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ORIGINAL PAPER

Early-life economic conditions and old-age male 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 during the Great Depression, proxied by county-level banking deposits during in utero and first years of life, influences old-age longevity. We find that a one-standard-deviation rise in per capita bank deposits is associated with an approximately 1.7 month increase in males' longevity at old age. 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 measures when exploring the relationship between early-life conditions and later-life mortality.

Keywords Mortality · Longevity · Great Depression · Historical data

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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).[1] 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 and 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 males' 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.[2] 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 and Schwartz 1963). Therefore, our measure captures the severity of the economic shock experienced by households 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 four 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 among male individuals. 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

[1] 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).

[2] 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.

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 mechanisms. 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-1900s 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 and Duque (2022) and Duque and 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.[3]

[3] 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 et al. (2016a, b) 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.

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 suggests potential mechanisms. Section 7 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 and Currie 2011; Currie 2009; Hayward and Gorman 2004; Steptoe and 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 health and mortality (Abiona and Ajefu 2023; Baird et al. 2011; Schoeps et al. 2018), cognitive development (Chang et al. 2022; Majid 2015; Yamashita and Trinh 2022), test scores (Almond et al. 2015; Shah and Steinberg 2017), education (Aizer et al. 2016a, b; Caruso and Miller 2015; Qian 2024), adulthood earnings (Black et al. 2007; Currie and Rossin-Slater 2015; Hoynes et al. 2016), and health outcomes over the life cycle (Fletcher 2018a, 2018b; Fletcher and Noghanibehambari 2024; Goodman-Bacon 2021; Miller and Wherry 2019; Noghanibehambari and Engelman 2022; Noghanibehambari and Fletcher 2023b; Persson and Rossin-Slater 2018). Early-life conditions could operate through these mediatory pathways to influence the trajectory of old-age longevity.[4] 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 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 and 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 and 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 and 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

[4] Although these mediatory channels are post-birth outcomes, there is another channel through which economic conditions affect infants' health: maternal stress. Financial distress resulting from worsening economic conditions may impose mental pressure among pregnant mothers, resulting in adverse birth outcomes (Carlson 2015; Lindo 2011). Besides, the impact of changes in local economic condition is different across sociodemographic groups (Hoynes et al. 2012). In Appendix J, we show that the association between early-life bank deposits and later-life mortality are significantly larger for low-educated mothers, further supporting the maternal stress mechanism.

childhood conditions, including parents' socioeconomic status, mother's marital status, mother's labor force status, and parents' nativity. Montez and 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 (2010a, b) 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 and Schmitz (2021) and Schmitz and 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 and Fletcher (2024) 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 and 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.[5] In so doing, we assign the current year of deposits to our in utero deposit measure for births occurring in the months of October through December. For births occurring between January to September, the in utero measure is a weighted average of current and previous year's deposits. The weights are proportionate to the fraction of pregnancy that overlap with current and previous year. For instance, consider an infant born in March 1930. This infant has been exposed to

[5] One concern is that there is evidence that economic conditions are associated with birth outcomes, including preterm birth (Salazar et al. 2023). Therefore, the assumption of a 9-month gestation might not accurately capture the actual exposure. In Appendix K, we show that the effects are comparable and even slightly larger when we use a 7-month pre-birth weighted average.

three months of current year's (1930) deposits and six months of the previous year's (1929) deposits during prenatal development. Hence, the weights are $\frac{3}{9}$ and $\frac{6}{9}$ for 1930 and 1929 deposits, respectively. 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 3042 counties in 47 states born between 1926 and 1936 who died between 1975 and 2005.[6] 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 Fig. 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 Fig. 1 illustrates the distribution of age at death by county of birth for cohorts born in 1930. Figure 2 illustrates the time-series evolution of bank deposits per capita for different census divisions. Not surprisingly, the figure reveals a significant drop in deposits as the Great Depression hit the economy and started to recover from the years 1933-1934, as the economy starts to recover. There is also a visual correlation between these two measures and long-run longevity. In a cross-sectional and correlational manner, Fig. 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:

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

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 covers several counties within the same state that have similar economic and demographic conditions (Bogue 1951). SEA was introduced in census

[6] 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.

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. 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 Eq. 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

Table 1 Summary statistics

Variable	Observations	Mean	Std. Dev.	Min	Max
Death age (months)	1,221,113	772.666	104.369	457.000	959.000
Death age (years)	1,221,113	63.880	8.716	38.000	79.000
Birth year	1,221,113	1930.192	3.106	1926.000	1936.000
Death year	1,221,113	1994.575	8.314	1975.000	2005.000
Birth month	1,221,113	6.422	3.422	1.000	12.000
Death month	1,221,113	6.494	3.505	1.000	12.000
Deposits per capita (STD)	1,221,113	0.000	1.000	−.584	50.792
Deposits per capita	1,221,113	330.917	566.430	0.000	29101.057
Total deposits ($1B)	1,221,113	.304	1.084	0.000	12.594
Drop in deposits $>5\%$ and $\leq 10\%$	1,221,113	.100	.301	0.000	1.000
Drop in deposits $> 10\%$	1,221,113	.230	.421	0.000	1.000
White	1,221,113	.914	.279	0.000	1.000
Black	1,221,113	.080	.271	0.000	1.000
Father SEI 1^{st} quartile	1,221,113	.239	.426	0.000	1.000
Father SEI 2^{nd} quartile	1,221,113	.227	.419	0.000	1.000
Father SEI 3^{rd} quartile	1,221,113	.226	.418	0.000	1.000
Father SEI 4^{th} quartile	1,221,113	.202	.401	0.000	1.000
Father SEI missing	1,221,113	.041	.200	0.000	1.000
Mother education <HS	1,221,113	.603	.489	0.000	1.000
Mother education =HS	1,221,113	.275	.446	0.000	1.000
Mother education >HS	1,221,113	.049	.217	0.000	1.000
Mother education missing	1,221,113	.070	.256	0.000	1.000
Work related per capita relief spending	1,221,113	5.000	9.499	0.000	376.468
Average homeownership	1,221,113	.501	.137	.025	.907
Share of literate	1,221,113	.811	.194	0.000	1.000
Average occupational income score	1,221,113	23.730	4.105	11.784	29.717
Share of married females	1,221,113	.609	.031	.289	.739
County-level data:					
Retail sales per capita	20,258	174.998	98.567	0.000	782.439
State-level data:					
Income per capita	376	445.600	195.139	122.988	1151.417
1960 census data:					
Years of schooling	403,493	7.897	2.938	0.000	15.000
Educ >1 year of college	403,493	.174	.379	0.000	1.000
Educ >2 years of college	403,493	.132	.339	0.000	1.000
Educ >3 years of college	403,493	.096	.295	0.000	1.000
Educ >4 years of college	403,493	.073	.261	0.000	1.000
Wage and salary income	403,493	1622.633	1873.754	0.000	19,866.486
Total income	403,493	1879.734	2068.231	−7906.861	19,866.486
Socioeconomic index	342,766	34.440	21.715	3.000	96.000
Occupational prestigious score	340,401	35.806	12.377	9.300	81.500
Occupational education score	340,346	18.558	22.373	.800	100.000

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

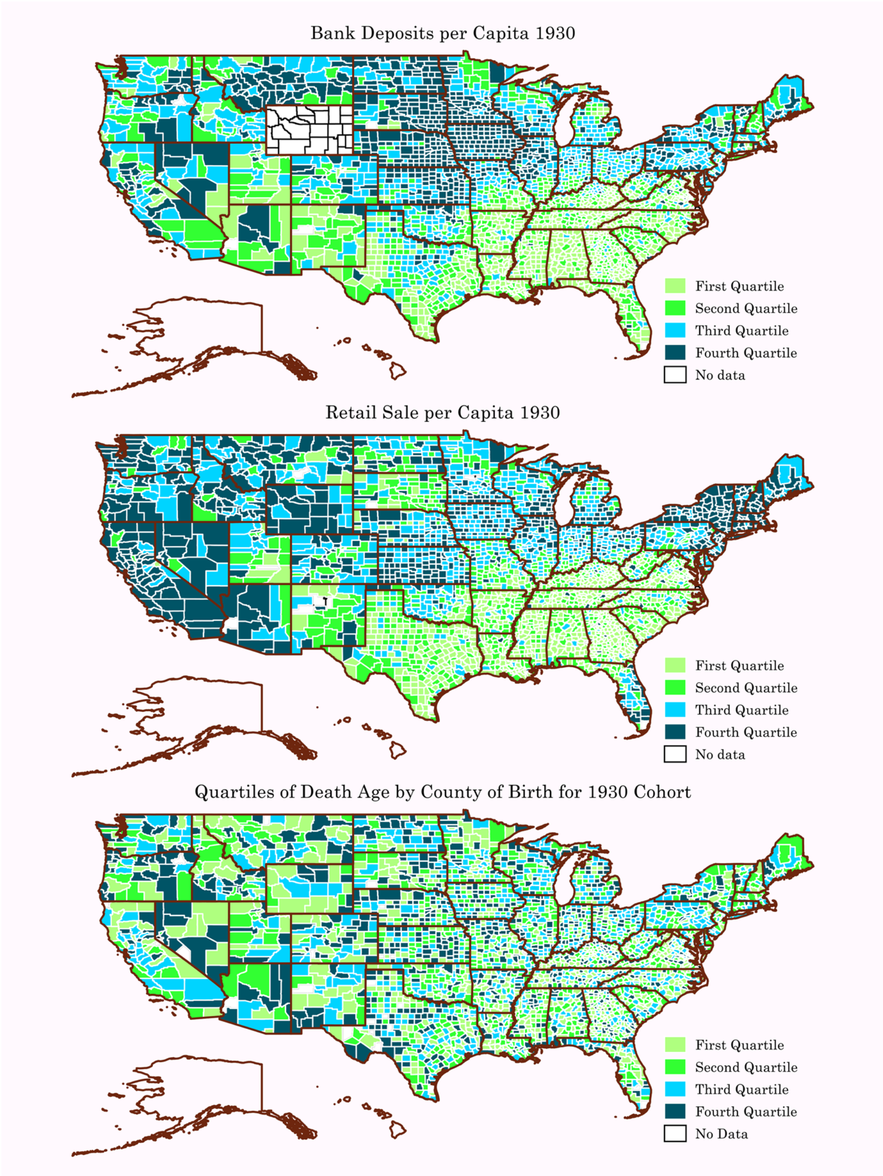

Fig. 1 Geographic distribution of variables. The colors in the map are based on the county's quartile rank in the nation's distribution of the respective variable

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.

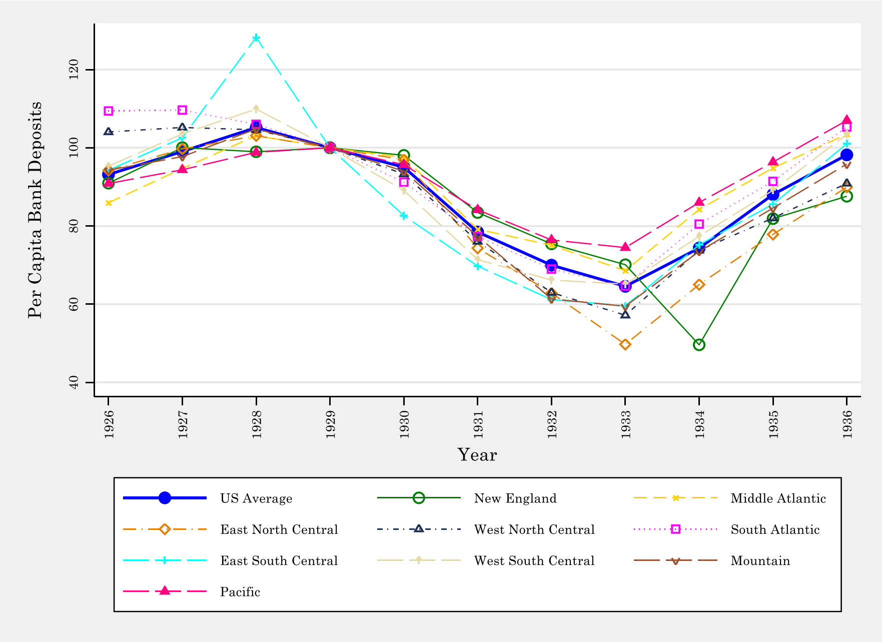

Fig. 2 Time-series evolution of per capita bank deposits across census regions

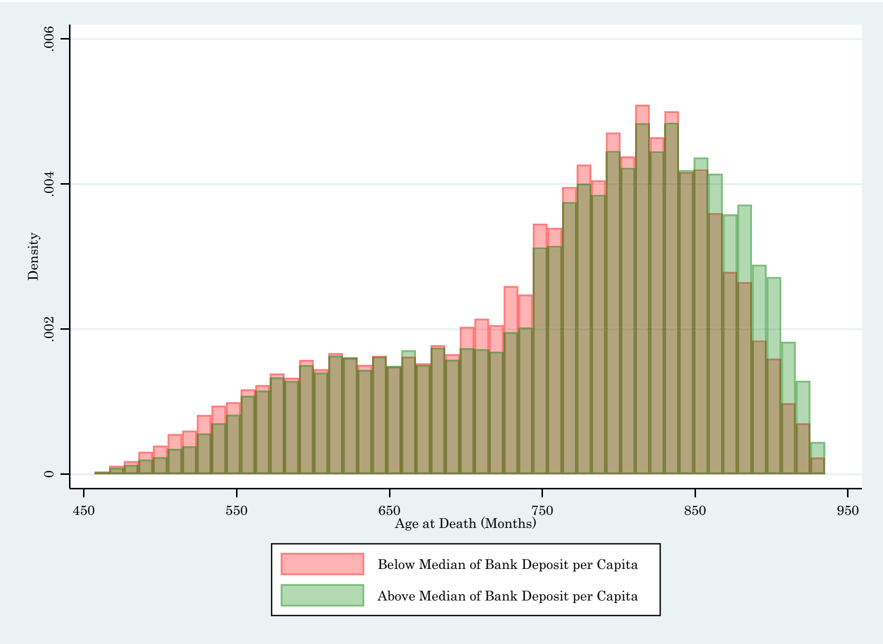

Fig. 3 Density distribution of age at death by county above/below median bank deposits per capita

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 (STD)	−.002	.012	.0007	.009	.002***	−.102	.002**
	(.001)	(.013)	(.001)	(.016)	(.0006)	(.071)	(.001)
Observations	1,221,113	1,133,633	1,221,113	1,068,262	1,221,113	1,040,576	1,221,113
R-squared	.118	.088	.041	.074	.045	.051	.026
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

Standard errors, clustered at the county level, are in parentheses. Regressions include county fixed effects, birth-year fixed effects, and SEA-by-birth-year fixed effects. 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

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

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 Eq. 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.[7] To further complement this analysis, in Appendix M, we employ Heckman two-step correction model to correct for biases arising from truncation and selection (Heckman 1979). We find coefficients that are slightly larger than those of the main results (Section 5.2), suggesting that selection, at worst, induces a small downward bias into our regressions.

A final concern of endogeneity is regarding reverse causality as areas with healthier people might enjoy better economic conditions, hence higher bank deposits. In Appendix G, we empirically examine whether longevity of individuals can explain variations in bank deposits. Nonetheless, the results provide no evidence of this concern.

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.

A growing literature document the influence of family characteristics and local conditions as significant determinants of long-run health outcomes (Almond et al. 2018; Hayward and Gorman 2004). Therefore, a priori, we need to account for these potential confounders into our regressions (columns 2–4 of Table 4). However, the fact that coefficients do not change across these columns lends credibility to the exogeneity of bank deposits, i.e., deposits are not correlated with other well-known

[7] In Appendix E, we also re-do this analysis using the Numident data (death years of 1988–2005). As reported in Appendix Table E-1, we observe a small though statistically significant coefficient for successful merging between Numident death records and the 1940 census. However, the point estimates are minuscule and become insignificant for further subsample analyses.

determinants of later-life health, at least with regard to the observable and available variables.[8] 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 five income percentiles is associated with about 0.8 months higher age at death.[9] 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 one 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.[10] Therefore, the marginal effect of Table 4 is equivalent to the effect of 0.43 years of schooling on longevity. Aizer et al. (2016a, b) 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.[11] 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 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

[8] The difference in R-squared of the full specification of column 4 and column 1 is relatively small, roughly 1.4% of baseline R-squared of column 1. However, the coefficients of race, father socioeconomic index, and maternal education are all statistically significant. Therefore, the minor change in R-squared does not refer to the lack of relevance of these variables.

[9] This number is the average association for both men and women. We should emphasize that our sample covers males only.

[10] The sample covered in their study covers males only, hence comparable to our study sample.

[11] Their study also focuses on male individuals only, hence comparable to our study sample.

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)	.00009	.0001	.00008	.0002**
	(.00009)	(.0001)	(.0001)	(.0001)
Observations	22,284,291	19,635,027	12,940,582	10,364,728
R-squared	.063	.063	.068	.066
Mean DV	0.056	0.058	0.060	0.058
%Change	0.158	0.331	0.131	0.407

Standard errors, clustered at the county level, are in parentheses. Regressions include county fixed effects, birth-year fixed effects, SEA-by-birth-year fixed effects, individual race dummies, maternal education dummies, and paternal education and socioeconomic status dummies. Regressions also include county-by-birth-year covariates including 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

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

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)	1.559***	1.586***	1.594***	1.725**
	(.577)	(.592)	(.585)	(.720)
Observations	1,221,113	1,221,113	1,221,113	1,221,113
R-squared	.102	.103	.103	.103
Mean DV	772.939	772.939	772.939	772.939
%Change	0.202	0.205	0.206	0.223
County FE	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes
SEA-by-birth-year FE	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes
Family controls	No	No	Yes	Yes
County controls	No	No	No	Yes

Standard errors, clustered at the county level, are in parentheses. Individual controls include dummies for race. 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

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

accordingly (Currie and 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, year, and SEA-year fixed effects.[12] The results are reported in Table 5. Deposits are positively associated with birth rates. A one-standard-deviation change in per capita deposits is associated with 0.22 additional births per 1000 women in the county, equivalent to roughly 0.6 percent rise in the mean of the outcome, a quite small change. In column 2 of this table, we observe an almost identical implied effect when we look at log fertility rate.

A similar concern arises due to the association between bank deposits and infant mortality rate. In columns 3 and 4, we observe that higher bank deposits are associated with lower infant mortality rates. However, the point estimates are small and noisy, which limits further interpretation.

To further examine fertility response, we use full-count census of 1940 and examine selective fertility response of parents to changes in deposits based on maternal education. These results, reported and discussed in Appendix D, do not provide a consistent and strong evidence of selective fertility behavior by maternal education.

5.4 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 and Rossin-Slater 2015; Ludwig and 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 four new variables for assigning deposits at two years before birth and two years after birth. We include these variables as separate

[12] Since this analysis examines the contemporaneous impact of economic conditions, the "year" variable is the same as "birth year."

regressors in the full specifications of Eq. 1. The results are reported in Table 6. We observe small and insignificant association for two-years prior to birth. However, the coefficient of one year prior to birth suggests a significant effect of 1 month increase in longevity. This is expected as this variable partly captures the in utero period, especially for cohorts born in the beginning months of the current year. Interestingly, we observe quite small and insignificant associations for post-birth periods. The overall results imply two pieces of information. First, the results do not pick up on the overall pre-existing improvements in health. Second, the in utero period is the more correlated with economic conditions in contrast with other post-natal ages.

5.5 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 2018; Atherwood 2022; Cutler et al. 2007; Van den Berg et al. 2015; Duque and Schmitz 2021; Granados et al. 2009; Myrskylä, 2010b; Schmitz and 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 7, 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.[13] The correlation is 32.5 months (column 3). The marginal effect becomes smaller (about the same 80 percent 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. For instance, the observed coefficient of column 5 is the result of comparing longevity of differential exposure to bank deposits. Higher deposits (and income) might be the result of healthier cohorts in specific regions which then is reflected in higher (children's) longevity. Therefore, these results imply the need to control for

[13] 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.

Table 5 The association between deposits per capita and births rates

	Outcomes:			
	Births rate (per 1000 women)	Log birth rate	Infant mortality rate (per 1000)	Log infant mortality rate
	(1)	(2)	(3)	(4)
Deposits per capita (STD)	.224*	.006*	−.107	−.002
	(.127)	(.003)	(.213)	(.003)
Observations	29,170	29,170	29,170	28,836
R-squared	.909	.919	.719	.680
Mean DV	34.685	3.515	60.572	4.061
%Change	0.647	0.176	−0.177	−0.053

Standard errors, reported in parentheses, are clustered at the county level. Regressions include county fixed effects, birth-year fixed effects, and SEA-by-birth-year fixed effects. The regressions are weighted using county-level mean of population over the sample period

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

county level effects and focus on within-county changes in economic conditions (as measured by bank deposits).

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.

5.6 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 Eq. 1. The results are reported in columns 1–3 of Table 8. 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

Table 6 Assigning deposits per capita at pre-prenatal and postnatal periods

	Outcome: death age (months)			
	(1)	(2)	(3)	(4)
Two years before birth	.669			
	(.516)			
One year before birth		.970*		
		(.515)		
One year after birth			.122	
			(.661)	
Two years after birth				−.283
				(.575)
Observations	1,215,134	1,213,430	1,150,655	1,152,367
R-squared	.103	.103	.091	.091
Mean DV	772.990	773.002	776.069	776.062

Standard errors, clustered at the county level, are in parentheses. Regressions include county fixed effects, birth-year fixed effects, SEA-by-birth-year fixed effects, individual race dummies, maternal education dummies, and paternal education and socioeconomic status dummies. Regressions also include county-by-birth-year covariates including 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

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

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.[14]

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 8. 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 8 and this drop in income, one can deduce a roughly 6.1 months drop in later-life longevity.[15] This is an economically large effect.

[14] In Appendix L, we use both retail sale per capita and bank deposits in the same regression. We find insignificant effects of retail sale but large and significant coefficient for bank deposits. We argue that as these two variables are highly correlated and contain similar information, most of the variations of retail sale is captured by bank deposits.

[15] This number is calculated using the marginal effect of column 4 of Table 4 (1.7), the marginal effect of column 6 of Table 8 (0.42), and the change of 1.5 standard-deviations in state-level income, as follows: $\left(\frac{1}{0.42}\right) \times 1.5 \times 1.7$.

Table 7 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)	(8)
State-level income per capita	9.390***	1.302*						
	(1.269)	(.743)						
State-level deposits per capita			32.512***	6.206				
			(6.582)	(6.039)				
County-level deposits per capita					.416	−.533***	2.408***	2.998***
					(.831)	(.043)	(.893)	(.833)
Observations	771,158	771,158	771,158	771,158	771,158	771,158	771,158	771,158
R-squared	.010	.053	.003	.053	.00002	.052	.052	.068
Mean DV	754.850	754.850	754.850	754.850	755.112	755.112	755.112	755.112
%Change	1.244	0.173	4.307	0.822	0.055	-0.071	0.319	0.397
State FE	No	Yes	No	Yes	No	Yes	No	No
County FE	No		No	No	No	No	Yes	Yes
Birth-year FE	No	Yes	No	Yes	No	Yes	Yes	Yes
SEA-by-birth-year FE	No	No	No	No	No	No	No	Yes
Region-by-birth-year FE	No	Yes	No	Yes	No	Yes	No	No

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 regressions are weighted using county-level mean of population over the sample period

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

Table 8 The correlations between deposits per capita and other economic variables (1929–1936)

	County-level outcome:			State-level outcome		
	Per capita retail sale (STD)			Per capita income (STD)		
	(1)	(2)	(3)	(4)	(5)	(6)
Deposits per Capita (STD)	.864***	.180***	.145***	.721***	.380***	.417***
	(.277)	(.068)	(.052)	(.176)	(.078)	(.071)
Observations	20,237	20,236	19,574	376	376	376
R-squared	.318	.979	.990	.504	.964	.980
Mean DV	0.000	0.000	−0.009	0.000	0.000	0.000
County FE		Yes	Yes	No	No	No
State FE		No	No	No	Yes	Yes
Year FE		Yes	Yes	No	Yes	Yes
SEA-by-year FE		No	Yes	No	No	No
Region-by-year FE		No	No	No	No	Yes

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

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

5.7 Robustness checks

In Table 9, 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.

There is evidence that season of birth and season of death may influence health and longevity of individuals (Simmerman et al. 2009; Vaiserman 2021). In column 5, we control for seasonality in birth and death outcomes by adding birth-month and death-month fixed effects.[16] 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 columns 7–8, we replace the outcome with a dummy to indicate longevity beyond age

[16] We add 11 dummies for birth-month and 11 separate dummies for death month into our regressions.

Table 9 Robustness checks

	Column 4 Table 4	Adding county-by-individual FE	Adding county-by-parental covariates FE
	(1)	(2)	(3)
Deposits per capita (STD)	1.725**	1.729**	1.797**
	(.720)	(.718)	(.707)
Observations	1,221,113	1220764	12,20,880
R-squared	.103	.103	.105
	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)	2.392***	1.725**	.002**
	(.590)	(.702)	(.001)
Observations	1,221,113	1,221,113	1,221,113
R-squared	.103	.104	.098
	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)	.006***	.005*	1.702**
	(.001)	(.003)	(.747)
Observations	1,221,113	1,221,113	1,218,545
R-squared	.133	.066	.102

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 column 9, we check for sensitivity to 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.

We implement two sets of additional robustness checks to complement this section. First, there are concerns of the confounding influence of contemporaneous conditions on longevity. Since DMF data does not report place of death, we employ Numident death records as an alternative data source which provides state of death. In Appendix H, we show that the results are fairly robust to including time-invariant state-of-death features and time-variant state-level characteristics. Second, another robustness check could also include measures of healthcare access as potential confounding influence on infants' health and their later-life longevity. We have information on County Health Departments and measures of medical staff at the county level for a subsample of mostly rural southern counties. We report and discuss the results in Appendix I. We find almost identical coefficients for specifications with/without these additional county covariates.

5.8 Alternative measures

In Table 10, 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 dummies 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.

5.9 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.

5.10 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 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.[17]

[17] In this section, we avoid restricting the sample based on migration status. The main reason is that the population of migrants and non-migrants differ systematically. In Appendix F, we show that non-migrants in the 1960 census are significantly lower educated and have lower socioeconomic status than migrants where migration is based on state-of-birth and current state of residence. Therefore, selection based on migration is endogenous and causes sample selection concerns. However, in Appendix F, we examine the effects among migrants and non-migrants and find that the estimated effects are fairly similar among these two groups.

Table 10 Alternative measures

	Outcome: death age (months)	
	(1)	(2)
Total deposits (STD)	2.195***	
	(.825)	
Drop in deposits > 5% and < 10%		−.602**
		(.281)
Drop in deposits > 10%		−.592*
		(.334)
Observations	1,206,663	1,208,367
R-squared	.103	.103
Mean DV	773.034	773.021

Standard errors, clustered at the county level, are in parentheses. All regressions include county fixed effects, birth-year fixed effects, SEA-by-birth-year fixed effects, and county trend. All regressions include individual, parental, and county covariates. Individual controls include dummies for race. 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

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

We also restrict the sample to male individuals born between the years 1926–1936.[18] We implement regressions that include, in addition to individual covariates, PUMA and region-birth-year fixed effects.[19] The results are reported in Table 11. We find 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 0.02 additional years of schooling (column 1), 38 basis points increase in the probability of any college education (column 2), $57 higher wage income (column 6), and 0.34 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 5.6. Such changes in deposits are associated with about 1.42 percentage points fall in the probability of college education and 1.3 units drop in socioeconomic score.[20]

[18] We use the variable indicating birth year in the 1960 censuses to identify cohorts born between 1926 and 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.

[19] Since in many cases PUMA contains several SEAs, most of the identifying variation comes from PUMA-year level, we avoid using SEA-year fixed effects. Instead, we include region-year fixed effects.

[20] These figures are calculated by multiplying the associated change in bank deposits during this period, using the correlation between income and bank deposit in Table 8, and a drop of 1.5 SD in income during this period, hence an inflating factor of $\frac{1.5}{0.4} = 3.75$.

Table 11 Exploring the mechanisms of impact using 1960 census

	Years of schooling	Education ≥ 1 year of college	Education ≥ 2 years of college	Education ≥ 3 years of college	Education ≥ 4 years of college
	(1)	(2)	(3)	(4)	(5)
Deposits per capita (STD)	.019	.003**	.003**	.002*	.002**
	(.015)	(.001)	(.001)	(.001)	(.001)
Observations	195,260	195,260	195,260	195,260	195,260
R-squared	.110	.058	.051	.044	.039
Mean DV	8.150	0.249	0.203	0.154	0.126
%Change	0.243	1.551	1.503	1.806	2.174
	Wage income	Total personal income	Socioeconomic index	Occupational prestigious score	Occupational educational score
	(6)	(7)	(8)	(9)	(10)
Deposits per capita (STD)	57.318***	64.643***	.344***	.181***	.265***
	(9.881)	(14.645)	(.075)	(.031)	(.065)
Observations	195,260	195,260	189,108	186,036	185,986
R-squared	.137	.163	.108	.101	.057
Mean DV	3289.221	3730.190	35.390	37.126	19.342
%Change	1.743	1.733	0.973	0.488	1.375

Standard errors, reported in parentheses, are clustered at the county-Puma level. Regressions include PUMA-county fixed effects, and region-by-birth-year fixed effects. The regressions are weighted using county-Puma-level mean of population over the sample period

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

We can use the values reported by similar studies to understand how much of the effects could operate through these channels. Fletcher and Noghanibehambari (2023) 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.[21] 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 0.3 months if it solely operates through increases in college education. This number can explain only 17.6 percent of the observed reduced-form effect.[22] 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.[23] 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 0.08 months increase in longevity, equivalent to roughly 4.8 percent of the reduced-form marginal effect in Table 4.[24]

7 Conclusion

The Great Depression was an extraordinary event in the economic history of the US. From 1929 to 1933, real output contracted by more than 25 percent and the unemployment rate increased from 3.2 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 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 are important for later-life longevity. We also argue that endogenous

[21] Their study uses CenSoc-Numident data and covers both men and women. Their heterogeneity analyses by race suggests slightly larger effects among male individuals.

[22] The treatment-on-treated calculation of Fletcher and Noghanibehambari (2023) 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% of the marginal effect of the reduced-form effect of deposits in longevity reported in column 4 of Table 4 ($100 \times 0.11/1.7$).

[23] Their study covers a male-only sample, similar to the study sample of the current study.

[24] 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: $\frac{0.34 \times 12 \times 0.037}{1.7} \times 100$.

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 mechanisms. However, we argue that between 6 and 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.

The Great Depression represented a unique and distinct period in the US history with severe and long-lasting downturn in economic activities. Its length and depth were unprecedented, and the economic knowledge of true policy response was limited. Therefore, it provides a different landscape compared with later recessions to examine the long-term health impacts. In the meantime, it shares some commonalities with other economic crises, such as stock market crash, resource scarcity, as well as the availability of relief programs. However, one should exercise caution in generalizing these results for other periods.

As a final note, it is important to acknowledge that while bank deposits can offer insights into local economic conditions, they may also be influenced by other factors, such as banking policies or management of commercial banks. For instance, there are shreds of evidence for the period of the Great Depression that commercial banks remained profitable and could pass the 1930s with minimal loss (Alcidi and Gros 2011). Therefore, banking deposits may understate the true exposure to economic condition, and, hence, our results provide a lower bound of true associations.

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Data availability The data and replication codes are available upon request from the corresponding author.

Declarations

Disclaimer The content is solely the responsibility of the authors and does not necessarily represent the official views of the Social Security Administration or the NIA.

Conflict of interest The authors declare no competing interests.

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