The Early Bird Catches the Worm: The Effect of Birth Order on Old-Age Mortality

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Previous studies explore the role of birth order in children's and adults' outcomes. This literature usually provides evidence of disadvantage of children with higher birth order. A narrow strand of this literature explores the association between birth order and old-age mortality. This study re-visits the birth-order-longevity relationship using US data. We employ Social Security Administration death records between the years 1988 and 2005 linked to the 1940 full-count census and implement family fixed effect strategy to compare within-sibling differences in the outcome. The findings suggest that later-born children live, on average, 1–3 fewer months of life. The observed associations are exclusively concentrated among whites. However, the results do not point to significant heterogeneity based on family socioeconomic status, maternal education, and gender. Additional analyses suggest that higher birth order is associated with negative early educational outcomes.

Introduction

What can explain levels and disparities in health during adulthood and old ages? Although several strands of empirical research offer insight into contemporaneous determinants, other studies point to the relevance of early life conditions and childhood circumstances (Almond, Currie, and Duque 2018; Almond and Currie, 2011a, 2011b; Almond and Mazumder, 2005; Banerjee et al., 2010; Currie, 2009; Currie and Vogl, 2013; Deming, 2009; Gagnon and Mazan, 2009; Hoynes, Schanzenbach, and Almond 2016; Myrskylä, 2010). Some of these studies explore how social programs and policy interventions during *in utero*, infancy, and childhood can change the trajectory of children's outcomes and lead to accrued benefits that can be detected in adulthood, improved educational attainment, labor market

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outcomes, and health outcomes (Goodman-Bacon 2018, 2021; Haeck and Lefebvre, 2016; Miller and Wherry, 2019; Wherry and Meyer, 2016). Other studies explore how family socioeconomic status influences children's outcomes and later-life health. These familial factors range from the material resources available to mothers during prenatal development to their residential location and the accompanying neighborhood effects (Currie and Stabile, 2003; Hayward and Gorman, 2004; Maccini and Yang, 2009; Murasko, 2009; Taylor, 2010). For instance, Løken, Mogstad, and Wiswall (2012) document strong and nonlinear effects of family income on childhood education and intelligence quotient (IQ), with the largest effects at the bottom quantiles of income. Halpern-Manners et al. (2020) employ a twin-fixed-effect strategy and show that education significantly improves old-age longevity. Therefore, childhood circumstances can influence later-life health outcomes through mediatory channels such as education and income.

This long chain of cause-and-effect is studied not only on levels of family resources but also on differential exposures of children to these resources. One aspect of disparate exposure to family material resources is through birth order. Birth order shapes the timing, level, and type of investments provided to children. Price (2008) shows that lower birth order children receive more parental time but lower levels of material resources than higher born siblings because parents appear to equalize inputs in a given time period (i.e., year) rather than at each child-age. This equalization in time inputs works together with the regularity of the age-earnings profile of parents to produce differentials in timing, levels, and types of investments. A second key differential family experience shaped by birth order is the presence and relative-age of siblings. Additional siblings both dilute the effects of household resources (parental time, household income) but also serve as their own resource, as potential teachers, role models, caregivers, and care receivers. Earlier-born children accrue more inputs from their families at early ages and have larger cumulative consumption of resources, including parental time (Ejrnæs and Pörtner, 2004; Horton, 2015). For instance, later-born children may benefit from lower material resources since per-child resource allocation would decrease as the sibship size grows. Conversely, families may accumulate more material resources over the years, benefiting later-borns exposed to higher cumulative resources. Later-born children may also receive more parental attention and consume more material resources at later ages when earlier-born children leave the household. These increased resources at later ages may be far less beneficial than the same resources provided at earlier ages (Pavan 2015; Cunha et al. 2010).[1]

The current study aims to extend our understanding of the impacts of birth order on later-life old-age mortality using US data. The contribution of this paper to the ongoing literature is twofold. First, previous studies using US data employ datasets with limited geographic coverage and racial composition and rely on cross-family correlations (Smith et al., 2009). In contrast, this study is the first to examine this question in a large sample that covers all US regions. Moreover, this study accounts for unobserved heterogeneity in family fertility decisions and other family level factors by employing cross-siblings comparisons. Second, the large sample size allows for a wide range of heterogeneity analysis by gender, demographic, and socioeconomic characteristics.

The rest of the paper is organized as follows. "**Literature background: Pathways between birth order and mortality**" section reviews the literature on birth order. This section also reviews the contextual background of the period under study. "**Data and methods**" section discusses the data sources, sample construction strategy, and the econometric method. "**Results**" section reviews the results. "**A note on the magnitude of the results**" section discusses the economic significance of the findings. "**Conclusion**" section concludes the paper.

Background

Literature background: Pathways between birth order and mortality

Studies in a number of fields examine the short-term and long-term effects of birth order on a wide array of outcomes. In Table 1, we show a brief list of these studies listed by scope of impact, outcome, finding, and heterogeneity in the results. In this section, we start by reviewing studies that evaluate the influence of birth order on short-run outcomes during infancy and childhood. We then move to studies that examine longer run outcomes. Finally, we go over the narrow literature on birth order and old-age mortality.

Later-born children have, on average, higher health endowments at birth (Aparicio et al., 2020; Brenøe and Molitor, 2017). Brenøe and Molitor (2017) use Danish administrative data and apply a family fixed effect strategy, and show that, during earlier pregnancies, women are more likely to smoke and encounter pregnancy complications. In later pregnancies, children have higher health at birth.[2] Côté, Blanchard, and Lalumière (2003) show that later-born children have higher birth weight and the health advantage differs by the gender of the previous child.[3] However, this disadvantage of earlier-born children vanishes after the first year of life.

In contrast to the early life health benefits to higher birth order, a host of studies show that higher birth order confers disadvantages by young adulthood, including education[4], IQ[5], and self-reported health (Barclay and Myrskylä, 2014; Behrman and Taubman, 1986; Conley and Glauber, 2006; Damian and Roberts, 2015; Healey and Ellis, 2007; Hotz and Pantano, 2015; Kessler, 1991).[6] For instance, Black, Devereux, and Salvanes (2005) use administrative data on the entire population of Norway and implement a family fixed effect strategy. Their results suggest that

TABLE 1 Brief review of studies on birth order

Time effects measured	Outcomes	Findings	Heterogeneity
Short-term	Birth weight	Mixed (Côté, Blanchard, and Lalumière 2003): Quebec, Canada	Gender
Short-term	Birth outcomes	Later-borns> (Brenøe and Molitor, 2017): Denmark	Family size
Short term	Parental time with children	First-borns> (Price, 2008): USA	Gender
Medium-term	Height	First-borns> (Jayachandran and Pande, 2017): India	Gender–religion
Medium-term	Cognitive measures	First-borns> (Pavan, 2015): USA	NA
Medium-term	Nutritional status	First-borns> (Horton, 2015): Philippines	Parental education and socioeconomic measures
Long-term	IQ	First-borns> (Black, Devereux, and Salvanes 2011): Norway	Family size
Long-term	Fitness outcomes	First-borns> (Barclay and Myrskylä, 2014): Sweden	NA
Long-term	Health behavior	Mixed (Black, Devereux, and Salvanes 2016): Norway	NA
Long-term	Education	Later-borns> (Ejrnæs and Pörtner, 2004): Philippines	Parental education and socioeconomic measures
Long-term	Education	First-borns> (Booth and Kee, 2009): UK	Family size and maternal education
Long-term	Education	First-borns> (Haan, 2010): USA	Family size
Long-term	Education and earnings	First-borns> (Black, Devereux, and Salvanes 2005): Norway	Family size
Long-term	Education and earnings	First-borns> (Kantarevic and Mechoulan, 2006): USA	Race
Long-term	Income	First-borns> (Bertoni and Brunello, 2016): 11 European countries	NA

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TABLE 1 (Continued)

Time effects measured	Outcomes	Findings	Heterogeneity
Long-term	Managerial occupational choice	First-borns> (Grinberg, 2015): USA	Maternal education
Long-term	Mortality risks	Later-borns> (Barclay and Kolk, 2015): Sweden	NA
Long-term	Mortality risks	Later-borns> (Modin, 2002): Sweden	Gender
Long-term	Suicide mortality risks	Later-borns> (Saarela, Cederström, and Rostila 2016): Finland	Ethnicity
Long-term	Mortality risks	Later-borns> (Rostila, Saarela, and Kawachi 2014): Sweden	Gender
Long-term	Mortality risks	No difference (Smith et al., 2009): USA	NA
Long-term	Mortality risks	No difference (O'Leary et al., 1996): USA	Gender
Long-term	Mortality risks	Later-borns> (Donrovich, Puschmann, and Matthijs 2014): Belgium	Gender

second-and-higher-born children have, on average, 0.3–0.9 fewer years of schooling compared to their first-born siblings. They also document significant differences in earnings and employment by birth order. Booth and Kee (2009) use the British Household Panel Survey and document that later-born children have lower educational attainments than their first-born sibling. Kantarevic and Mechoulan (2006) investigate the birth order effect in the Panel Study of Income Dynamics. They document a negative association between birth order and education. This relationship persists in their earnings differences and is larger among large black families. Haan (2010) shows that birth order is negatively associated with educational attainments. She offers differential parental investment as a potential mediatory channel. Parents transfer more often and higher amounts to their earlier-born children.[7]

In the longer run, several studies examine the impact of birth order on labor market outcomes and point to the disadvantage of later-borns for adulthood income, while other studies fail to find any associations (Kessler 1991). Bertoni and Brunello (2016) use data from 11 European countries and find that first-born children earn roughly 14 percent higher than their later-born siblings in their first job[8]. They argue that this is primarily due to their relatively better initial placement as a result of their higher education. They show that the relative disadvantage of later-borns disappears after 10 years as they catch up and move to better-paid jobs over the years. They conclude that the key factor is the lower risk aversion of later-born children, which drives them to move frequently between jobs[9].

Several strands of the literature suggest that education, earnings, health, and other developmental outcomes may lead to lower longevity (Brodish and Hakes, 2016; Chetty et al., 2016; Fletcher et al., 2021; Fletcher, 2015; Fletcher and Frisvold, 2014; Gathmann, Jürges, and Reinhold 2015; Hayward et al., 2015; Karraker, Schoeni, and Cornman 2015; Kinge et al., 2019; Lleras-Muney, 2005; Nelson and Fritzell, 2014). Given the results of the previous literature on the associations of birth order with education, income, and health, one may expect to observe differences in longevity and mortality by birth order. A narrow strand of literature evaluates the birth-order-mortality association. For instance, Barclay and Kolk (2015) use Swedish population register data and implement a within-family comparison analysis to explore the birth-order-mortality relationship. They control for age, mother's age at birth, and cohort dummies. They find significant and robust effects of a positive association. Higher birth order is associated with higher mortality risks, specifically from cancers of the respiratory system and external causes. Modin (2002) compares the mortality outcomes of siblings of different birth orders across families and finds that later-born children face higher risks of all-cause mortality. Furthermore, she finds that children's socioeconomic status during adulthood can explain all the observed associations. O'Leary et al. (1996) use the Terman Life-Cycle Study data that include a small sample of high ability subjects born between 1900 and 1925 in California and examine the role of birth order on mortality by gender and cause of death. They find mixed, inconclusive, and in most cases insignificant differences in mortality by birth cohort. Saarela, Cederström, and Rostila (2016) employ Finnish population register data and show that second-and-higher-born children have a 27–72 percent higher risk of suicide mortality, while there is no consistent birth order effect for other causes of death. Rostila, Saarela, and Kawachi (2014) document a similar association between birth order and suicide using Swedish data. Donrovich, Puschmann, and Matthijs (2014) use 533 observations from Antwerp, Belgium and apply a cross-siblings comparison and find a positive birth-order-mortality association.

However, very few studies investigate the birth-order-mortality relationship in the case of the United States. The main reason is the scarcity of datasets containing information on the family structure during childhood and mortality data of siblings in old age. Smith et al. (2009) use 12,000 sib-pairs born in Utah and explore the correlational links between mortality and a series of early-to-middle-life markers, including family socioeconomic status, family size, parental age at offspring birth, parental mortality, religious upbringing, and birth order. They employ cross-family comparisons and fail to find birth order effects on mortality. Birth order is not the primary variable of interest in their study. Moreover, they fail to account for unobserved heterogeneity across families that could endogenously determine sibship size and their children's health capital.

Background, research question, and study limitations

As mentioned at the end of "**Literature background: Pathways between birth order and mortality**" section, the literature on birth order and mortality is limited. The current study extends this literature by evaluating the birth-order-longevity relationship in the United States. We use death record data from Social Security Administration over the years 1988–2005 linked with the full-count 1940 census. We employ a family fixed effect strategy to compare the siblings' outcomes within a family who differ in their birth order. There are three primary concerns regarding the choice of cohorts in our analyses which we discuss below. First, since we attempt to infer birth order using census data (see "**Data and methods**" section), we focus on birth cohorts of 1922–1940. In fact, about 65 percent of cohorts are born after the Great Depression. This period of US history experienced large and sharp changes in economic conditions. From 1929 to 1933, the US Gross Domestic Product (GDP) fell by about 30 percent. It took the economy almost a decade to reach its pre-1929 levels. The changes in economic conditions could, in various ways, affect fertility and family structure (Sobotka, Skirbekk, and Philipov 2011; Schaller, Fishback, and Marquardt 2020). During the same period, small-scale welfare programs underwent structural changes and experienced exponential expansions. In 1935, Social Security Administration was established as an essential part of New Deal programs. These welfare programs have been shown to affect short-run and long-run health outcomes (Stuckler et al. 2012; Fishback, Haines, and Kantor 2007; Modrek et al. 2022; Noghanibehambari and Engelman 2022; Aizer et al. 2016). Therefore, one concern is regarding the generalizability of the presented results to other birth cohorts.

Second, improvements in health care and medical technology resulted in sharp declines in infant mortality rates, specifically since the mid-1920s (Singh and Yu 2019). Although we include birth cohort fixed effects to absorb the temporal factors, families may benefit from these improvements at differential rates. Therefore, later-born children have an advantage in such an environment and are exposed to less challenging mortality regimes. This fact could also be problematic given that we infer birth order based on the number of children in the household using the 1940 census. If there are earlier-born children who had died prior to 1940, our measure of birth order becomes inaccurate. Although we should be aware of these issues in interpreting the results, we also attempt to explore the effects for other birth cohorts as a robustness check. We use cross-census linking rules to merge 1940 with the 1930 census and replicate our results with earlier birth cohorts. The results (reported and discussed in Appendix B in the Supporting Information) suggest similar effects as the main results.

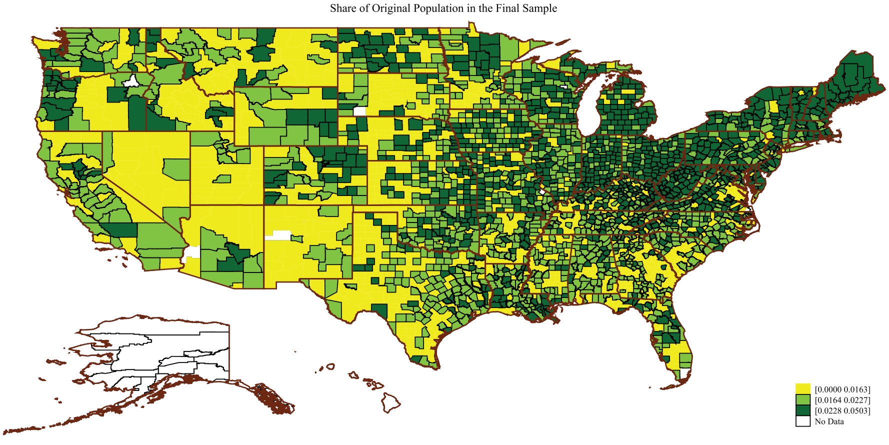
The third concern is the potential influence of local policies. During the early decades of the twentieth century, states started to adopt various policies that could influence short-run and long-run health outcomes. For instance, states experienced a second wave of compulsory schooling and child labor laws that are shown to have spillovers in health outcomes (Fletcher, 2015; Lleras-Muney, 2005; Mazumder, 2008). We address this concern by including a double interaction between birth-state and birth cohort fixed effects to fully absorb state-level policy changes that evolve over cohorts. Moreover, we include county-by-cohort fixed effects to fully control for changes in local economic and sociodemographic conditions that could influence health outcomes.

Data and methods

The primary source of data is Social Security Administration Numerical Identification (Numident) death records extracted from the CenSoc Project (Goldstein et al. 2021). The Numident data contain death records of people who ever had a social security number. Goldstein et al. (2021) provide a publicly available version of this linkage that covers the years 1988–2005 with information on historical identification variables required for merging with the 1940 census. There are four advantages of using these data. First, it provides a wide range of family covariates during individuals' childhood, including family structure. Second, it allows me to recognize siblings in death data based on their location in the 1940 census. This aspect of the data enables a within-sibling comparison strategy. Third, the sample size is between 15 and 20 times larger than previous studies and contains roughly 180,000 sibling groups. The larger sample size enables the study to explore the potential heterogeneity effects across a wide range of demographic and socioeconomic characteristics. Fourth, compared to previous studies on birth order and mortality that cover a specific geographic region (Modin, 2002; O'Leary et al., 1996; Smith et al., 2009), the current data offer universal geographic coverage across US states. This makes the results more representative of the whole population.

The 1940 census is extracted from Ruggles et al. (2020). The census does not record the birth order of children. However, one can infer the birth order by sorting children within each family using the information on parents and the birth date of siblings.[10] We impose some age restrictions

FIGURE 1 Match rate of the within-sibling sample to the original population of 1940 census



to mitigate the measurement error in the birth order measure due to absent children. We exclude those above age 18 as they are more likely to have left their original household. Since older mothers may have children who have already left the household, we restrict the sample to those whose mother's age is at most 36.[11] We also exclude mothers whose age of eldest child is above 18.[12] Moreover, to avoid including adoptees, we remove families with more than 15 identified children to the same mother and families that the reported race of parents and children differ. Even with these sample restrictions, there could be children who have died during early infancy or mothers who have had a child at a very early age who have left the family before 1940. These cases add noise to birth order measures. For instance, suppose that the middle child in a three-children family had died or left the household before 1940 (before even their elder sibling turned 18) and that both present children appear in Numident death records. In that case, the comparison is based on a third-born versusa first-born, while the data show a comparison between a second-born and a first-born. If the relationship between birth order and longevity is monotonous (as the main findings suggest), the measurement error displaces the comparison across different birth orders. Moreover, we also show the results where we stratify the sample by the mother's age group and family size. The robust evidence across the strata suggests that these measurement errors do not drive the results.

These restrictions leave us with an original population of 18,285,054 children. We merge this with Numident data to get a sample of 1,579,588 children who died between the years 1988 and 2005 (hereafter pooled sample). Finally, since the main identification strategy exploits within-sibling variations, we restrict the sample to children with at least one sibling in Numident data. This leaves a final sample with 395,945 observations from 179,379 families (hereafter sibling sample).[13] Figure 1 shows the geographic distribution of the share of observations in the final sibling sample relative to the original population across counties. Although no clustering pattern exists at one specific state or region, counties in the Midwest and Northeast regions reveal higher shares in the final sample.

In later analyses of the paper, we also use Social Security Administration Death Master Files (DMF) data extracted from Goldstein et al. (2021). We should note that while the DMF covers relatively longer death years (1975–2005), it contains death records of males only.

Table 2 reports summary statistics of the final sample. Age at death varies between 47 to 83 years, with an average of 67.2. About 28 percent of observations are first-borns. The demographic characteristics of the final sample are quite similar to the original population (see Appendix A in the Supporting Information). The share of whites, blacks, and Hispanics is 89.9 percent, 9.7 percent, and 2 percent in the final sample and 89.6 percent, 10 percent, 2.4 percent in the original population, respectively.[14] However, females are underrepresented in the final sample (42.5 percent) versus the original population (49.4 percent).[15] More importantly, there are differences in the father's education, mother's education, and father's socioeconomic index. Children with higher socioeconomic status are more likely to be in the final sample. We also observe a similar pattern of discrepancies when comparing the within-sibling sample to the characteristics of the pooled sample. One explanation is that larger families with more children, who have different characteristics for observable and unobservable reasons, are more likely to be observed in Numident data (assuming the same probability of death per child) and appear in the within-sibling sample.

To account for these differences and make the final sample representative of the original population, we use the inverse probability weighting scheme in all analyses. The construction of weights is as follows. In the original population, we create a binary variable that equals one if the observation survives to the within-sibling sample and zero otherwise. We regress the indicator for successful survival on fixed effects of birth state and birth year as well as all demographic and family-level socioeconomic characteristics using logistic regression. We then calculate the predicted probability of successful survival for each observation. We use the inverse of this amount as the weighting rule in regressions. These *inverse probability weights* assume higher weights for observations with a lower likelihood of survival from the original population to the final sample and vice versa. The weights are, in essence, a conventional solution for attrition issues in panel data analysis (Halpern-Manners et al., 2020; Vandecasteele and Debels, 2007; Weuve et al., 2012). Moreover, We show the results of unweighted regressions in Appendix B in the Supporting Information. The similarities in the effects rule out the concerns regarding the representativeness of the final sample.

We start the analysis by comparing across-family differences in mortality of children with higher versus lower birth orders, conditional on a rich

TABLE 2 Summary statistics of the within-sibling sample

Variable	Mean	SD	Min	Max
Death Age (Years)	67.215	5.956	47	83
Birth Order = 1	0.283	0.451	0	1
Birth Order = 2	0.287	0.453	0	1
Birth Order = 3	0.194	0.395	0	1
Birth Order ≥ 4	0.236	0.424	0	1
Female	0.424	0.494	0	1
Race: White	0.902	0.298	0	1
Race: Black	0.095	0.293	0	1
Race: Other Races	0.004	0.059	0	1
Ethnicity: Hispanic	0.019	0.138	0	1
Female Sibling	0.423	0.466	0	1
Female	0.424	0.494	0	1
Birth Space = [1,2]	0.356	0.479	0	1
Birth Space = [3,5]	0.221	0.415	0	1
Mother's Age at Birth	22.75	4.175	15	36
Father's Education ≤ High School	0.72	0.449	0	1
Father's Education = High School	0.222	0.415	0	1
Father's Education ≥ Some College	0.038	0.192	0	1
Father's Education Missing	0.019	0.138	0	1
Mother's Education ≤ High School	0.644	0.479	0	1
Mother's Education = High School	0.309	0.462	0	1
Mother's Education ≥ Some College	0.029	0.169	0	1
Mother's Education Missing	0.017	0.129	0	1
Father's Socioeconomic Index < Median	0.499	0.5	0	1
Father's Socioeconomic Index ≥ Median	0.501	0.5	0	1
Father's Labor Force Status = Active	0.964	0.187	0	1
Father's Occupation = Blue Collar	0.023	0.149	0	1
Father's Occupation = Farm	0.216	0.411	0	1
Father's Occupation = Other	0.762	0.426	0	1
Total Number of Children in the Household	4.504	1.925	2	15
Mother's Labor Force Status = Active	0.076	0.266	0	1
Ownership of Dwelling = Owner	0.324	0.468	0	1
Observations		382,619		

set of covariates. Specifically, we use the following ordinary least square regressions:

$$DA_{ipfb} = \alpha_0 + \alpha_1 BO_2 + \alpha_3 BO_3 + \alpha_4 BO_{4\leq} + \alpha_5 X_i + \alpha_6 Z_f + \zeta_{pb} + \varepsilon_{ifpb}, \quad (1)$$

where the measure of mortality is the age at death (DA). The parameter BO represents birth order of the child i from family f whose place of birth and/or residence is p and who belongs to birth cohort b. First-borns serve as the reference group. Therefore, coefficients of α_1, α_2, and α_3 measure the differences in longevity of second, third, and fourth-and-higher born children compared to first-borns, respectively. In X, we include a series of individual covariates as follows. We calculate the average birth spacing of each child to their neighboring siblings. We generate two dummies for 0–2 and 3–5 years of birth spacing and include them in X. Moreover, we calculate the share of females in one's focal siblings to measure the gender composition that a child is exposed to.[16] We also interact the child's gender with the siblings' gender composition, birth spacing dummies, and dummies to indicate mother's age at the time of birth. The matrix Z contains family controls including dummies for parental race composition, mother's education, father's education, father's socioeconomic index, father's labor market participation status, mother's labor market participation status, father's occupation, father's house-owner status, and indicators for missing values of these variables.

Birth cohort fixed effects account for temporal differences in health outcomes across cohorts that could be reflected in their longevity. We also control for place-specific factors that influence health outcomes by including birth-state and the 1940 county-of-residence fixed effects in regressions. Moreover, about 65 percent of individuals in the final sample are born post-1929, when the Great Depression hit the US economy. Studies provide suggestive evidence of a link between local economic conditions around birth and childhood and old-age mortality (Van Den Berg et al. 2006, 2011; Scholte, Van Den Berg, and Lindeboom 2015; Noghanibehambari et al. 2022). The economic exposures during the 1930s have also been examined in the case of the Dust Bowl and the resulting agricultural failure (Cutler, Miller, and Norton 2007; Noghanibehambari and Fletcher 2022; Arthi 2018; Atherwood 2022). There is also evidence that the New Deal programs during the 1930s had an influence on later-life health and mortality (Modrek et al. 2022; Noghanibehambari and Engelman 2022). To account for these place-based confounders that vary across cohorts, we interact place fixed effects with birth-year fixed effects. Therefore, the parameter ζ represents birth-state-by-birth-year and county-by-birth-year fixed effects.

Comparing the mortality of later-borns to earlier-borns could lead us to spurious correlations for three reasons. First, families choose to have higher birth order children because of reasons unobserved to the researcher. The endogenous fertility decisions impose a sample selection bias that cannot be adjusted by including observable controls such as sibship size. Second, parents may differ in their level of differential investment in the health and education of their later-born children. This differential behavior may be correlated with parental education (Abufhele, Behrman, and Bravo 2017; Grätz and Torche 2016) as well as other unobserved parental characteristics. These disparate attitudes may contaminate the birth-order-mortality relationships as we cannot observe and control for those traits. Third, there are genetic factors that determine some health outcomes and are also correlated with birth order (Sadovnick, Yee, and Ebers 2005; Sulloway 2007; Turner, Pihur, and Chakravarti 2011). It is difficult to control for these factors as there are very few datasets that contain mortality-related genomes as well as other information required for this analysis.

The empirical method attempts to rule out family-level confounders by comparing within-sibling outcomes. It adds family fixed effects to Equation (1), as follows:

$$DA_{ifpb} = \alpha_0 + \alpha_1 BO_2 + \alpha_3 BO_3 + \alpha_4 BO_{4\leq} + \alpha_5 X_i + \xi_f + \zeta_{pb} + \varepsilon_{ifpb.} \qquad (2)$$

The parameter ξ represents the family fixed effect. All other parameters are the same as Equation (1).

Results

Across-sibling results

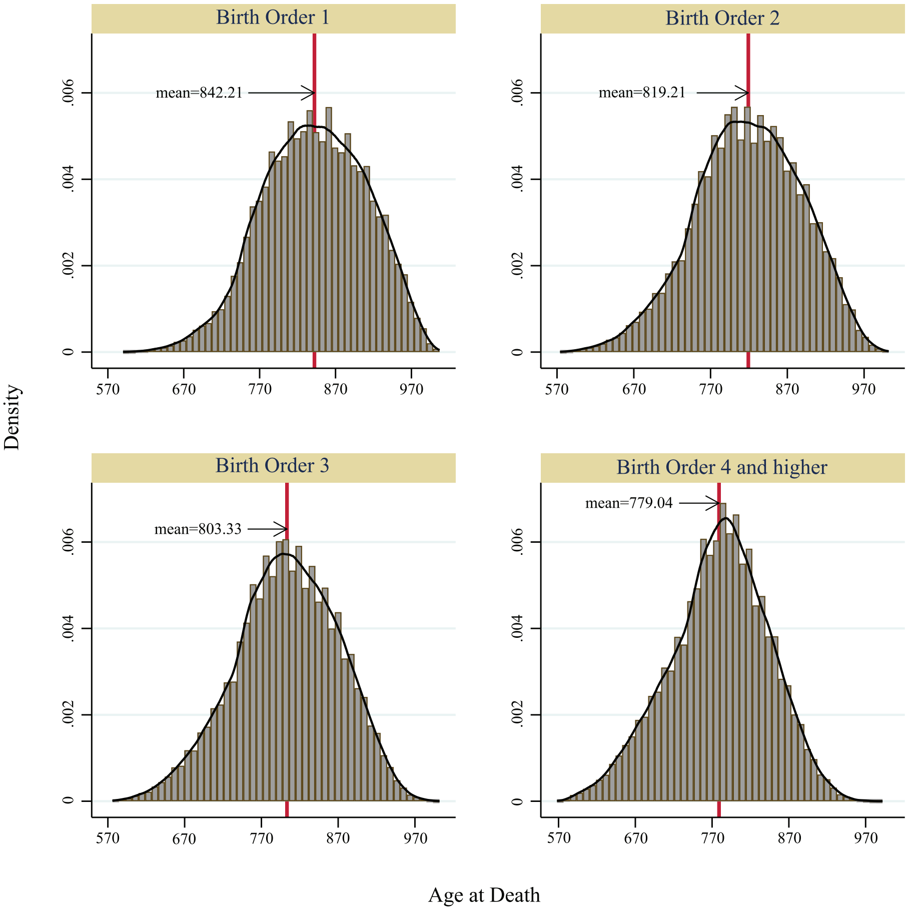
Figure 2 shows the cross-family density distribution of birth order and marks the mean variable within each panel. On average and visually, later-borns reveal lower longevity, and this negative relationship is a monotonous function of birth order. We use OLS regressions to partial out a series of individual, family, and place confounders. The results are reported in Table 3 for the pooled sample and sibling sample in the left and right panels, respectively.[17] We find negative and statistically significant differences between later-borns' and first-borns' longevity across families both in the pooled sample and sibling sample. However, the observed associations diminish in size as we add controls in the second column of each panel. We should note that these results do not fully account for unobserved family characteristics and parental behavior. Our primary identification design relies on the within-siblings comparisons that we present in the following section.

Within-sibling results

The results of family fixed-effect models, introduced in Equation (2), are reported in Table 4. We start with a parsimonious model that includes family and birth-year fixed effects and slightly add more covariates across consecutive columns. The marginal effects are substantially larger than the cross-family comparison results, suggesting that unobserved family confounders, such as within-family cross-sibling discriminatory behavior and investments, likely bias the OLS results of Table 3.

The fully parametrized specification of column 4 implies that later-born children have shorter lives compared to their first-born siblings. Being a second-born, third-born, and fourth-and-higher-born child is associated with 0.1, 0.19, and 0.28 years lower longevity (equivalent to

FIGURE 2 Density distribution of age at death by birth order



1.2, 2.3, and 3.4 months). These results are in line with the findings of Barclay and Kolk (2015) that birth order increases the risks of all-cause mortality among Swedish birth cohorts of 1938–1960. However, the results reported here contradict the findings of Smith et al. (2009) that study early and middle-life determinants of mortality in a small sample of the Utah population and find birth-order-mortality correlations that point to higher risks of mortality for first-born siblings. However, the documented associations of their study are statistically indistinguishable from zero.

Heterogeneity by family structure

One potential source of heterogeneity is regarding the family size and specifically sibship size (Fletcher and Kim, 2019; Masquelier, 2013). In Table 5, we explore the results across 2-child, 3-child, and 4-and-more-child

TABLE 3 The OLS association between birth order and mortality using pooled sample and sibling sample

	Outcome: Age at death (years), samples:			
	Pooled sample		Sibling sample	
	(1)	(2)	(3)	(4)
Birth Order = 2	−0.055***	−0.047***	−0.024	−0.020
	(0.009)	(0.010)	(0.023)	(0.023)
Birth Order = 3	−0.072***	−0.056***	−0.048*	−0.040
	(0.012)	(0.013)	(0.027)	(0.027)
Birth Order ≥ 4	−0.100***	−0.083***	−0.081***	−0.067**
	(0.014)	(0.019)	(0.029)	(0.030)
Observations	1,576,951	1,576,951	382,619	382,619
R^2	0.468	0.468	0.478	0.481
Fixed effects	√	√	√	√
Controls		√		√

NOTE: Standard errors, clustered at the family level, are in parentheses. Controls include a continuous variable measuring the gender composition of siblings (1 = all siblings female) interacted with a gender dummy, dummies for birth spacing between the previous and later sibling (1–2 years/3–5 years) interacted with a gender dummy, maternal age at the time of birth interacted with a gender dummy, dummies for race (black/white), a dummy for ethnicity (Hispanic), dummies for father's education (less than high school/high school/some college and more), mother's education (less than high school/high school/some college and more), father's socioeconomic index (below and above median), father's labor force status (active), mother's labor force status (active), father's occupation (white-collar/farm/other), ownership of dwelling (owner), family's total number of children, and missing indicators for missing values of parental characteristics. The set of fixed effects include county of residence at 1940 by birth-year fixed effects, and birth-state by birth-year fixed effects. The regressions are weighted using the inverse probability weights where the probabilities are based on logit regressions of a successful merging (a dummy that is 1 if the observation is merged with Numident and is present in the respective sample, and 0 otherwise) on observables and fixed effects.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

families in columns 1–3, respectively. The negative associations of each birth order diminish as the sibship size grows. For instance, the coefficient of second-borns reduces (in magnitude) by 47 percent from a 2-child family to a 4-more-child family. In addition, the marginal effects monotonically decrease by birth order, similar to the main results of Table 4. For instance, the marginal effect of third-borns is roughly 2.4 and 2.1 times the marginal effect of second-borns in 3-child and 4-more-child families, respectively (columns 2 and 3).

We further show the results by maternal age at first birth in columns 4 and 5. We observe larger coefficients for all later-born coefficients among younger mothers (age at first birth ≤ 22).

Heterogeneity by sociodemographic characteristics

Several studies suggest the role of parental sociodemographic and socioeconomic features in determining short-run and long-run health outcomes (Almond, Currie, and Duque 2018; Black, Devereux, and Salvanes 2016; Brandt, Deindl, and Hank 2012; Currie, 2009; Fletcher, 2009). In Table 6, we explore the heterogeneity of the results by race, gender, maternal education, and paternal socioeconomic score. In so doing, we interact a non-white

TABLE 4 The association between birth order and mortality using within family comparison strategy

	Outcome: Age at death (years)			
	(1)	(2)	(3)	(4)
Birth Order = 2	−0.058**	−0.087***	−0.088***	−0.100***
	(0.026)	(0.028)	(0.028)	(0.031)
Birth Order = 3	−0.084**	−0.130***	−0.131***	−0.194***
	(0.039)	(0.042)	(0.042)	(0.047)
Birth Order ≥ 4	−0.188***	−0.238***	−0.240***	−0.281***
	(0.056)	(0.059)	(0.059)	(0.066)
Observations	386,904	386,904	386,902	382,619
R^2	0.694	0.695	0.695	0.752
Family FE	√	√	√	√
Birth-year FE	√	√	√	√
Controls		√	√	√
County and birth-state FE			√	√
County-birth-year and birth-state-birth-year FE				√

NOTE: Standard errors, clustered at the family level, are in parentheses. Controls include a continuous variable measuring the gender composition of siblings (1 = all siblings female) interacted with a gender dummy, dummies for birth spacing between the previous and later sibling (1–2 years/3–5 years) interacted with a gender dummy, and maternal age at the time of birth interacted with a gender dummy. The regressions are weighted using the inverse probability weights where the probabilities are based on logit regressions of a successful merging (a dummy that is 1 if the observation is merged with Numident and is present in the respective sample, and 0 otherwise) on observables and fixed effects.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE 5 Heterogeneity across different family structure

	Outcome: Age at death (years), samples:				
	2-Child families	3-Child families	4-and-more-Child families	Mother's age < 30	Mother's age ≥ 30
	(1)	(2)	(3)	(4)	(5)
Birth Order = 2	−0.162	−0.191**	−0.084*	−0.129**	−0.060
	(0.102)	(0.084)	(0.045)	(0.055)	(0.069)
Birth Order = 3		−0.453***	−0.177***	−0.223**	−0.171*
		(0.140)	(0.060)	(0.091)	(0.095)
Birth Order ≥ 4			−0.282***	−0.315***	−0.210*
			(0.082)	(0.119)	(0.126)
Observations	36,758	58,798	236,285	176,007	106,615
R^2	0.807	0.794	0.757	0.765	0.767

NOTE: Standard errors, clustered at the family level, are in parentheses. All regressions include family fixed effects, county-by-birth-year fixed effects, birth-state-by-birth-year fixed effects, and controls. Controls include a continuous variable measuring the gender composition of siblings (1 = all siblings female) interacted with a gender dummy, dummies for birth spacing between the previous and later sibling (1–2 years/3–5 years) interacted with a gender dummy, and maternal age at the time of birth interacted with a gender dummy. The regressions are weighted using the inverse probability weights where the probabilities are based on logit regressions of a successful merging (a dummy that is 1 if the observation is merged with Numident and is present in the respective sample, and 0 otherwise) on observables and fixed effects.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE 6 Heterogeneity across subsamples based on parental sociodemographic characteristics

	Outcome: Age at death (years)			
	(1)	**(2)**	**(3)**	**(4)**
Birth Order = 2 × Non-white	0.182*			
	(0.096)			
Birth Order = 3 × Non-white	0.324***			
	(0.107)			
Birth Order ≥ 4 × Non-white	0.418***			
	(0.112)			
Birth Order = 2 × Mother's Schooling < 12		−0.005		
		(0.008)		
Birth Order = 3 × Mother's Schooling < 12		−0.013		
		(0.009)		
Birth Order ≥ 4 × Mother's Schooling < 12		−0.018*		
		(0.011)		
Birth Order = 2 × Father's SEI			−0.001	
			(0.001)	
Birth Order = 3× Father's SEI			−0.0001	
			(0.002)	
Birth Order≥4 × Father's SEI			−0.002	
			(0.002)	
Birth Order = 2 × Female				−0.028
				(0.060)
Birth Order = 3 × Female				−0.018
				(.066)
Birth Order ≥ 4 × Female				−0.045
				(0.064)
Female				0.432***
				(0.131)
Birth Order = 2	−0.113***	−0.056	−0.078*	−0.084**
	(0.033)	(0.069)	(0.043)	(0.041)

/...

TABLE 6 (Continued)

	Outcome: Age at death (years)			
	(1)	(2)	(3)	(4)
Birth Order = 3	−0.231***	−0.095	−0.204***	−0.187***
	(0.049)	(0.087)	(0.059)	(0.056)
Birth Order ≥ 4	−0.327***	−0.136	−0.226***	-0.249***
	(0.068)	(0.104)	(0.077)	(0.073)
Observations	375,861	375,861	375,861	375,861
R^2	0.752	0.752	0.752	0.752

NOTE: Standard errors, clustered at the family level, are in parentheses. All regressions include family fixed effects, county-by-birth-year fixed effects, birth-state-by-birth-year fixed effects, and controls. Controls include a continuous variable measuring the gender composition of siblings (1 = all siblings female) interacted with a gender dummy, dummies for birth spacing between the previous and later sibling (1–2 years/3–5 years) interacted with a gender dummy, and maternal age at the time of birth interacted with a gender dummy. The regressions are weighted using the inverse probability weights where the probabilities are based on logit regressions of a successful merging (a dummy that is 1 if the observation is merged with Numident and is present in the respective sample, and 0 otherwise) on observables and fixed effects.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

dummy, female dummy, mother's years of schooling, and father's socioeconomic score with birth order variables. We should note that the main effects of these interaction variables are absorbed in family fixed effects.

We observe positive and significant coefficients for double-interaction of birth order variables with non-whites.[18] As the main effects of birth order suggest, the implied associations are larger and negative among whites. However, we do not observe heterogeneity based on maternal education (column 2) and paternal socioeconomic status (column 3). The double-interaction coefficients are small in magnitude and statistically insignificant. In column 4, we show the birth order interactions with a female dummy. For birth orders 3 and 4-and-higher, we observe negative interaction terms, suggesting higher influence of birth order on longevity of female children. However, the coefficients are statistically insignificant which limit additional interpretations.

Additional robustness checks

In Appendix E in the Supporting Information, we explore the sensitivity of the results to alternative derivative outcomes. We show that the birth order associations are robust when we replace the outcomes with binary indicators of living beyond specific ages. We also show the robustness to alternative functional form when we replace the outcome with log of age at death.

In Appendix F in the Supporting Information, we examine the heterogeneity across subsamples. We find virtually zero coefficients for those residing in West. We also find larger associations for earlier cohorts.

In Appendix G in the Supporting Information, we evaluate the robustness of the results to using an alternative data that cover longer death window. Specifically, we use DMF data that report death to male individuals over the years 1975–2005 (14 additional death years). We find very similar results suggesting that limited death window does not affect the estimates. As an additional check, we also replicate the main regressions using CenSoc-provided weights. This weighting scheme is based on matching birth cohorts with their longevity using the Human Mortality Database (HMD) with the purpose of accounting for left and right truncations in Numident death records. We report and discuss the results in Appendix B in the Supporting Information. We observe coefficients that are almost identical to those weighted by inverse probability weights used in the paper.

Mechanisms

For reasons discussed in "**Data and methods**" section, we restrict the sample to children below age 18. Therefore, we are unable to observe their completed education or labor market outcomes in 1940. To overcome this data limitation and to investigate potential mechanism channels, we construct a variable that partly reflects early educational outcomes. We define an age-specific education variable that measures the relative years of schooling of a child with respect to their K-12-eligible years. For instance, 13-year-olds should have had 8 years of education as they are exposed to 8 years of K-12 schooling. A child with 6 years of schooling has studied, on average, three-quarters of their eligible schooling years. Therefore, the age-specific education of the child is 0.75. In the analyses of this section, we drop children under 5 as they are below school age.

We explore the birth order associations with age-specific education using the same empirical method of Equation (2). The results are reported in column 1 of Table 7. The results show significant negative associations. With respect to the mean of the outcome, later-borns have 9.8–27.5 percent lower age-specific education compared to their first-born sibling.

In column 2 of Table 7, we show the birth order effects on the probability of school attendance. The coefficients are negative for all later-born children and mostly statistically significant, although small in magnitude. For fourth-and-higher-born children, we observe a reduction of 1.6 percentage points in the likelihood of attending school while they are at school age (off a mean of 0.83). These are early educational outcomes and do not reveal a longer run educational channel. Relying on these early outcomes and studies that show the birth order effects on completed education, to the extent that education increases longevity, they could point to a possible mechanism channel (Fletcher et al., 2021; Fletcher, 2015; Fletcher and Frisvold, 2014; Jamison, Jamison, and Hanushek 2007; Lacroix et al., 2019; Lleras-Muney, 2005; Malamud, Mitrut, and Pop-Eleches 2021).

TABLE 7 Exploring potential mechanisms

	Outcomes:					
	Age-specific education	School attendance	Occupation: Professional workers	Occupation: managers, proprietors, officials	Occupation: Craftsmen, foremen, kindred workers	Occupation: Other
	(1)	(2)	(3)	(4)	(5)	(6)
Birth Order = 2	−0.058***	−0.002	−0.000	−0.012***	−0.001	0.014***
	(0.002)	(0.002)	(0.001)	(0.003)	(0.001)	(0.004)
Birth Order = 3	−0.114***	−0.007**	0.001	−0.020***	−0.002	0.021***
	(0.004)	(0.003)	(0.002)	(0.006)	(0.002)	(0.007)
Birth Order ≥ 4	−0.166***	−0.016***	−0.002	−0.025**	−0.000	0.028**
	(0.006)	(0.004)	(0.004)	(0.010)	(0.004)	(0.011)
Observations	234,665	277,135	24,251	24,251	24,251	24,251
R^2	0.774	0.850	0.725	0.656	0.642	0.667
Mean DV	0.580	0.825	0.003	0.013	0.002	0.983

NOTE: Standard errors, clustered at the family level, are in parentheses. The sample of columns 1–2 is restricted to children above age 5. The sample of columns 3–6 is restricted to children aged 14–18. All regressions include family fixed effects, county-by-birth-year fixed effects, birth-state-by-birth-year fixed effects, and controls. Controls include a continuous variable measuring the gender composition of siblings (1 = all siblings female) interacted with a gender dummy, dummies for birth spacing between the previous and later sibling (1–2 years/3–5 years) interacted with a gender dummy, and maternal age at the time of birth interacted with a gender dummy. The regressions are weighted using the inverse probability weights where the probabilities are based on logit regressions of a successful merging (a dummy that is 1 if the observation is merged with Numident and is present in the respective sample, and 0 otherwise) on observables and fixed effects.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Another potential pathway is occupational choice. We restrict the sample to children at least 14 years old and explore the likelihood of having white-collar occupations for later-born children versus first-borns. We report the results in columns 3–5 of Table 7. We observe negative and, in some cases, statistically significant coefficients. Besides, we also observe a higher likelihood of having reported other types of occupations, among which are more blue-collar jobs (column 6). To the extent that early occupational choice may influence health outcomes, these results point to potential pathways between birth order and mortality (Fletcher, 2012; Kelly et al., 2014).

A note on the magnitude of the results

Although the scarcity of similar studies using US data and specifically for the longevity outcomes limits the scope for comparison of magnitudes, we can compare these effects with other early-life determinants of later-life longevity. For instance, Fletcher and Noghanibehambari (2021) explore the effects of college openings during the 1940s–1950s on education and old-age longevity of 1923–1940 cohorts using Numident data. Their treatment-on-treated calculations suggest that having a college education is associated with roughly 1 year higher age at death. The birth order effects on longevity are roughly 10–28 percent of the estimated effect of a college degree. Noghanibehambari and Fletcher (2022) examine the impact of *in utero* and childhood exposure to the 1930s Dust Bowl and topsoil erosions on old age longevity. They find intent-to-treat effects of about 1-month lower longevity. Therefore, our results suggest later-born effects on longevity that are 1–2.8 times the intent-to-treat effect of the Dust Bowl exposure during early life.

Moreover, we compare the effects across studies to explore to what degree the birth order effects operate through improved educational attainments.[19] Halpern-Manners et al. (2020) use Numident-DMF data and implement a twin fixed-effect strategy to explore the effects of education on mortality. They find that an additional year of education is associated with 0.35 years higher age at death. In addition, Black, Devereux, and Salvanes (2005) document that second-borns and third-borns attain 0.34 and 0.53 fewer years of schooling. Kantarevic and Mechoulan (2006) show that in the Panel Study of Income Dynamics, first-borns have roughly 0.5 higher years of completed education. Assuming a disadvantage of 0.5 years in education and combining with the reported effects of Halpern-Manners et al. (2020), one can calculate an educational-induced longevity disadvantage of 0.2 years. This is quite similar to the reduced-form effects of Table 4, and suggests that the birth order effects operate primarily through educational channels.

Conclusion

A growing body of research explores the relevance of early-life conditions in explaining the disparities in achievements and outcomes during adulthood and old ages. One strand of this literature explores how birth order, as a marker of fewer available resources during childhood, affects later-life outcomes such as education, income, personality traits, risk preference, occupational choice, and health outcomes. The current study adds to this ongoing research by exploring the associations between birth order differences within siblings with their adulthood and old-age longevity.

We find negative associations between birth order and longevity for both between-family and within-family analyses. However, our findings that use within-family comparisons suggest substantially larger reductions in longevity among later-born siblings. Second-born, third-born, and fourth-and-higher-born children live about 1–3 fewer months of life. The effects are monotonic by birth order and grow in magnitude as we look at higher birth orders. Together, these findings suggest a large role for within family processes, such as parental favoritism, underlying the long term effects of birth order. Additional analyses suggest that the effects are more pronounced among whites. Somewhat surprisingly, the results do not point to differences in birth-order-longevity association based on gender, family socioeconomic status, and maternal education, suggesting that the sources of within-family inequality, such as parental favoritism, may be general to the population rather than specific to these measures of individual and family status. Further analyses point to negative birth order effects on early educational outcomes. Later-born children have lower age-specific education and are less likely to attend school. We posit that educational channels can explain a considerable portion of the birth-order-mortality association.

Roser et al. (2013) show that between the years 1700 and 2013, the average life expectancy of a 50-year-old person increased from 70.6 to 82.8 years. Therefore, the disadvantage of a later-born child is roughly 0.8–2.3 percent of the improvements in longevity over three centuries. Smith and Bradshaw (2006) document that life expectancy at birth in the United States increased from 56.3 (58.7) for males (females) to 60.8 (65.3) for cohorts born between 1922 and 1940 (cohorts in our final sample). Using the average increases in life expectancy and the results of Table 4, we find that the disadvantage of being a later-born is roughly equivalent to 1.5–4.3 percent of the overall longevity improvements of these cohorts.

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Notes

1 The so-called *resource dilution hypothesis* states that accrued material, cognitive, and interpersonal resources are a decreasing function of sibship size (Barclay and Kolk, 2015; Barclay and Myrskylä, 2014; Blake, 1981; Hertwig, Davis, and Sulloway 2002; Jæger, 2009). On the other hand, the so-called *confluence hypothesis* suggests that children's intellectual development requires cognitive stimulation to adapt to the dynamically changing family environment. Cognitive stimulation becomes harder to acquire as sibship size grows (Barclay and Kolk, 2015; Jæger, 2009; Zajonc, 1976; Zajonc and Sulloway, 2007). Scholars in various fields, including economics, sociology, demography, medical sciences, and psychology, investigate the later-life effects of birth order. Although there is no conclusive consensus in this field, they generally find that later-born children, on average, have adverse later-life health outcomes, intelligence, cognitive development, IQ, education, and earning (Bertoni and Brunello, 2016; Black et al., 2011; Jayachandran and Pande, 2017; Malak et al., 2019; Pfouts, 1980; Spears et al., 2019).

2 In addition, there is also evidence that a child's birth order is associated with risks of neonatal and infant mortality (Cabrera, 2011; Coffey and Spears, 2021; Cohen, 1975; Miller et al., 1992). Spears et al. (2019) show that, after controlling for a child's number of siblings, one can observe early-life survival advantages for later-borns.

3 Aparicio et al. (2020) use administrative data from Spain and document a positive relationship between birth weight and unemployment rate. In addition, they document that birth weight increases with birth order. The offsetting effect of birth order can explain about 20 percent of the countercyclical behavior of birth weight

4 Contrary to these findings, Ejrnæs and Pörtner (2004) construct a model of intra-household allocation that accounts for endogenous fertility. They use a longitudinal dataset from the Philippines and find that later-born children have better educational outcomes and spend more time on educational activities. They posit that these results are in line with model predictions.

5 Other studies also explore the birth order impact on IQ and find negative associations (Belmont and Marolla, 1973; Black, Devereux, and Salvanes 2011; Damian and Roberts, 2015; Pfouts, 1980).

6 Jayachandran and Pande (2017) document that a birth order gradient is an important determinant in the observed height disadvantage of Indian children. They posit that parental favoritism for eldest sons makes the birth-order-height gradient steeper than in similarly poor regions in sub-Saharan Africa. Black, Devereux, and Salvanes (2016) use a large Norwegian dataset to explore the birth order effects across a wide range of health behavior and outcomes. They find that firstborns are less likely to smoke, and they tend to report better physical and mental health. However, in a similar study, Barclay and Myrskylä (2014) use Swedish military conscription data, employ a within-family comparison strategy across various sibship sizes, and find that earlier-borns have better fitness outcomes.

7 Havari and Savegnago (2020) use European survey data to document the intergenerational impact of birth order. They show that children of later-born parents have lower education compared with children's outcomes of earlier-born parents.

8 Grinberg (2015) find that first born children in the National Longitudinal Survey of Youth (NLSY) are more likely to choose managerial and leadership occupations. Other studies find weak or mixed results for the effect of birth order on leadership personality (Andeweg and Berg, 2003; Tricarichi and Jalajas, 2019).

9 However, Lejarraga et al. (2019) do not find consistent evidence that later-borns have a higher propensity to take risks.

10 The Numident reports the exact date of birth of the deceased. Therefore, there is less concern for measurement errors in reported age.

11 If mothers enter the maternity ward before 18 and their first child leaves the household before 18, the birth order measure becomes noisy. However, we show the results across mothers' age (rather than a pooled sample of all ages). Also, in 1940, only 0.09 percent of mothers have children and are less than 18 years old. In addition, there is only 4.5 percent of individuals who are less than 18 and do not live with their parents.

12 These age restrictions result in a limited window of birth cohorts: 1922–1940. However, in Appendix C in the Supporting Information, we show that the results are robust to other birth cohorts. Specifically, we use cross-census linking and employ the full-count 1930 census and find comparable birth-order-longevity relationship for birth cohorts of 1910-1930.

13 Summary statistics of original sample and pooled (merged with Numident) sample are reported in Appendix A in the Supporting Information.

14 As per census definitions, we define white and black based on race categories. Hispanic is defined based on ethnicity and so it is exclusive of race.

15 This occurs more often in linking historical data as women usually change their names after marriage.

16 Côté, Blanchard, and Lalumière (2003) show that the birth weight of newborns depend on the gender of previous child. Cools and Patacchini (2019) document the earning difference of adults with an older brother to those with and older sister and find a 7 percent earnings penalty for those who have an older brother.

17 In Appendix I in the Supporting Information, we show the full regression results.

18 In Appendix H in the Supporting Information, we explore the heterogeneity of the results by race and implement regressions across subsamples of whites and non-whites, separately. We find larger and significant associations in the white subsample. Among non-whites, we observe smaller and insignificant coefficients.

19 As we discuss in "**Literature background: Pathways between birth order and mortality**" section, the birth order literature is mixed and do not provide a consensus argument regarding potential mediatory channels. However, the evidence weighs toward a negative birth order effect in educational outcomes, more than earnings and other labor market outcomes (Bertoni and Brunello, 2016; Booth and Kee, 2009).

Data Availability Statement

The data and programs are available upon request from the corresponding author.

Conflict of Interest

The authors declare no conflict of interest.

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