Early-Life Economic Conditions and Old-Age Mortality: Evidence from Historical County-Level Bank Deposit Data*

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Abstract

This paper studies the long-run mortality effects of in-utero and early-life economic conditions. We examine how local economic conditions experienced in the Great Depression, proxied by county level banking deposits during in-utero and first years of life, can influence old-age longevity. We find that a one-standard-deviation rise in per capita bank deposits is associated with an approximately 1.7 months increase in longevity at old age. The effects are robust across a wide array of specification checks. Additional analyses comparing state-level versus county-level economic measures provide insight on the importance of controlling for local-level confounders and exploiting more granular

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measures in exploring the relationship between early-life conditions and later-life mortality.

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1. Introduction

Increases in life expectancy have been one of the most significant improvements in households' welfare over the last century (Cutler et al., 2006; Deaton, 2003).[7] In the past 100 years, Americans have enjoyed an overall gain of approximately 25 years in life expectancy at birth. Motivated by the growing body of research showing that conditions experienced in utero and early childhood are key determinants of health and human capital outcomes later in life (Almond et al., 2018; Almond & Currie, 2011; Barker, 1990), we provide new evidence on the relationship between early-life economic conditions and old age mortality, by focusing on the most severe economic recession in American history, the Great Depression.

Using micro-level data on the universe of deaths from 1975 and 2005 obtained from the Social Security Administration death records and linked with the 1940 U.S. Complete Count Census, this paper analyzes how individual longevity is affected by local economic shocks before birth and during the first years of life. We proxy local economic conditions with annual, county-level per capita bank deposits.[8] We focus on this measure because financial distress in the banking system played a major role in propagating the contraction of economic activity and employment during the Great Depression (Bernanke, 1983; Fisher, 1933; Friedman & Schwartz, 1963). Therefore, our measure captures the severity of the economic shock experienced by households

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[7] Several explanations have been provided in the literature for such a staggering improvement, including rising incomes and economic development (Acemoglu & Johnson, 2007; Costa, 2015; Fogel, 1994; Preston, 1975), the introduction of new drugs and scientific innovations (Bleakley, 2007), and/or public health investments (Anderson et al., 2022; Cutler & Miller, 2005).

[8] The deposits reflect both the supply and the demand of credit in the local area. For instance, an economic downturn destroys local jobs and reduces earnings and subsequent savings. On the other hand, it also affects market expectations about the future of the economy and influences the decisions of firms in their demand for credit. However, we show that the equilibrium quantity co-moves strongly and significantly with alternative measures of the economy such as income and retail sales

across different regions while, unlike all other measures of economic conditions used in the literature, having the advantage of being available at the county and year level prior to 1929.

Our empirical model uses a two-way fixed effects strategy that compares the longevity of cohorts born before, during, and after the Great Depression and who were differentially exposed to local economic shocks around the time of birth, controlling for county-level time-invariant characteristics, temporal shocks, and within-state county-group-by-year fixed effects. Our findings suggest that a one-standard-deviation increase in bank deposits during the in-utero period, which is equivalent to roughly 4 times the drop in deposits between the years 1929-1933 (peak-to-trough of the Great Depression), is correlated with a roughly 1.7 months higher age at death in old age. The effect is quite robust across a wide array of specifications. To assess whether these relationships are an artifact of overall trends in health improvements/disruptions, we implement a placebo test and show that the effects become indistinguishable from zero for deposits at pre-prenatal ages. We also argue that these effects are not driven by endogenous demographic changes in the sample due to changes in fertility, early-life survival, or migration. Additional analysis suggests that improvements in educational attainments and income in adulthood are potential mechanism channels. However, using estimates from previous studies, we posit that these channels only partially reflect the pathways.

The motivation of the current study stems from the limited evidence of in-utero economic conditions and later-life longevity. Van Den Berg et al. (2006) were the first to document the adverse effects of national economic conditions around birth on life expectancy using historical data for the Netherlands. The authors found that cohorts born during an economic boom lived 1.6 years longer (or 4 percent longer relative to the life expectancy of 39 years) than those born during economic recessions. No effects were found when booms were experienced during early childhood. Similarly, Lindeboom et al. (2010), also using data from the Netherlands, showed that children born during the Potato famine of the mid-nineteen hundreds lived 2.5-4 fewer years as adults compared to those born before the nutritional shock and with more pronounced impacts for children from lower class families.

We contribute to the literature in several ways. First, we contribute to a small literature that analyzes the link between economic conditions and mortality in the context of the Great Depression that has found mixed results. While Granados et al (2009) showed a negative correlation between the GDP per capita and the national mortality rate, Stuckler et al. (2012) found no effect between changes in bank suspensions and changes in mortality except for an increase in suicide rates. Fishback et al. (2007) in contrast, showed a small decline in death rates during the 1930s due to increases in New Deal spending. Cutler et al. (2007) found little evidence that early life exposure to the Depression affected long-term health (including mortality) using longitudinal data from the Health and Retirement Study (HRS) linked to regional-level macroeconomic data. More recently, however, Schmitz & Duque (2022) and Duque & Schmitz (2021) revisited this question in the HRS using macroeconomic data linked to the state of birth and found improvements in the magnitude and precision of the effects on old age health and mortality when economic outcomes were measured at the state level as opposed to the regional level. Importantly, these effects were localized to the in utero period specifically as opposed to the pre-conception, postnatal, childhood, or early adolescent periods. Thus, from an empirical perspective, we also contribute to the literature by using longitudinal bank deposit data measured at the county level before and during the Great Depression to explore within-state geographic variation in economic conditions, as opposed to prior studies that relied on region-level and state-level variations in the shock. In addition, our data source contains millions of observations, which adds power to our statistical tests and allows the research design to search for potential heterogeneity in the effects across different demographic groups.[9]

The rest of the paper is organized as follows. Section 2 reviews the literature. Section 3 introduces data sources. Section 4 discusses the empirical framework. Section 5 reviews the results. Section 6 explores endogenous fertility. Section 7 suggests potential mechanism channels. Section 8 concludes the paper.

2. Literature Review

A growing body of research evaluates the long-term effects of early life adversities (Almond et al., 2018; Almond & Currie, 2011; Currie, 2009; Hayward & Gorman, 2004; Steptoe & Zaninotto, 2020). Several studies provide suggestive evidence for the relevance of in-utero and early-life conditions for short-term and later-life outcomes, including child mortality (Baird et al., 2011; Schoeps et al., 2018), cognitive development (Chang et al., 2022; Majid, 2015; Yamashita & Trinh, 2022), test scores (Almond et al., 2015; Shah & Steinberg, 2017), education (Aizer, Stroud, et al., 2016; Caruso & Miller, 2015), adulthood earnings (Black et al., 2007; Currie & Rossin-Slater, 2015; Hoynes et al., 2016), and health outcomes over the life cycle (Fletcher, 2018; Fletcher et al., 2010; Fletcher & Noghanibehambari, 2022; Goodman-Bacon, 2021; Miller & Wherry, 2019; Noghanibehambari & Engelman, 2022; Persson & Rossin-Slater, 2018). Early-life conditions could operate through these mediatory pathways to influence the trajectory of old-age longevity. For instance, den Berg et al. (2015) employ a longitudinal panel of observations in Dutch registries covering about two centuries and showed that men who were born during an

[9] Our paper also contributes to an emerging literature that has looked at the role of economic resources–through specific welfare programs—on longevity. Two recent studies by Aizer, Eli, et al. (2016) and Aizer et al. (2020) that focused on the introduction of the U.S. Mother's Pension program in 1937 and the largest youth training program in history implemented during the New Deal, respectively, showed significant effects of these government interventions on beneficiaries' life expectancy.

economic boom (versus a recession) are more likely to be married during adulthood and at old ages and have a lower risk of mortality. They argue that, among men, marriage has a protective effect against mortality. Grimard et al. (2010) use data from Mexico and show that socioeconomic status measures during childhood significantly affect old-age health outcomes even after accounting for education and income. Bengtsson & Broström (2009) use data from Sweden and show that early life disease loads affect old-age mortality and socioeconomic status. However, they do not find evidence that the early-life health environment effect on later-life mortality operates through wealth and socioeconomic channels. Gagnon & Bohnert (2012) employ data from Canada and show that family wealth and the socioeconomic status during early life affect mortality during old ages among males. Their results fail to provide evidence of this association among females.

Hayward & Gorman (2004) use data from the National Longitudinal Survey of Older Men and show that men's mortality is correlated with an array of early-life and childhood conditions, including parents' socioeconomic status, mother's marital status, mother's labor force status, and parents' nativity. Montez & Hayward (2011) use the Health and Retirement Study to explore early-life family socioeconomic status on later-life mortality. They find significant positive correlations between risks of mortality during adulthood and a series of early-life adversities, including perceived poverty during childhood, having a low-educated father, and self-reported poor childhood health. On the other hand, Myrskylä (2010) finds weak and modest effects of early-life conditions on adult mortality using data from several developed European countries. The results suggest a strong correlation between period effects and mortality rather than early-life effects.

Studies that examine the role of economic conditions on later-life mortality outcomes exploit measures of economic conditions at various levels of aggregation and find different results.

For instance, Van Den Berg et al. (2006) use historical longitudinal data from the Netherlands to explore economic conditions in early life on old-age mortality. They exploit the cyclicality of national Gross Domestic Product (GDP) as a proxy for economic conditions and find that being born during a boom versus a recession results in 8 percent lower mortality rates. Arthi (2018) explores the persistent effects of in-utero and childhood exposure to state-level measures of the Dust Bowl, a devastating environmental shock followed by agriculture failure and reductions in income, on later-life human capital and health. The result suggests long-lasting effects on income, disability, and college completion. Similarly, Duque & Schmitz (2021) and Schmitz & Duque (2022) find a connection between state-level in utero exposure to wages, employment, and car sales during the Great Depression and late-life aging outcomes and mortality. However, an earlier paper by Cutler et al. (2007) that used exposure to economic variation at the census-region level during the Great Depression failed to find any evidence that fetal exposure to economic conditions was associated with disability and chronic disease later in life. Atherwood (2022) implements county-level Dust Bowl measures and explores the effects of young adulthood exposure on later-life longevity. He finds insignificant average effects. Noghanibehambari & Fletcher (2022) re-examine the effects of early-life exposure to the Dust Bowl on old-age longevity. They employ difference-in-difference method and account for county-level heterogeneity. They find intent-to-treat effects of about 1 month reduction in longevity among cohorts that were severely affected by their county-of-birth topsoil erosion in early life.

3. Data and Sample Selection

The primary data source is the Social Security Administration Death Master File (hereafter DMF) records extracted from the CenSoc Project database (Goldstein et al., 2021). The DMF data contains death records among males between 1975-2005 that are linked to the full-count 1940 census. Therefore, they have a wide array of early-life social and economic variables. There are three advantages of DMF data over similar data sources that contain mortality information necessary for our research design. The availability of county identifiers in the 1940-census allows for more granular and detailed environmental information as opposed to virtually all other data sources with state identifiers. Second, the DMF builds a longitudinal panel that contains millions of observations while similar longitudinal data provides several thousand (e.g., Health and Retirement Study). Third, the DMF-census-linked data offers a wide array of family-level covariates, including parental education and a socioeconomic score that can be used in our balancing tests, in analysis of heterogeneous impacts by family resources, and adds the robustness to our identification strategy.

We proxy local economic conditions using changes in bank deposits compiled by the Federal Deposit Insurance Corporation (FDIC) and taken from Manson et al. (2017). The data reports total annual deposits in all state and federal banks in each county (except Wyoming and DC) over the years 1920-1936 as of December of each year. Our choice of this proxy is based on two facts. First, later in the paper, we will illustrate the positive association between changes in bank deposits and similar proxies of state-level and county-level economic covariates. Second, similar studies show an association between banking crises and city-level economic conditions during a similar period (Stuckler et al., 2012).

To infer county of birth from the DMF-census sample, we take two approaches. First, we use cross-census linking rules provided by the Census Linking Project to merge 1940-census records with 1930-census. In our sample, we are able to match 48 percent of cohorts born 1926-1930 to their 1930 census records. For these matched records, we use county-of-residence in 1930 as county-of-birth. For unmatched cohorts of 1926-1930, and for all cohorts of 1931-1936, we continue to infer the county-of-birth using the second approach. Specifically, we use information from three variables reported in 1940-census. First, we use information on the county of residence in 1935 as the benchmark proxy for location of birth using the fact that the 1940 Census reports the migration status from five years prior. Second, we use county of residence in 1940 if the individual reported having stayed in the same house since 1935. Third, if the migration status is missing and the person's state of birth is the same as state of residence in 1940, we again use county of residence in 1940 as the proxy for the county of birth. To further reduce the migration issue, we limit the sample to children up to 15 years old as older children usually leave their original household. Doing so will limit the sample to cohorts born after 1926.

Since our purpose is to explore in-utero exposures, we calculate a weighted average of deposits for the nine months before birth, assuming an average of nine months of gestation. In so doing, we assign the current year of deposits to our in-utero deposit measure for months of October through December. For other months, the in-utero measure is a weighted average of current and previous year's deposits, where weights are based on the number of months of a year that overlap with the last nine-month period. For instance, March deposits takes three-ninths of the current year's deposits and six-ninths of the previous year's deposits. We then merge DMF-census sample to bank deposit data based on inferred county-of-birth and the weighted average of previous nine-month deposits.

To control for other county-level sociodemographic changes, we use full-count decennial censuses from Ruggles et al. (2020) for the decennial years 1920-1940. We then linearly interpolate covariates for inter-decennial years. Moreover, for the analysis to explore the association of bank deposits with other economic variables, we use county-level retail sale per capita and state-level income per capita extracted from Fishback et al. (2007).

The final sample includes 1,221,113 individuals from 3,042 counties in 47 states born between 1926-1936 who died between 1975-2005.[10] Summary statistics of the final sample are reported in Table 1. The average per capita bank deposit is $331. Over the sample period, roughly 10 percent of individuals are born in counties that experienced a 5 to 10 percent drop in total banking deposits relative to the county-specific previous year's value. About 23 percent of individuals live in counties that experienced a drop of more than 10 percent in deposits. The top and middle panels of Figure 1 depict the cross-sectional geographic distribution of per capita deposits and per capita retail sales. There is a visual correlation between our proxy for economic conditions (bank deposits) and a measure of local consumption expenditure (retail sales per capita). The bottom panel of Figure 1 illustrates the distribution of age-at-death by county-of-birth for cohorts born in 1930. There is also a visual correlation between these two measures and long-run longevity. In a cross-sectional and correlational manner, Figure 3 depicts the differences in the density distribution of age at death in the subsample of above-median per capita deposit (in green) versus below median per capita deposit (in red). While these are suggestive figures, they do not convey any informative interpretation of the statistical association.

4. Econometric Method

Our identification strategy exploits within-county and over-time variations in bank deposits. Specifically, we implement regressions of the following form:

$$DA_{icsb} = \alpha_0 + \alpha_1 PCBD_{csb^*} + \alpha_2 X_{icsb} + \alpha_3 Z_{csb} + \xi_c + \zeta_b + \eta_{sb} + \varepsilon_{icsb}, \quad (1)$$

Where the outcome is age at death (DA) of individual i born in county-of-birth c State Economic Area s and birth year b. State Economic Areas (SEA) are geographic boundaries that

[10] The bank deposit data does not include data for Wyoming and DC. Also, the 1940 census does not include Alaska and Hawaii. These states are therefore omitted from the sample.

covers several counties within the same state that have similar economic and demographic conditions (Bogue, 1951). SEA was introduced in census 1950 and then was applied to counties in 1940 census. Since the period of the Great Depression was accompanied by vast changes in economic conditions, we prefer exploiting within-SEA across-counties variations to better isolate the impacts of economic conditions. The parameter $PCBD$ represents per capita bank deposits assigned to each individual based on county-of-birth and the average of nine months leading to birth (b^*). To ease the interpretation, we standardize this variable with respect to the mean and standard deviation of the sample. In X, we include as individual controls dummies for race, gender, and ethnicity. The matrix X also contains parental characteristics, including dummies for maternal education, paternal education, and socioeconomic status. The parameter Z represents a series of county-by-birth year covariates constructed based on full-count decennial censuses 1920-1940 and interpolated for inter-decennial years. These covariates include the share of homeowners, the share of married people, and the average occupational income score. The county fixed effects, represented by ξ, control for time-invariant unobserved features of counties. To account for temporal cohort-level changes in longevity, we add birth cohort fixed effects, represented by ζ. To account for all SEA-by-year divergence in the outcome and other time-varying local determinants, we include SEA-by-birth-year fixed effects represented by η. Therefore, the identifying variation comes from changes in bank deposits across counties within an SEA-year. Finally, ε is a disturbance term. We cluster standard errors at the county level to account for serial correlation in the error term. The coefficient of interest is α_1 that, conditional on covariates and fixed effects, captures the effect of one-standard-deviation (from mean) change in per capita bank deposits on later-life old-age longevity.

5. Results

5.1. Endogeneity Concerns

One potential concern in our analysis is that economically improving areas attract more people and induce migration. Similarly, a recession may affect different areas to varying degrees and generates in/out-migration. If certain characteristics in migrant subpopulations correlate with their later-life health and longevity, the link between deposits and longevity is contaminated by endogeneity. For instance, if whites are more (less) likely to migrate after a county is hit by a recession, the coefficients of equation 1 can overstate (understate) the true effects since whites have higher longevity for reasons not necessarily captured by a race dummy. To explore this source of bias, we ask whether changes in bank deposits are associated with changes in observable characteristics, conditioning on county and SEA-year fixed effects. The results of such balancing tests are reported in Table 2. We do not observe any statistical association between bank deposits and probability of being white, mother's schooling, father's schooling, and father's socioeconomic score. Moreover, the estimated effect sizes are economically small. For instance, based on percent change from the mean of the dependent variable reported in the fifth row, the effects suggest that a one-standard deviation change in bank deposits is correlated with 0.27 percent change in white, 0.15 percent change in maternal schooling, and 0.32 percent change in paternal socioeconomic score. Therefore, the big picture extracted from these numbers is a failure in finding robust and strong evidence of demographic and socioeconomic changes due to changes in bank deposits that could unbalance the sample by contaminating the long-run relationships.

Another concern is that linkage rates between DMF and the 1940-census may be predicted by early-life shocks, such as changes in deposits. While the linking rules are primarily based on name commonality, and we have little prior concern for this being correlated with local economic conditions, we explore merging issues empirically by evaluating the correlation between the merging of DMF-census records and deposits. In so doing, we start by imposing sample selections as discussed in section 3. We also follow the procedure described in section 3 to infer county-of-birth. The sample selections result in 22,285,131 observations before merging with the DMF death records. The successful merge dummy takes a value of one if DMF is merged with the census records and zero otherwise. We then regress this variable on deposits per capita, conditional on a full specification of equation 1. The results are reported in Table 3 for the full sample, sample of whites, sample of people with low educated mothers, and sample of persons with low socioeconomic score fathers, in columns 1-4, respectively. We find small and insignificant coefficients between deposits and successful merging. For instance, a one-standard-deviation change in banking deposits per capita results in an insignificant 0.8 basis point increase in the probability of merging, equivalently 0.15 percent change from the mean of the outcome.

5.2. Main Results

The main results of the paper are reported in Table 4. We start with a model that only includes county and SEA-by-birth-year fixed effects in column 1 and gradually add additional covariates to the model across consecutive columns. The marginal effect of bank deposits per capita is virtually unchanged across specifications. Since there is enough evidence to believe that race/gender (as individual covariates), mother's education and father's socioeconomic index (as parental characteristics), and county-level demographic and economic covariates are significant determinants of long-run health outcomes, the fact that coefficients do not change across columns lends credibility to the exogeneity of bank deposits, at least with regards to the observable and available variables. The results of the full specification of column 4 suggest that, on average, a one-standard-deviation change (from the mean) in per capita deposits in utero is associated with 1.7 months higher longevity during old ages. We put this number into perspective by comparing it with the coefficients of other variables. For instance, the marginal effect of a black dummy (not reported here) is -15.3 (se=1.1). Therefore, a one-standard-deviation change in bank deposits is equivalent to roughly 11 percent of the black-white gap in longevity. The difference in average life expectancy between the US and other OECD countries is 49.2 months (76.6 years versus 80.7 years, respectively). Thus, the impact of a one-standard-deviation rise in bank deposits is equivalent to roughly 3.5 percent of the US-OECD-countries gap in longevity.

Another way to understand the magnitude of the finding is to compare with other determinants of longevity and the impact of other early-life exposures. For instance, Chetty et al. (2016) investigate the income-longevity relationship in the US using individual tax returns linked with death records. They document that each 5-income-percentile is associated with about 0.8 months higher age-at-death. Therefore, the effect of a one-standard-deviation rise in bank deposits in county-year-of-birth is equivalent to the impact that moving up the income ladder by about 1 percentile has on longevity. Halpern-Manners et al. (2020) examine the education-mortality relationship using twin fixed effect strategy. They document that each additional year of education is associated with about 4 months higher longevity. Therefore, the marginal effect of Table 4 is equivalent to the effect of 0.43 years of schooling on longevity. Aizer, Eli, et al. (2016) examine the impacts of the Mother's Pension (MP) program, a state-local government joint initiative to help poor single mothers with cash transfers prior to social security era, on children's old-age longevity. They find treatment-on-treated effects of about 1-year additional life to children whose mothers were selected for the MP benefits. The MP benefits usually lasted for three years and transferred about 30-40 percent of pre-transfer maternal income. Therefore, our finding is equivalent to a one-time transfer of 13-17 percent of income to poor families.

5.3. Effects during Pre-prenatal and Postnatal Periods

While the literature points to the relevance of conditions in utero for later-life outcomes, several studies also show associations between post-birth exposures and conditions and long-run outcomes (Chyn, 2018; Currie & Rossin-Slater, 2015; Ludwig & Miller, 2007). This section complements the main results by investigating the effects of bank deposits experienced during postnatal ages and later-life longevity. Moreover, we also examine the association between changes in deposits during pre-prenatal period on longevity. The idea is that if the deposits are capturing general improvements in health conditions rather than in-utero economic condition shocks, we would observe similar effects for the exposure measures assigned for pre-prenatal period. Therefore, this test provides a placebo check to assess the validity of the main results.

In so doing, we generate two new variables for assigning deposits at two years up to gestation and two years after birth. We include these two variables as well as our primary in-utero exposure measure in the full specifications of equation 1 and allow for the effects of these variables to compete within a regression. The results are reported in Table 5. Across specifications, the coefficients of the pre-prenatal development period suggest small and insignificant correlations providing support for the empirical strategy. The correlations between bank deposits and postnatal ages are also small in magnitude (relative to the deposit's assignment during in-utero), and their coefficients are statistically insignificant. The effect of exposure during in-utero suggests positive, relatively large, and statistically significant association between deposits and old-age longevity.

5.4. Exploring the Relevance of County-Level Variations

As we discussed in section 1, an advantage of our study compared to the previous literature is in part that we use a more granular measure of local economic conditions (i.e., county-level) versus national, census-region, or state-level measures of other studies (Arthi, 2018a; Atherwood, 2022; Cutler et al., 2007; Van den Berg et al., 2015; Duque & Schmitz, 2021; Granados et al., 2009; Myrskylä, 2010b; Schmitz & Duque, 2022; Scholte et al., 2015; Van Den Berg et al., 2006; Van den Berg et al., 2009). To show that this granularity is essential in this context, we explore the correlations of economic conditions at the state level with longevity in our sample. Specifically, we use state-level income per capita and deposits per capita aggregated at the state level. Since the state-level income per capita is available only from 1929, we restrict the sample to cohorts of 1929-1936. In all regressions of this section, we include individual and family covariates.

As reported in column 1 of Table 6 the unconditional correlation between state-level income per capita and death age is 9.4 months. However, a large portion of the observed correlation can be explained by unobserved state and cohort characteristics. Controlling for state and cohort time-invariant confounders reduces the coefficient by about 86 percent (column 2). Next, we aggregate the deposits from the county level to the state level and show the correlations with age at death in columns 3-4.[11] The correlation is 35.5 months (column 3). The marginal effect becomes smaller (about the same 80% reduction as in Columns 1 and 2) and noisy once we include state and cohort fixed effects (column 4).

In the next step, we use county-level deposits per capita and replicate the results across different specifications. In column 5, we show that the correlation (excluding fixed effects) is about 0.4 months and statistically insignificant. Adding state-fixed effects provides a negative and small coefficient. These results suggest the existence of state and county level variables correlated with bank deposits that have offsetting effects, again suggesting the need to control for county level effects and focus on within-county changes in economic conditions (as measured by bank deposits).

[11] We use county-level per-capita deposits and use a weighted average of this value for each state where weights are mean county population over the sample period.

In column 7, we include county fixed effects. The correlation between county level economic circumstances at the time of birth and later life longevity becomes positive and relatively large in magnitude. In column 8, we control for other local-level confounders by including SEA-year fixed effects. The results suggest an increase of 2.9 months due to a one-standard-deviation change in bank deposits.

5.5. Validity of Bank Deposits as a Proxy for Economic Conditions

To gauge the magnitude of the paper's main results and to explore the validity of bank deposits as a proxy for local economic conditions, we explore the relationship between per capita deposits and other local and state-level economic variables. The main limitation of this exercise is the scarcity of local-level data for the time period of this study. We know of no dataset that contains measures of county-level income or county-level unemployment rates during this time period (and that starts prior to the Great Depression). However, income data is available at the state level for the years 1929-1940. Also, retail sale data is available at the county level which is, arguably, a reasonable measure of consumption. These data are available for post-1929 years and are taken from Fishback et al. (2007). Thus, the analysis sample for both retail sales and income cover the years 1929-1936 since our sample ends in the year 1936.

We merge per capita retail sales at the county and year level with our bank deposit sample and implement regressions similar to equation 1. The results are reported in columns 1-3 of Table 7. We start by showing the unconditional correlation in column 1, adding county and year fixed effects in column 2, and then implementing a full specification in column 3. The unconditional correlation suggests a very strong co-movements between bank deposits and retail sales. However, fixed effects explain a large portion of this correlation. The correlation of the full model of column 3 that includes SEA-by-year fixed effects is still statistically and economically significant. A one-standard-deviation rise in per capita deposits is associated with 0.15 standard-deviations increase in per capita retail sale.

For the state-level income analysis, we aggregate our final sample at the state-level and merge it with income data at the state-year level and implement regressions that include state, year, and region-by-year fixed effects. These results are reported in columns 4-6 of Table 7. The full specification of column 6 implies that for a one-standard-deviation change in deposits per capita the income per capita changes by 0.4 standard deviations. Between the years 1929 and 1933 (peak to trough of the Great Depression), income per capita decreased from \$611 to \$326, a decrease of about 1.5 times its standard deviation over the sample period. Using figures from Table 7 and this drop in income, one can deduce a roughly 6.1 months drop in later-life longevity.[12] This is an economically large effect.

5.6. Robustness Checks

In Table 8, we explore the robustness of the main results to alternative specifications. In column 1, we replicate the results of the full specification of Table 4 as the benchmark comparison. In column 2, we allow for the time-invariant effects of counties to vary by individual covariates. In column 3, we add county-by-parental-characteristics fixed effects so that the unobserved time-invariant features of a county can be absorbed differently by families with different sociodemographic backgrounds. The resulting marginal effects are almost identical to the main results. In column 4, we control for all unobserved county characteristics that evolve linearly over cohorts by including county-by-birth-year linear trend. The effect rises and remain statistically significant.

[12] This number is calculated using the marginal effect of column 4 of Table 4 (1.7), the marginal effect of column 6 of Table 7 (0.42), and the change of 1.5 standard-deviations in state-level income, as follows: $\left(\frac{1}{0.42}\right) \times 1.5 \times 1.7$.

In column 5, we control for seasonality in birth and death outcomes by adding birth-month and death-month fixed effects. The resulting coefficient is comparable to that of column 1.

In column 6, we check for robustness of the functional form by replacing the outcome with the log of age at death. The effect suggests 0.2 percent change in the outcome as a result of one-standard-deviation change in deposits, an effect that is almost identical to the percent change effect shown in column 4 of Table 4. In column 7-8, we replace the outcome with a dummy to indicate longevity beyond age 70 and 65, respectively. A one-standard-deviation increase in in-utero bank deposits per capita is associated with 62 and 53 basis-points higher likelihood of living beyond age 70 and 65. These effects are equivalent to roughly 2.6 and 1.1 percent change from the mean of their respective outcomes.

In columns 9, we check for sensitivity of county-level clustering. We find that the errors are larger when we implement a two-way clustering by county and state-year. However, the resulting coefficient is still statistically significant at the 5 percent level.

5.7. Alternative Measures

In Table 9, we replicate the main results using alternative measures of banking conditions. In column 1, we ignore the differences in county population as a deflating channel for the effects of deposits and use total deposits as the main independent variable. The resulting marginal effect suggests 2.2 months higher longevity as a result of a one-standard-deviation change in deposits. In column 2, we replace the independent variable with two dummy indicating that banking deposits in a specific county and year have dropped between 5-10 and more-than-10 percent relative to the county's previous year's deposits, candidate measures of the banking crisis. The result suggests a reduction of 0.6 months in longevity for both measures of banking crisis. We should note that 33 percent of cohorts have experienced a banking crisis during their prenatal period, per our definition of crisis (see Table 1). Overall, these results add to the general picture that the economic conditions of early life have significant effects on later-life longevity.

5.8. Heterogeneity by Subsamples

In Appendix A, we replicate the main results across two subpopulations: whites and blacks. We find that the longevity of black people is more strongly connected with economic conditions. The marginal effect of black sample is roughly 2.5 times that of the white sample, although the effects in both samples are significant.

During the period of the study and specifically for post-1934 years, many counties in the Southern Plains region experienced the Dust Bowl. We examine how the effects vary by Dust Bowl exposure counties in Appendix C. We find that for counties exposed to the Dust Bowl, the correlations are about five times larger, though statistically insignificant. The effect on other counties is almost identical to that of the main results of the paper.

5.9. Sensitivity to Death Window and Gender Selection

The DMF reports death records for males in the years from 1975 to 2005. As an alternative source of data that covers both genders, we use Numident death records of the Social Security Administration extracted from the Censoc Project (Goldstein et al., 2021). Numident is also linked to the 1940 census but reports the death to both females and males for death years 1988-2005. We explore whether the results are sensitive to gender selection and death selection of DMF in Appendix B. We show that when we restrict the sample to Numident death years (i.e., 1988-2005) the effect drops by about 65 percent (column 2 of Appendix Table B-1). This is quite comparable to the Numident results (column 5 of Appendix Table B-1). Therefore, the effects are larger as we expand the death window to cover earlier deaths, suggesting that the Great Depression accelerated the age of mortality.

Looking at the male subsample of Numident reveals an effect of 0.8 months while the female subsample suggests an insignificant effect of 0.2. Therefore, the results are primarily driven by males, suggesting that early-life economic conditions are more relevant to the health of males than females. This fact is in line with studies that show the exposures in early-life are more impactful for males (Clark et al., 2021; Clay et al., 2019; Rosa et al., 2019; Smith et al., 2011; Wang et al., 2017; Weinberg et al., 2008).

6. Fertility Response

While the balancing tests of Table 2 are inconsistent with strong demographic compositional changes due to the differences in the survival of subpopulations, one may be concerned that parents observe the economic condition and plan their fertility accordingly (Currie & Schwandt, 2014; Schaller, 2016; Schaller et al., 2020). However, the literature on economic conditions and fertility is not conclusive (Black et al., 2013; Cohen et al., 2013; Docquier, 2004). Moreover, little empirical research has been done for our study period (Fishback et al., 2007). To explore the selective fertility of parents to the observed deposit changes, we use county-level births data over the years 1926-1936 extracted from Bailey et al. (2016). The data offers three main variables that are specifically useful for the analysis of this section: general birth rate, share of births to whites, and share of births to blacks. Since over time more counties appear in the sample and the effects may be driven by differential fertility of new counties, we balance the county-year sample so that each county has appeared in at least 5 years. This leaves us with roughly 896 counties. We merge this with deposit data and implement regressions that include the county and SEA-year fixed effects. The results are reported in Table 10. Deposits are positively associated with birth rates. A one-standard-deviation change in per capita deposits is associated with 0.18 additional births per 1,000 women in the county, equivalent to roughly 0.5 percent rise in the mean of the outcome, a quite small change. Although we find evidence of procyclical fertility, the results fail to provide evidence of compositional changes in births. Specifically, there is no change in the share of births to white and black mothers (columns 2 and 3). The marginal effects are statistically insignificant and economically small, as implied by percent changes from the mean of the outcome. For instance, a one-standard-deviation rise in deposits is associated with a three basis-point decrease in the share of births to whites, roughly 0.05 percent change from the mean. If we believe that whites have higher longevity for unobservable reasons, then the associated reduction in their fertility could understate the effects in the main results.

Economic conditions could also affect the survival of fetuses into birth. Fetal death selection is disproportionately higher among males to a degree that sex ratio at birth has been used as a proxy for fetal death (Sanders & Stoecker, 2015). We explore the effects of changes in local economic conditions on sex ratio at birth using the 1940 full-count Census. We report and discuss the results in Appendix D. We find insignificant effects that are very small in magnitude. We further examine selective fertility response of parents to changes in deposits based on maternal education. These results, also reported in Appendix D, do not provide a consistent and strong evidence of selective fertility behavior by maternal education.

7. Mechanism

The results so far suggest that early-life economic conditions have moderate and robust effects on later-life longevity. To establish a candidate mediatory link, we explore the effects on later-life education-income profiles. However, in the 1940 Census, the cohorts of our final sample (born in 1926-1936) had not completed their education. In addition to this issue, post-1940 censuses do not provide county identifiers. To overcome this problem, we use the 1960 census in which we have a below-state geographic identifier: Public Use Microdata Area (PUMA).

PUMA is a Census-defined geographic boundary that identifies places based on their population. In urban areas with a higher population (and population density), a county contains several PUMAs. In rural areas with a lower population, several counties are grouped to form one PUMA. We convert our deposit data into PUMA level by aggregating the deposits for several-county PUMAs and assigning similar values to different PUMAs within a county that covers several PUMAs. We then merge this with observations in the 1960 census based on PUMA and birth year. To alleviate the migration issue, we restrict the sample to individuals whose state of birth is the same as state-of-residence in 1960. We also restrict the sample to cohorts born between the years 1928-1936.[13] We implement regressions that include, in addition to individual covariates, PUMA and state-birth-year fixed effects.[14] The results are reported in Table 11 The results suggest a strong statistical association between per capita deposits in the birth year and educational outcomes and measures of the socioeconomic index. For instance, a one-standard-deviation rise in deposits is associated with 0.04 additional years of schooling (column 1), 57 basis points increase in the probability of any college education (column 2), $27 higher wage income (column 6), and 0.18 units increase in the socioeconomic score (column 8). We can scale up these effects using changes in state-level income per capita from peak to trough of the Great Depression (years 1929-1933) and its link to deposits as discussed in section 5.5. Such changes in deposits are associated with about 0.13 years drop in years of schooling, roughly 2 percentage-points fall in the probability of college education, and 0.6 units drop in socioeconomic score.

[13] We use the variable indicating birth year in the 1960 censuses to identify cohorts born between 1928-1936. The reason behind this cohort selection is to have a sample of cohorts similar to the DMF-census-linked sample used in the main analysis of the paper.
[14] Since in many cases PUMA contains several SEAs, we most of the identifying variation comes from PUMA-year level, we avoid using SEA-year fixed effects. Instead, we include state-year fixed effects.

We can use the values reported by similar studies to understand how much of the effects could operate through these channels. Fletcher & Noghanibehambari (2021) explore the effects of new college opening during adolescence on later-life longevity. They find that having a college education raises the age at death by about 1.6 years. Combining this figure with the marginal effect of column 2, one can deduce that a one-standard-deviation increase in deposits raises the age at death by 0.1 months if it solely operates through increases in college education. This number can explain only 6.4 percent of the observed reduced-form effect.[15] In another study to explore the effects of education on mortality, Halpern-Manners et al. (2020) implement a twin-strategy and find that an additional year of schooling is associated with 0.34 years higher age at death. Using the coefficient of column 1, we can infer that, had only the effects operated only through improvements in schooling, a one-standard-deviation rise in deposits leads to 0.15 months increase in longevity, equivalent to roughly 8.8 percent of the reduced-form marginal effect in Table 4.[16]

8. Conclusion

The Great Depression was an extraordinary event in the economic history of the United States. From 1929 to 1933, real output contracted by more than 25 percent and the unemployment rate increased from 3.2 percent to 25 percent, reaching the highest levels ever documented. Despite its magnitude, previous literature has found little evidence that the Great Depression affected adult mortality. In this paper we provide new evidence on this link by using local banking deposits, as a proxy for economic conditions and credit market, during in-utero and year of birth can influence

[15] The treatment-on-treated calculation of Fletcher & Noghanibehambari (2021) suggests 1.6 years of increased longevity as a result of college education. We combine this number with the estimated effect of column 2 of Table 11 assuming that the effects solely operate through college education channel. Hence, a one-standard deviation rise in deposits is associated with 0.0057×1.6 years or 0.11 months of additional life. This number is 6.4 percent of the marginal effect of the reduced-form effect of deposits in longevity reported in column 4 of Table 4 ($100 \times 0.11/1.7$).

[16] This is calculated using 0.34 years effect of Halpern-Manners et al. (2020), column 4 of Table 4 (1.7), and column 1 of Table 11 (0.037), as follows: $\frac{0.34 \times 12 \times 0.037}{1.7} \times 100$

old-age longevity. We find that a one-standard-deviation rise in per capita bank deposits is associated with about 1.7 months higher age at death during old ages. The effect is statistically significant, economically meaningful, and robust across a wide array of specification checks.

A battery of balancing tests rules out significant changes in demographic and family socioeconomic characteristics associated with changes in deposits. Moreover, we fail to find any associations between deposit changes in postnatal ages and later-life longevity suggesting that only conditions in-utero and first year of life is important for later-life longevity. We also argue that endogenous fertility response of parents from different demographic groups does not affect the main results. Additional analysis suggests quite strong associations between bank deposits and retail sale and income per capita, which implies that banking deposits are indeed a reasonable proxy to capture local economic conditions. In addition, we show that improvements in education-income profile during adulthood are potential mechanism channels. However, we argue that between 6-9 percent of the link between early-life deposits and later-life longevity can be explained by modest changes in educational outcomes. These small effects on potential mediatory outcomes suggest that the economic conditions operate through other non-labor-market channels to impact longevity such as changes in health capital that can be detected in old ages.

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Tables

Table 1 - Summary Statistics

Variable	Observations	Mean	Std. Dev.	Min	Max
Death Age (Months)	1221113	772.66643	104.36921	457	959
Death Age (Years)	1221113	63.88002	8.71601	38	79
Birth Year	1221113	1930.1929	3.10607	1926	1936
Death Year	1221113	1994.5757	8.31481	1975	2005
Deposits per Capita (STD)*	1221113	0	1	-.58422	50.79202
Deposits per Capita	1221113	330.91799	566.43036	0	29101.057
Total Deposits ($1B)**	1221113	.30424	1.08423	0	12.59437
Drop in Deposits>5% & ≤10%***	1221113	.10077	.30102	0	1
Drop in Deposits>10%	1221113	.23087	.42139	0	1
White	1221113	.91466	.27939	0	1
Black	1221113	.08022	.27164	0	1
Father SEI 1st Quartile	1221113	.23911	.42654	0	1
Father SEI 2nd Quartile	1221113	.22718	.41901	0	1
Father SEI 3rd Quartile	1221113	.22679	.41876	0	1
Father SEI 4th Quartile	1221113	.20241	.40179	0	1
Father SEI Missing	1221113	.04182	.20017	0	1
Mother Education <HS	1221113	.60398	.48907	0	1
Mother Education =HS	1221113	.27552	.44678	0	1
Mother Education >HS	1221113	.04961	.21714	0	1
Mother Education Missing	1221113	.07089	.25664	0	1
Work-Related per Capita Relief Spending	1221113	5.00054	9.49932	0	376.46896
Average Homeownership	1221113	.50192	.13717	.02538	.90763
Share of Literate	1221113	.81183	.1943	0	1
Average Occupational Income Score	1221113	23.73044	4.10521	11.78472	29.7175
Share of Married Females	1221113	.60946	.03199	.28999	.73985
County-Level Data:					
Retail Sale per Capita	20258	174.9985	98.5671	0	782.43945
State-Level Data:					
Income per Capita	376	445.60084	195.13996	122.98853	1151.4171
The 1960-Census Data:					
Years of Schooling	403493	7.89761	2.93819	0	15
Educ>1 Year of College	403493	.17413	.37923	0	1
Educ>2 Year of College	403493	.13247	.339	0	1
Educ>3 Year of College	403493	.0963	.295	0	1
Educ>4 Year of College	403493	.07397	.26172	0	1
Wage and Salary Income	403493	1622.6333	1873.754	0	19866.486
Total Income	403493	1879.7341	2068.2311	-7906.8618	19866.486
Socioeconomic Index	342766	34.44022	21.7157	3	96
Occupational Prestigious Score	340401	35.80639	12.37729	9.3	81.5
Occupational Education Score	340346	18.55855	22.3739	.8	100

Notes. The statistics are weighted using county-level mean population.
* The abbreviation STD represents standardized variable.
** Total deposits measure average county-year total deposits in all banks reported in the data. Its unit in this table is billions of dollars.
*** Percent drop is the drop in a county's deposits with respect to the county's previous year's deposits.

Table 2 - Balancing Tests

	Outcomes:						
	White	Mother's Schooling	Mother's Education Missing	Father's Schooling	Father's Education Missing	Father's SEI	Father's SEI Missing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Deposits per Capita	-.00254	.012	.00072	.00918	.002***	-.10234	.00237**
(STD)	(.00161)	(.01309)	(.00132)	(.01603)	(.00068)	(.07191)	(.00098)
Observations	1221113	1133633	1221113	1068262	1221113	1040576	1221113
R-squared	.11896	.08817	.04105	.07459	.04562	.0515	.02668
Mean DV	0.944	8.125	0.063	8.083	0.016	32.052	0.043
%Change	-0.270	0.148	1.138	0.114	12.513	-0.319	5.504
County FE	✓	✓	✓	✓	✓	✓	✓
SEA-by-Birth-Year FE	✓	✓	✓	✓	✓	✓	✓

Notes. Standard errors, clustered at the county-level, are in parentheses. The regressions are weighted using county-level mean of population over the sample period. The "%Change" values are calculated using the estimated marginal effects of row 1 divided by the mean of dependent variables reported in row 4.

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

Table 3 - The Correlation between Successful Merging of DMF-Census data and Per Capita Deposits

	Outcome: Successful DMF-1940-Census Merging; Subsamples:			
	Full Sample	Whites	Mother Education less than High School	Father's SEI below Median
	(1)	(2)	(3)	(4)
Deposits per Capita (STD)	.00008 (.00009)	.00007 (.00009)	-.00006 (.00013)	.00011 (.00009)
Observations	22285131	19603408	12940827	10365020
R-squared	.06454	.06571	.06851	.06721
Mean DV	0.057	0.058	0.060	0.058
%Change	0.148	0.125	-0.107	0.188
County FE	✓	✓	✓	✓
SEA-by-Birth-Year FE	✓	✓	✓	✓
Family Controls	✓	✓	✓	✓
County Controls	✓	✓	✓	✓

Notes. Standard errors, clustered at the county-level, are in parentheses. The regressions are weighted using county-level mean of population over the sample period. The "%Change" values are calculated using the estimated marginal effects of row 1 divided by the mean of dependent variables reported in row 4.
*** p<0.01, ** p<0.05, * p<0.1

Table 4 - Main Results: The Association between in-Utero Bank Deposits and Old-Age Longevity

	Outcome: Death Age (Months)			
	(1)	(2)	(3)	(4)
Deposits per Capita (STD)	1.55965***	1.58676***	1.59411***	1.72552**
	(.57764)	(.59254)	(.585)	(.72026)
Observations	1221113	1221113	1221113	1221113
R-squared	.10219	.10312	.1036	.1036
Mean DV	772.939	772.939	772.939	772.939
%Change	0.202	0.205	0.206	0.223
County FE	✓	✓	✓	✓
SEA-by-Birth-Year FE	✓	✓	✓	✓
Individual Controls		✓	✓	✓
Family Controls			✓	✓
County Controls				✓

Notes. Standard errors, clustered at the county-level, are in parentheses. Individual controls include race and gender dummies. Parental controls include father's socioeconomic status dummies and mother's education dummies. County-by-birth-year covariates include share of white-collar workers, share of blue-collar workers, share of farmers, and share of literate people. The regressions are weighted using county-level mean of population over the sample period. The "%Change" values are calculated using the estimated marginal effects of row 1 divided by the mean of dependent variables reported in row 4.
*** p<0.01, ** p<0.05, * p<0.1

Table 5 - Assigning Deposits per Capita at Pre-prenatal, Prenatal, and Postnatal Periods

	Outcome: Death Age (Months)		
	(1)	(2)	(3)
Two Years Pre-Prenatal	-.54054	-.54144	-.43639
	(.65818)	(.66487)	(.82259)
During In Utero	2.14054**	2.13353**	1.96145**
	(1.07014)	(1.07366)	(.95119)
Two Years Postnatal	-.82156	-.83721	-.65997
	(.67746)	(.68141)	(.70487)
Observations	1142972	1142972	1142972
R-squared	.09109	.09161	.09161
Mean DV	776.140	776.140	776.140
County FE	✓	✓	✓
SEA-by-Birth-Year FE	✓	✓	✓
Individual Controls	✓	✓	✓
Family Controls		✓	✓
County Controls			✓

Notes. Standard errors, clustered at the county-level, are in parentheses. Individual controls include race and gender dummies. Parental controls include father's socioeconomic status dummies and mother's education dummies. County-by-birth-year covariates include share of white-collar workers, share of blue-collar workers, share of farmers, and share of literate people. The regressions are weighted using county-level mean of population over the sample period. The "%Change" values are calculated using the estimated marginal effects of row 1 divided by the mean of dependent variables reported in row 4.

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

Table 6 - Comparing the Effects of State-Level Economic Conditions with the Effects of Local Economic Conditions on Longevity

	Outcome: Death Age (Months)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7
State-Level Income Per Capita	9.3903***	1.3029*					
	(1.26957)	(.74357)					
State-Level Deposits per Capita			32.51253***	6.20618			
			(6.58206)	(6.03937)			
County-Level Deposits per Capita					.41644	-.53373***	2.4080
					(.83148)	(.04354)	(.893
Observations	771158	771158	771158	771158	771158	771158	7711
R-Squared	.01005	.05348	.0036	.05348	.00002	.05241	.052
Mean DV	754.850	754.850	754.850	754.850	755.112	755.112	755.1
%Change	1.244	0.173	4.307	0.822	0.055	-0.071	0.3
State FE		✓		✓		✓	
County FE							✓
Birth Year FE		✓		✓		✓	✓
SEA-by-Birth-Year FE							
Region-by-Birth-Year FE		✓		✓		✓	

Notes. Standard errors are in parentheses. Standard errors of columns 1-4 are clustered at the state-level. Standard errors of columns 5-8 are clustered at the county-level. The regres
mean of population over the sample period.
*** p<0.01, ** p<0.05, * p<0.1

Table 7 - The Correlations between Deposits per Capita and other Economic Variables (1929-1936)

	County-Level Outcome: Per Capita Retail Sale (STD)			State-Level Outcome Per Capita Income (STD)		
	(1)	(2)	(3)	(4)	(5)	(6)
Deposits per Capita (STD)	.86484***	.18034***	.14568***	.72179***	.38056***	.41758***
	(.2771)	(.06852)	(.05264)	(.17681)	(.07804)	(.07123)
Observations	20237	20236	19574	376	376	376
R-squared	.31861	.97957	.99003	.50444	.96404	.98016
Mean DV	0.000	0.000	-0.009	0.000	0.000	0.000
County FE		✓	✓			
State FE					✓	✓
Year FE		✓	✓		✓	✓
SEA-by-Year FE			✓			
Region-by-Year FE						✓

Notes. Standard errors, reported in parentheses, are clustered at the county-level for columns 1-2 and state-level for columns 3-4. The regressions are weighted using county-level/state-level mean of population.
*** p<0.01, ** p<0.05, * p<0.1

Table 8 - Robustness Checks

	Column 4 Table 4	Adding County-by-Individual FE	Adding County-by-Parental Covariates FE
	(1)	(2)	(3)
Deposits per Capita (STD)	1.72552**	1.72981**	1.79781**
	(.72026)	(.71853)	(.70769)
Observations	1221113	1220764	1220880
R-squared	.1036	.10389	.10501
	Adding County-by-Birth-Year Trend	Adding Birth-Month and Death-Month FE	Outcome: Log Age at Death
	(4)	(5)	(6)
Deposits per Capita (STD)	2.39254***	1.72548**	.00244**
	(.59011)	(.70252)	(.00103)
Observations	1221113	1221113	1221113
R-squared	.10396	.10447	.09848
	Outcome: Death Age > 70 Years	Outcome: Death Age > 65 Years	Two-Way Clustering SE at County by State-Birth-Year Level
	(7)	(8)	(9)
Deposits per Capita (STD)	.00625***	.00531*	1.70281**
	(.00157)	(.00309)	(.74797)
Observations	1221113	1221113	1218545
R-squared	.13318	.06653	.10269

Notes. Standard errors, clustered at the county-level (except for columns 9-10), are in parentheses. All regressions include county fixed effects, birth-year fixed effects, county-division fixed effects, and county trend. All regressions include individual, parental, and county covariates. Individual controls include race and gender dummies. Parental controls include father's socioeconomic status dummies and mother's education dummies. County-by-birth-year covariates include share of white-collar workers, share of blue-collar workers, share of farmers, and share of literate people. The regressions are weighted using county-level mean of population over the sample period.

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

Table 9 - Alternative Measures

	Outcome: Death Age (Months)	
	(1)	(3)
Total Deposits (STD)	2.19549***	
	(.82583)	
Drop in Deposits>5% and <10%		-.60259**
		(.28177)
Drop in Deposits>10%		-.59231*
		(.33466)
Observations	1206663	1208367
R-squared	.1035	.10352
Mean DV	773.034	773.021

Notes. Standard errors, clustered at the county-level, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, county-division fixed effects, and county trend. All regressions include individual, parental, and county covariates. Individual controls include race and gender dummies. Parental controls include father's socioeconomic status dummies and mother's education dummies. County-by-birth-year covariates include share of white-collar workers, share of blue-collar workers, share of farmers, and share of literate people. The regressions are weighted using county-level mean of population over the sample period.
*** p<0.01, ** p<0.05, * p<0.1

Table 10 - The Association between Deposits per Capita and Births Rates

	Outcomes:		
	Births per 1000 Women	Share of Births to Whites	Share of Births to Blacks
	(2)	(3)	(4)
Deposits per Capita (STD)	.18997**	-.00036	.00022
	(.08947)	(.00112)	(.00105)
Observations	8365	8365	8365
R-squared	.92043	.97239	.98336
Mean DV	36.824	0.701	0.297
%Change	0.516	-0.052	0.075
County FE	✓	✓	✓
Year FE	✓	✓	✓
SEA-Year FE	✓	✓	✓

Notes. Standard errors, reported in parentheses, are clustered at the county-level. The regressions are weighted using county-level mean of population over the sample period.
*** p<0.01, ** p<0.05, * p<0.1

Table 11 - Exploring the Mechanisms of Impact Using 1960 Census

	Outcomes:								
	Years of Schooling	Education≥ 1 Year of College	Education≥ 2 Years of College	Education≥ 3 Years of College	Education≥ 4 Years of College	Wage Income	Total personal Income	Socioeconomic Index	Occ Pre S
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Deposits per Capita	.03648**	.00573***	.00477***	.00353**	.004***	27.07686***	30.1783***	.17735*	.0
(STD)	(.01797)	(.00148)	(.00149)	(.00144)	(.00146)	(6.77593)	(8.75529)	(.09199)	(.
Observations	310975	310975	310975	310975	310975	310975	310975	269899	2(
R-squared	.12647	.04356	.03782	.03092	.02832	.39537	.43729	.12008	.(
Mean DV	7.916	0.179	0.138	0.102	0.080	1766.282	2032.139	35.233	3
%Change	0.461	3.202	3.457	3.456	5.005	1.533	1.485	0.503	(
Puma-County FE	✓	✓	✓	✓	✓	✓	✓	✓	
State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	

Notes. Standard errors, reported in parentheses, are clustered at the county-Puma-level. The regressions are weighted using county-Puma-level mean of population over the sample period.
*** p<0.01, ** p<0.05, * p<0.1

Figures

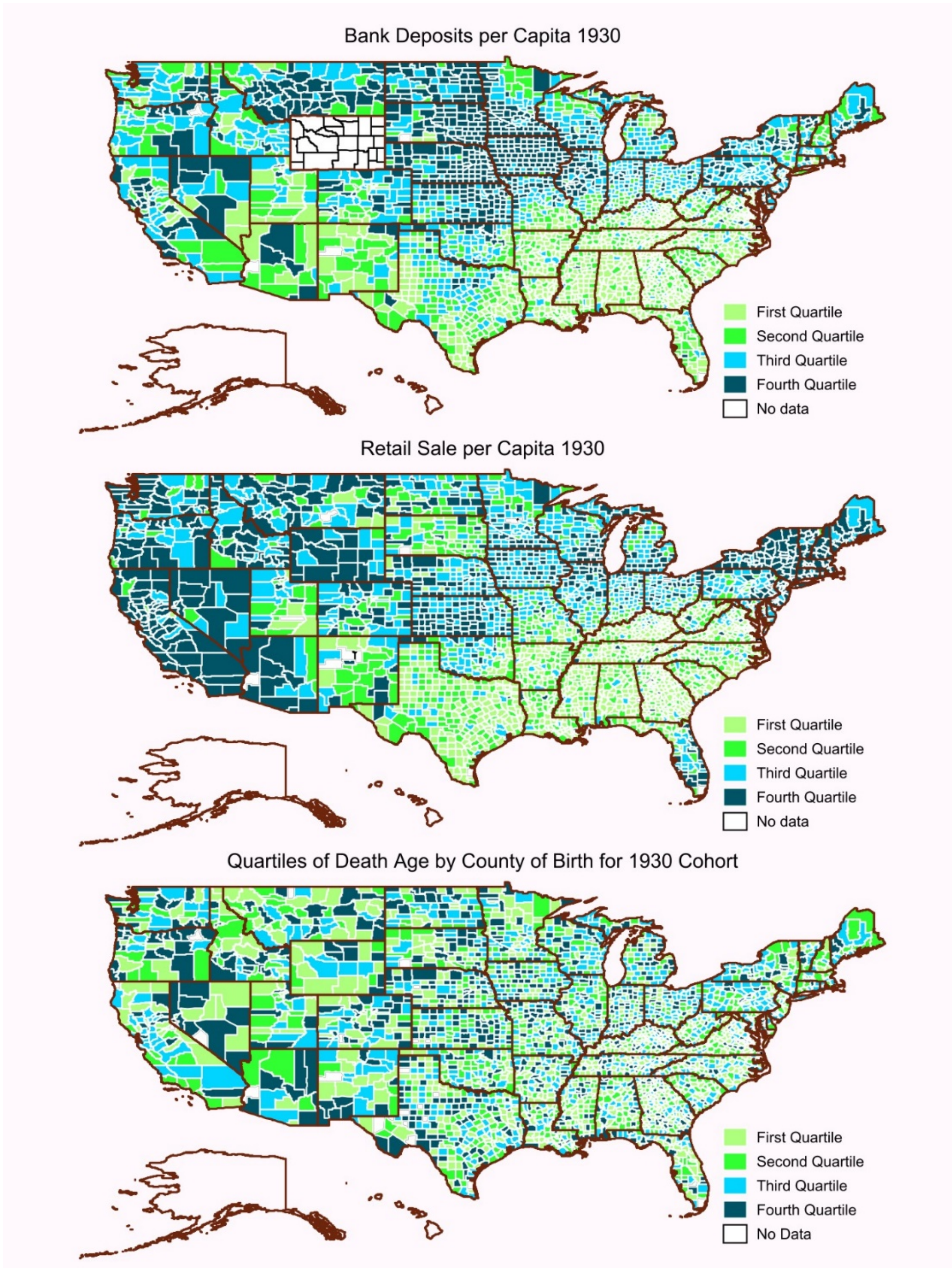

Figure 1 - Geographic Distribution of Variables

Notes. The colors in the map are based on the county's quartile rank in the nation's distribution of the respective variable.

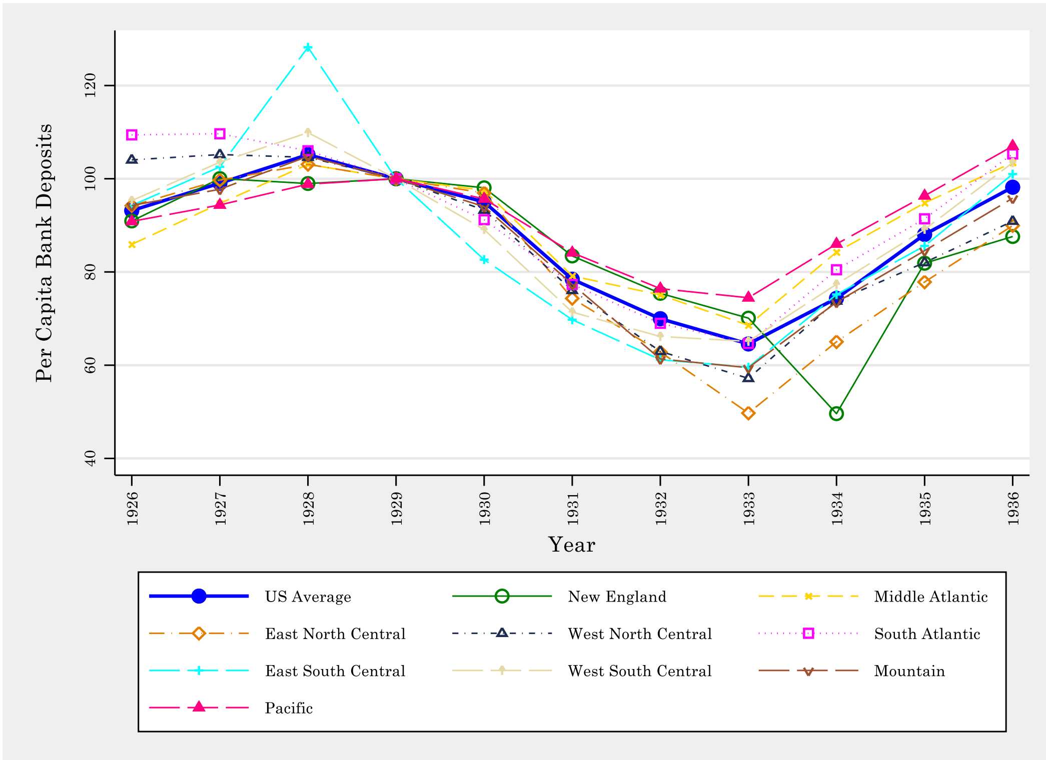

Figure 2 - Time-Series Evolution of Per Capita Bank Deposits across Census Regions

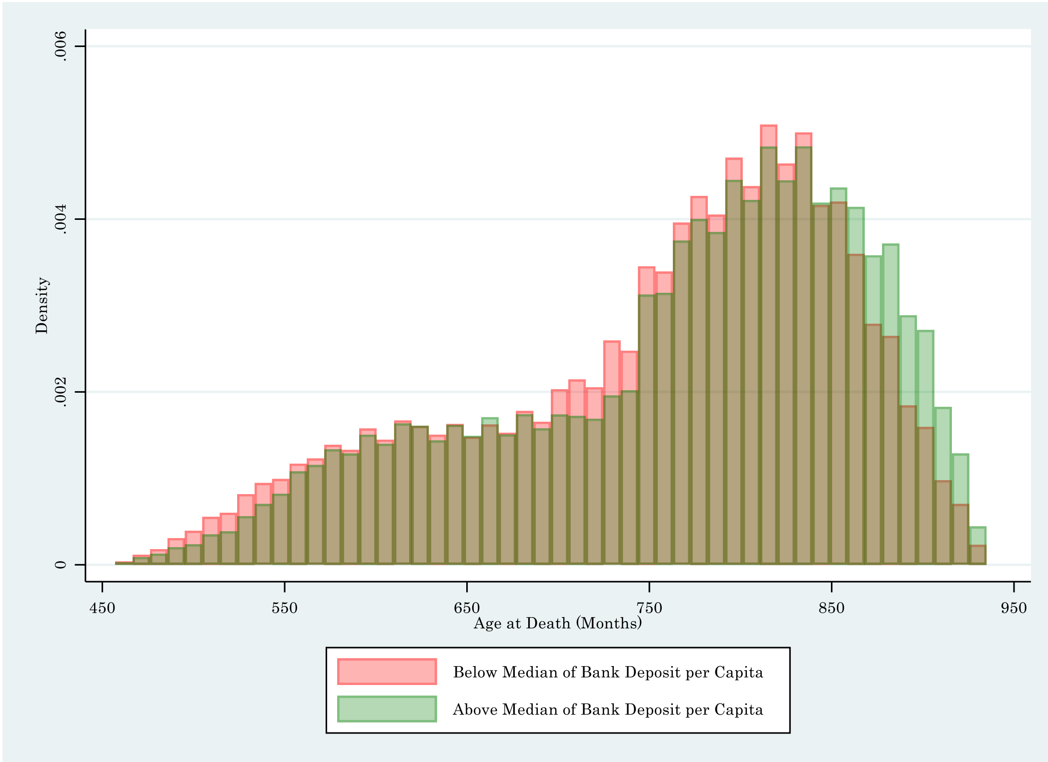

Figure 3 - Density Distribution of Age at Death by County above/below Median Bank Deposit Per Capita

Appendix A

In Appendix Table A-1, we replicate the main results across subsamples of whites and blacks. We observe relatively larger coefficients among whites. A one-standard-deviation rise in bank deposits per capita during in-utero is associated with 4.9 and 1.9 months higher longevity among blacks and whites, respectively.

Appendix Table A-1 - The Effects across Subsamples

	Outcome: Age at Death (Months)	
	Blacks	Whites
	(1)	(2)
Deposits per Capita (STD)	4.85125**	1.93562***
	(2.42706)	(.70076)
Observations	95690	1116246
R-squared	.13416	.10225
Mean DV	754.521	773.914
%Change	0.643	0.250
County FE	✓	✓
SEA-by-Birth-Year FE	✓	✓
Individual Controls	✓	✓
Family Controls	✓	✓
County Controls	✓	✓

Notes. Standard errors, clustered at the county-level, are in parentheses. Individual controls include race and gender dummies. Parental controls include father's socioeconomic status dummies and mother's education dummies. County-by-birth-year covariates include share of white-collar workers, share of blue-collar workers, share of farmers, and share of literate people. The regressions are weighted using county-level mean of population over the sample period.
*** p<0.01, ** p<0.05, * p<0.1

Appendix B

In this appendix, we replicate the main results using Numident death records (1988-2005) as an alternative data source. We start by replicating the full specification of column 4 of Table 4 in the first column of Appendix Table B-1. We then restrict DMF data to the coverage years of Numident data, i.e., years 1988-2005, in column 2. We reach a significant coefficient of 0.65, about 38 percent of the size of the main results. Using male deaths in Numident, we observe an effect of 0.8 months (column 3). Comparing columns 2 and 3, we observe a relatively larger effect in Numident data. However, focusing on female death records reveal an insignificant effect of 0.3 months (column 4). Pooling both genders in Numident data suggests a significant effect of 0.55 months (column 5).

Appendix Table B-1 - Sensitivity of Results to Death Window Coverage

	Outcome: Age at Death (Months)				
	DMF, Males, Death Years 1975-2005	DMF, Males, Death Years 1988-2005	Numident, Males, Death Years 1988-2005	Numident, Females, Death Years 1988-2005	Numident, All, Death Years 1988-2005
	(1)	(2)	(3)	(4)	(5)
Deposits per Capita (STD)	1.72552**	.64944***	.79878***	.27035	.54779***
	(.72026)	(.24692)	(.18345)	(.31548)	(.16828)
Observations	1221113	938470	1336616	976853	2327996
R-squared	.1036	.2605	.26177	.27642	.26492
Mean DV	772.939	819.901	820.745	827.705	823.705
%Change	0.223	0.079	0.097	0.033	0.067
County FE	✓	✓	✓	✓	✓
SEA-by-Birth-Year FE	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓
Family Controls	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓

Notes. Standard errors, clustered at the county-level, are in parentheses. Individual controls include race and gender dummies. Parental controls include father's socioeconomic status dummies and mother's education dummies. County-by-birth-year covariates include share of white-collar workers, share of blue-collar workers, share of farmers, and share of literate people. The regressions are weighted using county-level mean of population over the sample period.
*** p<0.01, ** p<0.05, * p<0.1

Appendix C

One potential heterogeneity is across counties with differential exposure to the Dust Bowl and the resulting soil erosions. Although Dust Bowl started mainly after 1935, they could impose selection bias to our results if economic conditions have differential impact on health of newborns in exposed and unexposed counties. We use data from Hornbeck (2011) to distinguish counties with high and medium soil erosion due to the Dust Bowl from all other counties. We then replicate the main results across exposed and unexposed counties. The results are reported in Appendix Table C-1. A one-standard-deviation increase in bank deposits per capita during the prenatal development is associated with 9.1 and 1.7 months higher longevity in exposed and unexposed counties, respectively. However, the effect of exposed counties is insignificant. Therefore, we rule out the concern that behavior of Dust Bowl exposed counties drive the main results.

Appendix Table C-1 - Heterogeneity of the Results across Counties by their Exposure to Dust Bowl

	Outcome: Age at Death (Months)	
	Dust Bowl Counties	All Other Counties
	(1)	(2)
Deposits per Capita (STD)	9.05849	1.73333**
	(10.3922)	(.72988)
Observations	174684	1043160
R-squared	.12838	.103
Mean DV	771.158	772.973
%Change	1.175	0.224
No of Counties	776	2278
County FE	✓	✓
SEA-by-Birth-Year FE	✓	✓
Individual Controls	✓	✓
Family Controls	✓	✓
County Controls	✓	✓

Notes. Standard errors, clustered at the county-level, are in parentheses. Individual controls include race and gender dummies. Parental controls include father's socioeconomic status dummies and mother's education dummies. County-by-birth-year covariates include share of white-collar workers, share of blue-collar workers, share of farmers, and share of literate people. The regressions are weighted using county-level mean of population over the sample period.
*** p<0.01, ** p<0.05, * p<0.1

Appendix D

In section 6, we discussed potential fertility responses to local economic conditions. We found evidence of procyclicality in the fertility behavior of parents but did not find evidence for changes in the composition of births based on race. One source of changes in the composition of births is selection by fetal deaths. Several studies suggest that conditions prior to and during pregnancy affect fetal death and that the incidence of fetal death is disproportionately higher among male fetuses (Quintana-Domeque & Ródenas-Serrano, 2017; Sanders & Stoecker, 2015). Therefore, fetal death and sex ratio at birth are strongly connected (Bruckner & Catalano, 2007; Catalano et al., 2005; James & Grech, 2017; Morse & Luke, 2021). We can also assess the selective survival of fetuses during pregnancy by evaluating the sex ratio at birth. However, the natality data over the period of the study is limited to race (white and black) disaggregation and does not provide more information on the share of females in births at the county level. To overcome this limitation and assess this source of selection, we turn to the 1940 full-count Census. We limit the data to cohorts born between 1926-1936 to have the same cohorts as the final sample of the paper. We use the same procedure as described in section 3 to infer the county of birth. We use the information on gender to define a dummy indicating female. We then collapse the data at the county-year-of-birth level. The idea is that, conditional on survival up to 1940, the observed sex ratios in the 1940 Census could be used as a proxy for sex ratio at birth. We then merge this data with bank deposit data at the county-year level and implement regressions similar to equation 1. The results are reported in Appendix Table D-1 across columns for different specifications. We observe statistically insignificant effects that are very small in magnitude. In the fully parametrized model of column 3 that includes controls for the county-year share of different races, county fixed effects, and SEA-year fixed effects, a one-standard-deviation rise in deposits is associated with a 0.6 basis-points rise in sex ratio at birth (share of females to males), equivalent to 0.012 percent change with respect to the mean of the outcome.

Another interesting fertility selection is differential births response to economic conditions by maternal education. Since the natality data used in section 6 lacks information on maternal education, we use the full-count 1940 Census, focus on the 1926-1936 cohorts, and infer the county of birth as described in section 3. We then collapse the sample at the county-year-of-birth level and merge it with county-level deposit data. We assume that, conditional on survival into 1940, the full-count Census reveals the universe of births and can be used to examine the selective fertility response to deposits by maternal education. In so doing, we regress measures of maternal education from the collapsed Census data on per capita deposits, conditional on county and SEA-year fixed effects. The results are reported in Appendix Table D-2 for a continuous measure of the mother's schooling (column 1) and three binary measures of the mother's education (columns 2-4). The estimated effects are very small in magnitude. For instance, a one-standard-deviation change in deposits is associated with roughly 0.04 percent change in the mother's schooling (from the mean of the outcome). Except for mother education less than high school, the coefficients are statistically insignificant.

Appendix Table D-1 - Deposits and Sex Ratio at Birth Using 1940 Full-Count Census

	Outcome: Sex Ratio		
	(1)	(2)	(3)
Deposits per Capita (STD)	.00013 (.00018)	.00014 (.00015)	.00006 (.00022)
Observations	33468	33468	32400
R-squared	.12232	.12365	.23157
Mean DV	0.493	0.493	0.493
%Change	0.026	0.028	0.012
County and Year FE	✓	✓	✓
Controls		✓	✓
SEA-by-Year FE			✓

Notes. Standard errors, clustered at the county-level, are in parentheses. Controls include share of whites and blacks in each cell.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-2 - Deposits and Mothers' Schooling Using 1940 Full-Count Census

	Outcomes:			
	Mother's Years of Schooling	Mother's Education < HS	Mother's Education = HS	Mother's Education > HS
	(1)	(2)	(3)	(4)
Deposits per Capita (STD)	-.00293	.00067*	-.00079	-.00008
	(.00251)	(.00037)	(.0005)	(.00008)
Observations	32373	32400	32400	32400
R-squared	.98387	.98448	.98181	.93932
Mean DV	8.189	0.568	0.292	0.059
%Change	-0.036	0.118	-0.271	-0.137
County and Year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
SEA-by-Year FE	✓	✓	✓	✓

Notes. Standard errors, clustered at the county-level, are in parentheses. Controls include share of whites and blacks in each cell. *** p<0.01, ** p<0.05, * p<0.1