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Dust to Feed, Dust to Gray: The Effect of *in Utero* Exposure to the Dust Bowl on Old-Age Longevity

Hamid Noghanibehambari and Jason Fletcher

ABSTRACT Intensive agriculture and deep plowing caused topsoil erosion and dust storms during the 1930s, affecting agricultural income and land values for years. Given the growing literature on the relevance of *in utero* and early-life exposures, it is surprising that studies focusing on links between the Dust Bowl and later-life health have produced inconclusive and mixed results. We reevaluate this literature and study the long-term effects of *in utero* and early-life exposure to topsoil erosion caused by the 1930s Dust Bowl on old-age longevity. Specifically, using Social Security Administration death records linked with the full-count 1940 census, we conduct event studies with difference-in-differences designs to compare the longevity of individuals in high- versus low-topsoil-erosion counties before versus after 1930. We find intent-to-treat reductions in longevity of approximately 0.85 months for those born in high-erosion counties after 1930. We show that these effects are not an artifact of preexisting trends in longevity. Additional analyses suggest that the effects are more pronounced among children raised in farm households, females, and those whose mothers had lower education. We also provide suggestive evidence that reductions in adulthood income are a likely mechanism for the effects we document.

KEYWORDS Mortality • Dust Bowl • Drought • Early-life exposures • Historical data

Introduction

Several recent and growing strands of research have emphasized the relevance of prenatal development and early-life periods for a battery of later-life outcomes (Almond et al. 2018; Almond and Currie 2011; Barker 1990; Barker et al. 2002). Health endowment at birth and the gradual accumulation of health capital through early childhood influence infants' and children's physical, cognitive, and socioemotional well-being, and disturbances in initial health capital during this critical period could alter later-life outcomes. For instance, studies have documented negative short- and long-term consequences of *in utero* and early-life exposures to income shocks, agricultural crop failure, pollution, natural disasters, stress, toxic chemicals, and nutritional shocks (Baird et al. 2016; Billings and Schnepel 2018; Currie and Schmieder 2009; Lindeboom et al. 2010; Sanders 2012; Scholte et al. 2015; Torche 2018; van den Berg et al. 2011). These prenatal and early-life shocks can be translated

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into adverse health outcomes during infancy and early childhood, which appear in such medium- and long-run outcomes as cognitive development (Aizer, Stroud et al. 2016; Berthelon et al. 2021), test scores (Sanders 2012; Shah and Steinberg 2017), educational attainments (Almond et al. 2009; Fuller 2014), adulthood earnings (Behrman and Rosenzweig 2004; Black et al. 2007), health during adulthood (Maruyama and Heinesen 2020), hospitalization during adulthood (Miller and Wherry 2019), and old-age mortality (Goodman-Bacon 2021; van den Berg et al. 2011). These studies point to so-called scarring effects. However, a narrower strand of research has revealed null or positive effects through mortality selection (Bozzoli et al. 2009; Catalano et al. 2008; Catalano et al. 2019; Ekamper et al. 2015; Kannisto et al. 1997) or nonmonotonic effects (Lleras-Muney and Moreau 2022).[1]

An important example of such environmental adversity emerged during the Dust Bowl era in the U.S. Great Plains. During the late nineteenth and early twentieth centuries, farmers in the U.S. Great Plains expanded agricultural production and deeply plowed the virgin topsoil. The ever-growing practice eliminated the native grasslands required to cover and retain ground soil. The loss of topsoil coverage and severe droughts during the 1930s caused self-perpetuating wind erosion. The erosion of unprotected land also facilitated overland flow and surface runoff from rainwater and stormwater. Topsoil erosion caused inevitable agricultural crop failures, negative shocks to local economies, and inflating agricultural product prices (Hansen and Libecap 2015; Hornbeck 2012).

The Dust Bowl era produced adversities that could have translated into adverse health outcomes among vulnerable populations, specifically infants, with potentially long-run consequences. Several studies exploring similar shocks to agricultural failures and other environmental impacts have found significant effects on long-run outcomes (Barreca et al. 2021; Le and Nguyen 2021, 2022; Lindeboom et al. 2010). However, the literature on the Dust Bowl's later-life health effects is mixed, providing inconclusive evidence. Cutler et al. (2007) explored *in utero* Dust Bowl exposure and found no meaningful effects on adulthood height, body mass index, disability, diseases, or mortality. Similarly, Atherwood (2022) found no impact of childhood exposure to the Dust Bowl on old-age longevity. However, Arthi (2018) documented sizable increases in disability rates for adults with childhood exposure to the Dust Bowl. These studies used a cross-sectional analysis. In the absence of time-varying treatment variables, they compared individuals born in different census regions (Cutler et al. 2007), states (Arthi 2018), or counties (Atherwood 2022). Therefore, they could not account for time-invariant unobserved heterogeneity in individuals'

[1] For instance, Ekamper et al. (2015) examined long-term mortality effects of *in utero* exposure to the Dutch Hunger Winter of 1944–1945, finding no effects on mortality due to cancer and cardiovascular diseases. Catalano et al. (2008), employing data from Scandinavian countries, argued that colder ambient temperature exposure during the gestational period is associated with more male fetal deaths and a longer life span among surviving infants. Catalano et al. (2019) found that individuals exposed to the 1773 Swedish Famine early in life lived 4.2 years longer than Swedish individuals in the last half of the century. Kannisto et al. (1997) examined the effects of Finland's 1866–1868 famine on the affected cohorts' mortality and longevity. They found that *in utero* and early-life exposure to the famine was associated with higher mortality rates up to age 17. However, from ages 17 to 80, the mortality outcomes of exposed and unexposed cohorts were very similar, suggesting that the short-run influence of the famine did not translate to long-term adverse effects for longevity and life span.

birthplaces. Further, Atherwood (2022) focused on exposure in early adulthood and a subset of Great Plains counties.

In this study, we aim to reevaluate the later-life health effects of *in utero* and early-life exposure to severe topsoil erosion of the 1930s on old-age longevity. We address the drawbacks of previous studies by implementing a difference-in-differences model to account for unobserved county characteristics. This novel approach provides new insights into the later-life effects of the Dust Bowl. Moreover, unlike Atherwood (2022), we focus on *in utero* and early-life exposures and examine all U.S. counties.

The topsoil erosion of the 1930s, along with extreme droughts and dust clouds, affected agricultural production, income, food accessibility, and air quality. These shocks may have impacted infants' and children's initial health capital, which could be revealed in their old-age health and mortality outcomes. We ask whether *in utero* and early-life exposure to topsoil erosion during the 1930s can be detected in old-age mortality outcomes. We employ data from the 1988–2005 Social Security Administration (SSA) death records linked with the full-count 1940 census. We compare longevity outcomes of individuals born in high-erosion counties (treated counties) versus low-erosion counties (control counties) in the 1930s versus the preceding decade. We implement balancing tests to examine the changes in the demographic composition of births due to exposure to higher levels of soil erosion. In comparing treated and control counties before 1930, we do not observe a discernible difference in demographic and socioeconomic characteristics. After 1930, we find some evidence of increases in the share of females, individuals whose fathers had lower education, and individuals with lower socioeconomic status (SES). These changes are quite small in magnitude, and their implied confounding influence on mortality is not likely to drive the main results or underestimate the effects. In addition, only a small part of the pathway between early-life topsoil erosion and later-life mortality can be explained by the selection of births from lower SES families. Our main results reveal significant and negative intent-to-treat effects. Relative to individuals born in counties categorized as low-erosion farmlands, individuals born in high-erosion counties after the 1930s lived 0.85 fewer months.

Results of event studies reveal no preexisting trends in the longevity of more-affected counties versus less-affected counties up to 10 years before 1930. We conduct robustness checks and include extensive controls and fixed effects to test the sensitivity of the results. Moreover, we show that the effects are not driven by seasonality in births and deaths and are robust to alternative functional forms and longevity measures. Heterogeneity analyses suggest relatively larger effects among females and individuals with low-educated mothers. The effects also reveal heterogeneity by region: larger soil-erosion effects on counties in the South and Midwest, where the direct effects were concentrated, in contrast to other regions experiencing less erosion and no dust.

To explore the potential mechanisms of education and labor market outcomes, we employ 1960 census data and implement a similar identification strategy. We find suggestive evidence of increases in elementary and middle school completion, similar to Arthi's (2018) findings and suggesting substitutability between working on farms and attending school in childhood. However, the education effects do not translate into high school completion, college attendance, or income. Further, individuals born in high-erosion areas experience large and significant reductions in adulthood income.

The policy implications of these results lie in two aspects of the event that could relate to other types of environmental catastrophes and natural disasters. First, mortality is an extreme and precise measure of health. Our detection of adverse effects on the longevity of affected cohorts more than half a century later denotes considerable negative life cycle impacts and likely deteriorated old-age health and well-being. Therefore, policymakers aiming to promote lifelong health outcomes could focus on early-life events to prevent adverse later-life outcomes. Second, in events related to climate change and environmental phenomena, individual efforts are suboptimal, private solutions do not account for the externalities associated with intensive and uncontrolled farming, and more collective decision-making is required (Hansen and Libecap 2015). These situations call for government interventions and more collective actions. Our results point to the negative externalities of such events for health outcomes and highlight the role of policy interventions.

This study's contributions to the literature are twofold. First, we reevaluate the literature on the Dust Bowl's later-life health impacts. Contrary to the findings of Cutler et al. (2007) and Atherwood (2022), we find negative, sizable, and significant effects on longevity outcomes. Second, we contribute to the literature on *in utero* and early-life exposures and later-life health outcomes. Specifically, we add to the small but growing literature on the long-term health effects of environmental events and disasters (Barreca et al. 2021; Currie et al. 2015; De Rubeis et al. 2021; Rosales-Rueda 2018).

Literature Review

Soil erosion and resulting crop failure could influence old-age mortality of individuals exposed in infancy via several channels. In this section, we explore these potential channels and the relevant literature.

Agricultural income reductions are the primary channel through which climatic shocks in early life might affect future health and life cycle outcomes. Hyland and Russ (2019) explored the long-run effects of drought exposure during infancy and childhood on adult outcomes using data from several sub-Saharan African countries. They found suggestive evidence for reductions in wealth, education, height, and intergenerational effects on the next generations' birth outcomes. Because the effects were exclusively driven by rural residents, the researchers argued that their findings operated through distortions in agricultural outputs. Le and Nguyen (2021) examined the impact of *in utero* exposure to floods and droughts on childhood health outcomes, finding significant reductions in anthropometric outcomes between ages 1 and 5. Molina and Saldarriaga (2017) investigated the effects of temperature fluctuation on birth outcomes in several Andean countries and documented that temperature deviation from the location-specific, long-term path was associated with increased food insecurity and adverse birth outcomes. Feeny et al. (2021) studied the long-run gender gap consequences of early-life exposure to rainfall shocks in Vietnam, providing evidence that women were less likely to be employed than males if they were exposed to rainfall anomalies during their first two years of life. Shah and Steinberg (2017) showed that higher rainfall in rural India increased local wages, which were associated with increases in human capital investments during early life but decreased investments in children aged 5–16. They argued that the substitutability of schooling with labor wages reduced children's human capital formation. Maccini and Yang (2009) documented that among women, exposure to higher rainfall in the birth year was associated with higher schooling and improved measures of self-reported health. Banerjee et al. (2010) identified the later-life health impacts of shocks to agricultural income induced by phylloxera pests in French vineyards during the late nineteenth century. They found significant and relatively large intent-to-treat effects on height but no effects on life expectancy.

In particular, changes in income and agricultural output can affect children's food insecurity and mothers' prenatal nutritional intake. Studies exploring famine exposure impacts and later-life effects of governmental social programs have typically found relatively large impacts operating through changes in food access (Abiona 2022; Almond et al. 2011; Almond and Mazumder 2011; East 2020; Hernández-Julián et al. 2014; Hoynes et al. 2016; Karimia and Basu 2018; Majid 2015; Neelsen and Stratmann 2011; Painter et al. 2005). For instance, Haeck and Lefebvre (2016) found that a nutritional assistance program for pregnant mothers in Canada increased birth weight by roughly 70 grams. Lindeboom and colleagues (2010) explored the impacts of *in utero* and early-life nutritional shocks induced by the Dutch Potato Famine (1846–1847) on later-life and old-age longevity. They exploited the regional and temporal variations in potato and rye prices to proxy for early-life food availability and found that cohorts exposed in early life had their longevity reduced by 2.5–4 years. Roseboom et al. (2006) investigated the later-life health impacts of fetal exposure to the 1944–1945 Dutch famine, finding that *in utero* famine exposure was associated with adulthood glucose intolerance, coronary heart diseases, and disturbed blood coagulation. In a similar study, van Abeelen et al. (2012) found higher risks of mortality among women exposed to the Dutch famine during their prenatal development. Rosales-Rueda (2018) documented that early-life exposure to Ecuador's El Niño floods substantially reduced household income, food consumption, and maternal breastfeeding; *in utero* exposure to floods significantly increased the incidence of low birth weight, lowered childhood test scores, and decreased height among children aged 5–7.

A channel of impact between the Dust Bowl, in particular, and long-run health outcomes is fetal exposure to pollution. A relatively large literature has documented the short- and long-run health impacts of pollution among infants and children (Bharadwaj et al. 2017; Chay and Greenstone 2003; Currie and Neidell 2005; Currie et al. 2009; Currie and Schmieder 2009; Currie and Walker 2011; Currie et al. 2014; Sanders 2012; Simeonova et al. 2021). For instance, Moreira et al. (2020) showed that Saharan dust intrusion across municipalities in Spain was associated with a higher share of low birth weight infants. Altindag et al. (2017) explored the environmental and health impacts of yellow dust outbreaks in South Korea, created when high-speed surface winds from China and the deserts of Mongolia carry dust clouds into East Asian countries. They found that yellow dust increased air pollution and subsequently decreased the birth weight and gestational age of affected infants. In a similar review study, Hasunuma et al. (2019) demonstrated that exposure to the Asian dust event resulted in adverse health outcomes, higher mortality, and increases in hospitalization.

These effects were observed among various age groups, from infancy to adulthood. Currie and Schwandt (2016) found that fetal exposure to the dust cloud created by the 9/11 terrorist attacks significantly and negatively affected birth outcomes. Jones (2020) examined the pollution and infant health effects of a series of dust storms in the United States in 2010–2017, finding sizable increases in the incidence of low birth weight and preterm birth resulting from dust-driven pollution increases.

Some studies suggest that natural disasters and climatic catastrophes might impact health endowments at birth through less direct channels, such as prenatal maternal stress (Álvarez-Aranda et al. 2020; Caruso and Miller 2015; Glynn et al. 2001; Hetherington et al. 2021; Kim et al. 2017; Nandi et al. 2018; Torche 2011). For instance, Currie and Rossin-Slater (2013) found that fetal exposure to hurricanes was associated with increases in infants' abnormal conditions and meconium aspiration syndrome. Noghanibehambari (2022) documented that the effects of *in utero* exposure to earthquakes during the first trimester had negative and significant effects on old-age longevity.

Most evidence pertains to the potential negative effects of early-life adversities and exposures (a scarring effect), but several studies suggest a positive influence through selection effects: early-life mortality and survival of the fittest (Bozzoli et al. 2009; Catalano et al. 2008; Catalano et al. 2019; Ekamper et al. 2015; Kannisto et al. 1997). For instance, Kannisto et al. (1997) examined the effects of the 1866–1868 famine in Finland on the longevity of cohorts exposed to the famine early in life. They found mortality increases up to age 17 and no increases in survival rates after age 17. Their results indicate mortality selection and no negative legacies of early-life exposures. Similar to the ongoing discussion between selection versus a scarring effect, the Dust Bowl literature offers mixed evidence. Hornbeck (2012) examined the impacts on high- and medium-erosion counties versus low-erosion counties and documented long-run effects on agricultural production, agricultural income, and agricultural land value. Arthi (2018) explored *in utero* and childhood exposure using cross-state variation, finding significant increases in disability during adulthood and negative impacts on college completion and fertility. However, she documented positive effects on high school completion rates and argued that children, who would substitute schooling with farm work, were more likely to continue schooling when job prospects were scarce. Cutler et al. (2007) exploited Dust Bowl exposure variations across census regions and examined later-life health and mortality outcomes. They found no significant impacts on height, measures of chronic conditions, or disability.

These last two studies relied on variations across regions and states, large geographic areas with potentially wide heterogeneity of exposure and the resulting effects on health outcomes. A more precise framework would examine substate exposures to obtain a more accurate measure and account for within-state variations. Atherwood (2022) explored county exposures to the Dust Bowl and later-life longevity, employing Death Master Files (DMF) data and a subsample of Great Plains counties to compare the longevity outcomes of those who resided during their childhood in high- versus low-erosion counties. He failed to find a significant impact of Dust Bowl exposure on longevity.

The important drawback of the previous research designs was failing to account for unobserved heterogeneity in treated versus control counties. Their designs employed cross-sectional estimates and compared individuals born or living in different places with potentially different exposures to the Dust Bowl.[2] To overcome this limitation, we introduce an empirical model that accounts for time-invariant unobserved features of counties.

Moreover, Atherwood's (2022) analysis was limited to Great Plains counties, thereby omitting some topsoil erosion counties (see Figure A1; tables and figures designated with an "A" appear in the online appendix). Further, Atherwood focused on exposure in early adulthood, whereas we focus on early-life and childhood exposure. In addition, his data covered deaths of only males, whereas our data cover males and females, thereby uncovering potential gender differences.

Data Sources and Sample Construction

Our primary data source is the SSA-reported Numerical Identification System death records, the so-called Numident database, extracted from the Censoc Project (for an outline, see Goldstein et al. 2021). The SSA-recorded Numident data cover deaths of females and males during 1988–2005. The primary advantage of Censoc–Numident data is that they are linked at the individual level to the full-count 1940 census, creating a longitudinal panel of unprecedented size with extensive information on family characteristics and granular geographic detail on the 1940 place of residence. The lowest geographic area that the public-use full-count 1940 census provides is the county. The 1940 census asked respondents to report their 1935 county of residence (if different than that in 1940). Given our primary purpose of exploring *in utero* and early-life effects, we infer the county of birth based on the given information. We assume that the 1935 county of residence is also the county of birth. We exclude the following respondents from the sample: (1) those whose 1935 place of residence is not available and who reported having migrated during the last five years and (2) those whose 1935 county information is missing and state of birth differs from their 1940 state of residence. If the state of birth is the same as the 1940 state of residence, the 1935 county is missing, and the respondent did not migrate in the last five years, we can safely assume that the 1940 county of residence is the same as the county of birth. To further mitigate migration-related measurement issues with our county-of-birth variable, we limit the sample to cohorts born after 1920. Despite these efforts, our county-of-birth measure still contains measurement error that might be correlated with exposure to the Dust Bowl and later-life mortality (Long and Siu 2018).

We extract the data for soil erosion and Dust Bowl from Hornbeck (2011), who used Soil Conservation Service data to construct county-level data on the share of farmlands' topsoil that has eroded. He categorized these fraction measures into three variables based on the cumulative fraction of a county's farmlands' topsoil that eroded in the 1930s: high-fraction erosion, medium-fraction erosion, and low-fraction erosion.

For our analysis, we build a high-erosion dummy variable that equals 1 if the share of high-fraction erosion exceeded 75%. All other counties are considered to have low

[2] Atherwood (2022) examined county data, Arthi (2018) used state data, and Cutler et al. (2007) used data on the census region.

erosion. Figure A1 shows the geographic distribution of high- and low-erosion counties across the country. Given the scarcity of related agricultural and soil erosion data for the 1930s, our data do not provide over-time variation in the county soil erosion data, although we can examine potential variation indirectly by estimating effects separately by each birth year. This limitation is common in studies of short- and long-term effects of the Dust Bowl.

We merge the county-level data on soil erosion with Numident–census data based on individuals' county of birth. Whereas previous studies focused on counties affected by wind erosion and historically recognized as Dust Bowl counties (Atherwood 2022), we examine all U.S. counties and show heterogeneity across regions. We do not limit the geographic coverage of our sample because although erosion was more severe in the Great Plains than elsewhere, the effects were detectable in many areas as far away as the prairies of Canada (McLeman et al. 2014; Schubert et al. 2004). Further, drought-driven erosion was more widespread and potentially impacted agricultural products elsewhere in the country.

For additional analyses related to endogenous fertility, we use data from Bailey et al. (2016) covering births and deaths for a subsample of more-populous counties. We use 1960 census data from Ruggles et al. (2020) to analyze mechanism channels. We also draw on 1920–1940 decennial census data from Ruggles et al. to construct county covariates, interpolating them for interdecennial years.

Summary statistics for the final sample are provided in Table 1. The age at death varies from 47.6 to 85.6 years, with an average of approximately 71.2 years. Approximately 8% of individuals were born in high-erosion counties. The sample underrepresents females and overrepresents Whites because the Numident–census linking is primarily based on name commonalities and information on birthplace and age. Given that females usually changed their names after marriage, they are less likely to be linkable. Although non-Whites are underrepresented in the sample, they represent their respective populations on other sociodemographic features (Breen and Osborne 2022).

Econometric Method

The identification strategy exploits the spatial variations in county-specific cumulative topsoil erosion combined with rises in dust clouds and droughts in the 1930s versus a decade earlier. We implement difference-in-differences analyses using ordinary least-squares regressions of the following form:

$$y_{icsb} = \alpha_0 + \alpha_1 Post_b \times HErosion_{cs} + \alpha_2 \mathbf{X}_i + \alpha_3 \mathbf{Z}_{csb} + \xi_c + \zeta_{sb} + \varepsilon_{icsb}, \quad (1)$$

where y is the outcome (age at death) of individual i in county c in state s who was born in year b. *Post* is a dummy variable that equals 1 for post-1930 years and 0 otherwise. *HErosion* is a dummy variable indicating high topsoil erosion in the county over the 1930s decade. The matrix $\mathbf{X}$ contains individual and family controls, including dummy variables for race, gender, maternal education, and paternal SES. $\mathbf{Z}$ includes several county controls constructed using county values in full-count decennial censuses and interpolated for interdecennial years. These controls include

Table 1 Summary statistics

Variable	Mean	SD	Min.	Max.
Numident–1940 Census Data				
Death age (months)	854.47565	69.10555	572	1,028
Birth year	1927.1297	3.83597	1920	1940
Death year	1998.3376	4.75345	1988	2005
High erosion	.07662	.26598	0	1
High erosion × Post-1930	.02159	.14533	0	1
Female	.42877	.49490	0	1
White	.92604	.26170	0	1
Black	.07054	.25606	0	1
Other race	.00341	.05830	0	1
Hispanic	.01385	.11685	0	1
Paternal SES 1st quartile	.20628	.40463	0	1
Paternal SES 2nd quartile	.23355	.42309	0	1
Paternal SES 3rd quartile	.19965	.39973	0	1
Paternal SES 4th quartile	.23699	.42524	0	1
Paternal SES missing	.04353	.20405	0	1
Maternal education < high school	.58219	.49320	0	1
Maternal education = high school	.26960	.44375	0	1
Maternal education > high school	.06158	.24040	0	1
Maternal education missing	.08663	.28129	0	1
County Covariates				
Share homeowners	.51516	.13121	.01811	.90763
Share children <5 years old	.40899	.12508	.13409	1.09902
Share literate	.88620	.15143	0	1
Share married	.60847	.03182	.26571	.77807
Average occupational score	23.42786	4.08945	11.78472	32.76429
Number of observations		1,819,066		
1960 Census Data				
Years of schooling	7.9491	2.89429	0	15
>4	.92799	.25850	0	1
>5	.88956	.31343	0	1
>6	.79513	.40361	0	1
>7	.72758	.44520	0	1
>8	.64402	.47881	0	1
>9	.57231	.49474	0	1
>10	.18027	.38441	0	1
>11	.13116	.33758	0	1
>12	.08944	.28538	0	1
Log total personal income	5.3709	3.53372	0	9.89684
Log wage income	4.82951	3.84557	0	10.12667
Female	.50815	.49993	0	1
Black	.09593	.29449	0	1
White	.89788	.30280	0	1
Number of observations		645,778		
Birth and Infant Death Data				
Infant mortality rate per 100,000 births	61.93882	28.64321	0	1,000
Birth rate per 1,000 women	39.87917	11.38456	0	149.79358
Share births to Whites	.67368	.23898	0	1
Share births to Blacks	.32546	.23886	0	1
Number of observations		60,330		

Table 2 Main results for effect of exposure on age at death (months)

	(1)	(2)	(3)	(4)	(5)
High Erosion × Post-1930	–.84895*	–.86141*	–.90603*	–.89264*	–.84591*
	(.3973)	(.41969)	(.41985)	(.41997)	(.42258)
Number of Observations	1,818,426	1,818,422	1,818,422	1,818,422	1,818,422
R^2	.32596	.32643	.32927	.32959	.32959
Mean	854.811	854.811	854.811	854.811	854.811
% Change	–0.099	–0.101	–0.106	–0.104	–0.099
Birth Year Fixed Effects	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓
Birth-Year-by-Birth-State Fixed Effects		✓	✓	✓	✓
Individual Covariates			✓	✓	✓
Family Controls				✓	✓
County Covariates					✓

Notes: Standard errors, clustered on the county, are shown in parentheses. Regressions are weighted using county farmland in 1930.

*$p < .05$

the share of homeowners, the share of children younger than 5, the share of literate people, the share married, and the average occupational score. The parameters ξ and ζ represent county and fixed effects for state by birth year. Finally, ε is a disturbance term. Standard errors are clustered at the county level. Following Hornbeck (2012), we weight the regressions using the farmland area of the county in 1930.[3]

Results

Main Results

We report our main results in Table 2. We start with regressions that include only fixed effects for county and birth year, adding more covariates across consecutive columns. When we add fixed effects for state by birth year, the magnitude of the marginal effects increases slightly (column 2). However, the effects in columns 2–5 are quite robust and similar as we add more controls. The full specification, shown in column 5, suggests that cohorts born in high-erosion counties versus low-erosion counties post-1930 versus pre-1930 live 0.85 fewer months.[4]

To put the effect of high erosion into perspective, we compare the number with the coefficients of other individual covariates in the same regressions. Specifically, we use

[3] The online appendix (section C) shows the results of unweighted regressions and examines the sensitivity of the results to alternative weights. The estimated coefficients suggest stability to alternative weights and unweighted regressions.

[4] The online appendix (section F) explores the sensitivity of our results across alternative models. The results are robust across a wide array of specification checks, functional form checks, an alternative difference-in-differences estimation method, alternative cohort restrictions, and alternative standard error clustering levels.

the female–male longevity gap (coefficient on female = 7.21, SE = 0.11) and the Black–White gap in longevity (coefficient on Black = –4.23, SE = 0.23). The effects of high erosion represent 11.7% of the female–male gap and 20.1% of the Black–White gap.

Hornbeck (2012) found that high-erosion counties experienced a 12% reduction in retail sales per capita, equivalent to approximately a 0.11-standard-deviation change in retail sales per capita in their sample. Noghanibehambari et al. (2022) found that *in utero* and early-life local economic conditions, proxied with bank deposits per capita, were significantly associated with old-age longevity. Furthermore, they validated their proxy by showing a correlation between bank deposits and alternative measures of local economic conditions (including retail sales per capita). They found that a 1-standard-deviation decrease in bank deposits per capita was associated with a 0.21-standard-deviation drop in retail sales per capita and an age at death that was roughly 2.7 months lower. Using the cumulative effects reported by Hornbeck (2012) and these effects, we reach a back-of-the-envelope effect of 1.4 months due to worsening economic conditions resulting from high topsoil erosion in the county of birth—slightly larger than the marginal effect of 0.85 months shown in Table 2.

The average life expectancy at birth in the United States increased by approximately 8 years between 1920 and 1940. The intent-to-treat effect of 0.85 months on longevity represents roughly a 1% change in longevity across cohorts in our final sample. Another way to gauge the economic significance of the results is to compare them with other studies exploring the determinants of longevity. For instance, Chetty et al. (2016) explored the income–longevity relationship across income percentiles and found that for each additional income percentile, longevity increased by roughly 1.9 months—a relatively constant factor across different baseline percentiles. At the mean of the population, a 1-percentile change represents roughly $8,000 (in 2020 dollars). Therefore, our results suggest that the intent-to-treat effects of early-life exposure to the Dust Bowl might be offset by approximately $3,600 in additional annual income. Fletcher and Noghanibehambari (forthcoming) explored the effects of college opening on college education and mortality, finding intent-to-treat effects of 0.13 months of additional longevity for each additional four-year college opening as a result of increases in college education. Compared with our estimated intent-to-treat effects, the negative effects of high topsoil erosion offset the positive effects of roughly seven new four-year colleges in the local area.

Heterogeneity Across Subsamples

Table 3 explores the heterogeneity of results across subsamples based on sociodemographic characteristics. Column 1 replicates the main results shown in column 5 of Table 2. Columns 2 and 3 display results for White and non-White individuals. We conduct this heterogeneity analysis because of evidence of racial disparity in exposure and effects of environmental shocks (Berberian et al. 2022; Park et al. 2020). Further, as we demonstrate later in the article, shocks to income and food accessibility are part of the pathways between early-life exposure to the Dust Bowl and old-age mortality, and these shocks have a greater impact on non-Whites, who have lower SES, on average. Indeed, the estimated coefficient drops by approximately 19% for Whites and increases by approximately 75% among non-Whites.

Table 3 Heterogeneity in age at death (months) across subsamples

	Full Sample (1)	White (2)	Non-White (3)	Female (4)	Male (5)	Father Is a Farmer (6)	Maternal Education <College (7)
High Erosion × Post-1930	–.84591*	–.68795	–1.51431	–.91756	–.81182	–2.50945*	–.95668*
	(.42258)	(.45282)	(1.16205)	(.67242)	(.55319)	(1.21229)	(.43175)
Number of Observations	1,818,422	1,684,118	134,036	779,673	1,038,744	219,587	1,578,090
R^2	.32959	.32752	.35891	.33689	.32131	.35807	.33198
Mean	854.811	855.514	844.501	860.479	850.593	846.752	853.954
% Change	–0.099	–0.080	–0.179	–0.107	–0.095	–0.296	–0.112

Notes: Standard errors, clustered on county, are shown in parentheses. Regressions include fixed effects for county and state by birth year. All regressions include individual, family, and county covariates. Regressions are weighted using county farmland in 1930.

*$p < .05$

Columns 4 and 5 of Table 3 examine heterogeneity by gender. The estimated effect of high erosion is slightly larger among females than males (–0.92 vs. –0.81)—a differential gender impact that has also been shown by other studies. Several studies found larger and more persistent impacts of health shocks among females (Ae-Ngibise et al. 2019; Bharadwaj and Lakdawala 2013; Chen et al. 2020; Muchomba and Chatterji 2020; Wang et al. 2017).

Column 6 focuses on those whose fathers worked in farm-related occupations. The effect size of high erosion suggests considerably larger impacts for these individuals, with a coefficient roughly 2.9 times that of the main results. The effects seem to operate largely through income channels, given that topsoil erosion produced a large income shock among farmers.

Column 7 shows effects for individuals with low-educated mothers. The marginal effect rises in magnitude and remains statistically significant. Overall, the effects are more pronounced for non-Whites, females, individuals whose fathers farmed, and individuals whose mothers had lower education.

We also examine the heterogeneity of the results across census regions because the dosage and intensity of topsoil erosion differed systematically across regions. Figure A1 reveals a higher concentration of counties with high topsoil erosion in the South and Midwest. If the effects operate primarily through direct impacts of agriculture and income shocks rather than spillovers from other counties or states, one may expect higher impacts in these regions. The online appendix (section G) shows that the effects of high erosion are primarily concentrated in counties in the West and especially in the South and Midwest.

Finally, we examine heterogeneity by migration status, finding that the effects are primarily confined to nonmigrants (i.e., those who stayed in their birth state until 1940). These results are also reported in the online appendix (section G). The effect flips sign and becomes statistically insignificant for those who migrated (i.e., died in a state other than their birth state).

Endogeneity Concerns

The estimated coefficients from Eq. (1) potentially provide biased estimates. The first concern regards the likely composition changes among cohorts in the treated and control groups. These treated–control cohort differences could bias the estimates if other dimensions associated with each group are also correlated with our measures of erosion exposure. For instance, if lower SES individuals were more likely to be exposed to erosion shocks, the estimated coefficients would be overestimates, given that these individuals had a lower age at death due to unobserved factors. The differential exposure or the differential cohort composition of the final sample could result from several factors, including endogenous postevent migration, birth composition, fetal and infant deaths, and survival into adulthood. However, two pieces of empirical evidence are inconsistent with these endogeneity sources. First, we directly test for changes in composition differences among the final sample by implementing balancing-test-type event studies. In these event studies, we assume that the event occurred in 1930. We compare the characteristics of individuals in high-erosion counties with those in low-erosion counties in different years relative to 1929–1930. We group event times into two-year bins. To explore the pre- and posttrends in sociodemographic characteristics, we use individual and family characteristics as the outcomes. In all regressions, we include state–year and county fixed effects. The results from regressions in which the high-erosion measures are interacted with event-time dummy variables are reported in Figures 1 and 2. For comparison, we standardize all outcomes.

We observe small decreases in the share of Whites for 1934–1940 in high-erosion counties (top left panel, Figure 1). Given the inclusion of very few individuals of other races in the data, the results for Blacks (top right panel, Figure 1) reflect the effects on Whites. The share of females also increased slightly for high-erosion measures in the 1932–1933 group (bottom left panel, Figure 1). In addition, fathers' education decreased slightly for the 1932–1933 cohorts, and mother's education decreased slightly for the 1936–1940 cohorts (top panels, Figure 2). Further, paternal SES decreased for the 1936–1937 cohort (bottom left panel, Figure 2). Although these exceptions provide point estimates that are larger than other coefficients, they are barely significant at conventional levels. Further, besides these exceptions, almost all other pre-1930 and post-1930 coefficients in both groups are not statistically significant. Moreover, the effects do not reveal a consistent pattern of change during the 1920s and 1930s.

We identify four additional reasons why these suggestive compositional changes may not influence the results of Eq. (1). First, their implied effects on longevity are substantially smaller than the reduced-form effects. For instance, our regressions estimate that the implied confounding influence of paternal schooling is roughly 0.4% of the effects of the high-erosion dummy variable on age at death.[5] Second, we observe changes for a limited number of cohorts. The average changes for post-1930 versus pre-1930 cohorts are very small and insignificant for most of the outcomes studied here.

[5] The coefficient on father's years of schooling for the 1932–1933 cohorts is −0.11 (SE = 0.047)—a 1.4% change. The comparative coefficient in the fully parametrized regression of Eq. (1) is 0.26 (SE = 0.018). Therefore, the confounding influence of lower father's years of schooling for this group is roughly 0.004 months—about 0.4% of the effect shown in column 5 of Table 2.

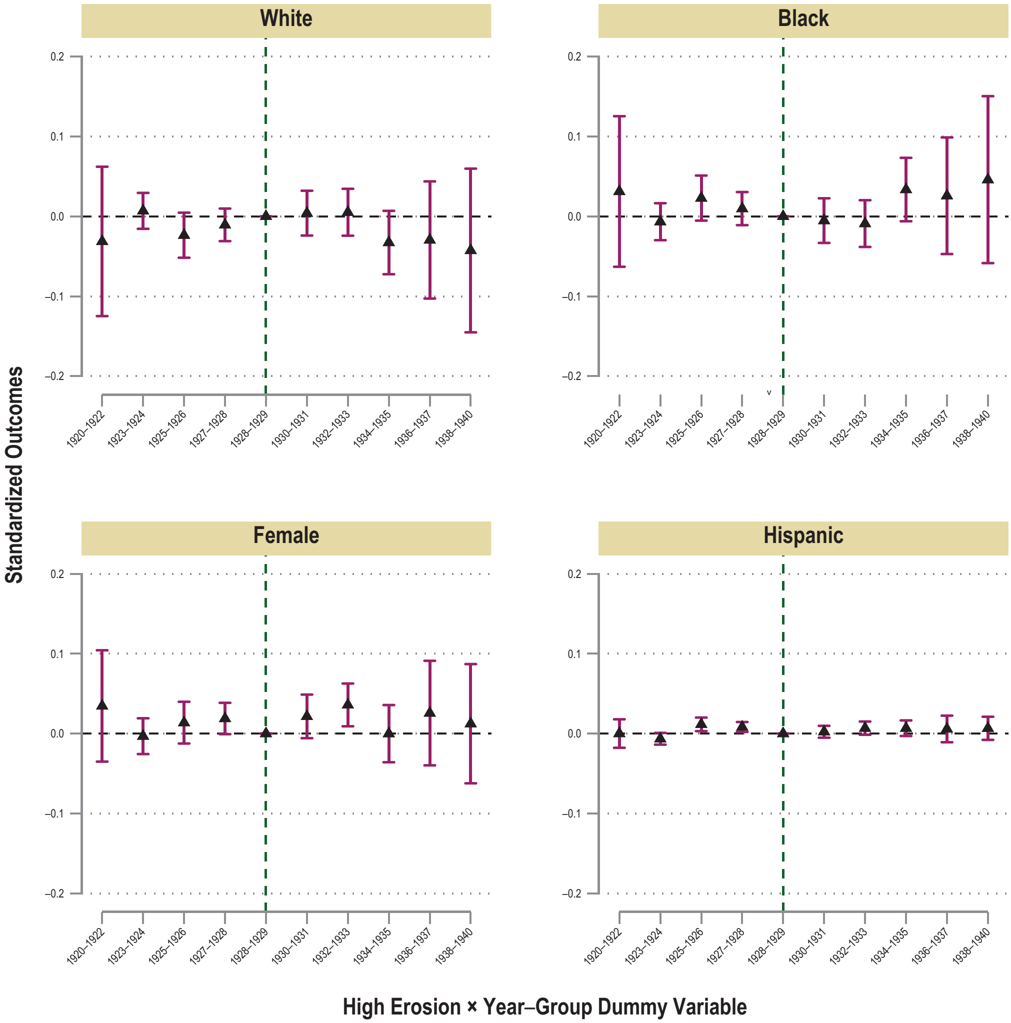

Fig. 1 Point estimates and 95% confidence intervals from event study exploring cross-cohort race, ethnicity, and sex compositional changes and topsoil erosion measures. Standard errors are clustered on the county. Regressions include state–year and county fixed effects and are weighted using county farmland in 1930.

We conduct this analysis through balancing tests, reported and discussed in the online appendix (section B). For instance, the difference-in-differences coefficient of the high-erosion dummy variable on White and female suggests insignificant changes of 18 and 40 basis points, respectively, which is equivalent to roughly a 0.2% and 0.9% change compared with the outcome's mean. We complement this analysis by implementing the same balancing tests using the original cohorts in the full-count census. This analysis eliminates the potential concern of the observed effects of those who were selected in Numident data by looking at the population of these cohorts. Again, we observe quite small and insignificant changes in the population composition for those in high-erosion

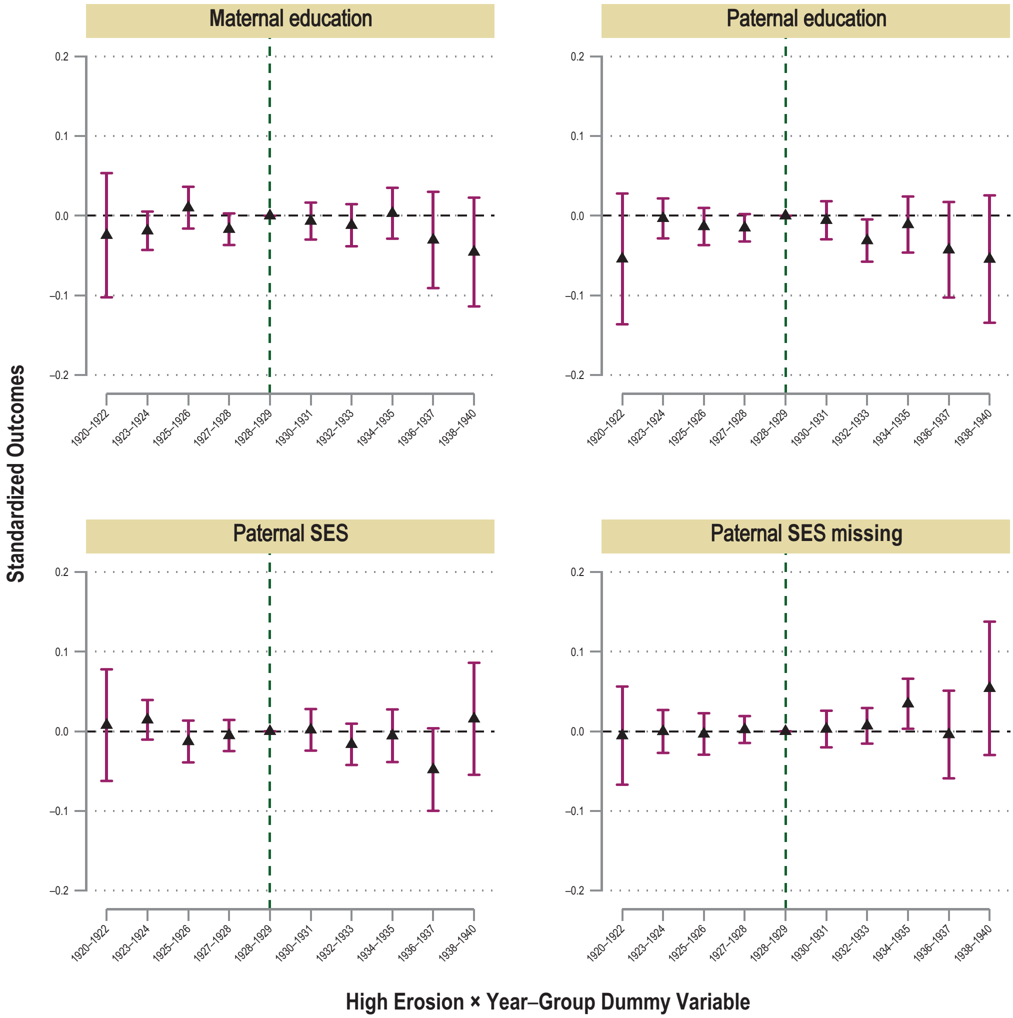

Fig. 2 Point estimates and 95% confidence intervals from event study exploring parental education and paternal SES change across years and topsoil erosion measures. Standard errors are clustered on the county. Regressions include state–year and county fixed effects and are weighted using county farmland in 1930.

counties post-1930. Third, we would expect to observe a consistent pattern across various sociodemographic and socioeconomic outcomes if there was a noticeable compositional change in family formation and the sample's sociodemographic characteristics. However, the pattern is not consistent. We observe (small and insignificant) increases in the share of Whites and females (pointing to longevity improvements) but (small and insignificant) decreases in paternal SES and maternal education (see section B, online appendix). Fourth, as we discuss later, these insignificant changes are also consistent with our analyses on fertility and infant mortality.

In line with the earlier balancing-test analysis of endogenous changes in sociodemographic characteristics, we can implement a direct test of endogenous fertility and

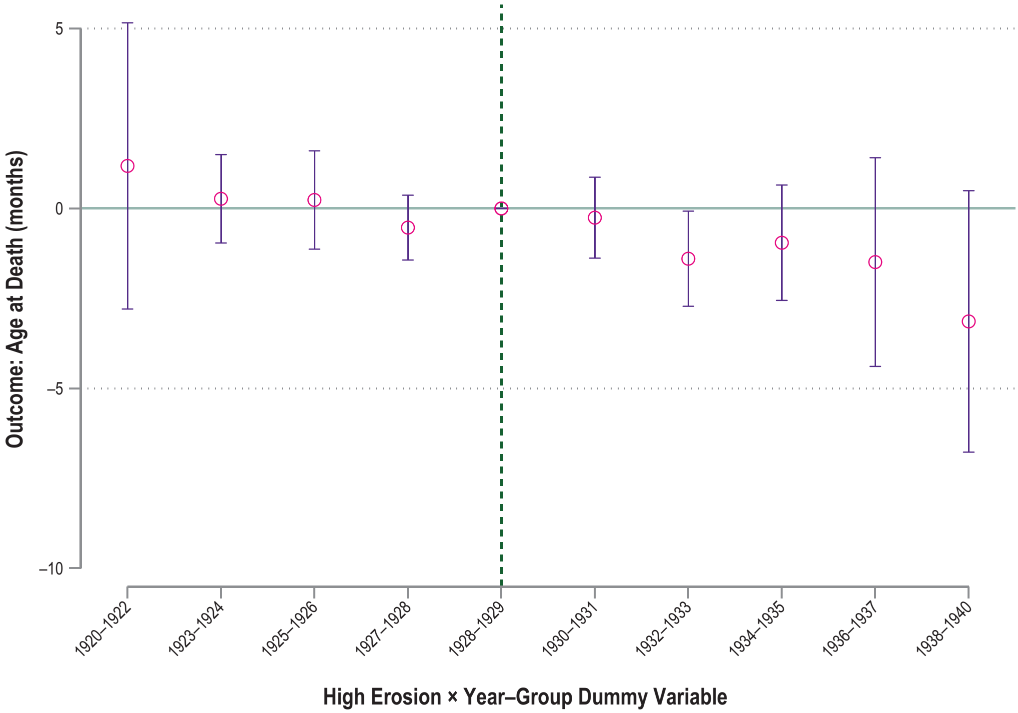

Fig. 3 Point estimates and 95% standard errors from event study exploring *in utero* effects of Dust Bowl exposure on old-age mortality. Standard errors are clustered on the county. Regressions include fixed effects for county and state by birth year. All regressions include individual, family, and county covariates and are weighted using county farmland in 1930.

mortality selection. We have limited natality information for a subset of the United States in 1920–1940. We merge this county–year panel dataset with the topsoil erosion database and implement regressions similar to Eq. (1). We do not observe changes in infant mortality or birth rates, nor do we observe changes in the composition of births to Whites and Blacks (see Table E1, online appendix).

Second, the difference-in-differences coefficients of Eq. (1) pick up the pre-1930 differences across high- versus low-erosion counties. Such differences introduce preexisting longevity trends if the structural differences also appear in other health dimensions of high- versus low-erosion counties that persist in the long-run outcomes. For instance, deep plowing and intensive agriculture could have triggered soil erosion before the 1930s, specifically in counties with higher cumulative erosion recorded in the 1930s. This potential pre-1930 soil erosion could have affected land values and employment in those counties (Hornbeck 2012). To explore the concern of preexisting longevity trends due to the differential trajectory of treated and control counties, we implement an event-study analysis in which we assume that the event occurred at the onset of the 1930s and the event time is the birth year relative to the event date. We aggregate event-time coefficients into two-year bins. We implement the full specifications of Eq. (1) and replace the time dummy variable with our event-study coefficients. The results are reported in Figure 3. Almost all of the pretrend coefficients are statistically and economically insignificant. The effects started rising in magnitude after 1930. This rise is more discernible for 1932–1933 cohorts, for whom the event-time coefficient becomes statistically significant. Although the 1934–1940 coefficients are statistically insignificant, they are quite similar in size to the 1932–1933 coefficients and considerably larger than all pretrend coefficients. From this observed pattern of coefficients, we can deduce that the Dust Bowl period had negative impacts for high-erosion counties and that these effects do not reflect preexisting longevity trends.

Third, the 1940 census and Numident data are potentially endogenously linked. If Numident observations with a higher likelihood of merging with the 1940 census have characteristics that are correlated with both their erosion exposure and their health outcomes that can be detected in old-age mortality outcomes, then the estimates of Eq. (1) likely weigh toward those endogenously determined features rather than the effects of erosion. Our empirical test of this possibility reveals no evidence of an endogenous linking direction (online appendix, section E). We observe no statistically meaningful associations between the exposure variables and the likelihood of merging from the original population.

Comparison With Alternative Data Sources

Our data source covers deaths occurring in 1988–2005. One concern in this censored-structure data is left and right truncation, which could bias the results if the early-life exposure effects appear at younger ages, inhibiting detection at older ages. Similarly, the effects could have a latent aspect such that they are concentrated in older ages. We explore these potential age-specific concentrations of effects using an alternative data source that covers earlier death records: the DMF data from Goldstein et al. (2021), covering deaths occurring in 1975–2005. We implement the same sample selection criteria and employ the same regressions as in Eq. (1). We report and discuss our comparisons in the online appendix, section H. We find significant effects of comparable size. We also argue that the inclusion of the earlier death window might slightly increase the magnitude of the effects.

Another concern is the exclusion of post-2005 deaths in the death window of the Numident data. To explore the sensitivity of the results to later deaths, we use Vital Statistics death records of all deaths up to 2020. These data are disadvantaged in that they do not contain the county of birth and report only the state of birth for post-1979 death years. We use an aggregated version of topsoil erosion data and implement regressions with an identification strategy comparable to Eq. (1). We report and discuss the results in the online appendix, section D. We argue that this truncation underestimates the true effects (similar to the analysis with the DMF data in section H). Our back-of-the-envelope calculation suggests that high erosion is associated with roughly 2.8 months lower longevity (if we could expand the death window to pre-1988 and post-2005 deaths).

Moreover, the truncation of the death window could confound the estimated effects if the selection process is correlated with exposure measures in our equations. To address this issue, we employ the Heckman two-step correction model (Heckman 1979), which estimates the selection process using the universe of the original cohort in the first step and calculates inverse Mills ratios that are added to the second-step equation as an additional control to correct for selection bias. As shown in section I of the online appendix, the estimated effects are comparable to those reported in Tables 2 and 3, suggesting that nonrandom selection due to truncation is not likely to confound mortality equations.

Potential Mechanisms

The Dust Bowl may have affected later-life mortality through several channels. The immediate impact of this environmental catastrophe was on agricultural production. In section J of the online appendix, we use county-level production data for selected years with available data for 1920–1940 to examine the effects of topsoil erosion on total agricultural production using regressions similar to Eq. (1). We find reductions of roughly 50% to 90% for the county-level production of oats, wheat, barley, rye, and potatoes. This substantial impact on agricultural output may have affected food supply, food security, and food prices. This channel is plausible: food prices and food insecurity are more relevant for lower SES individuals, as evidenced in the larger effects for non-Whites and individuals from lower education families in Table 3.

A second channel is reductions in income from agriculture and from the spillover effects of the agricultural sector on other sectors. We empirically test this possibility using full-count census data for a similar period (1920–1940) and implementing regressions similar to Eq. (1). As shown in section J of the online appendix, we observe reductions in farm status, as would be expected if the Dust Bowl worsened job prospects and profits in the farming sector. We also observe reductions in the average county-level SES. However, we find a much larger drop in SES among those with farm-related occupations. Furthermore, although average county-level labor force participation rates did not change, labor force participation among individuals with farm-related occupations decreased significantly. This channel is also plausible, given the much larger longevity effects among children whose fathers were farmers (Table 3, column 6).

Another possible channel is air quality changes. Although we are unaware of detailed pollution data available for the period of study (limiting our ability to test this channel directly), studies suggest that dust storms and dust clouds contain health-detrimental pollutants (Heft-Neal et al. 2020; Jones 2020). Moreover, *in utero* and early-life exposures to pollution negatively and significantly impact health outcomes, with lingering effects across the life course (Carpenter and Bushkin-Bedient 2013; Currie et al. 2014).

Another potential pathway is the influence of the Dust Bowl on mental pressure and stress. Stress may directly affect young children's health with long-lasting consequences. For instance, early-life stress is associated with adulthood metabolic syndromes (Pervanidou and Chrousos 2012), adverse cognitive functioning (Hedges and Woon 2010), and negative effects on neural systems, with implications for psychological and behavioral development (Smith and Pollak 2020). Moreover, environmentally induced maternal stress during prenatal development has adverse effects on infants and results in low birth weight and preterm birth (Duncan et al. 2017; Kim et al. 2017; Torche 2018). Maternal stress releases stress hormones (e.g., cortisol) that pass through the placenta and affect fetal development by inhibiting organ growth and leading to preterm birth. Premature birth is associated with a higher likelihood of low birth weight, with potential short- and long-run effects on health and social outcomes across the life course (Almond et al. 2005; Fletcher 2011). Although stress is a likely channel, lack of detailed data for the study period limits our ability to test the associated links.

These channels can potentially affect a wide range of outcomes across the life course. Some of these outcomes could be mediatory channels through which the initial impacts affect old-age health and mortality. For instance, several studies have suggested that changes in education and income are potential pathways between early-life shocks and old-age health (Adhvaryu et al. 2019; Almond et al. 2018; Currie and Vogl 2013; Kesternich et al. 2015). However, information on education and income is not available in the Numident data. Moreover, the 1940 census information on education and income is unreliable because cohorts have not completed their education or have yet to enter the labor market. Another barrier to exploring education–income channels is that the public-use censuses do not report county of residence for samples from 1950 onward. Although the IPUMS extracts of Ruggles et al. (2020) de-identify some counties on the basis of population and other geographic identifiers, these data are far from universal and usually identify less than 17% of counties.

The census began providing a below-state and universal-coverage geographic identifier, Public-Use Microdata Area (PUMA), in 1960. The PUMA boundaries vary over time and are defined using population and population densities. A PUMA covers several counties in rural areas with low population densities, but several PUMAs constitute one county in urban areas. We employ PUMA and the IPUMS extract's de-identified counties to construct a new geographic boundary that is the greater of the county and PUMA. We name this geographic variable *PUMA-county*. We build a crosswalk between counties and PUMA-counties and aggregate the soil erosion database into the PUMA-county level. We then merge the PUMA-county-level soil erosion database with the 1960 census using individuals' PUMA-county and birth year. We implement similar sample selections as in the main analysis and focus on 1920–1940 cohorts with full information on education and income, regardless of their labor force status. We then implement regressions similar to Eq. (1), with measures of education and income as the outcomes.

Columns 1–9 of Table 4 display the results for dummy variables indicating that the individual had more than 4–12 years of schooling, respectively. The main purpose of this set of definitions is to explore the levels of education at which the effects are concentrated. We observe increases in those who finished elementary and secondary schooling. However, the effects flip sign and become small and insignificant for schooling beyond 9 years (i.e., high school education and above). Specifically, we do not observe statistical changes for high school graduate and college education (columns 8 and 9). Arthi (2018) found increases in education that were more concentrated in high school completion. However, our results suggest that children were more likely to finish elementary and middle school and enter high school but were less likely to finish high school and go to college owing to exposure to high erosion. Consistent with Arthi (2018), we argue that in the absence of the Dust Bowl, these children would have worked on the farm and that the reductions in agricultural employment pushed them toward entering high school.

Table 4 Exploring potential mechanisms using the 1960 census

	Years of Schooling									Log Total Personal Income	Log Wage Income
	>4 (1)	>5 (2)	>6 (3)	>7 (4)	>8 (5)	>9 (6)	>10 (7)	>11 (8)	>12 (9)	(10)	(11)
High Erosion × Post-1930	.02676**	.04208**	.03481**	.02807**	.01860*	.02012*	–.00073	–.00086	.00118	–.26662**	–.35049**
	(.00729)	(.00951)	(.00877)	(.00780)	(.00814)	(.00804)	(.00847)	(.00652)	(.00458)	(.07501)	(.09324)
Number of Observations	645,778	645,778	645,778	645,778	645,778	645,778	645,778	645,778	645,778	645,058	645,778
R^2	.11965	.14072	.12926	.11937	.10938	.10335	.06642	.05974	.05012	.32987	.22662
Mean	0.928	0.890	0.795	0.728	0.644	0.572	0.180	0.131	0.089	5.371	4.830

Notes: Standard errors, clustered on the county, are shown in parentheses. Regressions include fixed effects for county-PUMA and state by birth year. All regressions include individual covariates. Regressions are weighted using IPUMS-provided person weights.

$^*p<.05$; $^{**}p<.01$

Columns 10 and 11 explore the effects on log total income and log wage income, respectively. The coefficients suggest 27% and 35% reductions in total income and wage income, respectively. These results are also consistent with Arthi's (2018) documentation of effects on later-life disability. In contrast, the effects seem to contradict those of education in columns 1–6. However, the effects of education appear only to push those with elementary schooling to attend secondary schooling. These improvements do not translate into high school graduation or college attendance. The literature on education and income usually suggests larger improvements in high school graduation and college education rates (Murnane 2013). More importantly, the primary channel between education and health is changes in income and SES. As implied from Table 4, the improvements in primary and secondary education (columns 1–6) do not translate into improvements in personal income. Hence, the effects on education may not benefit health outcomes later in life. On the other hand, the relatively large effects on income signify worse socioeconomic outcomes during adulthood. The adverse socioeconomic effects and the observed reductions in income could persist through old age and appear in old-age health and mortality outcomes (Biggs et al. 2010; Chetty et al. 2016; Cristia 2009; Demakakos et al. 2015; Gong et al. 2019; Hajat et al. 2011).

Discussion and Conclusion

Individuals worldwide are increasingly facing natural disasters and environmental catastrophes, partly owing to climate change (Banholzer et al. 2014; Cappelli et al. 2021). However, media attention and government interventions are usually short-lived. A recently developed literature provides evidence of the medium-run outcomes of early-life and childhood exposures, but little is known about the long-run outcomes (Aizer, Eli et al. 2016; Almond et al. 2018). Understanding the full costs for exposed populations is important in cost–benefit calculations and relief policy designs. We attempted to complement this literature and contribute to our growing understanding of this topic by evaluating the effects of *in utero* and early-life exposure to Dust Bowl–driven soil erosion on later-life mortality.

We employed SSA death records linked to the full-count 1940 census. We implemented difference-in-differences designs to compare the longevity outcomes of individuals born in high-erosion counties versus low-erosion counties in the 1930s (the Dust Bowl era) versus earlier. Our results suggest statistically significant longevity reductions of 0.85 months for those born in high-erosion counties. We used the universe of cohorts born in the 1930s and observed in 1940 to extract the potential total life-years lost due to early-life exposure to the Dust Bowl. We implemented the same inference method as in the main analysis to assign their county of birth. A simple back-of-the-envelope calculation suggests that 190,842 life-years were lost among cohorts born in high-erosion counties.[6]

[6] In total, 2,694,248 children were born in the 1930s (observed in the full-count 1940 census) in high-erosion counties. Multiplying this number by the marginal effect of high erosion in column 5 of Table 2 (0.85 months) and dividing by 12 leads to roughly 190,842 years.

We implemented event studies to examine the concerns over preexisting health and longevity trends. We found small increases in the share of females and small reductions in father's education and father's SES among exposed cohorts, implying a limited role in the selection of births based on family SES that could partially lead to the observed effects on longevity. However, these selection effects are relatively minor, and most coefficients are insignificant.

We conducted robustness checks to show that our results are not sensitive to additional covariates and fixed effects, alternative outcomes, functional forms, alternative difference-in-differences estimations, alternative clustering levels, or alternative subsamples. In addition, our heterogeneity analyses revealed larger effects among females and individuals whose mothers had lower education.

We used the 1960 census to explore potential mechanisms. Regression results revealed that exposed individuals were more likely to attend secondary school. However, we did not find improvements in high school graduation and college attendance rates. On the contrary, we found relatively large effects on income. We argue that the scarcity of agricultural jobs pushed children to attend secondary school, but the negative health effects overcame the potentially small effects of education such that the net effects on income were negative, statistically significant, and relatively large in magnitude. We posit that the income and wage reductions signify worsened SES, which could persist over the life cycle and influence old-age health and longevity.

Finally, our study has some limitations. First, we assumed that the county of residence in 1935 and 1940 (obtained from the 1940 census) could provide a reliable measure for the county of birth. However, individuals might have migrated from their county of birth by 1935, and their migration could have resulted from the Dust Bowl. Migration adds measurement error to our regressions and, more importantly, could be correlated with individuals' later-life longevity (Black et al. 2015; Rasulo et al. 2012). Second, the data we used provide truncated death coverage (1988–2005), potentially inducing sample selection that downwardly (upwardly) biases estimates if mortality is higher (lower) at younger ages before 1988 as a result of early-life exposure to the Dust Bowl. Similarly, selection could result in underestimated (overestimated) coefficients if the mortality effects are stronger (weaker) at older ages—specifically, as observed after 2005. Although we found suggestive empirical evidence to rule out this concern, the death years were not selected randomly, and the data still might have produced biased estimates. Further, as discussed earlier in this article (and in section D, online appendix), the effects could have been much larger had we had information on deaths before 1988 and after 2005. Third, we did not have access to individuals' other later-life outcomes and so could not implement a more comprehensive analysis in exploring mechanisms. This data limitation forced us to use a sample of similar cohorts from the 1960 census to examine the effects on education and income. Therefore, we advise using caution in interpreting the results of the mechanisms analysis because it does not contain the same individuals as those in the final sample. ■

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Hamid Noghanibehambari (corresponding author)
noghanih@apsu.edu

Noghanibehambari • College of Business, Austin Peay State University, Clarksville, TN, USA; https://orcid.org/0000-0001-7868-2900

Fletcher • La Follette School of Public Affairs, University of Wisconsin–Madison, Madison, WI, USA; https://orcid.org/0000-0001-8843-0563