NBER WORKING PAPER SERIES

DUST TO FEED, DUST TO GREY:
THE EFFECT OF IN-UTERO EXPOSURE TO THE DUST BOWL ON OLD-AGE LONGEVITY

Hamid Noghanibehambari
Jason Fletcher

Working Paper 30531
http://www.nber.org/papers/w30531

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2022

The authors claim that they have no conflict of interest to report. The authors would like to acknowledge financial support from NIA grants (R01AG060109, R01AG076830) and the Center for Demography of Health and Aging (CDHA) at the University of Wisconsin-Madison under NIA core grant P30 AG17266. We thank Joshua Goldstein for helpful comments. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Dust to Feed, Dust to Grey: The Effect of In-Utero Exposure to the Dust Bowl on Old-Age Longevity
Hamid Noghanibehambari and Jason Fletcher
NBER Working Paper No. 30531
October 2022
JEL No. I12,I15,J0

ABSTRACT

Intensive agriculture and deep plowing resulted in top-soil erosion and dust storms during the 1930s. These effects have been shown to affect agricultural income and land values that persisted 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 find inconclusive and mixed results. This paper re-evaluates this literature and studies the long-term effects of in-utero and early-life exposure to top-soil erosion caused by the Dust Bowl of the 1930s on old-age longevity. Specifically, we employ Social Security Administration death records linked with the full-count 1940 census and implement event studies and difference-in-difference designs to compare the longevity of individuals in high/medium versus low top-soil erosion counties post-1930 versus pre-1930. We find intent-to-treat reductions in longevity of about 0.9 months for those born in high erosion counties post-1930. We show that these effects are not an artifact of preexisting trends in longevity. Additional analyses suggest the effects are more pronounced among children raised in farm households, females, and those with lower maternal education. We also provide suggestive evidence that reductions in adulthood income are a likely mechanism channel.

Hamid Noghanibehambari
University of Wisconsin-Madison
1180 Observatory Drive
Unit 4401
Madison, WI 53706
USA
noghanibeham@wisc.edu

Jason Fletcher
University of Wisconsin-Madison
La Follette School of Public Affairs
1225 Observatory Drive
Madison, WI 53706
and NBER
jfletcher@lafollette.wisc.edu

1. Introduction

Several recent and growing strands of research in various settings emphasize the relevance of prenatal development and early-life periods for a battery of outcomes later in life (Almond et al., 2018; Almond & Currie, 2011; Barker, 1990; Barker et al., 2002). Health endowment at birth and the gradual accumulation of health capital in the early years of life sets out a trajectory for the physical, cognitive, and socio-emotional aspects of infants and children. Therefore, disturbances in the initial health capital during this critical period could lead to disruptions in the pathways of later-life outcomes. For instance, studies document negative short-term 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 & Schnepel, 2018; Currie & 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 into adverse health outcomes during infancy and early childhood, which in turn appear in a wide array of medium-run and long-run outcomes, including cognitive development (Aizer et al., 2016; Berthelon et al., 2021), test scores (Sanders, 2012; Shah & Steinberg, 2017), educational attainments (Almond et al., 2009; Fuller, 2014), adulthood earnings (Behrman & Rosenzweig, 2004; Black et al., 2007), health during adulthood (Maruyama & Heinesen, 2020), hospitalization during adulthood (Miller & Wherry, 2019), and later-life old-age mortality outcomes (Goodman-Bacon, 2021; van den Berg et al., 2011).

During the late nineteenth and early twentieth centuries, American Great Plains farmers expanded agricultural production and implemented deep plowing of the virgin top-soil. The ever-growing practice eliminated native grasslands required to cover and retain ground soil. The loss of top-soil coverage combined with severe droughts during the 1930s caused a series of selfperpetuating wind erosions through the Aeolian processes. The erosion of unprotected land also facilitated overland flows and surface runoffs caused by rainwater and storm-water. Top-soil erosions resulted in inevitable agricultural crop failures, negative shocks to local economies, and pushing up agricultural product prices (Hansen & Libecap, 2015; Hornbeck, 2012). The resulting adversities of the infamous Dirty Thirties or the Dust Bowl era could be translated into adverse health outcomes among the vulnerable population and specifically infants, with potentially long-run consequences. This is expected as several studies that explore similar shocks to agricultural failures, and other environmental impacts find significant effects that can be detected in long-run outcomes (Barreca et al., 2021; Le & Nguyen, 2021, 2022; Lindeboom et al., 2010). However, the literature on the later-life health effects of the Dust Bowl is mixed and provides inconclusive evidence. Cutler et al. (2007) explore in-utero Dust Bowl exposure and finds no meaningful effects on adulthood height, BMI, disability, diseases, and mortality. In a similar study, Atherwood (2022) also finds no impact of childhood exposure to the Dust Bowl on old-age longevity. However, Arthi (2018) documents sizeable increases in disability rates for adults with childhood exposure to the Dust Bowl. Our paper enters at this point of the literature and aims to re-evaluate the later-life health effects of in-utero and early-life exposure to severe top-soil erosions of the 1930s on old-age longevity.

The top-soil erosions of the 1930s combined 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 unearthed in their old-age health and mortality outcomes. We ask whether in-utero exposure to the top-soil erosions during the 1930s can be detected in old-age mortality outcomes. We employ data from Social Security Administration Death records over the years 1988-2005 linked with the full-count 1940 census. We compare longevity outcomes of people born in high and medium erosion counties versus low erosion counties over the 1930 years versus before. We implement a series of balancing tests to examine the changes in the demographic composition of births as a result of exposure to higher levels of soil erosion. Comparing treated and control counties pre-1930, we do not observe a discernible difference in demographic and socioeconomic characteristics. Post-1930, we find some evidence of increases in the share of females, those with lower father education, and with lower socioeconomic status. This implies a change in the composition of births and early-life mortality selection of the Dust Bowl era. A higher sex ratio at birth suggests higher fetal and infant mortality rates among males, consistent with the *fragile male* hypothesis. In addition, we posit that part of the pathway between early-life top-soil erosion and later-life mortality can be explained by the selection of births from lower socioeconomic status families. Our main results point to significant and negative intent-to-treat effects. Relative to counties categorized as low erosion farmlands, individuals born in high erosion and medium erosion counties post-1930s live 0.9 and 0.2 months shorter lives, respectively. These effects are equivalent to 24 and 14 percent of the white-black gap in longevity in our regressions, respectively. Although the effects of medium erosion counties are noisy, they become larger in magnitude and statistically significant when we exclude the sample of counties located in the South.

We implement event studies and show that there are no preexisting trends in the longevity of more affected counties versus less affected counties up to ten years before the start of the 1930s. We carry out a wide array of robustness checks and extensive sets of 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. We also show that the effects are robust to alternative functional forms and longevity measures. A series of heterogeneity analyses suggest relatively larger effects among females and those with low-educated mothers. The effects also reveal heterogeneity by region and suggest larger effects of soil erosion on counties located in the West and Midwest, which experienced much of the direct effects, while other regions experienced smaller levels of erosion (and not dust).

To search for mechanism paths, we employ 1960 census data and implement a similar identification strategy to explore the effects on education and labor market outcomes. We find suggestive evidence of increases in high school completion. This is also similar to the effects found by Arthi (2018) and suggests some degrees of substitutability between working on farms and attending school during childhood. However, the education effects are not translated into high school completion, college attendance, and income. During adulthood, those born in high erosion areas experience large and significant reductions in their 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. The fact that we could detect negative effects more than half a century later on the longevity of affected cohorts denotes considerable negative life-cycle impacts and likely deteriorated old-age health and well-being. Therefore, policymakers that aim at promoting lifelong health outcomes may focus on early-life events as an effective tool for the prevention of adverse later-life outcomes. Second, in cases 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 & Libecap, 2015). These situations call for government interventions and more collective actions. Our results add to the negative externalities of such events for health outcomes and highlight the role of policy interventions.

The contributions of this paper to the literature are twofold. First, we re-evaluate the literature on the later-life health impacts of the Dust Bowl. Contrary to previous findings of Cutler et al. (2007) and Atherwood (2022), we find negative, sizeable, 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 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).

The rest of the paper is organized as follows. Section 2 provides a literature review. Section 3 discusses data sources. Section 4 introduces the empirical method. Section 5 overviews the results. In section 6, we explore mechanism channels. We depart some concluding remarks in section 7.

2. Literature Review

Soil erosion and the resulting crop failure could influence old-age mortality of affected infants through several channels. In this section, we explore these potential channels and the relevant literature.

The primary channel through which climatic shocks during early life may affect future health and life-cycle outcomes is reductions in agricultural income. Hyland & Russ (2019) explore the long-run effects of exposure to drought during infancy and childhood on adult outcomes using data from several sub-Saharan African countries. They find suggestive evidence for reductions in wealth, education, height, and intergenerational effects on the next generations' birth outcomes. Since the effects are exclusively driven by rural residents, they argue that the findings operate through distortions in agricultural outputs. Le & Nguyen (2021) explore the impact of extreme rainfall variability during fetal growth on childhood health outcomes. They find significant reductions in anthropometric outcomes between ages 1-5 as a result of in-utero exposure to floods and droughts. Molina & Saldarriaga (2017) explore the effects of temperature fluctuation on birth outcomes in several Andean countries. They find that temperature deviation from the location-specific long-term path is associated with increased food insecurity and adverse birth outcomes. Feeny et al. (2021) explore the long-run gender gap consequences of early-life exposure to rainfall shocks in Vietnam. They find that women are less likely to be employed relative to males if they were exposed to rainfall anomalies during their first two years of life. Shah & Steinberg (2017) show that higher local wages induced by higher rainfall in rural India are associated with differential impacts on human capital formation. The rainfall-induced increases in wages are associated with increases in human capital investments during early life but negatively impact investments of children aged 5-16. They argue that substitutability of schooling with labor wage is a channel to reduce the human capital formation of children. Maccini & Yang (2009) investigate the effects of early-life income shocks on adulthood schooling and health in Indonesia. They find that among women, exposure to higher rainfall in the year of birth is associated with higher schooling and improved measures of self-reported health. Banerjee et al. (2010) identify the later-life health impacts of shocks to agricultural income induced by phylloxera pests in French vineyards during the late nineteenth century. They find significant and relatively large intent-to-treat effects on height but fail to detect any impact on life expectancy. Duque et al. (2020) explore the effects of macroeconomic conditions during early-life and childhood on old-age well-being. They use state-year variations in economic conditions during the Great Depression as the proxy for economic conditions and show that macroeconomic indicators in early childhood are strongly associated with a range of health outcomes during old-age, including metabolic syndrome and mortality.

One specific effect of changes in income and agricultural output can be detected in children's food insecurity and prenatal maternal nutritional intake. Studies that explore famine exposure impacts and later-life effects of governmental social programs usually find relatively large impacts operating through changes in food access (Abiona, 2022; Almond et al., 2011; Almond & Mazumder, 2011; East, 2018, 2020; Hernández-Julián et al., 2014; Hoynes et al., 2016; Karimia & Basu, 2018; Majid, 2015; Neelsen & Stratmann, 2011; Painter et al., 2005). For instance, Haeck & Lefebvre (2016) examine the effect of a nutritional assistance program for pregnant mothers in Canada on birth outcomes and find improvements in birth weight of about 70 grams. Lindeboom, Portrait, & van den Berg (2010) explore 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 exploit the regional and temporal variations in potato and rye prices to proxy for early-life food availability. They find that cohorts exposed to the potato famine in their early-life reveal 2.5-4 years lower longevity. Roseboom et al. (2006) investigate the later-life health impacts of fetal exposure to the Dutch famine of 1944-1945. They find that in-utero famine exposure is associated with adulthood glucose intolerance, coronary heart diseases, and disturbed blood coagulation. In a similar study, Van Abeelen et al. (2012) find higher risks of mortality among women exposed to the Dutch famine during their prenatal development. Rosales-Rueda (2018) explore the health effects of early-life exposure to El Nino floods in Ecuador. She finds sizable reductions in household income, food consumption, and maternal breastfeeding. For health outcomes and fetal exposure to flood, she documents significant increases in low birth weight, reductions in childhood test scores, and lower height among children of 5-7 years old.

Another channel of impact between the Dust Bowl and long-run health outcomes is fetal exposure to pollution. A relatively large literature documents the short-run and long-run health impacts of pollution specifically among infants and children (Bharadwaj et al., 2017; Chay & Greenstone, 2003; Currie et al., 2009, 2014; Currie & Neidell, 2005; Currie & Schmieder, 2009; Currie & Walker, 2011; Sanders, 2012; Simeonova et al., 2021). For instance, Moreira et al. (2020) show that Saharan dust intrusion across municipalities in Spain is associated with a higher share of low birth weight infants. Altindag et al. (2017) explore the environment and health impacts of Yellow Dust outbreaks in South Korea, dust clouds carried by high-speed surface winds from China and the deserts of Mongolia into East Asian countries. They find that the occurrence of Yellow Dust rises air pollution and subsequently decreases the birth weight and gestational age of affected infants. In a similar review study, Hasunuma et al. (2019) show that exposure to the Asian dust event results in higher mortality and hospitalization. Currie & Schwandt (2016) explore the effect of the dust cloud created by the 9/11 terrorist attacks on infants' health outcomes. They find that fetal exposure to the pollution of toxic materials created by the aftermath dust is associated with significant negative effects on birth outcomes. Jones (2020) examines the pollution and infant health effects of a series of dust storms in the US over the years 2010-2017. He finds sizeable increases in incidences of low birth weight and preterm birth as a result of increases in dust-driven pollution.

Some studies suggest that natural disasters and climatic catastrophes may impact health endowment at birth through less direct channels such as prenatal maternal stress (Álvarez-Aranda et al., 2020; Caruso & Miller, 2015; Glynn et al., 2001; Hetherington et al., 2021; Kim et al., 2017; Nandi et al., 2018; Torche, 2011). For instance, Currie & Rossin-Slater (2013) explore the impact of stress-induced by hurricanes on birth outcomes. They show that fetal exposure to hurricanes is associated with increases in infants' abnormal conditions and meconium aspiration syndrome.

Noghanibehambari (2022) explores the effects of in-utero exposure to earthquakes on old-age longevity. He finds negative and significant effects for exposure during the first trimester.

While the bulk of evidence weighs on the potential negative effects of dust clouds and agricultural disasters on short-run and long-run health outcomes, the Dust Bowl literature offers mixed evidence. Hornbeck (2012) examines the impacts on high and medium erosion counties versus low erosion counties and documents long-run effects on agricultural production, agricultural income, and agricultural land value. Arthi (2018) explores in-utero and childhood exposure using cross-state variation and finds significant increases in disability during adulthood. She also finds negative impacts on college completion and fertility. However, she finds positive effects on high school completion rates and argues that children, who would substitute schooling with farm work, are more likely to continue schooling with the scarcity of job prospects. Cutler et al. (2007) exploit cross-census-region variations in exposure to the Dust Bowl and examine later-life health and mortality outcomes. They fail to find significant impacts on height, measures of chronic conditions, and disability. These two studies rely on variations across region and state, large geographic areas with potentially wide heterogeneity of exposure and the effects of exposure on health outcomes. A more precise framework is to look at sub-state exposures to obtain a more accurate measure and account for within-state variations. Atherwood (2022) explores county exposures to the Dust Bowl and later-life longevity. He employs Death Master Files (DMF) data and a subsample of Great Plain counties to compare the longevity outcomes of those who resided during their childhood in high versus low erosion counties. He fails to find a significant impact of Dust Bowl exposure on longevity. The important drawback of his research design is that it fails to account for unobserved heterogeneity in treated versus control counties. His research design is basically comparing the longevity of those born in high and medium erosion counties to those born in low erosion counties within the same state, conditional on a limited set of county controls. We overcome this important limitation by introducing an empirical model that accounts for time-invariant unobserved features of counties.

3. Data Sources and Sample Construction

The primary data source used in this study is the Numerical Identification System death records reported by Social Security Administration (SSA), the so-called Numident database, extracted from the Censoc Project outlined in Goldstein et al. (2021). The Numident data covers deaths that occurred to both females and males over the years 1988-2005 and were recorded by SSA. The primary advantage of Censoc-Numident data is that it is linked at the individual level to the full-count 1940 census. Therefore, it makes a longitudinal panel of unprecedented size with a wide array of information on family characteristics as well as granular geographic detail of place of residence in 1940. The lowest geographic area that the public-use full-count 1940 census provides is the county. The census provides information on the county of residence in 1940 and asks the respondent to report their county of residence in 1935 (if different than that of 1940). Since the primary purpose of this study is to explore in-utero and early-life effects, we need to infer the county of birth based on the given information. In so doing, we start by assuming that the county of residence in 1935 is the county of birth. If the information of the place of residence in 1935 is not available and the respondent reported that they have migrated during the last five years, we exclude the person from the sample. Similarly, if the 1935 county information is missing and the respondent's state-of-birth is different than state-of-residence in 1940, we also exclude the observation from the sample. If the state-of-birth is the same as the 1940 state-of-residence, the 1935 county is missing, and the respondent has not migrated in the last five years, it is safe to assume that the 1940 county-of-residence is the same as county-of-birth. To further mitigate the issue regarding migration and the resulting measurement issue in our county-of-birth variable, we limit the sample to cohorts born after 1920.

The data for soil erosion and Dust Bowl is extracted from Hornbeck (2011). He uses data from the Soil Conservation Service and constructs county-level data of the share of farmlands' top-soil that is eroded. He categorizes these fraction measures into three variables based on the cumulative severity of erosion over the 1930s: high fraction erosion, medium fraction erosion, and low fraction erosion. The first measure is the fraction of a county's farmland's top-soil that is highly eroded, and so on for the other two measures. We build dummy variables to indicate whether a county is considered high/medium/low erosion using these three measures. In so doing, our high erosion dummy variable equals one if the share of high fraction erosion is above 75 percent. Equivalently, only 25 percent of the farmland is considered medium and low erosion. Similarly, the medium erosion indicator equals one if the fraction of high erosion is between 50 and 75 or the fraction of medium erosion is more than 50 percent. All other counties are considered low erosion. Figure 1 shows the geographic distribution of high, medium, and low erosion counties across counties. Due to 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 though we are able to examine potential variation indirectly by estimating effects separately by each birth year. This is a common limitation in studies of short-term and long-term effects of the Dust Bowl.

We merge the county-level soil-erosion data with Numident-census data based on the county of birth of individuals. While previous studies focus on a subset of counties that are specifically affected by the wind erosions and historically recognized as the Dust Bowl counties, we focus on all US counties while showing the heterogeneity by different regions. There are two main reasons not to limit the geographic coverage of the sample. First, while the erosion hit the

Great Plains more severely than others, the effects were detectable in many areas as far as the prairies of Canada (McLeman et al., 2014; Schubert et al., 2004). Second, the drought-driven erosions were a more universal phenomenon that could impact agricultural products elsewhere in the country.

For further analyses related to endogenous fertility, we use data from Bailey et al. (2016). This data covers births and deaths data for a subsample of more-populated counties. For analysis of mechanism channels, we employ the 1960 census data extracted from Ruggles et al. (2020). We also construct a series of county covariates extracted from decennial censuses 1920-1940 and interpolate them for inter-decennial years. These data are also extracted from Ruggles et al. (2020).

Summary statistics of the final sample are provided in Table 1. The age at death varies between 47.6 and 85.6 years with an average of approximately 71.2 years. About 8 and 32 percent of observations live in high and medium erosion counties, respectively. The sample underrepresents females and overrepresents white people. This is because the Numident-census linking is primarily based on name commonalities and information on place of birth and age. Since females usually change their names after marriage, they are less likely to be linkable. Although nonwhites are underrepresented in the sample, they represent their respective populations with regard to other sociodemographic features (Breen & Osborne, 2022).

4. Econometric Method

The identification strategy exploits the spatial variations in county-specific cumulative top-soil erosions combined with rises in dust clouds and droughts of the 1930s versus a decade earlier. We implement difference-in-difference analyses using ordinary least square regressions of the following forms:

$$y_{icsb} = \alpha_0 + \alpha_1 Post_b \times HErosion_{cs} + \alpha_2 Post_b \times MErosion_{cs} + \alpha_3 X_i + \alpha_4 Z_{csb} + \xi_c + \zeta_{sb} + \varepsilon_{icsb} \tag{1}$$

Where y is the outcome (ag at death) of individual i in county c in state s and born in year b. The variable $Post$ is a dummy that equals one for post-1930 years and zero otherwise. The variable $HErosion$ ($MErosion$) is a dummy variable indicating high (medium) erosion of top-soil in each county over the 1930 decade (see section 3). The matrix X contains individual and family controls including dummies for race, gender, maternal education, and paternal socioeconomic status. In Z, we include several county controls that are constructed using county values in full-count decennial censuses and interpolated for inter-decennial years. These controls include the share of homeowners, share of children less than 5 years old, share of literate people, share of married, and average occupational score. The parameters ξ and ζ represent county and state-by-birth-year fixed effects. 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.[4]

5. Results

5.1. Concerns over Endogeneity

The estimated coefficients of equation 1 could potentially provide biased estimates of the true impacts for several reasons which we discuss below. The first concern is regarding likely changes in the composition of cohorts in the treated and control subsamples. These treated-control cohort differences could bias the estimates if there are other dimensions associated with each group that is also correlated with our soil erosion exposure measures. For instance, if people of lower

[4] In Appendix B, we show the results of unweighted regressions and examine the sensitivity of the results to alternative weights. The estimated coefficients suggest stability to alternative weights and unweighted regressions.

socioeconomic status are more likely to be exposed to the erosion shocks, the estimated coefficients overestimate the true effects since these people have lower age at death due to unobserved factors. The differential exposure or the differential cohort composition of the final sample could be the result of several sources, including endogenous post-event migration, endogenous birth composition, endogenous fetal and infant deaths, and endogenous survival into adulthood. We argue against these sources of endogeneity by providing two pieces of empirical evidence. First, we directly test for changes in differences in the composition of the final sample by implementing a series of balancing-test-type event studies. In these event studies, we assume that the event occurs at the onset of the 1930s. We compare the characteristics of people in high and medium erosion counties with low erosion counties in different years relative to the years 1929-1930. We group event-times into two-year bins. To explore the pre-post-trends in sociodemographic characteristics, we use individual and family characteristics as the outcomes. In all regressions, we include state-year and county fixed effects. These results are reported in Figure 2 through Figure 5. In each figure, the results of two top (and similarly two bottom) panels come from the same regressions in which the high erosion (left panel) and medium erosion (right panel) measures are interacted with event-time dummies. For comparison purposes, we standardized all outcomes.

We observe small decreases in the share of whites for the years 1934-1935 in high erosion counties (top-left panel of Figure 2). Since there are very few people of other races, the results on black (two bottom panels of Figure 2) are a reflection of the effects on white. There is also a slight increase in the share of females for high erosion measures in the 1932-1933 group (top left panel of Figure 3). This fact suggest there is a change in the composition of births, for example, due to a higher male fetal death selection. It also points to potential raises in male infant mortality rate which led a higher female survival into adulthood, consistent with *fragile male* hypothesis (Clark et al., 2021; Drevenstedt et al., 2008; James & Grech, 2017; Rosa et al., 2019; Weinberg et al., 2008).

In addition, we observe small decreases in fathers' education for 1932-1933 cohorts in high erosion counties (bottom-left panel of Figure 4). Although post-1930 coefficients of father's socioeconomic score suggest slight decreases in the outcome, they are statistically insignificant (top-left panel of Figure 5). In addition, the share of fathers with missing information on the socioeconomic index rises for 1934-1935 cohorts in high erosion counties (bottom-left panel of Figure 5). Overall, these results offer some evidence of selection of births based on father education and socioeconomic status. Since lower socioeconomic status families, on average, have children with lower health endowment and potentially lower longevity, part of the results in the reduced-form analysis (section 5.2) could operate through this selection channel.

Besides these exceptions, almost all other pre-and-post-1930 coefficients in both groups are statistically insignificant. Moreover, the effects do not reveal a consistent pattern of rising/falling during the 1920s and 1930s. We complement the analyses of these event studies by employing the difference-in-difference estimations of 1 with sociodemographic characteristics as outcomes. The estimated results, reported and discussed in Appendix A, provide a similar story as the event studies discussed in this section.

Second, we have limited natality information for a subset of US counties over the years 1920-1940. We employ this county-year panel dataset and merge it with the top-soil erosion database and implement regressions similar to equation 1. The results are reported in Table 2. We do not observe any change in infant mortality rates and birth rates. Moreover, there is no change in the composition of births to whites and blacks.

The second concern is that the difference-in-difference coefficients of equation 1 pick up the pre-1930 differences across high-medium versus low erosion counties. Such differences introduce preexisting trends in longevity if the structural differences also appear in other health dimensions of high-medium versus low erosion counties that persist in the long-run outcomes. For instance, deep plowing and intensive agriculture could have triggered soil erosion in the years prior to the 1930s specifically in counties with higher cumulative erosion recorded in the 1930s. These potential pre-1930 soil erosions could have affected land values and employment in those counties (Hornbeck, 2012). To explore the concern of preexisting trends in longevity due to the differential trajectory of treated and control cohort-counties, we implement an event study analysis in which we assume the event occurs 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 equation 1 and replace the erosion measures with our event study coefficients. The results are reported in Figure 6 for high erosion and medium erosion exposures in the top and bottom panels, respectively. Almost all of the pre-trend coefficients are statistically and economically insignificant in both panels. In the top panel of high erosion measures, the effects start to rise in magnitude for post-1930 years. However, in the bottom panel, we do not observe a discernible post-trend.

The third concern is the potential endogenous linking between the 1940 census and Numident data. If observations in Numident with a higher likelihood of merging with the 1940 census have some 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 equation 1 likely weigh toward those endogenously-determined features rather than the effects of erosion. To investigate this source of bias, we start with the full sample of cohorts born in 1920-1940 and implement similar sample selections as our Numident-census sample. We then merge it with the soil erosion database. Next, we merge it with the final sample of Numident data and create a dummy variable to indicate successful merging. We regress this variable on measures of soil erosion conditional on county and state-by-birth-year fixed effects. The results are reported in Table 3 for the full sample and several subsamples across columns. The estimated coefficients fail to provide any evidence of endogenous merging. The effects are statistically insignificant and economically small in magnitude.

5.2. Main Results

The main results of the paper are reported in Table 4. We start with regressions that include only county and birth year fixed effects and add more covariates across consecutive columns. When we add state-by-birth-year fixed effects, the marginal effects slightly rise in magnitude (column 2). However, in columns 2-5, the effects are quite robust and similar as we add more controls. The full specification of column 5 suggests that cohorts in high and medium erosion counties versus low erosion counties post-1930 versus pre-1930 have 0.9 and 0.2 months shorter lives, respectively. However, the coefficients of medium erosion counties are statistically insignificant.

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 the female-male gap in longevity (implied by the coefficient of female, not reported in this table) and the black-white gap in longevity (implied by the coefficient of black, not reported in this table). The effects of high erosion represent a 12.5 percent of female-male gap and 18 percent of the black-white gap in longevity.

Hornbeck (2012) finds that high erosion counties experienced a 12 percent reduction in retail sales per capita, equivalent to about 0.11 standard-deviations change in retail sales per capita in their sample. Noghanibehambari et al. (2022) explore the effects of in-utero and early-life economic conditions on old-age mortality. They proxy local economic conditions with bank deposits per capita and find significant associations between deposits per capita and old-age longevity. Furthermore, they attempt to validate their proxy by showing that bank deposits are correlated with alternative measures of local economic conditions including retail sales per capita. They find that a one-standard-deviation decrease in bank deposits per capita is associated with 0.21 standard-deviations drop in retail sales per capita and roughly 2.7 months lower age at death. Using the cumulative effects reported by Hornbeck (2012) and these effects, we reach a back-of-an-envelope effect of 1.4 months due to worsening economic conditions as a result of high erosion. This is slightly larger than the marginal effect of 0.9 months in Table 4.

The average life expectancy at birth in the US increased by about 8 years between the years 1920-1940. The intent-to-treat effect of 0.9 months on longevity represents about a 1 percent 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 that explore the determinants of longevity. For instance, Chetty et al. (2016) explore the income-longevity relationship across income percentiles and find that for each additional income percentile longevity increases by about 1.9 months, a relatively constant factor across different baseline percentiles. Fletcher & Noghanibehambari (2021) explore the effects of college opening on college education and mortality. They find intent-to-treat effects of 0.13 months additional longevity for each additional 4-year college opening as a result of increases in college education. In comparison with our estimated intent-to-treat effects, the negative effects of high top-soil erosion offset the positive effects of roughly 7 new 4-year college openings in the local area.

5.3. Robustness Checks

We explore the sensitivity of our results across alternative models in Table 5. To have a benchmark comparison, we replicate the full specification of column 5 of Table 4 in column 1. In column 2, we add additional family controls including the father's wage income in 1940, number of siblings, father's employment status, and a dummy for ownership of the dwelling. Although we lose some of the observations due to missing values, the effects are very similar in magnitude to those of column 1. In columns 3 and 4, we add to the full model of column 1 a series of county-by-individual-characteristics dummies and county-by-parental-characteristics dummies, respectively. Therefore, we allow for time-invariant features of counties to have differential effects among households and individuals with different sociodemographic characteristics. The estimated marginal effects are quite similar to those of column 1.

We control for seasonality in birth and death by including birth-month and death-month fixed effects in column 5. The effects are quite comparable to the baseline estimates. In column 6, we add a battery of additional county-by-year controls including population, share of females, share of whites, share of blacks, share of other races, share of Hispanics, share of immigrants, share of children less than 5, share of literate people, and share of married people. The estimated coefficients are slightly smaller than those of column 1 but remain statistically significant.

In column 7, we check the sensitivity to the functional form by replacing the outcome with log of age at death. The marginal effect of high erosion suggests a 0.11 percent reduction in longevity. In column 8, we replace the outcome with a dummy indicating age at death greater than

70 years. The estimated effect of high erosion suggests 63 basis-points reductions in the likelihood of living beyond age 70, off a mean of 0.52.

Furthermore, we explore the sensitivity of the difference-in-difference model to heterogeneity in treatment effect by replicating the main results using methods developed by Sun & Abraham (2021). The marginal effect of high erosion rises by about 5 percent.

In column 10, we show the robustness of statistical significance by employing a two-way cluster technique and clustering the standard errors at the county and state-by-birth-year levels. This clustering level account for both serial and spatial correlations in the error term. Standard errors are quite similar to the main results.

In columns 11 and 12, we use more restricted subsamples of cohorts, specifically, cohorts of 1925-1935 and 1925-1940, respectively. In both subsamples, the marginal effects are comparable to those of column 1 and remain statistically significant at conventional levels for high erosion exposure measure.

5.4. Heterogeneity across Subsamples

We explore the heterogeneity of results across subsamples based on sociodemographic characteristics in Table 6. Column 1 replicates the main results of column 5 of Table 4. Column 2 focuses on the subsample of white individuals. The marginal effect of medium erosion rises in magnitude by about 35 percent while that of high erosion drops by about 13 percent. In column 3, we restrict the sample to females. The estimated effect of high erosion rises by about 12 percent although becomes statistically insignificant. This differential impact across gender has also been shown by other studies. Several studies find larger health shock impacts and more persistent impacts among females (Ae-Ngibise et al., 2019; Bharadwaj & Lakdawala, 2013; Chen et al., 2020; Muchomba & Chatterji, 2020; Wang et al., 2017).

In column 4, we focus on those whose fathers work in farm-related occupations. The effect size of high erosion suggests considerably larger impacts. The coefficient of high erosion becomes roughly 2.4 times that of the main results. This fact suggest that a large portion of effects operate through income channels as the top-soil erosions had a large income shock among farmers. Nonetheless, the effect of medium erosion flips sign and remain statistically insignificant.

In column 5, we focus on individuals with low-educated mothers. The marginal effect of high erosion rises in magnitude and remain statistically significant. The big picture of this table suggests lower heterogeneity by race but considerable differences in effects by gender and maternal education. The effects are more pronounced for females and for those with lower maternal education.

In Table 7, we also examine the heterogeneity of the results across different census regions. The effects of high erosion are primarily concentrated in counties located in South, Midwest and West regions. In the South and Midwest, high erosion is associated with 4.4 and 2.9 months lower longevity, about 3-5 times the effect of the full sample. On the other hand, the effect of medium erosion is more pronounced in Southern counties, suggesting about 3 months lower longevity. However, the effects become positive for counties in Northeast and are statistically insignificant for both medium and high erosion measures. However, only about 15 percent of treated counties are located in this region.

The Dust Bowl and its long-lasting legacies for local economies could force out-of-state migration. The Numident data also provides information on state-of-death, which we use to build a measure of migration. We define a migrant as a person whose state-of-birth is different than state-of-death[5]. We then explore whether our top-soil erosion measures are correlated with long-term cross-state migration. The results are reported in column 1 of Table 8. Both measures suggest positive correlations. Relative to the mean of the outcome, medium and high erosion counties are associated with a 1.5 and 1.8 percent increase in cross-state migrants. These positive effects raise an important source of heterogeneity regarding the mitigating impacts of individuals' migration status. Thus, we examine whether the negative effects have differential impacts among migrants and stayers. In so doing, we replicate the main results for the subsample of migrants and stayers in columns 2 and 3 of Table 8, respectively. The marginal effects suggest that the results are primarily confined to the non-migrant subsample. The effects on the migrant population are smaller in magnitude than the main results and statistically insignificant.

5.5. Comparison with Alternative Data Sources

Our data source covers deaths that occurred between the years 1988-2005. One concern is the left and right truncation of data. These types of sample selection bias the results if the early-life exposure effects appear in younger ages which makes them hard to detect in older ages. Similarly, the effects could have a latent aspect and are concentrated in older ages. We explore these potential age-specific concentration of the effects using an alternative data source that covers earlier death records. In so doing, we use DMF data from Goldstein et al. (2021) that covers death occurred between the years 1975-2005. We implement the same sample selection criteria and employ the same regressions as in equation 1. We report our comparisons in Table 9. We replicate the full-sample of Numident from column 5 of Table 4 in column 1. Columns 2 and 3 report the effects among females and males of Numident data, respectively.

[5] Nearly half of our observations die in a state other than their birth state. This is higher than more recent data, for example, Fletcher et al. (2022) note that approximately 1/3 of decedents in the Mortality Disparities in American Communities (MDAC) die in a state other than their birth state.

In columns 4 and 5, we replicate the results for the DMF data that covers only males. In column 4, we restrict the DMF data to cover deaths that occur between the years 1988-2005, similar to the death window of Numident. In column 5, we show the effects of the full death window of DMF.

The comparison between columns 3 and 4 (same-gender and death window samples) suggests that DMF offers larger coefficients for both medium and high erosion. Moreover, the full sample DMF suggests slightly smaller coefficients when we include earlier years of death records (comparing columns 4 and 5). For instance, the high erosion coefficient drops by about 9 percent from the death window of 1988-2005 to the death window of 1975-2005. If we extrapolate this drop for both genders, we can deduce a reduction in longevity of about 0.85 months for exposure to high erosion. Therefore, the inclusion of the earlier death window only slightly reduces the magnitude of the effects.

Another concern is the exclusion of post-2005 deaths in the death window of Numident data. In order to explore the sensitivity of the results to later deaths, we use Vital Statistics death records that cover the universe of deaths up to 2020. The disadvantage of this data is that it does not contain county of birth and reports only state of birth for post-1979 death years. We use an aggregated version of top-soil erosion data and implement regressions with a comparable identification strategy as in equation 1. We report and discuss the results in Appendix C. Our back-of-an-envelop calculation suggests that high erosion is associated with roughly 3 months lower longevity (if we were able to expand the death window to pre-1988 and post-2005 deaths).

6. Potential Mechanisms

Several studies suggest that, in addition to biological mechanisms, 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 & Vogl, 2013; Kesternich et al., 2015). However, the information on education-income is not available in Numident data. Moreover, the 1940 census information on education and income is also unreliable as 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 1950-onward samples. Although the IPUMS-extracts of Ruggles et al. (2020) de-identify some counties based on population and other geographic identifiers, they are far from universal and usually identify less than 17 percent of counties.

Starting in 1960, the census provides a below-state and universal-coverage geographic identifier, Public-Use Microdata Area (PUMA). The PUMA boundaries vary over time and are defined based on population and population densities. In rural areas with low population densities, PUMA covers several counties while in urban areas several PUMAs constitute one county. We employ PUMA and the IPUMS-extract's de-identified counties to construct a new geographic boundary that is the greater of 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 PUMA-county level. We then merge it with the 1960 census based on individuals' PUMA-county and birth year. We implement similar sample selections as in the main results and focus on cohorts of 1920-1940 with full information on education and income, regardless of their labor force status. We then implement regressions similar to equation 1 in which the outcomes are measures of education and income.

The results are reported in Table 10. In column 1 through 9, we use a series of dummy variables indicating the individual's years of schooling more than 4 to 12 years, respectively. The main purpose of this set of definitions is to explore at which levels of education the effects are concentrated. We observe increases in those who finish their elementary and secondary schools. However, the effects become insignificant for schooling beyond 9 years, i.e., high school education and above. Specifically, we do not observe any statistical change for high school graduate and college education (columns 8 and 9). Arthi (2018) also finds increases in education that are more concentrated in high school completion. However, our results suggest that children are more likely to enter high school as a result of higher erosion exposure. In line with Arthi (2018), we argue that these children would choose to work on the farm in the absence of the Dust Bowl and the reductions in agricultural employment push them towards entering high school.

In columns 10 and 11, we explore the effects on log total income and log wage income, respectively. High erosion coefficients suggest 28 and 38 percent reductions in total and wage income, respectively. These are also in line with the effects found by Arthi (2018) on later-life disability. In contrast, the effects seem to contradict those of education in column 1. However, the effects of education only appear 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-income usually suggests larger improvements for high school graduation and college education (Murnane, 2013). Overall, 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).

7. Conclusion

Throughout the world, people are facing ever-increasing natural disasters and environmental catastrophes, partly due to climate change (Banholzer et al., 2014; Cappelli et al., 2021). However, media attention and government interventions are usually short-lived. While a recently developed literature provides evidence of the medium-run outcomes and specifically for early-life and childhood exposures, little is known about the long-run outcomes (Almond et al., 2018; Bailey, Bleakley, et al., 2016). Understanding the full costs of exposed populations is important in cost-benefit calculations and relief policy designs. This paper attempted to complement this literature and contribute to our growing understanding of this topic by evaluating the long-run longevity effects of in-utero and early-life exposure to soil erosion driven by the Dust Bowl.

We employed death records from the Social Security Administration linked to the full-count 1940 census. We implemented difference-in-difference designs to compare the longevity outcomes of individuals who were born in high/medium top-soil erosion counties versus those in low erosion counties during the 1930s (Dust Bowl era) versus before. Our results suggested statistically significant reductions of 0.9 months in longevity for those born in high erosion counties. We use 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 implement the same inference method as described in section 3 to assign their county of birth. A simple back-of-an-envelop calculation suggests 202,106 life-years lost for cohorts born in high erosion counties.[6]

We implemented event studies to examine the concerns over preexisting trends in health and longevity. A series of event studies suggest small increases in the share of females, suggesting a higher fetal and infant deaths to males for those in high erosion counties post-1930s. We also observe some reductions in father education and father socioeconomic index among exposed

[6] Total number of children born during the 1930s (observed in the full-count 1940 census) in high erosion counties is 2,694,248. Multiplying this with the marginal effect of high erosion in column 5 of Table 4 (0.9 months) and dividing by 12 leads to roughly 202K years.

cohorts, implying selection of births based on family socioeconomic index that could partly lead to the observed effects on longevity. However, these selection effects are relatively small and the coefficients are in most cases insignificant.

We carry out a wide range of robustness checks to show that our results are not sensitive to additional covariates and fixed effects, alternative outcomes, functional forms, alternative difference-in-difference estimations, alternative clustering levels, and alternative subsamples. We also apply a series of heterogeneity analyses and find larger effects among females and those with lower maternal education.

We use the 1960 census to explore potential mechanisms. Regression results suggest that exposed individuals are more likely to go from elementary schools to secondary and high schools. However, we do not find improvements in high school graduation and college attendance. On the contrary, we find relatively large effects on income. We argue that while the scarcity of agricultural-related jobs pushes children to attend secondary schooling, the negative health effects overcome the potentially small effects of education in a way that the net effects on income are negative, statistically significant, and relatively large in magnitude. We posit the reductions in income and wages signify worse socioeconomic status that could persist over the life-cycle and appear in old-age health and longevity.

References

Abiona, O. (2022). Malnutrition pathway for the impact of in utero drought shock on child growth indicators in rural households. *Environment and Development Economics*, *27*(1), 20–39. https://doi.org/10.1017/S1355770X20000571

Adhvaryu, A., Fenske, J., & Nyshadham, A. (2019). Early life circumstance and adult mental health. *Journal of Political Economy*, *127*(4), 1516–1549. https://doi.org/10.1086/701606/SUPPL_FILE/2014095DATA.ZIP

Ae-Ngibise, K. A., Wylie, B. J., Boamah-Kaali, E., Jack, D. W., Oppong, F. B., Chillrud, S. N., Gyaase, S., Kaali, S., Agyei, O., Kinney, P. L., Mujtaba, M., Wright, R. J., Asante, K. P., & Lee, A. G. (2019). Prenatal maternal stress and birth outcomes in rural Ghana: Sex-specific associations. *BMC Pregnancy and Childbirth*, *19*(1), 1–8. https://doi.org/10.1186/S12884-019-2535-9/TABLES/4

Aizer, A., Stroud, L., & Buka, S. (2016). Maternal stress and child outcomes: Evidence from siblings. *Journal of Human Resources*, *51*(3), 523–555. https://doi.org/10.3368/jhr.51.3.0914-6664R

Almond, D., & Currie, J. (2011). Killing Me Softly: The Fetal Origins Hypothesis. *Journal of Economic Perspectives*, *25*(3), 153–172. https://doi.org/10.1257/JEP.25.3.153

Almond, D., Currie, J., & Duque, V. (2018). Childhood circumstances and adult outcomes: Act II. *Journal of Economic Literature*, *56*(4), 1360–1446.

Almond, D., Hoynes, H. W., & Schanzenbach, D. W. (2011). Inside the war on poverty: The impact of food stamps on birth outcomes. *Review of Economics and Statistics*, *93*(2), 387–403. https://doi.org/10.1162/REST_a_00089

Almond, D., Lena, E., & Mårten, P. (2009). Chernobyl's Subclinical Legacy: Prenatal Exposure to Radioactive Fallout and School Outcomes in Sweden. *The Quarterly Journal of Economics*, *124*(4), 1729–1772. https://doi.org/10.1162/QJEC.2009.124.4.1729

Almond, D., & Mazumder, B. (2011). Health capital and the prenatal environment: the effect of Ramadan observance during pregnancy. *American Economic Journal: Applied Economics*, *3*(4), 56–85. https://doi.org/10.1257/app.3.4.56

Altindag, D. T., Baek, D., & Mocan, N. (2017). Chinese Yellow Dust and Korean infant health. *Social Science & Medicine*, *186*, 78–86. https://doi.org/10.1016/J.SOCSCIMED.2017.05.031

Álvarez-Aranda, R., Chirkova, S., & Romero, J. G. (2020). Growing in the womb: The effect of seismic activity on fetal growth. *Economics & Human Biology*, *36*, 100815. https://doi.org/10.1016/J.EHB.2019.100815

Arthi, V. (2018). “The dust was long in settling”: Human capital and the lasting impact of the American Dust Bowl. *Journal of Economic History*, *78*(1), 196–230. https://doi.org/10.1017/S0022050718000074

Atherwood, S. (2022). Does a prolonged hardship reduce life span? Examining the longevity of young men who lived through the 1930s Great Plains drought. *Population and Environment 2022*, 1–23. https://doi.org/10.1007/S11111-022-00398-W

Bailey, M., Bleakley, H., Currie, J., Jensen, R., Foster, A., Margo, R., Aizer, A., Eli, S., Ferrie, J., & Lleras-Muney, A. (2016). The Long-Run Impact of Cash Transfers to Poor Families. *American Economic Review*, *106*(4), 935–971. https://doi.org/10.1257/AER.20140529

Bailey, M., Clay, K., Fishback, P., Haines, M., Kantor, S., Severnini, E., & Wentz, A. (2016). U.S. County-Level Natality and Mortality Data, 1915-2007. *Inter-University Consortium for Political and Social Research*. https://doi.org/https://doi.org/10.3886/E100229V4

Baird, S., Hicks, J. H., Kremer, M., & Miguel, E. (2016). Worms at Work: Long-run Impacts of a Child Health Investment. *The Quarterly Journal of Economics*, *131*(4), 1637–1680. https://doi.org/10.1093/QJE/QJW022

Banerjee, A., Duflo, E., Postel-Vinay, G., & Watts, T. (2010). Long-run health impacts of income shocks: Wine and phylloxera in nineteenth-century France. *The Review of Economics and Statistics*, *92*(4), 714–728.

Banholzer, S., Kossin, J., & Donner, S. (2014). The impact of climate change on natural disasters. *Reducing Disaster: Early Warning Systems for Climate Change*, *9789401785983*, 21–49. https://doi.org/10.1007/978-94-017-8598-3_2/FIGURES/9

Barker, D. (1990). The fetal and infant origins of adult disease. *BMJ: British Medical Journal*, *301*(6761), 1111.

Barker, D. J. P., Eriksson, J. G., Forsén, T., & Osmond, C. (2002). Fetal origins of adult disease:

strength of effects and biological basis. *International Journal of Epidemiology*, *31*(6), 1235–1239. https://doi.org/10.1093/IJE/31.6.1235

Barreca, A. I., Neidell, M., & Sanders, N. J. (2021). Long-run pollution exposure and mortality: Evidence from the Acid Rain Program. *Journal of Public Economics*, *200*, 104440. https://doi.org/10.1016/J.JPUBECO.2021.104440

Behrman, J. R., & Rosenzweig, M. R. (2004). Returns to birthweight. In *Review of Economics and Statistics* (Vol. 86, Issue 2, pp. 586–601). https://doi.org/10.1162/003465304323031139

Berthelon, M., Kruger, D., & Sanchez, R. (2021). Maternal stress during pregnancy and early childhood development. *Economics & Human Biology*, *43*, 101047. https://doi.org/10.1016/J.EHB.2021.101047

Bharadwaj, P., Gibson, M., Zivin, J. G., & Neilson, C. (2017). Gray matters: Fetal pollution exposure and human capital formation. *Journal of the Association of Environmental and Resource Economists*, *4*(2), 505–542. https://doi.org/10.1086/691591/ASSET/IMAGES/LARGE/FG5.JPEG

Bharadwaj, P., & Lakdawala, L. K. (2013). Discrimination begins in the womb: Evidence of sex-selective prenatal investments. *Journal of Human Resources*, *48*(1), 71–113. https://doi.org/10.3368/jhr.48.1.71

Biggs, B., King, L., Basu, S., & Stuckler, D. (2010). Is wealthier always healthier? The impact of national income level, inequality, and poverty on public health in Latin America. *Social Science & Medicine*, *71*(2), 266–273. https://doi.org/10.1016/J.SOCSCIMED.2010.04.002

Billings, S. B., & Schnepel, K. T. (2018). Life after lead: Effects of early interventions for children exposed to lead. *American Economic Journal: Applied Economics*, *10*(3), 315–344. https://doi.org/10.1257/app.20160056

Black, S. E., Devereux, P. J., & Salvanes, K. G. (2007). From the Cradle to the Labor Market? The Effect of Birth Weight on Adult Outcomes. *The Quarterly Journal of Economics*, *122*(1), 409–439. https://doi.org/10.1162/qjec.122.1.409

Breen, C. F., & Osborne, M. (2022). *An Assessment of CenSoc Match Quality*. https://doi.org/10.31235/OSF.IO/BJ5MD

Cappelli, F., Costantini, V., & Consoli, D. (2021). The trap of climate change-induced “natural” disasters and inequality. *Global Environmental Change*, *70*, 102329. https://doi.org/10.1016/J.GLOENVCHA.2021.102329

Caruso, G., & Miller, S. (2015). Long run effects and intergenerational transmission of natural disasters: A case study on the 1970 Ancash Earthquake. *Journal of Development Economics*, *117*, 134–150. https://doi.org/10.1016/j.jdeveco.2015.07.012

Chay, K. Y., & Greenstone, M. (2003). The Impact of Air Pollution on Infant Mortality: Evidence from Geographic Variation in Pollution Shocks Induced by a Recession. *The Quarterly Journal of Economics*, *118*(3), 1121–1167. https://doi.org/10.1162/00335530360698513

Chen, X., Tan, C. M., Zhang, X., & Zhang, X. (2020). The effects of prenatal exposure to temperature extremes on birth outcomes: the case of China. *Journal of Population Economics*, *33*(4), 1263–1302. https://doi.org/10.1007/S00148-020-00768-4/TABLES/9

Chetty, R., Stepner, M., Abraham, S., Lin, S., Scuderi, B., Turner, N., Bergeron, A., & Cutler, D. (2016). The Association Between Income and Life Expectancy in the United States, 2001-2014. *JAMA*, *315*(16), 1750–1766. https://doi.org/10.1001/JAMA.2016.4226

Clark, A. E., D'Ambrosio, C., & Rohde, N. (2021). Prenatal economic shocks and birth outcomes in UK cohort data. *Economics & Human Biology*, *41*, 100964. https://doi.org/10.1016/J.EHB.2020.100964

Cristia, J. P. (2009). Rising mortality and life expectancy differentials by lifetime earnings in the United States. *Journal of Health Economics*, *28*(5), 984–995. https://doi.org/10.1016/j.jhealeco.2009.06.003

Currie, J., Davis, L., Greenstone, M., & Reed, W. (2015). Environmental Health Risks and Housing Values: Evidence from 1,600 Toxic Plant Openings and Closings. *American Economic Review*, *105*(2), 678–709. https://doi.org/10.1257/AER.20121656

Currie, J., & Neidell, M. (2005). Air Pollution and Infant Health: What Can We Learn from California's Recent Experience? *The Quarterly Journal of Economics*, *120*(3), 1003–1030. https://doi.org/10.1093/QJE/120.3.1003

Currie, J., Neidell, M., & Schmieder, J. F. (2009). Air pollution and infant health: Lessons from New Jersey. *Journal of Health Economics*, *28*(3), 688–703. https://doi.org/10.1016/J.JHEALECO.2009.02.001

Currie, J., & Rossin-Slater, M. (2013). Weathering the storm: Hurricanes and birth outcomes. *Journal of Health Economics*, *32*(3), 487–503. https://doi.org/10.1016/J.JHEALECO.2013.01.004

Currie, J., & Schmieder, J. F. (2009). Fetal Exposures to Toxic Releases and Infant Health. *American Economic Review*, *99*(2), 177–183. https://doi.org/10.1257/AER.99.2.177

Currie, J., & Schwandt, H. (2016). The 9/11 Dust Cloud and Pregnancy Outcomes: A Reconsideration. *Journal of Human Resources*, *51*(4), 805–831. https://doi.org/10.3368/JHR.51.4.0714-6533R

Currie, J., & Vogl, T. (2013). Early-Life Health and Adult Circumstance in Developing Countries. *Annual Review of Economics*, *5*, 1–36. https://doi.org/10.1146/ANNUREV-ECONOMICS-081412-103704

Currie, J., & Walker, R. (2011). Traffic Congestion and Infant Health: Evidence from E-ZPass. *American Economic Journal: Applied Economics*, *3*(1), 65–90. https://doi.org/10.1257/APP.3.1.65

Currie, J., Zivin, J. G., Mullins, J., & Neidell, M. (2014). What Do We Know About Short- and Long-Term Effects of Early-Life Exposure to Pollution? *Annual Reviews*, *6*(1), 217–247. https://doi.org/10.1146/ANNUREV-RESOURCE-100913-012610

Cutler, D. M., Miller, G., & Norton, D. M. (2007). Evidence on early-life income and late-life health from America's Dust Bowl era. *Proceedings of the National Academy of Sciences*, *104*(33), 13244–13249.

De Rubeis, V., Lee, J., Anwer, M. S., Yoshida-Montezuma, Y., Andreacchi, A. T., Stone, E., Iftikhar, S., Morgenstern, J. D., Rebinsky, R., Neil-Sztramko, S. E., Alvarez, E., Apatu, E., & Anderson, L. N. (2021). Impact of disasters, including pandemics, on cardiometabolic outcomes across the life-course: a systematic review. *BMJ Open*, *11*(5), e047152. https://doi.org/10.1136/BMJOPEN-2020-047152

Demakakos, P., Biddulph, J. P., Bobak, M., & Marmot, M. G. (2015). Wealth and mortality at older ages: A prospective cohort study. *Journal of Epidemiology and Community Health*, *70*(4), 346–353. https://doi.org/10.1136/jech-2015-206173

Drevenstedt, G. L., Crimmins, E. M., Vasunilashorn, S., & Finch, C. E. (2008). The rise and fall of excess male infant mortality. *Proceedings of the National Academy of Sciences of the United States of America*, *105*(13), 5016–5021. https://doi.org/10.1073/PNAS.0800221105

Duque, V., Schmitz, L. L., & others. (2020). *The Influence of Early-life Economic Shocks on Long-term Outcomes: Evidence from the US Great Depression*.

East, C. N. (2018). The Effect of Food Stamps on Children's Health: Evidence from Immigrants' Changing Eligibility. *Journal of Human Resources*, 0916-8197R2. https://doi.org/10.3368/jhr.55.3.0916-8197r2

East, C. N. (2020). The Effect of Food Stamps on Children's Health. *Journal of Human Resources*, *55*(2), 387–427. https://doi.org/10.3368/JHR.55.3.0916-8197R2

Feeny, S., Mishra, A., Trinh, T. A., Ye, L., & Zhu, A. (2021). Early-Life exposure to rainfall shocks and gender gaps in employment: Findings from Vietnam. *Journal of Economic Behavior & Organization*, *183*, 533–554. https://doi.org/10.1016/J.JEBO.2021.01.016

Fletcher, J., & Noghanibehambari, H. (2021). *The Effects of Education on Mortality: Evidence Using College Expansions*. https://doi.org/10.3386/W29423

Fletcher, J., Schwarz, H., Engelman, M., Johnson, N., Hakes, J., & Palloni, A. (2022). Understanding Geographic Disparities in Mortality. *Demography*.

Fuller, S. C. (2014). The Effect of Prenatal Natural Disaster Exposure on School Outcomes. *Demography*, *51*(4), 1501–1525. https://doi.org/10.1007/s13524-014-0310-0

Glynn, L. M., Wadhwa, P. D., Dunkel-Schetter, C., Chicz-DeMet, A., & Sandman, C. A. (2001). When stress happens matters: Effects of earthquake timing on stress responsivity in pregnancy. *American Journal of Obstetrics and Gynecology*, *184*(4), 637–642. https://doi.org/10.1067/MOB.2001.111066

Goldstein, J. R., Alexander, M., Breen, C., Miranda González, A., Menares, F., Osborne, M., Snyder, M., & Yildirim, U. (2021). Censoc Project. In *CenSoc Mortality File: Version 2.0. Berkeley: University of California*. https://censoc.berkeley.edu/data/

Gong, G., Phillips, S. G., Hudson, C., Curti, D., & Philips, B. U. (2019). Higher us rural mortality rates linked to socioeconomic status, physician shortages, and lack of health insurance. *Health Affairs*, *38*(12), 2003–2010. https://doi.org/10.1377/HLTHAFF.2019.00722/ASSET/IMAGES/LARGE/FIGUREEX2.JP

EG

Goodman-Bacon, A. (2021). The Long-Run Effects of Childhood Insurance Coverage: Medicaid Implementation, Adult Health, and Labor Market Outcomes. *American Economic Review*, *111*(8), 2550–2593. https://doi.org/10.1257/AER.20171671

Haeck, C., & Lefebvre, P. (2016). A simple recipe: The effect of a prenatal nutrition program on child health at birth. *Labour Economics*, *41*, 77–89. https://doi.org/10.1016/J.LABECO.2016.05.003

Hajat, A., Kaufman, J. S., Rose, K. M., Siddiqi, A., & Thomas, J. C. (2011). Long-term effects of wealth on mortality and self-rated health status. *American Journal of Epidemiology*, *173*(2), 192–200. https://doi.org/10.1093/aje/kwq348

Hansen, Z. K., & Libecap, G. D. (2015). Small Farms, Externalities, and the Dust Bowl of the 1930s. *Journal of Political Economy*, *112*(3), 665–694. https://doi.org/10.1086/383102

Hasunuma, H., Ichinose, T., Ueda, K., Odajima, H., Kanatani, K., Shimizu, A., Takami, A., Takeuchi, A., Nishiwaki, Y., Watanabe, M., & Hashizume, M. (2019). Health Effects of Asian Dust Events: A Literature Review Update of Epidemiological Evidence. *Japanese Journal of Hygiene*, *74*(0). https://doi.org/10.1265/JJH.19010

Hernández-Julián, R., Mansour, H., & Peters, C. (2014). The Effects of Intrauterine Malnutrition on Birth and Fertility Outcomes: Evidence From the 1974 Bangladesh Famine. *Demography*, *51*(5), 1775–1796. https://doi.org/10.1007/S13524-014-0326-5

Hetherington, E., Adhikari, K., Tomfohr-Madsen, L., Patten, S., & Metcalfe, A. (2021). Birth outcomes, pregnancy complications, and postpartum mental health after the 2013 Calgary flood: A difference in difference analysis. *PLoS ONE*, *16*(2 February), e0246670. https://doi.org/10.1371/journal.pone.0246670

Hornbeck, R. (2011). *Replication data for: The Enduring Impact of the American Dust Bowl: Short and Long-run Adjustments to Environmental Catastrophe*. Harvard Dataverse. https://doi.org/10.7910/DVN/CLRPAV

Hornbeck, R. (2012). The enduring impact of the American Dust Bowl: Short- and long-run adjustments to environmental catastrophe. In *American Economic Review* (Vol. 102, Issue 4, pp. 1477–1507). https://doi.org/10.1257/aer.102.4.1477

Hoynes, H., Schanzenbach, D. W., & Almond, D. (2016). Long-run impacts of childhood access to the safety net. *American Economic Review*, *106*(4), 903–934. https://doi.org/10.1257/aer.20130375

Hyland, M., & Russ, J. (2019). Water as destiny – The long-term impacts of drought in sub-Saharan Africa. *World Development*, *115*, 30–45. https://doi.org/10.1016/j.worlddev.2018.11.002

James, W. H., & Grech, V. (2017). A review of the established and suspected causes of variations in human sex ratio at birth. *Early Human Development*, *109*, 50–56. https://doi.org/10.1016/J.EARLHUMDEV.2017.03.002

Jones, B. A. (2020). After the Dust Settles: The Infant Health Impacts of Dust Storms. *Journal of the Association of Environmental and Resource Economists*, *7*(6), 1005–1032. https://doi.org/10.1086/710242

Karimia, S. M., & Basu, A. (2018). The effect of prenatal exposure to Ramadan on children's height. *Economics and Human Biology, 30*, 69–83. https://doi.org/10.1016/j.ehb.2018.05.001

Kesternich, I., Siflinger, B., Smith, J. P., & Winter, J. K. (2015). Individual Behaviour as a Pathway Between Early-Life Shocks and Adult Health: Evidence from Hunger Episodes in Post-War Germany. *The Economic Journal*, *125*(588), F372–F393. https://doi.org/10.1111/ECOJ.12281

Kim, B., Carruthers, C. K., & Harris, M. C. (2017). Maternal stress and birth outcomes: Evidence from the 1994 Northridge earthquake. *Journal of Economic Behavior and Organization*, *140*, 354–373. https://doi.org/10.1016/j.jebo.2017.05.014

Le, K., & Nguyen, M. (2021). In-utero Exposure to Rainfall Variability and Early Childhood Health. *World Development*, *144*, 105485. https://doi.org/10.1016/J.WORLDDEV.2021.105485

Le, K., & Nguyen, M. (2022). Desert locust swarms and child health. *Economics & Human Biology*, *44*, 101094. https://doi.org/10.1016/J.EHB.2021.101094

Lindeboom, M., Portrait, F., & van den Berg, G. J. (2010). Long-run effects on longevity of a nutritional shock early in life: The Dutch Potato famine of 1846–1847. *Journal of Health Economics*, *29*(5), 617–629. https://doi.org/10.1016/J.JHEALECO.2010.06.001

Maccini, S., & Yang, D. (2009). Under the weather: Health, schooling, and economic consequences of early-life rainfall. *American Economic Review*, *99*(3), 1006–1026. https://doi.org/10.1257/aer.99.3.1006

Majid, M. F. (2015). The persistent effects of in utero nutrition shocks over the life cycle: Evidence from Ramadan fasting. *Journal of Development Economics*, *117*, 48–57. https://doi.org/10.1016/J.JDEVECO.2015.06.006

Maruyama, S., & Heinesen, E. (2020). Another look at returns to birthweight. *Journal of Health Economics*, *70*, 102269. https://doi.org/10.1016/j.jhealeco.2019.102269

McLeman, R. A., Dupre, J., Berrang Ford, L., Ford, J., Gajewski, K., Marchildon, G., Ford, L. B., Ford, J., Gajewski, K., & Marchildon, G. (2014). What we learned from the Dust Bowl: Lessons in science, policy, and adaptation. In *Population and Environment* (Vol. 35, Issue 4, pp. 417–440). Kluwer Academic Publishers. https://doi.org/10.1007/s11111-013-0190-z

Miller, S., & Wherry, L. R. (2019). The Long-Term Effects of Early Life Medicaid Coverage. *Journal of Human Resources, 54*(3), 785–824. https://doi.org/10.3368/jhr.54.3.0816.8173r1

Molina, O., & Saldarriaga, V. (2017). The perils of climate change: In utero exposure to temperature variability and birth outcomes in the Andean region. *Economics & Human Biology*, *24*, 111–124. https://doi.org/10.1016/J.EHB.2016.11.009

Moreira, I., Linares, C., Follos, F., Sánchez-Martínez, G., Vellón, J. M., & Díaz, J. (2020). Short-term effects of Saharan dust intrusions and biomass combustion on birth outcomes in Spain. *Science of The Total Environment*, *701*, 134755. https://doi.org/10.1016/J.SCITOTENV.2019.134755

Muchomba, F. M., & Chatterji, S. (2020). Disability among children of immigrants from India and China: Is there excess disability among girls? *Population Studies*, *74*(2), 263–281. https://doi.org/10.1080/00324728.2020.1762911

Murnane, R. J. (2013). US High School Graduation Rates: Patterns and Explanations. *Journal of Economic Literature, 51*(2), 370–422. https://doi.org/10.1257/JEL.51.2.370

Nandi, A., Mazumdar, S., & Behrman, J. R. (2018). The effect of natural disaster on fertility, birth spacing, and child sex ratio: evidence from a major earthquake in India. *Journal of Population Economics, 31*(1), 267–293. https://doi.org/10.1007/s00148-017-0659-7

NCHS. (2020). Multiple Cause-of-Death Data Files. In *National Center for Health Statistics*.

Neelsen, S., & Stratmann, T. (2011). Effects of prenatal and early life malnutrition: Evidence from the Greek famine. *Journal of Health Economics*, *30*(3), 479–488.

Noghanibehambari, H. (2022). In utero exposure to natural disasters and later-life mortality: Evidence from earthquakes in the early twentieth century. *Social Science & Medicine*, *307*, 115189. https://doi.org/10.1016/J.SOCSCIMED.2022.115189

Noghanibehambari, H., Fletcher, J., Schmitz, L., Duque, V., & Gawai, V. (2022). *Early-Life Economic Conditions and Old-Age Mortality*.

Painter, R. C., Roseboom, T. J., Bossuyt, P. M. M., Osmond, C., Barker, D. J. P., & Bleker, O. P. (2005). Adult Mortality at Age 57 After Prenatal Exposure to the Dutch Famine. *European Journal of Epidemiology 2005 20:8*, *20*(8), 673–676. https://doi.org/10.1007/S10654-005-7921-0

Rosa, M. J., Nentin, F., Bosquet Enlow, M., Hacker, M. R., Pollas, N., Coull, B., & Wright, R. J. (2019). Sex-specific associations between prenatal negative life events and birth outcomes. *Stress*, *22*(6), 647–653. https://doi.org/10.1080/10253890.2019.1608944/SUPPL_FILE/ISTS_A_1608944_SM4672.DOCX

Rosales-Rueda, M. (2018). The impact of early life shocks on human capital formation: evidence from El Niño floods in Ecuador. *Journal of Health Economics*, *62*, 13–44. https://doi.org/10.1016/J.JHEALECO.2018.07.003

Roseboom, T., de Rooij, S., & Painter, R. (2006). The Dutch famine and its long-term consequences for adult health. *Early Human Development*, *82*(8), 485–491. https://doi.org/10.1016/J.EARLHUMDEV.2006.07.001

Ruggles, S., Flood, S., Goeken, R., Grover, J., & Meyer, E. (2020). IPUMS USA: Version 10.0 [dataset]. *Minneapolis, MN: IPUMS*. https://doi.org/10.18128/D010.V10.0

Sanders, N. J. (2012). What doesn’t kill you makes you weaker: Prenatal pollution exposure and educational outcomes. *Journal of Human Resources*, *47*(3), 826–850. https://doi.org/10.3368/jhr.47.3.826

Scholte, R. S., van den Berg, G. J., & Lindeboom, M. (2015). Long-run effects of gestation during the Dutch Hunger Winter famine on labor market and hospitalization outcomes. *Journal of Health Economics*, *39*, 17–30. https://doi.org/10.1016/J.JHEALECO.2014.10.002

Schubert, S. D., Suarez, M. J., Pegion, P. J., Koster, R. D., & Bacmeister, J. T. (2004). On the Cause of the 1930s Dust Bowl. *Science*, *303*(5665), 1855–1859. https://doi.org/10.1126/SCIENCE.1095048/SUPPL_FILE/SCHUBERT_SOM.PDF

Shah, M., & Steinberg, B. M. (2017). Drought of opportunities: Contemporaneous and long-term impacts of rainfall shocks on human capital. *Journal of Political Economy*, *125*(2), 527–561. https://doi.org/10.1086/690828/SUPPL_FILE/2013254DATA.ZIP

Simeonova, E., Currie, J., Nilsson, P., & Walker, R. (2021). Congestion Pricing, Air Pollution, and Children's Health. *Journal of Human Resources*, *56*(4), 971–996. https://doi.org/10.3368/JHR.56.4.0218-9363R2

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175–199. https://doi.org/10.1016/J.JECONOM.2020.09.006

Torche, F. (2011). The effect of maternal stress on birth outcomes: exploiting a natural experiment. *Demography*, *48*(4), 1473–1491. https://doi.org/10.1007/s13524-011-0054-z

Torche, F. (2018). Prenatal Exposure to an Acute Stressor and Children's Cognitive Outcomes. *Demography*, *55*(5), 1611–1639. https://doi.org/10.1007/S13524-018-0700-9

Van Abeelen, A. F. M., Veenendaal, M. V. E., Painter, R. C., De Rooij, S. R., Dijkgraaf, M. G. W., Bossuyt, P. M. M., Elias, S. G., Grobbee, D. E., Uiterwaal, C. S. P. M., & Roseboom, T. J. (2012). Survival effects of prenatal famine exposure. *The American Journal of Clinical Nutrition*, *95*(1), 179–183. https://doi.org/10.3945/AJCN.111.022038

van den Berg, G. J., Doblhammer-Reiter, G., Christensen, K., den Berg, G. J., Doblhammer-Reiter, G., Christensen, K., van den Berg, G. J., Doblhammer-Reiter, G., Christensen, K., den Berg, G. J., Doblhammer-Reiter, G., & Christensen, K. (2011). Being born under adverse economic conditions leads to a higher cardiovascular mortality rate later in life: Evidence based on individuals born at different stages of the business cycle. *Demography*, *48*(2), 507–530. https://doi.org/10.1007/s13524-011-0021-8

Wang, J., Gao, Z. Y., Yan, J., Ying, X. L., Tong, S. L., & Yan, C. H. (2017). Sex differences in the effects of prenatal lead exposure on birth outcomes. *Environmental Pollution*, *225*, 193–200. https://doi.org/10.1016/J.ENVPOL.2017.03.031

Weinberg, J., Sliwowska, J. H., Lan, N., & Hellemans, K. G. C. (2008). Prenatal Alcohol Exposure: Foetal Programming, the Hypothalamic-Pituitary-Adrenal Axis and Sex Differences in Outcome. *Journal of Neuroendocrinology*, *20*(4), 470–488. https://doi.org/10.1111/J.1365-2826.2008.01669.X

Tables

Table 1 - Summary Statistics

Variable	Mean	SD	Min	Max
Numident-1940-Census Data:				
Death Age (Month)	854.47565	69.10555	572	1028
Birth Year	1927.1297	3.83597	1920	1940
Death Year	1998.3376	4.75345	1988	2005
Medium Erosion	.32078	.46678	0	1
High Erosion	.07662	.26598	0	1
Medium Erosion × Post-1930	.08789	.28313	0	1
High Erosion × Post-1930	.02159	.14533	0	1
Female	.42877	.4949	0	1
White	.92604	.2617	0	1
Black	.07054	.25606	0	1
Other Races	.00341	.0583	0	1
Hispanic	.01385	.11685	0	1
Father Socioeconomic Score 1st Quartile	.20628	.40463	0	1
Father Socioeconomic Score 2nd Quartile	.23355	.42309	0	1
Father Socioeconomic Score 3rd Quartile	.19965	.39973	0	1
Father Socioeconomic Score 4th Quartile	.23699	.42524	0	1
Father Socioeconomic Score Missing	.04353	.20405	0	1
Mother Education < High School	.58219	.4932	0	1
Mother Education = High School	.2696	.44375	0	1
Mother Education > High School	.06158	.2404	0	1
Mother Education Missing	.08663	.28129	0	1
County Covariates:				
Share of Homeowners	.51516	.13121	.01811	.90763
Share of Children < 5-years-old	.40899	.12508	.13409	1.09902
Share of Literate	.8862	.15143	0	1
Share of Married	.60847	.03182	.26571	.77807
Average Occupational Score	23.42786	4.08945	11.78472	32.76429
Observations		1819066		
1960-Census Data:				
Years of Schooling	7.9491	2.89429	0	15
Years of Schooling>4	.92799	.2585	0	1
Years of Schooling>5	.88956	.31343	0	1
Years of Schooling>6	.79513	.40361	0	1
Years of Schooling>7	.72758	.4452	0	1
Years of Schooling>8	.64402	.47881	0	1
Years of Schooling>9	.57231	.49474	0	1
Years of Schooling>10	.18027	.38441	0	1
Years of Schooling>11	.13116	.33758	0	1
Years of Schooling>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	.3028	0	1
Observations		645778		
Birth-Infant-Death Data:				
Infant Mortality Rate per 100,000 Births	61.93882	28.64321	0	1000
Birth Rate per 1000 Women	39.87917	11.38456	0	149.79358
Share of Births to Whites	.67368	.23898	0	1
Share of Births to Blacks	.32546	.23886	0	1
Observations		60330		

Table 2 - Endogenous Births and Infant Deaths

	Outcomes:			
	Infant Mortality Rate	Birth Rate	Share of Births to Whites	Share of Births to Blacks
	(1)	(2)	(3)	(4)
Medium Erosion × Post-1930	.29965	-.10856	-.00331	.00223
	(.74654)	(.45641)	(.00591)	(.00334)
High Erosion × Post-1930	.96117	-1.05308	-.00463	-.00293
	(.91843)	(.9651)	(.00624)	(.0045)
Observations	60249	60283	19188	19177
R-squared	.78545	.79456	.96055	.9766
Mean DV	63.052	36.740	0.732	0.266
%Change Medium	0.475	-0.295	-0.453	0.838
%Change High	1.524	-2.866	-0.632	-1.100

Standard errors, clustered on county, are in parentheses. Regressions include county and state-by-birth-year fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

Table 3 - Endogenous Numident-Census Merging

	Outcome: Successful Merging of Numident Death Records with The Target Population of 1940			
	Full Sample	Whites	Nonwhites	Maternal Education < HS
	(1)	(2)	(3)	(4)
Medium Erosion × Post-1930	.00087	.00094	.00124	.00076
	(.00061)	(.00065)	(.00111)	(.00069)
High Erosion × Post-1930	.00065	.00094	.00096	.00069
	(.00087)	(.00094)	(.00144)	(.00093)
Observations	16577045	14994802	1582095	9515538
R-squared	.02169	.02105	.02798	.0214
Mean DV	0.112	0.114	0.086	0.113
%Change Medium	0.780	0.822	1.438	0.673
%Change High	0.576	0.821	1.115	0.611

Standard errors, clustered on county, are in parentheses. Regressions include county and state-by-birth-year fixed effects. Regressions are weighted using county farmland in 1930.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4 - Main Results

	Outcome: Age at Death (Months)				
	(1)	(2)	(3)	(4)	(5)
Medium Erosion × Post-1930	-.12984	-.22404	-.23245	-.22794	-.19115
	(.24992)	(.26841)	(.2686)	(.26793)	(.26558)
High Erosion × Post-1930	-.89638**	-.96564**	-1.01418**	-.99868**	-.93643**
	(.40811)	(.44179)	(.44152)	(.44161)	(.44418)
Observations	1818426	1818422	1818422	1818422	1818422
R-squared	.32596	.32643	.32928	.32959	.32959
Mean DV	854.811	854.811	854.811	854.811	854.811
%Change Medium	-0.015	-0.026	-0.027	-0.027	-0.022
%Change High	-0.105	-0.113	-0.119	-0.117	-0.110
Birth Year FE	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓
Birth-Year-by-Birth-State FE		✓	✓	✓	✓
Individual Covariates			✓	✓	✓
Family Controls				✓	✓
County Covariates					✓

Standard errors, clustered on county, are in parentheses. Regressions are weighted using county farmland in 1930.
*** p<0.01, ** p<0.05, * p<0.1

Table 5 - Robustness Checks

	Column 5 Table 4	Adding More Family Controls	Adding County by Individual Covariates FE	Adding County by Family Covariates FE
	(1)	(2)	(3)	(4)
Medium Erosion × Post-1930	-.19115	-.21981	-.20291	-.2066
	(.26558)	(.28192)	(.26597)	(.26763)
High Erosion × Post-1930	-.93643**	-.90088*	-.93431**	-.88892**
	(.44418)	(.47908)	(.44775)	(.45076)
Observations	1818422	1593832	1818161	1818417
R-squared	.32959	.33406	.33155	.33402
	Adding Birth-Month and Death-Month FE	Adding More County Controls	Outcome: Log Age at Death	Outcome: Age at Death >70 Years
	(5)	(6)	(7)	(8)
Medium Erosion × Post-1930	-.18937	-.13814	-.00022	-.0007
	(.26542)	(.26406)	(.00033)	(.0021)
High Erosion × Post-1930	-.90628**	-.8228*	-.00115**	-.00629*
	(.44301)	(.44458)	(.00055)	(.00359)
Observations	1818422	1818422	1818422	1818422
R-squared	.33251	.3296	.33034	.19596
	Sun-Abraham DD	Two-Way Cluster SE at County and State-by-Birth-Year	Restricting Cohorts to 1925-1935	Restricting Cohorts to 1925-1940
	(9)	(10)	(11)	(12)
Medium Erosion × Post-1930	-.14670	-.19115	-.24417	-.24168
	(.29612)	(.28608)	(.29813)	(.29156)
High Erosion × Post-1930	-98681***	-.93643*	-.9987*	-1.08318**
	(.46549)	(.47886)	(.51843)	(.48871)
Observations	1819061	1818422	1221136	1257218
R-squared	0.32960	.32959	.22302	.26286

Standard errors, clustered on county (except for column 10), are in parentheses. Regressions include county and state-by-birth-year fixed effects. All regressions include individual, family, and county covariates. Regressions are weighted using county farmland in 1930.
*** p<0.01, ** p<0.05, * p<0.1

Table 6 - Heterogeneity across Subsamples

	Outcome: Age at Death (Months), Subsamples:				
	Full Sample	Whites	Females	Father Farmers	Mother Education < College
	(1)	(2)	(3)	(4)	(4)
Medium Erosion × Post-1930	-.19115	-.27474	-.2735	.5522	-.14724
	(.26558)	(.27489)	(.39198)	(.67948)	(.28629)
High Erosion × Post-1930	-.93643**	-.81722*	-1.04682	-2.26091*	-1.02616**
	(.44418)	(.47207)	(.7008)	(1.25049)	(.45583)
Observations	1818422	1684118	779673	219587	1578090
R-squared	.32959	.32752	.33689	.35807	.33198
Mean DV	854.811	855.514	860.479	846.752	853.954
%Change Medium	-0.022	-0.032	-0.032	0.065	-0.017
%Change High	-0.110	-0.096	-0.122	-0.267	-0.120

Standard errors, clustered on county, are in parentheses. Regressions include county and state-by-birth-year fixed effects. All regressions include individual, family, and county covariates. Regressions are weighted using county farmland in 1930.
*** p<0.01, ** p<0.05, * p<0.1

Table 7 - Heterogeneity across Regions

	Outcome: Age at Death (Months), Subsamples:			
	Northeast	Midwest	West	South
	(1)	(2)	(3)	(4)
Medium Erosion ×	.03522	-.17928	-.4396	-3.00973**
Post-1930	(.39421)	(.60346)	(.52562)	(1.40469)
High Erosion ×	1.11763	-2.98537***	-1.44847*	-4.41077***
Post-1930	(.86613)	(.89175)	(.82081)	(1.57793)
Observations	868085	466265	404399	79534
R-squared	.39137	.26039	.26975	.25926
Mean DV	854.811	862.371	857.165	863.448
%Change Medium	-0.018	-0.021	-0.051	-0.349
%Change High	-0.097	-0.346	-0.169	-0.511

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

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

Table 8 - Heterogeneity by Migration from Birth to Death

	Outcome: Migrant	Outcome: Age at Death, Subsample: Migrants	Outcome: Age at Death, Subsample: Non-Migrants
	(1)	(2)	(3)
Medium Erosion × Post-1930	.00696***	.43636	-.48439
	(.00228)	(.39539)	(.32576)
High Erosion × Post-1930	.00826**	-.10827	-1.45934**
	(.00413)	(.70694)	(.63319)
Observations	1818283	846900	971371
R-squared	.09152	.32343	.35129
Mean DV	0.463	847.775	860.869
%Change Medium	1.503	0.051	-0.056
%Change High	1.784	-0.013	-0.170

Standard errors, clustered on county, are in parentheses. Regressions include county and state-by-birth-year fixed effects. All regressions include individual, family, and county covariates. Regressions are weighted using county farmland in 1930.
*** p<0.01, ** p<0.05, * p<0.1

Table 9 - Comparing the Results with DMF Records

	Outcome: Age at Death (Months)				
	Numident, Both Genders, 1988-2005	Numident, Females, 1988-2005	Numident, Males, 1988-2005	DMF, Males, 1988-2005	DMF, Males, 1975-2005
	(1)	(2)	(3)	(4)	(5)
Medium Erosion × Post-1930	-.18715	-.26246	-.15998	-.59886	-.18366
	(.26546)	(.39211)	(.34483)	(.45874)	(.67764)
High Erosion × Post-1930	-.92573**	-1.01567	-.88874	-1.29073*	-1.19612
	(.44359)	(.70148)	(.57673)	(.73813)	(1.13698)
Observations	1819191	780006	1039180	735022	967520
R-squared	.3296	.33689	.32131	.32349	.13385
Mean DV	854.810	860.477	850.593	848.958	803.512
%Change Medium	-0.022	-0.031	-0.019	-0.071	-0.023
%Change High	-0.108	-0.118	-0.104	-0.152	-0.149

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

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

Table 10 - Exploring Potential Mechanisms Using Census 1960

	Outcomes:										
	Years of Schooling > 4	Years of Schooling > 5	Years of Schooling > 6	Years of Schooling > 7	Years of Schooling > 8	Years of Schooling > 9	Years of Schooling > 10	Years of Schooling > 11	Years of Schooling > 12	Log Total Personal Income	Log Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Medium Erosion × Post-1930	.00367	.00741	.01461***	.01324***	.01521***	.01568**	-.00338	-.00138	-.00174	-.02827	-.09837*
	(.00439)	(.0051)	(.00526)	(.00456)	(.00545)	(.00641)	(.00571)	(.00469)	(.00342)	(.05534)	(.05232)
High Erosion × Post-1930	.02792***	.04444***	.03946***	.03228***	.02344***	.0251***	-.00181	-.0013	.00063	-.2756***	-.38177***
	(.00745)	(.00957)	(.00893)	(.00786)	(.00826)	(.00824)	(.00903)	(.00694)	(.00489)	(.07732)	(.09492)
Observations	645778	645778	645778	645778	645778	645778	645778	645778	645778	645058	645778
R-squared	.11966	.14075	.12932	.11942	.10943	.1034	.06643	.05974	.05012	.32987	.22665
Mean DV	0.928	0.890	0.795	0.728	0.644	0.572	0.180	0.131	0.089	5.371	4.830

Standard errors, clustered on county, are in parentheses. Regressions include county-PUMA and state-by-birth-year fixed effects. All regressions include individual covariates. Regressions are weighted using IPUMS-provided person weights.
*** p<0.01, ** p<0.05, * p<0.1

Figures

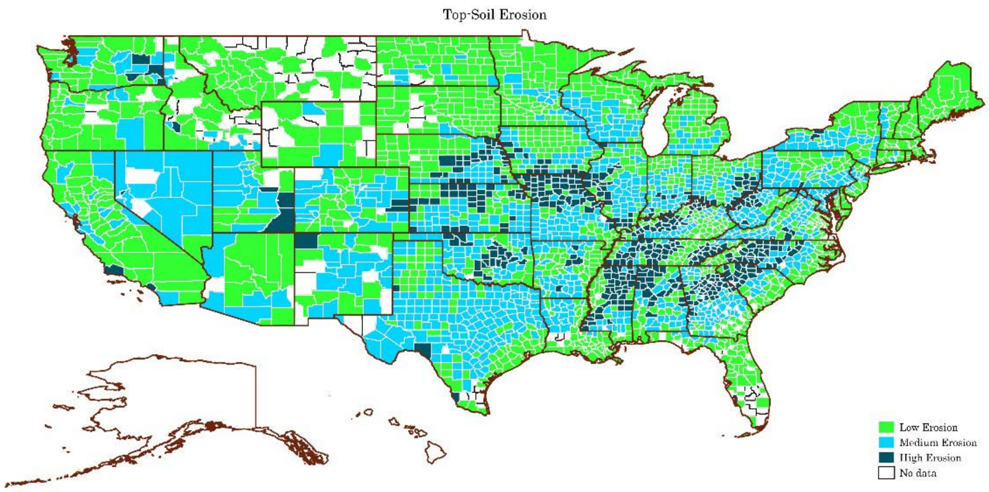

Figure 1 - Distribution of Top-soil Erosion

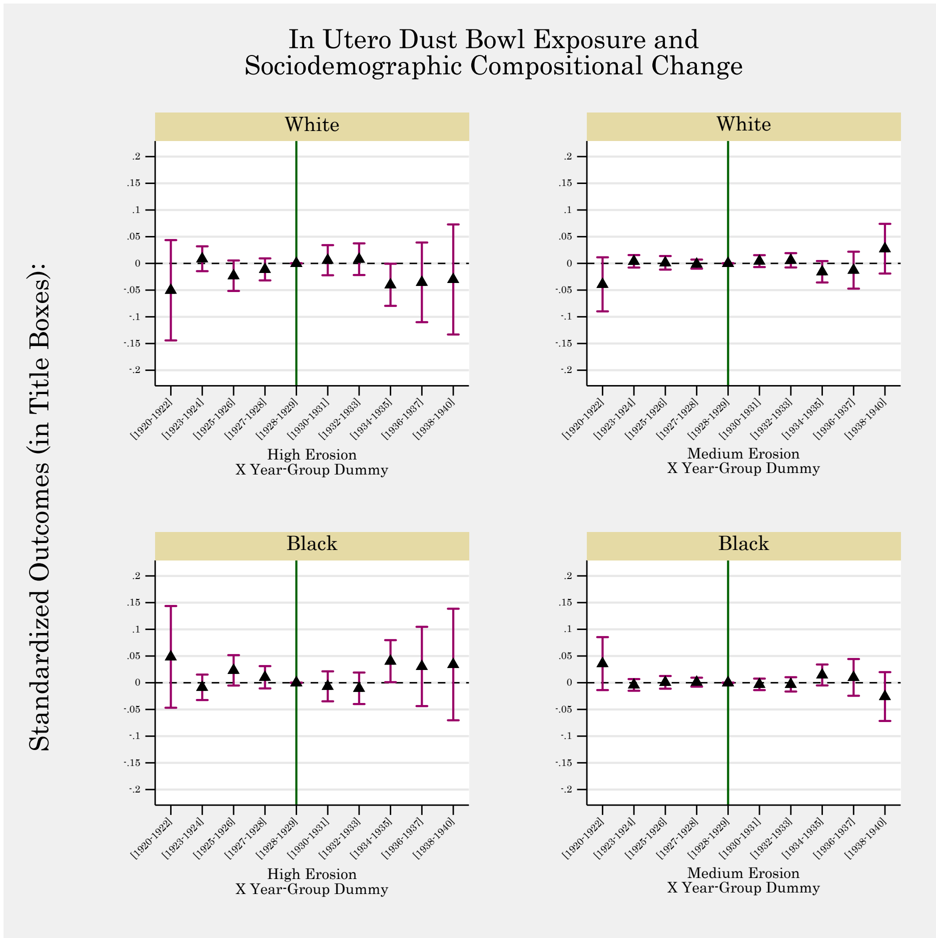

Figure 2 - Event Study Results to Explore Individual/Family Sociodemographic Change across Years and Topsoil Erosion Measures

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered on county. Regressions include state-year and county fixed effects. Regressions are weighted using county farmland in 1930.

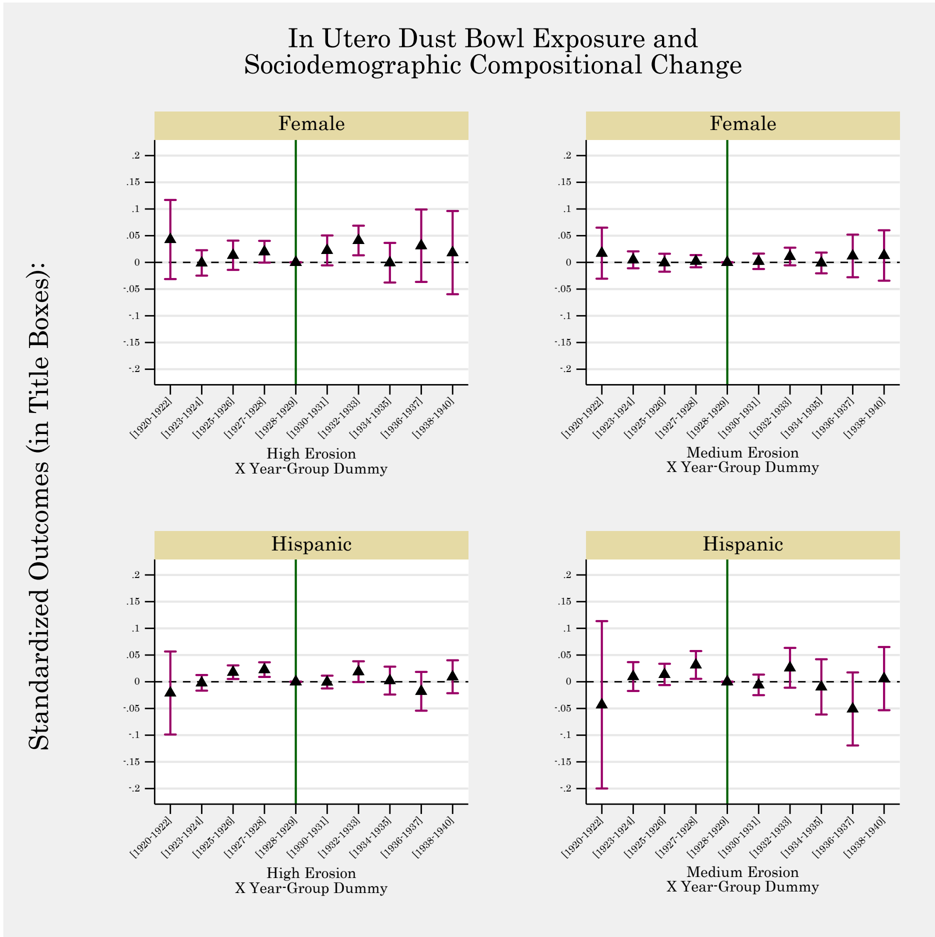

Figure 3 - Event Study Results to Explore Individual/Family Sociodemographic Change across Years and Topsoil Erosion Measures

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered on county. Regressions include state-year and county fixed effects. Regressions are weighted using county farmland in 1930.

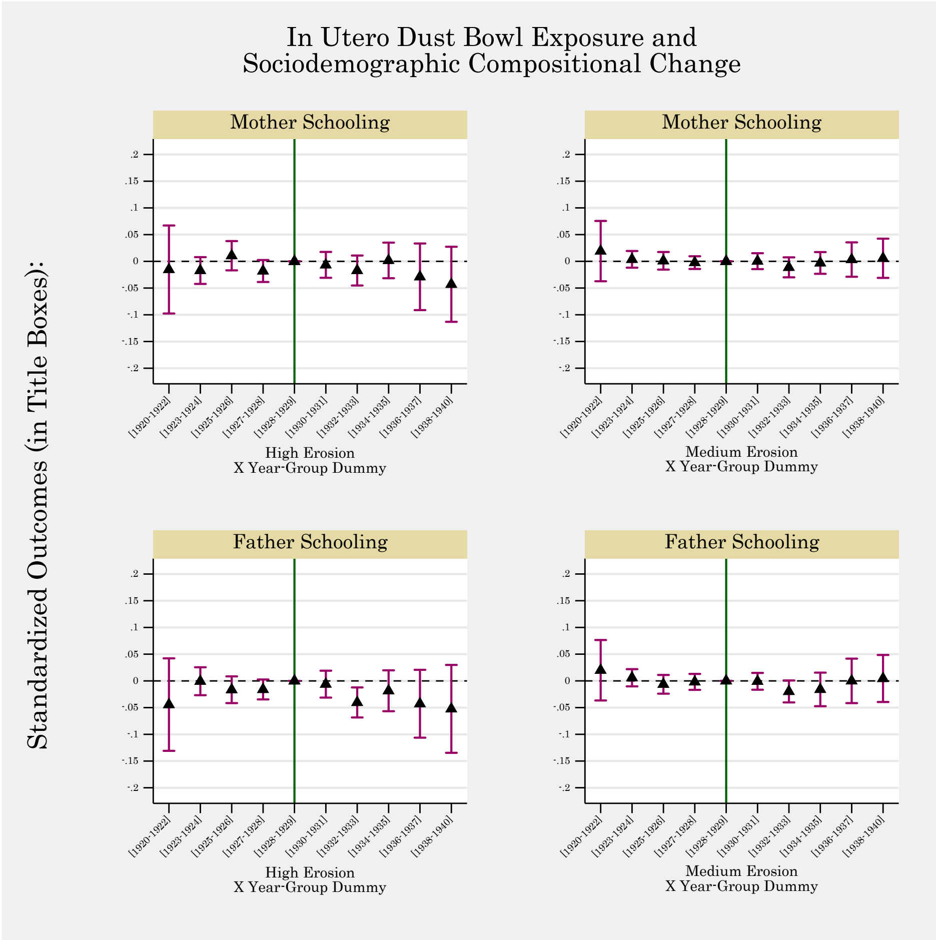

Figure 4 - Event Study Results to Explore Individual/Family Sociodemographic Change across Years and Topsoil Erosion Measures

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered on county. Regressions include state-year and county fixed effects. Regressions are weighted using county farmland in 1930.

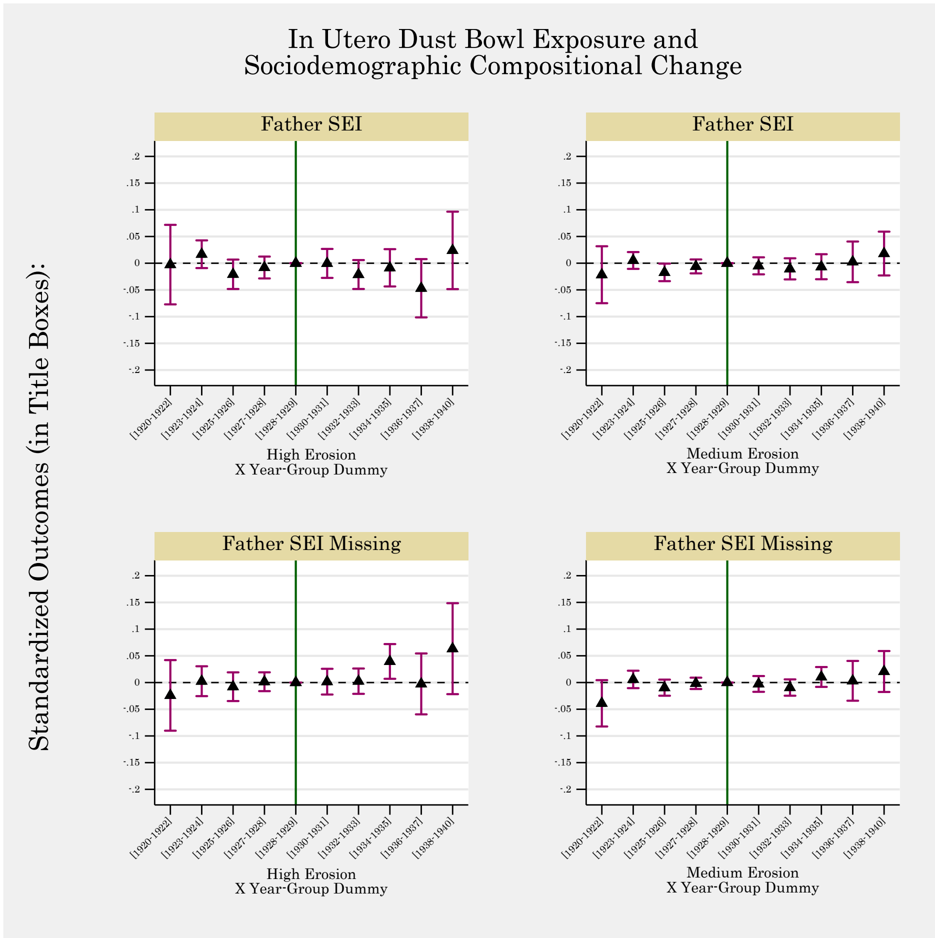

Figure 5 - Event Study Results to Explore Individual/Family Sociodemographic Change across Years and Topsoil Erosion Measures

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered on county. Regressions include state-year and county fixed effects. Regressions are weighted using county farmland in 1930.

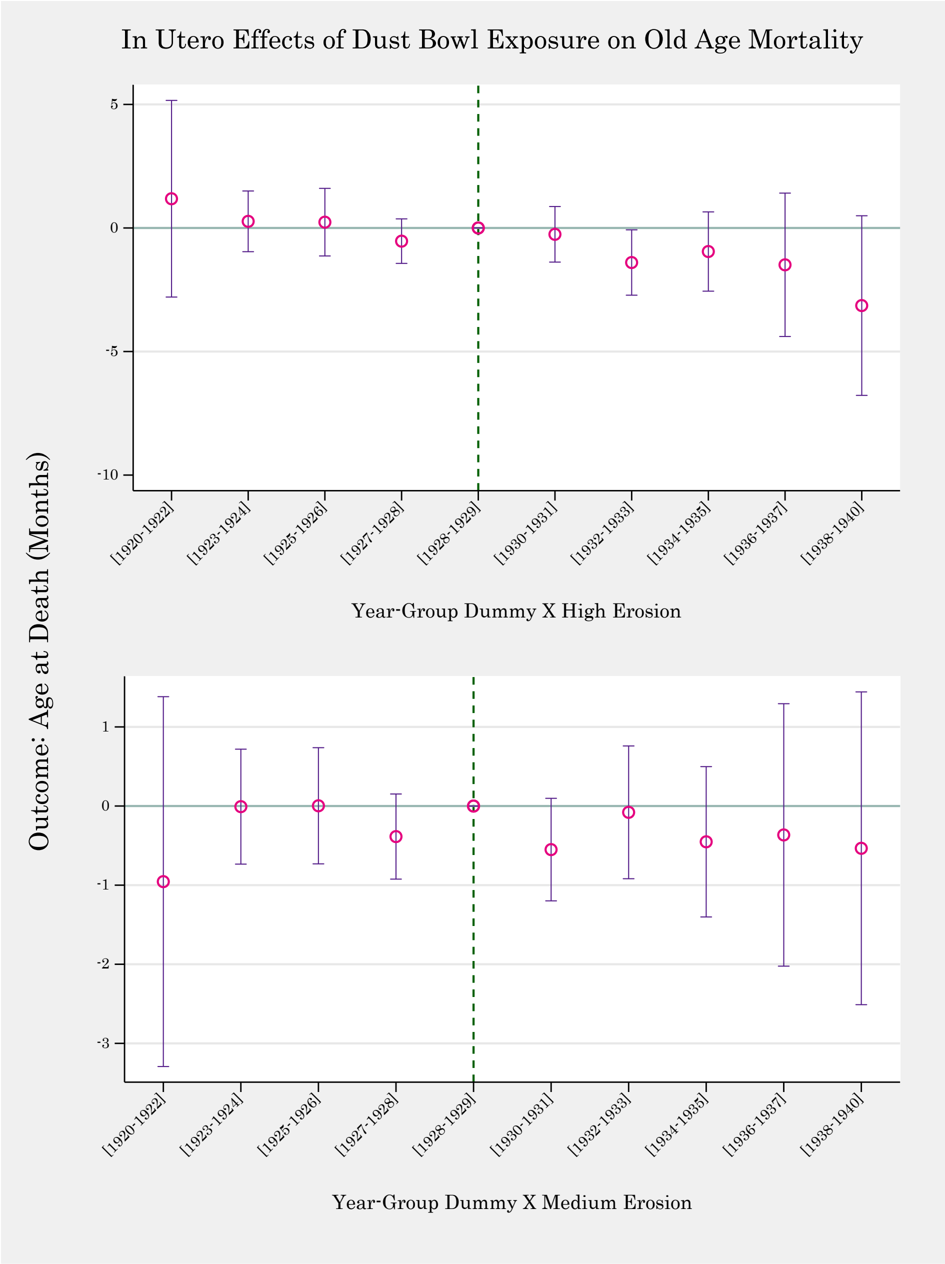

Figure 6 - Event Study Results

Notes. Point estimates and 95 percent standard errors are illustrated. Standard errors are clustered on county. Regressions include county and state-by-birth-year fixed effects. All regressions include individual, family, and county covariates. Regressions are weighted using county farmland in 1930.

Appendix A

In the main text of the paper, we show implemented a series of event studies to explore the balancing test of sociodemographic compositional change in the final sample for our treated versus control groups in pre-1930 years. In this appendix, we show the results for a difference-in-difference framework. In so doing, we regress dummies of individual and family characteristics on measures of erosion conditional on county and state-by-birth-year fixed effects. The results are reported in Appendix Table A-1. The estimated coefficients do not provide statistically significant associations between in-utero and early-life exposure to soil erosion measures and the likelihood of being white, black, female, paternal socioeconomic score, and maternal education. Moreover, the magnitude of the observed marginal effects is quite small in magnitude. This is more obvious when we compare the effects with the mean of the respective dependent variables reported in the last two rows of the table. Overall, these results are in line with the pattern of pre-trend coefficients in Figure 2 through Figure 5.

Appendix Table A-1 - Balancing Tests

	Outcomes:								
	White	Black	Female	Father's Socioeconomic Score	Father's Socioeconomic Score Missing	Mother's Education < High School	Mother's Education = High School	Mother's Education > High School	Mother's Education Missing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Medium Erosion × Post-1930	-.00055	.00062	.00069	.0239	.00023	.00384	-.00319	-.00051	-.00014
	(.00124)	(.00118)	(.00207)	(.12281)	(.00097)	(.00315)	(.00258)	(.00141)	(.0014)
High Erosion × Post-1930	.00154	-.00178	.0044	-.1542	.00239	.01064**	-.00424	-.00206	-.00435*
	(.00242)	(.00238)	(.0037)	(.19653)	(.0016)	(.00486)	(.0041)	(.00219)	(.00231)
Observations	1818422	1818422	1818422	1514672	1818422	1818422	1818422	1818422	1818422
R-squared	.24828	.26533	.00504	.08371	.01114	.06525	.05281	.0186	.05025
Mean DV	0.936	0.060	0.427	26.633	0.043	0.564	0.283	0.068	0.086
%Change Medium	-0.059	1.034	0.162	0.090	0.538	0.680	-1.126	-0.754	-0.159
%Change High	0.164	-2.972	1.030	-0.579	5.553	1.887	-1.498	-3.025	-5.055

Standard errors, clustered on county, are in parentheses. Regressions include county and state-by-birth-year fixed effects. Regressions are weighted using county farmland in 1930.
*** p<0.01, ** p<0.05, * p<0.1

Appendix B

In Appendix Table B-1, we show the robustness of the main results to alternative weighting schemes. In column 1, we replicate the benchmark results from column 5 of Table 4. In column 2, we use the total number of children aged 0-4 in the county in 1930. This measure is a proxy for the total pre-1930s fertility rate. We observe quite comparable marginal effects to column 1. In column 3, we use the number of farmers in the county in 1930 as an alternative proxy for the county's reliance on agriculture. The coefficients rise (in magnitude) by 37 percent for high erosion and 142 percent for medium erosion. Finally, in column 4, we implement unweighted regressions. Although the marginal effects drop slightly, they are similar to our main results.

Appendix Table B-1 - Robustness of the Results to Unweighted Regressions and Alternative Weights

	Outcome: Age at Death (Months)			
	Weighted by County Farmland in 1930	Weighted by County Number of Children Aged 0-4 in 1930	Weighted by County Number of Farmers in 1930	Unweighted Regressions
	(1)	(2)	(3)	(4)
Medium Erosion × Post-1930	-.19115	-.17052	-.4785*	-.12541
	(.26558)	(.22653)	(.27449)	(.21874)
High Erosion × Post-1930	-.93643**	-.88305**	-1.27396***	-.88766**
	(.44418)	(.43836)	(.4742)	(.44245)
Observations	1818422	1819061	1818454	1819061
R-squared	.32959	.33181	.33636	.32998
Mean DV	854.811	854.017	852.380	854.476
%Change Medium	-0.022	-0.020	-0.056	-0.015
%Change High	-0.110	-0.103	-0.149	-0.104
Birth Year FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Birth-Year-by-Birth-State FE	✓	✓	✓	✓
Individual Covariates	✓	✓	✓	✓
Family Controls	✓	✓	✓	✓
County Covariates	✓	✓	✓	✓

Standard errors, clustered on county, are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Appendix C

One concern in interpreting the main results of Table 4 is the limited death window of the Numident data. In section 5.5 and Table 9, we provided evidence that the results are stable in magnitude when we consider alternative data sources that include earlier deaths. In this appendix, we examine the sensitivity of the results to the inclusion of deaths that occurred after the end of the Numident window, i.e., post-2005 deaths. In so doing, we use Vital Statistics death records extracted from the National Center for Health Statistics (NCHS, 2020). The NCHS data covers the universe of mortality records in the US. The limitation of the data is that it does not provide information on the county of birth. However, from 1979, the NCHS data reports the state of birth, which we can use to examine the effects of the Dust Bowl and soil erosion on longevity. To do so, we aggregate the top-soil erosion data to the state level using the county-level share of farmlands as weights in the aggregation. We then merge it with the NCHS data at the birth-state level. In addition, to examine to what extent the effects change in state versus county-level aggregation of exposure measures, we also use Numident data and merge it with state-aggregated erosion data at the birth-state level. In the main results of the paper, we have cohorts born between 1920-1940. In the analysis of this appendix, we focus on a narrower set of cohorts, specifically those born between 1923-1939 to better match the NCHS data. In the paper, we follow a procedure to infer the county-of-birth (see section 3). The fact that we restrict the sample to non-migrants and that individuals leave their original households after age 17 leaves fewer individuals from 1920-1922 cohorts in the final sample. Moreover, since the 1940 census was enumerated in April, there are fewer 1940-born individuals in our Numident sample. To make the NCHS sample comparable to the final sample of Numident, we remove those born in 1940 and 1920-1922.

We implement regressions that include birth-state fixed effects, birth-year fixed effects, region-by-birth-year fixed effects, and individual covariates. The results are reported in Appendix Table C-1. In column 1, we use all Numident death records. In column 2, we restrict the Numident data to those records that are included in the final sample of the paper. The results suggest negative impacts on longevity. Consistent with Table 4, we observe the largest effects stemming from high erosion states. However, they are statistically insignificant and much smaller in magnitude due to the measurement error we induce by aggregating the county-level erosion measures to the state level. The high erosion coefficients are roughly 17-20 percent of the effects reported in Table 4. However, assuming this attenuation is held constant as we add additional years of death records, we can explore the possible impacts of the right-censored death window in our Numident analysis. In column 3, we rely on the subsample of 1923-1939 cohorts and replicate the results of column 2. We observe quite similar coefficients.

In column 4, we use the NCHS sample for the death window of 1988-2005. We observe negative impacts and larger effects from high erosion states. The coefficient of high erosion is roughly 80 percent larger than the effects found in state-aggregated Numident data. In column 5, we use all death records over the years 1979-2020. The effect of high erosion is about 2.3 times the effect in column 3. The marginal effect of low erosion increases substantially, changes sign, and becomes statistically significant. One interpretation of this finding is that the right censoring of death data in the Numident analysis produces a failure to find effects of low-erosion that accumulate over the life cycle and effect older age mortality (ages 75+) rather than earlier life mortality. We also find evidence that the right censoring of the death data in Numident attenuates our main results for high erosion exposure.

We can use the comparison of coefficients of columns 3 and 4 to extrapolate the effects of Table 4 to out-of-sample death records. A back-of-an-envelop calculation suggests that high erosion is associated with roughly 3 months lower longevity.[7]

[7] We multiply the ratio of high erosion effects of column 4 versus column 3 (rise in NCHS for post-2005) of Appendix Table C-1 by the coefficient of column 5 of Table 4, as follows: $\frac{0.939}{0.289} \times 0.936$

Appendix Table C-1 - Comparing the Effects with the NCHS Death Records at the Birth-State Level

	Outcome: Age at Death (Months), Sample:				
	Numident, Death Years 1988-2005, Birth Years 1920-1940	Numident Records in the Final Sample, Death Years 1988-2005, Birth Years 1920-1940	Numident Records in the Final Sample, Death Years 1988-2005, Birth Years 1923-1939	NCHS Death Records, Death Years 1988-2005, Birth Years 1923-1939	NCHS Death Records, Death Years 1979-2020, Birth Years 1923-1939
	(1)	(2)	(3)	(4)	(5)
Medium Erosion × Post-1930	-.04963	-.02574	-.00632	.00622	-.67285**
	(.14752)	(.3083)	(.30013)	(.28446)	(.26051)
High Erosion × Post-1930	-.19796	-.15804	-.1607	-.28879	-.93902
	(.14409)	(.19972)	(.19993)	(.46735)	(.628)
Observations	4222514	1814309	1642589	9,327,508	20,662,410
R-squared	.45952	.32825	.28899	.44212	.29129
Mean DV	837.042	854.409	850.381	832.924	903.355
%Change Medium	-0.006	-0.003	-0.001	0.001	-0.074
%Change High	-0.024	-0.018	-0.019	-0.035	-0.104

Notes. Standard errors, clustered on birth state, are in parentheses. Regressions include state and year of birth fixed effects. Regressions also include individual race, gender, and ethnicity dummies.
*** p<0.01, ** p<0.05, * p<0.1