Veterans versus Non-Veterans in Years Relative to the Bonus Payment Year

Notes. Point estimates and 95 percent confidence intervals are depicted. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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.

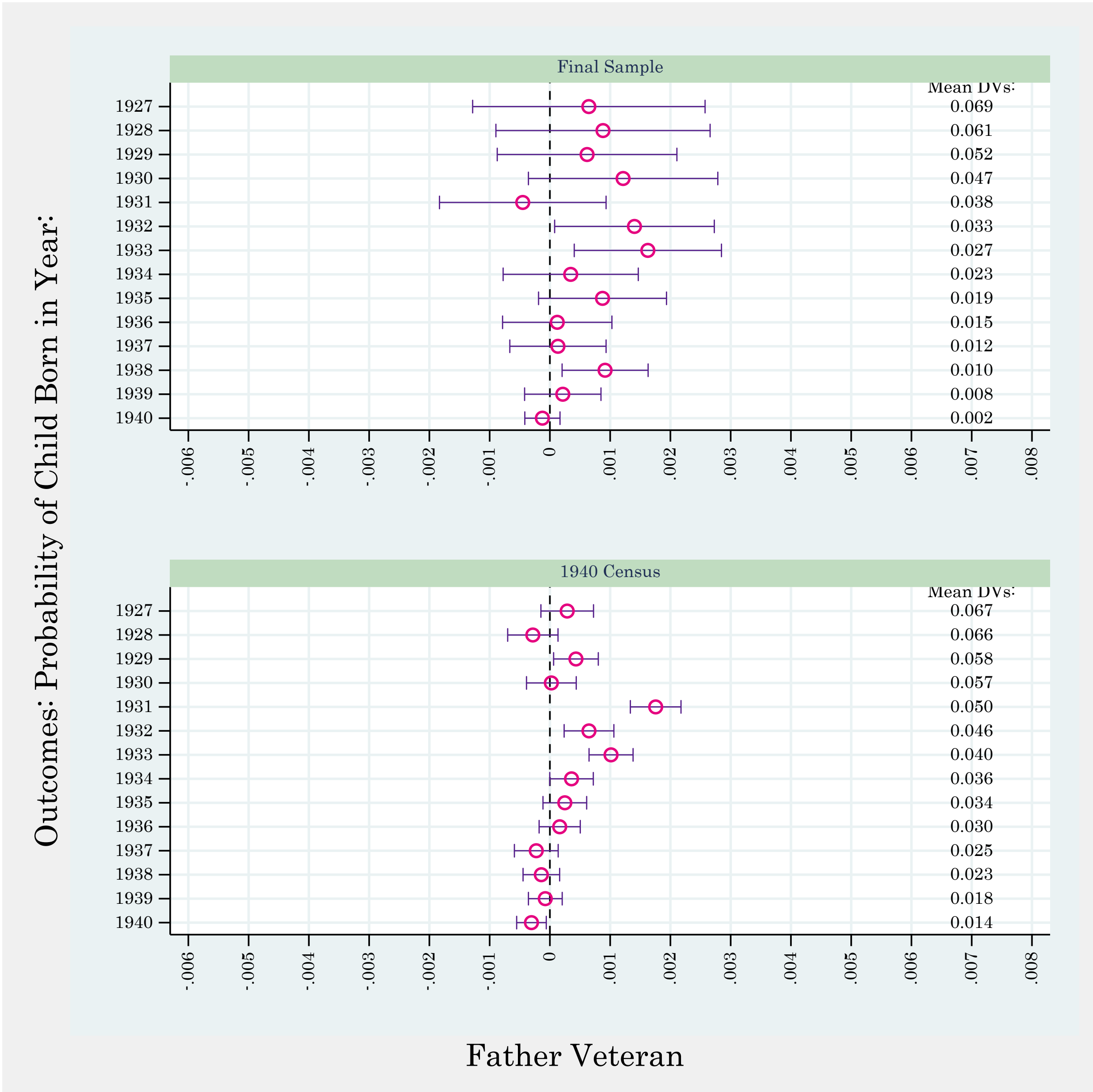

Figure 5 – Test of Endogenous Fertility among Veterans versus Non-Veterans in Years Relative to the Bonus Payment Year

Notes. Point estimates and 95 percent confidence intervals are depicted. Regressions include county fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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.

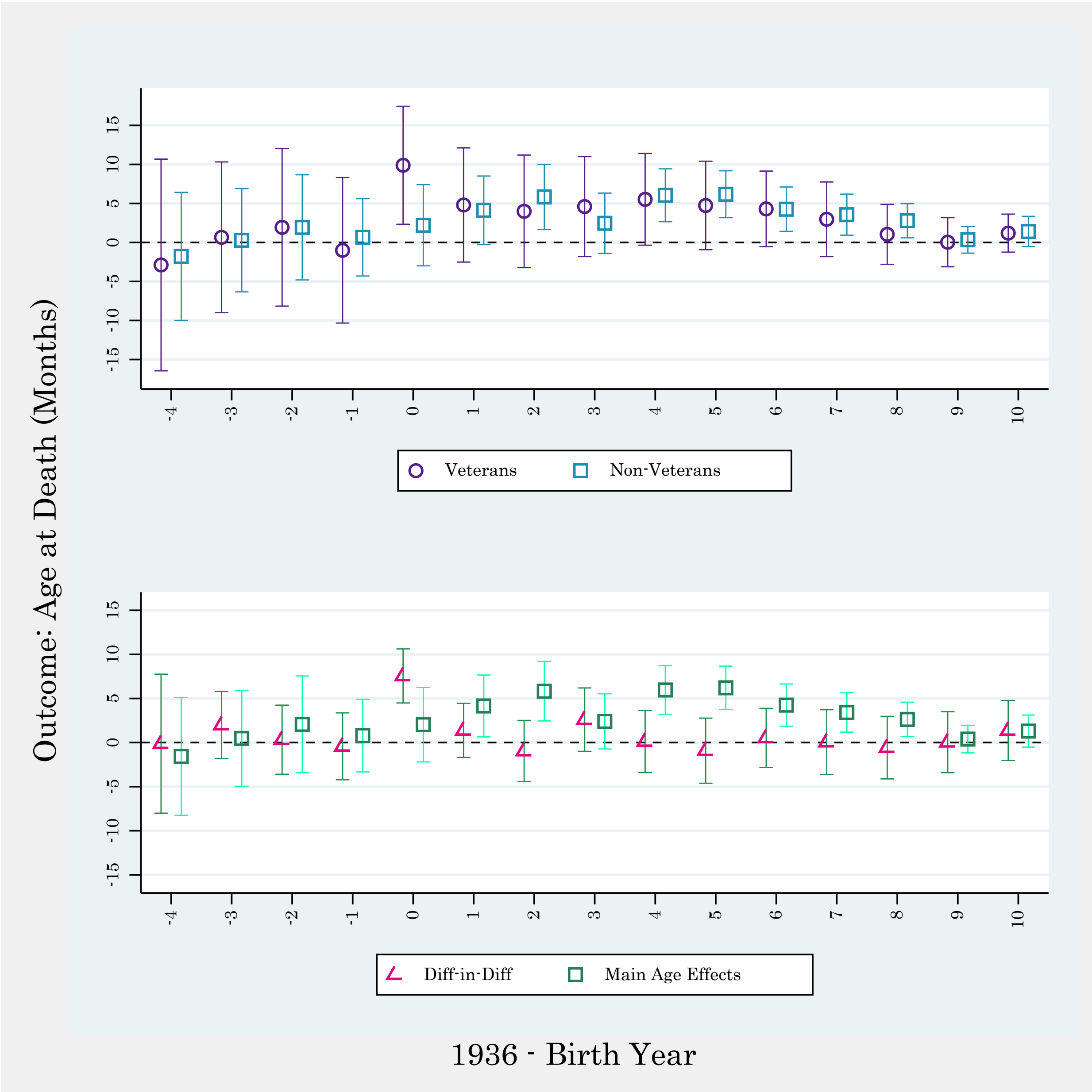

Figure 6 – Main Results: Changes in Longevity of Children of Veterans versus Non-Veterans, Born in Various Years Relative to the Bonus Payment Year

Notes. Point estimates and 95 percent confidence intervals are depicted. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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.

Appendix A

The main results of the paper are depicted as the difference-in-difference coefficients of the bottom panel of Figure 6. We report the coefficients and their standard errors across three columns of Appendix Table A-1. We add covariates to each consecutive column, although the coefficients are quite stable across specifications. In Appendix Table A-2, we group all pre-1936 and post-1936 years and replicate the results. For the subgroup of age at exposure of [-4,-1], i.e., born after 1936, we observe an increase of 1.8 months, although the point estimate is noisy. For the subgroup of age at exposure of [1,10], i.e., born prior to 1936, we observe and insignificant increase of one month. Consistent with the main results, the point estimate for those born in 1936 suggests an increase of 6.4 months in longevity.

Appendix Table A-1 – Reporting Regression Coefficients of Main Results

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Father Veteran × Age at Exposure=-4	-1.18153	-1.13935	-.12877
	(4.64771)	(4.7149)	(4.6841)
Father Veteran × Age at Exposure=-3	2.22555	2.30232	1.98806
	(2.05257)	(2.02004)	(2.26166)
Father Veteran × Age at Exposure=-2	.66141	.62664	.32195
	(2.34417)	(2.36138)	(2.3286)
Father Veteran × Age at Exposure=-1	-.63357	-.75736	-.42555
	(1.74342)	(1.75823)	(2.25268)
Father Veteran × Age at Exposure=0	**7.86239*** **	**7.94685*** **	**7.5554*** **
	(1.64884)	**(1.66561)**	**(1.8218)**
Father Veteran × Age at Exposure=1	.7712	.69968	1.38422
	(1.59378)	(1.60947)	(1.82476)
Father Veteran × Age at Exposure=2	-1.30493	-1.31101	-.96016
	(1.72042)	(1.73649)	(2.06275)
Father Veteran × Age at Exposure=3	2.83703	2.77026	2.59709
	(1.75146)	(1.77662)	(2.13605)
Father Veteran × Age at Exposure=4	.35575	.38757	.12596
	(1.64843)	(1.67003)	(2.09483)
Father Veteran × Age at Exposure=5	-.11578	-.20618	-.92357
	(1.48913)	(1.50474)	(2.1935)
Father Veteran × Age at Exposure=6	.05667	-.07647	.52431
	(1.52138)	(1.5439)	(1.99151)
Father Veteran × Age at Exposure=7	.33927	.42329	.04905
	(1.59902)	(1.62046)	(2.18586)
Father Veteran × Age at Exposure=8	-.20987	-.2792	-.57473
	(1.47562)	(1.50523)	(2.10617)
Father Veteran × Age at Exposure=9	.0106	-.00554	.03882
	(1.7337)	(1.76138)	(2.05529)
Father Veteran × Age at Exposure=10	.29133	.27284	1.37345
	(1.63706)	(1.6606)	(2.01679)
Observations	315659	315659	313897
R-squared	.32303	.3238	.32652
Mean DV	752.574	752.574	752.758
Fixed Effects	✓	✓	✓
Individual Covariates		✓	✓
Family Controls			✓

Notes. Robust standard errors are in parentheses. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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

Appendix Table A-2 - Replicating the Main Results Using Grouped Coefficients

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Father Veteran × Pre-1936 Age at Exposure	.34395	.30527	.45082
	(1.53604)	(1.55927)	(1.99927)
Father Veteran × Age at Exposure=0	7.81807***	7.90155***	7.55375***
	(1.65339)	(1.67018)	(1.82615)
Father Veteran × Post-1936 Age at Exposure	.05425	.01345	.23148
	(1.91833)	(1.94607)	(2.26408)
Observations	315659	315659	313897
R-squared	.32302	.32379	.3265
Fixed Effects	✓	✓	✓
Individual Covariates		✓	✓
Family Controls			✓

Notes. Robust standard errors are in parentheses. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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

Appendix B

In this appendix, we explore the heterogeneity of the results across subsamples. In columns 1 and 2 of Appendix Table B-1, we replicate the main results for nonwhite and white subsamples, respectively. Veterans of WWI were disproportionately white males (Hausman et al., 2016; Quincy, 2022). This fact is also quite noticeable when we look at the 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's 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, Appendix Table B-1). 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 Appendix Table B-1 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 7.8 months.

Appendix Table B-1 - Heterogeneity across Subsamples

	Outcome: Age at Death (Months), Subsamples:				
	Nonwhites	Whites	Mother Education<12	Father 1930 SEI below Median	1-2 Child Families
	(1)	(2)	(3)	(4)	(5)
Father Veteran × Age at Exposure=-4	28.52119	-3.5776	.91801	5.23288	4.22776
	(26.99471)	(3.99237)	(4.67787)	(4.732)	(4.76693)
Father Veteran × Age at Exposure=-3	-11.47494	.87974	1.64492	7.84858*	-1.04079
	(17.9101)	(1.95066)	(2.37218)	(4.13557)	(2.98987)
Father Veteran × Age at Exposure=-2	46.23386**	-1.60809	2.27577	2.92897	.52985
	(20.20672)	(2.4858)	(2.57115)	(4.65462)	(2.64709)
Father Veteran × Age at Exposure=-1	12.10072	-2.37145	-1.09978	7.35058*	-2.93225
	(16.43441)	(2.08388)	(2.17628)	(3.75707)	(2.69569)
Father Veteran × Age at Exposure=0	-20.05452	4.81504***	7.49102***	16.9211***	10.64049***
	(15.58411)	(1.67113)	(1.77592)	(3.22895)	(2.26988)
Father Veteran × Age at Exposure=1	-15.04819	.99398	1.44967	7.93674**	-.68556
	(14.74445)	(1.74777)	(1.71184)	(2.99554)	(2.00917)
Father Veteran × Age at Exposure=2	-18.38032	-2.28412	-1.05282	4.47038	-2.178
	(14.86146)	(1.87539)	(1.97079)	(2.86322)	(2.37066)
Father Veteran × Age at Exposure=3	-3.41942	1.40758	2.3455	5.27679*	3.45376
	(12.44317)	(1.80703)	(2.01497)	(2.77081)	(2.24411)
Father Veteran × Age at Exposure=4	-10.62176	-.83311	-.05867	1.55299	1.91399
	(14.18338)	(1.98802)	(1.96587)	(2.62658)	(2.2435)
Father Veteran × Age at Exposure=5	-25.56625*	-2.04047	-.77275	6.70526**	-2.48342
	(14.88751)	(2.16636)	(2.08585)	(2.96495)	(2.41648)
Father Veteran × Age at Exposure=6	-23.07539*	-.32711	1.1012	5.13102*	1.78465
	(13.69219)	(1.85868)	(1.82657)	(2.69305)	(2.05137)
Father Veteran × Age at Exposure=7	28.24781**	-2.8111	-.15851	2.66908	-1.28733
	(12.77833)	(1.90469)	(2.13015)	(2.99046)	(2.13942)
Father Veteran × Age at Exposure=8	-20.26905*	-.90871	-.50255	3.87605	1.82279
	(11.95777)	(1.75106)	(2.01499)	(2.93478)	(2.21742)
Father Veteran × Age at Exposure=9	-24.70676	-.88202	-.17892	3.92209	-.00805
	(14.72715)	(1.92259)	(1.93885)	(2.55953)	(2.20095)
Father Veteran × Age at Exposure=10	13.11557	.35512	1.26193	2.20917	-.46018
	(11.42553)	(1.80457)	(1.91649)	(2.75881)	(2.10412)
Observations	10435	302759	292603	164245	217366
R-squared	.61154	.31733	.3323	.37448	.34413
Mean DV	723.870	756.197	752.626	748.901	749.144

Notes. Robust standard errors are in parentheses. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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

Appendix C

In Appendix Table C-1, 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 Appendix Table A-1 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 0 suggests a 0.9 percent increase in longevity, respectively. The coefficient of age-at-exposure of 0 in column 1 implies a 0.82 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 3 percentage-points, equivalent to a 12.9 percent rise from the mean of the outcome.

In the main results, we use county-clustered 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. The coefficient of age-at-exposure of 0 remains statistically significant (with smaller standard errors).

Appendix Table C-1 - Robustness Checks

	Column 3 Appendix Table A-1	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
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Father Veteran × Age at Exposure=-4	-.12877	3.1211	-.24916	-.35912	.00065	-.00805	-.12877	-.18601
	(4.68419)	(4.11485)	(4.68083)	(4.58833)	(.00722)	(.01763)	(7.30489)	(2.19033)
Father Veteran × Age at Exposure=-3	1.98806	2.34513	1.75719	1.8096	.00237	-.00337	1.98806	2.07379
	(2.26171)	(2.05076)	(2.29376)	(2.22962)	(.00337)	(.00932)	(4.50587)	(2.08168)
Father Veteran × Age at Exposure=-2	.32195	-.80568	.1801	.42413	.00088	-.01354	.32195	.35634
	(2.32865)	(2.55254)	(2.352)	(2.31085)	(.0035)	(.01082)	(3.8381)	(2.03422)
Father Veteran × Age at Exposure=-1	-.42555	-.97005	-.49363	-.54345	-.00066	.00582	-.42555	-.60755
	(2.25272)	(2.2431)	(2.27593)	(2.24973)	(.00339)	(.01393)	(3.10029)	(1.71712)
Father Veteran × Age at Exposure=0	7.5554***	6.63879***	7.35125***	7.39801***	.01197***	.03316***	7.5554**	7.5706***
	(1.82184)	(1.84892)	(1.83417)	(1.81113)	(.00275)	(.00894)	(2.93593)	(1.96071)
Father Veteran × Age at Exposure=1	1.38422	-.3912	1.22625	1.33874	.00247	.00209	1.38422	1.39987
	(1.82479)	(1.90349)	(1.84614)	(1.82252)	(.00277)	(.00826)	(3.0254)	(1.99823)
Father Veteran × Age at Exposure=2	-.96016	-1.83322	-1.06072	-.90069	-.00146	-.00873	-.96016	-.93748
	(2.06279)	(1.91394)	(2.08636)	(2.0569)	(.00305)	(.00884)	(3.18159)	(1.99335)
Father Veteran × Age at Exposure=3	2.59709	.71978	2.5407	2.51148	.00387	-.00419	2.59709	2.61243
	(2.13609)	(2.00869)	(2.14682)	(2.14)	(.00322)	(.00926)	(3.18252)	(2.06231)
Father Veteran × Age at Exposure=4	.12596	-.0199	.04448	.00336	.00036	-.0144	.12596	.04329
	(2.09487)	(2.02566)	(2.10547)	(2.0889)	(.00313)	(.00922)	(2.74563)	(2.12239)
Father Veteran × Age at Exposure=5	-.92357	-2.20715	-.94566	-.95474	-.0009	-.01342	-.92357	-.91645
	(2.19355)	(2.07476)	(2.21323)	(2.18162)	(.00327)	(.00996)	(2.97403)	(2.34427)
Father Veteran × Age at Exposure=6	.52431	.37637	.54019	.44164	.00103	-.0051	.52431	.52679
	(1.99155)	(1.73542)	(1.99977)	(1.98886)	(.00296)	(.00869)	(2.72287)	(2.44452)
Father Veteran × Age at Exposure=7	.04905	-.17217	-.08321	-.0679	.00057	-.00127	.04905	.04916
	(2.18591)	(2.00627)	(2.2064)	(2.18938)	(.00327)	(.00925)	(2.71898)	(2.6015)
Father Veteran × Age at Exposure=8	-.57473	-1.45388	-.57097	-.57752	-.00053	-.01023	-.57473	-.598
	(2.10621)	(1.91422)	(2.10959)	(2.10793)	(.00314)	(.00949)	(2.68329)	(2.79588)
Father Veteran × Age at Exposure=9	.03882	-1.07613	.00564	-.02055	.00033	-.00649	.03882	.04989
	(2.05533)	(1.90953)	(2.05911)	(2.04864)	(.00303)	(.00971)	(2.59021)	(3.00976)
Father Veteran × Age at Exposure=10	1.37345	.90707	1.38672	1.34635	.00216	-.00843	1.37345	1.39473
	(2.01683)	(1.8503)	(2.00973)	(2.00657)	(.00303)	(.00886)	(2.53297)	(3.21884)
Observations	313910	312914	313910	313910	313910	313910	313910	313904
R-squared	.32653	.36398	.32659	.32723	.31424	.18221	.32653	.32662

Notes. Robust standard errors are in parentheses. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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

Appendix D

Appendix Table D-1 reports the estimated coefficients of the balancing tests that are illustrated in Figure 2 through Figure 4. Further, we group all coefficients for age at exposure of [-4,-1] into a post-1936 dummy variable and all coefficients for age at exposure of [1,10] into a pre-1936 dummy variable. Using these two variables in addition to the age at exposure of 0, we replicate the balancing test results in Appendix Table D-2. At the bottom of this table, we report the P-value of the difference between the coefficient of age at exposure of 0 and grouped pre-1936 and post-1936 variables, respectively. Since our results point to a considerable difference between the longevity of age at exposure of 0 versus other ages at exposure, we believe that this is an appropriate test to examine whether the results are driven by selective changes in sociodemographic and socioeconomic characteristics of veterans and nonveterans across different years. In most cases, the difference between the coefficient of age at exposure of 0 and the other two grouped variables is statistically insignificant.

Appendix Table D-1 - Reporting the Coefficients of Balancing Tests

	Outcomes:											
	White	Black	Father Education < 12	Father Education Missing	Mother Education < 12	Mother Education Missing	Father 1930 Socioeconomic Index	Father Homeowner 1930	Father No Children 0-2 in 1930	Father No Children 0-2 in 1940	Father Literacy 1930	Father Employed 1930
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Father Veteran × Age at Exposure=-4	-.00343	.00244	-.03829**	.02633**	-.00975	.02485**	1.71441	.03884	-.0028	.00039	.01812	.00667
	(.01702)	(.01684)	(.01754)	(.01317)	(.01075)	(.01242)	(1.11223)	(.02385)	(.0228)	(.02294)	(.01221)	(.01508)
Father Veteran × Age at Exposure=-3	-.00651	.00404	-.00559	.0129	-.00943	.00194	2.22812**	.03935*	.00748	-.01491	.00896	.00077
	(.01448)	(.01414)	(.01528)	(.01128)	(.00984)	(.01044)	(.98033)	(.02115)	(.02101)	(.02035)	(.01131)	(.01364)
Father Veteran × Age at Exposure=-2	.00072	-.00504	-.00264	.00332	-.0294***	.00368	2.69186***	.03323	.03839*	.04067**	.03216***	.02328*
	(.01214)	(.01167)	(.01444)	(.0103)	(.01024)	(.00996)	(.94081)	(.02031)	(.02034)	(.01951)	(.00991)	(.01224)
Father Veteran × Age at Exposure=-1	.0078	-.01041	-.00431	-.00389	-.02319***	-.00359	1.98918***	.0019	.03201**	.02395*	.01246	-.00632
	(.00914)	(.00884)	(.00984)	(.00643)	(.00704)	(.00601)	(.65565)	(.01471)	(.0145)	(.01454)	(.00827)	(.00979)
Father Veteran × Age at Exposure=0	-.00602	.00609	-.00523	-.00391	-.01168	-.00369	1.68387**	.01468	-.00961	-.01221	.01302	.00459
	(.0113)	(.01098)	(.01124)	(.00789)	(.00772)	(.00679)	(.7598)	(.01664)	(.01643)	(.01639)	(.00882)	(.01152)
Father Veteran × Age at Exposure=1	.0037	-.00718	-.00051	-.00574	-.02281***	-.01055*	1.42028*	.04363***	.0404**	.0105	-.00224	.00643
	(.01028)	(.00986)	(.01066)	(.00752)	(.00749)	(.00626)	(.72635)	(.01608)	(.01587)	(.01613)	(.00994)	(.00988)
Father Veteran × Age at Exposure=2	.00053	-.0003	.00368	-.00289	-.01153*	-.00078	1.22165*	.02331	.04582***	.03652**	.00579	-.00339
	(.00961)	(.00942)	(.01006)	(.00711)	(.00669)	(.00638)	(.67889)	(.01474)	(.01464)	(.01482)	(.00819)	(.00963)
Father Veteran × Age at Exposure=3	.00409	-.00578	-.01191	-.00378	-.03549***	-.00382	2.075***	.02351	.03441**	.03543**	.01146	.00482
	(.00896)	(.00877)	(.00951)	(.00634)	(.00674)	(.00592)	(.65686)	(.01447)	(.01454)	(.0147)	(.0081)	(.00915)
Father Veteran × Age at Exposure=4	-.00323	.00091	.00046	-.00348	-.02031***	-.00438	1.71229***	.0159	.01178	.01726	.01739**	.00727
	(.00851)	(.00829)	(.00911)	(.00626)	(.00629)	(.00562)	(.59751)	(.01336)	(.01336)	(.01355)	(.00766)	(.00846)
Father Veteran × Age at Exposure=5	.00615	-.00694	-.00253	-.0079	-.02706***	-.00517	1.98425***	.0256**	.00991	.04091***	.00789	.01513*
	(.0078)	(.00761)	(.00867)	(.00572)	(.0064)	(.0052)	(.59324)	(.01288)	(.01273)	(.01308)	(.00716)	(.00778)
Father Veteran × Age at Exposure=6	.00957	-.00949	-.01117	-.00092	-.02429***	-.00472	1.69932***	.00908	.00099	.02449*	.00594	.01612**
	(.00743)	(.0073)	(.00843)	(.00576)	(.00597)	(.00487)	(.55635)	(.01232)	(.01212)	(.01251)	(.00731)	(.00762)
Father Veteran × Age at Exposure=7	-.00741	.00403	-.01101	-.00239	-.02464***	-.00098	2.34518***	.04468***	.01172	.02145*	.00376	.00231
	(.00779)	(.00764)	(.00829)	(.00573)	(.00573)	(.00514)	(.55008)	(.01224)	(.01201)	(.01231)	(.00714)	(.00762)
Father Veteran × Age at Exposure=8	.00531	-.00447	.00141	-.00307	-.01423***	-.01011**	1.32618**	.02112*	.0071	.01839	.00508	.00583
	(.00708)	(.00698)	(.00791)	(.00534)	(.00547)	(.00466)	(.52317)	(.01161)	(.01163)	(.01184)	(.00668)	(.00734)
Father Veteran × Age at Exposure=9	.00078	-.0017	-.00816	.00094	-.01714***	-.00215	.97243*	.01281	.02015*	.02578**	.00194	-.00416
	(.00686)	(.00667)	(.0078)	(.00527)	(.00546)	(.00464)	(.51385)	(.01145)	(.01152)	(.01175)	(.0067)	(.00717)
Father Veteran × Age at Exposure=10	.00048	-.00254	.00579	.00214	-.01077**	-.0036	1.09277**	.0201*	.01886*	.03477***	.00147	.00934
	(.00664)	(.00651)	(.00749)	(.00514)	(.00525)	(.00453)	(.49811)	(.01123)	(.01114)	(.01137)	(.00642)	(.00696)
Observations	315659	315659	315659	315659	315659	315659	300479	315659	315659	315659	315659	315659
R-squared	.43397	.44067	.2214	.36901	.12002	.4202	.2087	.18388	.29107	.21066	.29061	.14054
Mean DV	0.892	0.099	0.906	0.040	0.952	0.033	23.454	0.378	0.476	0.377	0.927	0.918
P-Value	0.743	0.869	0.352	0.910	0.003	0.751	0.361	0.245	0.031	0.128	0.620	0.259
F-Stat	0.681	0.532	1.108	0.470	2.686	0.672	1.095	1.263	1.977	1.512	0.809	1.241

Notes. Robust standard errors are in parentheses. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. 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

Appendix Table D-2 - Balancing Tests Using Grouped Coefficients

	Outcomes:											
	White	Black	Father Education < 12	Father Education Missing	Mother Education < 12	Mother Education Missing	Father 1930 Socioeconomic Index	Father Homeowner 1930	Father No Children 0-2 in 1930	Father No Children 0-2 in 1940	Father Literacy 1930	Father Employed 1930
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Father Veteran × Pre-1936 Age at Exposure	.00218	-.00361	-.00352	-.00333	-.02185***	-.00459	1.62228***	.02516**	.02385**	.02652***	.0065	.00578
	(.0065)	(.00642)	(.0072)	(.0044)	(.0048)	(.00376)	(.47836)	(.01021)	(.00929)	(.0092)	(.00563)	(.006)
Father Veteran × Age at Exposure=0	-.00594	.00604	-.00549	-.00383	-.01161	-.00357	1.67806**	.0143	-.01018	-.01205	.01295	.00457
	(.01244)	(.01213)	(.01143)	(.00702)	(.0071)	(.00571)	(.82902)	(.01803)	(.01725)	(.0164)	(.01028)	(.01191)
Father Veteran × Post-1936 Age at Exposure	.00184	-.00448	-.00996	.00582	-.01964***	.00372	2.13446***	.02104	.02218*	.01649	.01685*	.00339
	(.00853)	(.0084)	(.00953)	(.0059)	(.00613)	(.00552)	(.64432)	(.01368)	(.0132)	(.01228)	(.00862)	(.00877)
Observations	315659	315659	315659	315659	315659	315659	300479	315659	315659	315659	315659	315659
R-squared	.43393	.44063	.22125	.36881	.11984	.42	.20866	.18377	.29094	.21053	.29051	.14039
P-Value of Diff btw Pre and 1936 Exposure	0.446	0.350	0.837	0.940	0.103	0.845	0.936	0.492	0.023	0.007	0.459	0.903
P-Value of Diff btw Post and 1936 Exposure	0.500	0.347	0.692	0.214	0.285	0.233	0.576	0.705	0.053	0.079	0.710	0.923

Notes. Robust standard errors are in parentheses. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. 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

Appendix E

One concern in interpreting the main results is the confounding influence of the data-linking procedure. This could be problematic if the selection of individuals in the final sample is a function of factors that are correlated with exposure measures in our regressions as well as their health and longevity. We can empirically test this using the original population in the 1940 census and examine whether the selection is correlated with the primary right-hand side variables of equation 1. In so doing, we link fathers in 1940 to their records in 1930 in order to infer World War I veteran status. We then restrict the sample based on age, father's age, and mother's age, as we did for the final sample of the paper. We then merge this data with the final sample and generate a dummy variable indicating successful merging. We regress this outcome on the right-hand side variables of equation 1. The results are reported in Appendix Table E-1 for regressions with and without controls. In both columns, we do not observe a meaningful and stylistically significant association between the primary exposure measures and the successful DMF-census merging outcome. In Appendix Table E-2, we show the results for grouped coefficients and observe similar findings. Specifically, the P-values for equality of age at exposure of 0 and the other two pre-1936 and post-1936 coefficients suggest that the tests fail to reject the corresponding hypotheses.

Appendix Table E-1 - The Association between Exposure Measures and Successful Census-DMF Data Linking

	Outcome: Successful DMF-Census Merging	
	(1)	(2)
Father Veteran × Age at Exposure=-4	.00057	.00053
	(.00129)	(.00129)
Father Veteran × Age at Exposure=-3	.00131	.00123
	(.00127)	(.00127)
Father Veteran × Age at Exposure=-2	-.00025	-.00031
	(.00123)	(.00123)
Father Veteran × Age at Exposure=-1	-.00012	-.00019
	(.00096)	(.00096)
Father Veteran × Age at Exposure=0	.00062	.00054
	(.00116)	(.00116)
Father Veteran × Age at Exposure=1	.00117	.00109
	(.00118)	(.00119)
Father Veteran × Age at Exposure=2	.00066	.00059
	(.00113)	(.00113)
Father Veteran × Age at Exposure=3	-.00125	-.00125
	(.00116)	(.00117)
Father Veteran × Age at Exposure=4	.00074	.00066
	(.00103)	(.00103)
Father Veteran × Age at Exposure=5	-.00048	-.0005
	(.00107)	(.00107)
Father Veteran × Age at Exposure=6	.00211**	.00205*
	(.00105)	(.00105)
Father Veteran × Age at Exposure=7	-.00001	-.00005
	(.0011)	(.0011)
Father Veteran × Age at Exposure=8	.00041	.00035
	(.00102)	(.00102)
Father Veteran × Age at Exposure=9	-.00046	-.00047
	(.00101)	(.00101)
Father Veteran × Age at Exposure=10	-.00077	-.00079
	(.00101)	(.00101)
Observations	5185600	5185600
R-squared	.01048	.01075
Mean DV	0.060	0.060
P-Value	0.269	0.310
F-Stat	1.225	1.164
Fixed Effects	✓	✓
Individual Covariates		✓

Notes. Robust standard errors are in parentheses. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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

Appendix Table E-2 - The Association between Exposure Measures and Successful Census-DMF Data Linking Using Group Coefficients

	Outcome: Successful DMF-Census Merging	
	(1)	(2)
Father Veteran × Pre-1936 Age at Exposure	.00016	.00009
	(.00076)	(.00076)
Father Veteran × Age at Exposure=0	.00058	.00051
	(.00116)	(.00116)
Father Veteran × Post-1936 Age at Exposure	.00016	.00012
	(.00064)	(.00064)
Observations	5185600	5185600
R-squared	.01047	.01075
P-Value of Diff btw Pre and 1936 Exposure	.7039	.7109
P-Value of Diff btw Post and 1936 Exposure	.6971	.7199
Fixed Effects	✓	✓
Individual Covariates		✓

Notes. Robust standard errors are in parentheses. Regressions include county fixed effects (interacted with a linear trend in birth year), birth year fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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

Appendix F

Appendix Table F-1 reports the coefficients that are depicted in the bottom panel of Figure 5. Further, we group the fertility outcomes for years before and after 1936 and replicate these regressions. These results are reported in Appendix Table F-2. Column 1 shows the difference between father veteran and nonveteran for the outcome of being born in 1925-1935 years, conditional on covariates and fixed effects. In column 2, report the regression coefficient for fertility in 1936. In column 3, we report on birth between the years 1937-1940. All coefficients imply economically small and statistically insignificant coefficients. Moreover, we cannot rule out the quality of coefficients in column 1 versus column 2 (p-value 0.8) and column 2 versus column 3 (p-value 0.7).

Appendix Table F-1 - Exploring Changes in Fertility across Different Years among Veterans versus Non-veterans

	Outcome: Birth Year in:				
	1926	1927	1928	1929	1930
	(1)	(2)	(3)	(4)	(5)
Father Veteran	.00252***	.00344***	.00481***	.006***	.00233
	(.00055)	(.00072)	(.0009)	(.00107)	(.00147)
Observations	314037	314037	314037	314037	314037
R-squared	.0111	.0144	.01824	.0245	.02258
Mean DV	0.024	0.030	0.036	0.042	0.054
	1931	1932	1933	1934	1935
	(6)	(7)	(8)	(9)	(10)
Father Veteran	-.00124	.00449**	.00122	.00214	.00412
	(.00179)	(.00207)	(.00233)	(.00263)	(.00291)
Observations	314037	314037	314037	314037	314037
R-squared	.02661	.03633	.04075	.0546	.0673
Mean DV	0.062	0.070	0.071	0.079	0.082
	1936	1937	1938	1939	1940
	(11)	(12)	(13)	(14)	(15)
Father Veteran	-.00181	-.00822***	.01071***	-.00199	-.0028*
	(.00312)	(.00311)	(.00345)	(.00313)	(.0016)
Observations	314037	314037	314037	314037	314037
R-squared	.07566	.10213	.11054	.12855	.13344
Mean DV	0.080	0.079	0.073	0.065	0.015

Notes. Robust standard errors are in parentheses. Regressions include county fixed effects, birth month fixed effects, and dummies for father's age and mother's age in 1940. Family controls include dummies for father's education, mother's education, father's socioeconomic status in 1930, father's home ownership in 1930, number of children in 1930, age of the youngest and oldest children in 1930, father's literacy in 1930, father's employment status in 1930, and father's labor force status in 1930. Individual controls include dummies for race and ethnicity. All right-hand side covariates and fixed effects are interacted with father's WWI veteran status 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

Appendix Table F-2 - Exploring Fertility Differences between Veterans and Nonveterans for Grouped Years

	Outcome: Birth Year in:		
	1925-1935	1936	1937-1940
	(1)	(2)	(3)
Father Veteran	.00326*	-.00098	-.00229
	(.00195)	(.00118)	(.00178)
Observations	314052	314052	314052
R-squared	.07308	.00847	.06251
Mean DV	0.689	0.080	0.231
P-Value of Diff btw Pre and 1936 Exposure	0.573		
P-Value of Diff btw Post and 1936 Exposure		0.858	

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

Appendix G

Appendix Table G-1 summarizes selective statistics across consecutive sample selection steps. The 1st panel reports the means and standard deviation of the selected variables in the full count 1940 census (number of observations ~ 131M). In the 2nd panel, we restrict the data to male individuals only. Next, restrict the sample to individuals born between 1920 and 1940. In the 4th panel, we restrict the sample to individuals whose fathers are present and unobserved in the household. The 5th panel restricts the sample based on the father's age. We then merge this with the 1930 census in order to extract information on the father's World War I veteran status. Finally, the 7th panel reports summary statistics of the selected variables for the sample merged with the DMF data, i.e., the final sample of the paper. In the final sample compared with the full count 1940 census, we observe more white individuals and fewer black individuals. The share of homeowners is considerably higher in the final sample (51%) compared with the full count 1940 census (40%). Further, compared with the original 1940 census, the final sample consists of households with higher maternal education and paternal socioeconomic index.

Appendix Table G-1 - Descriptive Statistics across Consecutive Sample Selections

	Full count 1940 census			Males			Birth Year ≥ 1920			Father Present at Home		
	Observations	Mean	SD	Observations	Mean	SD	Observations	Mean	SD	Observations	Mean	SD
White	131849230	.89971	.30039	66108160	.90158	.29789	24062203	.88527	.31869	20392403	.90545	.29259
Black	131849230	.096	.29459	66108160	.09346	.29108	24062203	.10935	.31208	20392403	.08949	.28546
House Owner	143605771	.39529	.48891	66108160	.42779	.49476	24062203	.4004	.48998	20392403	.41123	.49206
Father's Years of Schooling	46478797	7.61169	3.68085	24280064	7.57557	3.67874	19925261	7.7478	3.65623	19925261	7.7478	3.65623
Mother's Years of Schooling	52348054	7.87328	3.4178	27171531	7.83563	3.42152	21455850	8.10161	3.34707	19352846	8.1791	3.31686
Father's Socioeconomic Index	43770708	26.48813	21.35566	22880304	26.3049	21.26138	19342384	26.11074	21.18135	19342384	26.11074	21.18135

	Father's Birth Year 1890-1900			Merged with 1930			Merged with DMF		
	Observations	Mean	SD	Observations	Mean	SD	Observations	Mean	SD
White	7414718	.91989	.27146	2679518	.93972	.23801	312495	.96691	.17886
Black	7414718	.07572	.26456	2679518	.05726	.23234	312495	.0306	.17222
House Owner	7414718	.46792	.49897	2679518	.49955	.5	312495	.51661	.49972
Father's Years of Schooling	7241241	7.61396	3.66671	2623560	7.99855	3.53769	306529	8.05756	3.371
Mother's Years of Schooling	7263779	8.00619	3.31385	2630726	8.37866	3.17101	307342	8.37844	3.01904
Father's Socioeconomic Index	7083395	27.87083	21.95542	2569053	28.9144	22.29183	299663	29.23692	22.10175

Notes.