The Long Reach of Medical Access: Early-Life Physician Availability and Later-Life Mortality*

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Abstract

This study examines the long-term effects of early-life healthcare access on later-life mortality by focusing on the loss of Black physicians in the early 20th-century United States. Using newly linked individual-level death records and historical census data, we show that being born in a county that experienced the loss of a Black physician is associated with a 4.2-month reduction in longevity for Black individuals. We provide evidence to rule out concerns related to contemporaneous changes in county-level characteristics, shifts in the sociodemographic and socioeconomic composition of the sample, selection bias arising from linkage procedures, and pre-existing trends in longevity. The effects are more pronounced among males, individuals from lower socioeconomic backgrounds, and non-migrants. We also find suggestive evidence that reduced educational attainment, labor force participation, and income serve as mediating channels.

Keywords: Mortality, Longevity, Health Care Access, Early-Life Exposures, Physician Access

JEL Codes: I10, I14, J15, N31, N32

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1. Introduction

Access to medical care is widely viewed as a fundamental determinant of population health (Cutler, 2004; Cutler & Miller, 2005; Hollingsworth et al., 2024). The early decades of the 20th century were marked by sweeping expansions in public health infrastructure and clinical services that substantially increased access to care, particularly in rural and low-income communities (Duffy, 1992; Rosen, 2015). Federal initiatives such as the Sheppard–Towner Act of 1921 funded maternal and infant clinics in underserved areas, while large-scale campaigns against endemic diseases (including hookworm, pellagra, and malaria) raised population health in many disadvantaged regions (Beach et al., 2016; Bleakley, 2007, 2010; Bleakley & Lange, 2009; Moehling & Thomasson, 2012, 2014). At the same time, the spread of local health departments and clinics extended basic medical and sanitation services to populations that had previously faced severe barriers to care, contributing to substantial reductions in infant mortality and improvements in long-run health outcomes (Bauernschuster et al., 2023; Hoehn-Velasco, 2018, 2021; Hoehn-Velasco & Wrigley-Field, 2022). More recent work further demonstrates that early-life policy interventions that expand access to medical care can generate persistent improvements in adult health and survival (M. J. Bailey & Goodman-Bacon, 2015; Black et al., 2017; Goodman-Bacon, 2021). These findings are consistent with the fetal-origins hypothesis, which posits that prenatal and infant health conditions "program" the risk of chronic disease later in life, implying that adverse exposures during in utero and early childhood can have effects that only emerge decades later (Almond & Currie, 2011; Barker, 1990, 1992, 1994, 1995; Barker et al., 1993). Despite this theoretical and empirical background, relatively few studies have examined how the local supply of healthcare providers during infancy shapes longevity in old age. This paper connects these literatures by studying whether provider availability itself, measured at the local level and during the earliest years of life, has persistent effects on later life mortality.

In this paper, we fill that gap by focusing on physician supply among Black communities. Our focus on Black communities stems from the racially segregated structure of the healthcare system during the early decades of the 20th century. Black Americans often had very few physicians or clinics available to them, particularly in rural and low-income areas. As a result, the loss of even a small number of local Black doctors could leave an entire Black community without reliable access to medical care. Historical evidence documents these pronounced racial gaps in early-twentieth-century medical access (Bayer & McMillan, 2012; Byrd & Clayton, 2001;

Largent, 2018). Medical care was highly segregated, and Black physicians were often the primary, and in many places the only, plausible professional providers for Black patients. In such a setting, the exit, retirement, or death of a Black physician could generate a discrete reduction in effective access to medical care for Black infants and children, while creating minimal direct consequences for non-Black residents, who had access to a much larger pool of white physicians. Against this backdrop, we examine whether being born after a county loses its Black physician leads to worse survival decades later for Black individuals.

To address this research question, we assemble two primary data sources. First, we use individual-level mortality records from the Social Security Administration (Numident and the Death Master File) linked to the complete-count 1940 Census. Second, we measure the county-year supply of Black physicians by digitizing historical physician directories and define a "physician loss" event as a transition from at least one Black physician in a county-year to none in the following year, including persistent losses. Our baseline design is a differences-in-differences event-study framework that exploits the staggered timing of physician-loss events across counties. We compare Black and non-Black individuals born in the same county and year around the timing of a local Black-physician loss, while flexibly controlling for place and cohort effects. The identifying assumption is that, absent disruptions to Black-physician availability, racial differences in longevity would evolve similarly within counties across adjacent birth cohorts. We assess this assumption using event-study specifications and implement interaction-weighted estimators designed for staggered adoption with heterogeneous treatment effects.

The results indicate economically meaningful and statistically detectable long-run effects. Black individuals born after a county loses its Black physician live 4.2 fewer months on average, while the estimated effect for non-Black individuals is small and statistically indistinguishable from zero, consistent with the race-specific nature of the access shock in a segregated healthcare system. Event-study estimates show no evidence of differential pre-trends, and the decline in Black longevity emerges after the physician-loss event. Effects are larger for men, for individuals from lower socioeconomic backgrounds, and for non-migrants, and are stronger when counties experience larger reductions in the Black-physician inventory.

We also provide evidence on mechanisms consistent with a life-course channel linking early-life medical access to adult socioeconomic status and, ultimately, mortality. Using linked

1940 Census outcomes, exposure is associated with a 9.2 percent increase in the probability of not completing high school, reduced labor-force participation, and a sizeable reduction in wage income (about 45%). Exposure also reduces out-of-state migration by 1940, consistent with a channel in which limited early-life health inputs or constrained opportunities reduce mobility and later-life environments. These findings align with broader evidence that education and socioeconomic status are central determinants of adult health and mortality (Cutler et al., 2006; Fletcher, 2015; Galama et al., 2018; Lleras-Muney, 2005) and with models of dynamic complementarity in the formation of skills and capabilities early in life (Cunha & Heckman, 2007).

This paper makes two contributions to the literature. First, this is the first study to examine how the availability of physicians in local area during early life influences long-term health and longevity. It adds to a narrow but growing literature on the long-term impact of early-life healthcare access. For instance, Hoehn-Velasco (2018) and Hoehn-Velasco (2021) found that the introduction of county health departments in the early 20th century led to reductions in infant mortality and improvements in education, income, and longevity later in life. Similarly, Goodman-Bacon (2021) demonstrated that the rollout of Medicaid in the 1960s significantly lowered later life mortality, particularly for Black Americans who historically had limited access to healthcare and insurance coverage.

Second, this paper adds to the expanding body of research examining how in-utero and early-life conditions shape later-life health and mortality outcomes. Prior studies have documented that exposures to adverse early-life environments—such as pesticide contamination, natural disasters, poor economic conditions, and regressive policy interventions—can have lasting effects on old-age longevity (Aizer et al., 2016; Fletcher & Noghanibehambari, 2023, 2024; Lindeboom et al., 2010; Noghanibehambari & Engelman, 2022; Noghanibehambari & Fletcher, 2023a, 2023b, 2023c, 2024; Scholte et al., 2015; Van Den Berg et al., 2006). The present study contributes to this literature by investigating a previously understudied dimension of early-life adversity: disruptions in physician availability in a racially segregated healthcare system. We show that the loss of Black physicians during early life had lasting effects on the mortality of Black individuals. This highlights how unequal access to healthcare in racially segregated settings can lead to long-term negative consequences for population health.

The rest of the paper is organized as follows. Section 2 provides a short background on the "birth of a nation" movie. Section 3 discusses data sources and sample selection strategy. Section 4 introduces the econometric method. Section 5 reviews the results. Section 6 examines robustness checks. Section 7 explore mechanism channels. Section 8 discusses the magnitude of the results in comparison with the previous literature. Section 9 concludes the paper.

2. Background

At the turn of the 20th century, the American healthcare system was decentralized, unevenly regulated, and highly stratified by geography and race. At the turn of the twentieth century, the American healthcare system was fragmented, locally organized, and characterized by substantial disparities in access across regions and racial groups. Medical care was delivered primarily through small, private practices, charitable institutions, and a limited number of hospitals concentrated in urban areas. Most healthcare was paid for out of pocket, as public health insurance did not exist and private insurers largely avoided medical coverage (Stevens, 2009; Thomasson, 2002; Thomson et al., 2020). Charitable hospitals in large cities provided some care to low-income patients, but rural and segregated communities often relied on a small number of local physicians, midwives, or informal healers (Byrd & Clayton, 2012).

Physician supply expanded during this period, but distribution remained uneven. Rapid urbanization and industrialization drew physicians toward cities, where professional opportunities, hospitals, and paying patients were more abundant (Rothstein, 1987; Starr, 1978). Many rural counties had few or no physicians, and access to medical care varied widely across local labor markets. These geographic disparities were compounded by racial segregation. Although Black Americans made up roughly one-tenth of the population around 1900, they accounted for only a small fraction of the physician workforce (Byrd & Clayton, 2012; Chrisinger, 2024; Ly, 2021). In the South and other segregated regions, Black patients often relied on a limited number of Black physicians practicing in segregated clinics or hospitals, as white physicians frequently refused to treat Black patients (Ward, 2003).

Healthcare demand was high during this era. Infectious diseases such as tuberculosis, pneumonia, gastrointestinal infections, and childhood illnesses accounted for a large share of deaths (Cutler et al., 2006; Preston & Haines, 2014). Infant and maternal mortality rates were substantial, and the 1918 influenza pandemic dramatically reduced life expectancy by raising

mortality (Fletcher, 2018; Myrskylä et al., 2013; Noymer & Garenne, 2000). Under these conditions, the availability of a local physician could be critical, particularly for prenatal care, childhood illnesses, and acute infectious diseases.

At the same time, the early 20th century witnessed efforts to professionalize medical practice and raise training standards. Reforms led by professional organizations and philanthropic foundations sought to standardize curricula, strengthen licensing, and link medical education more closely to hospitals. The 1910 Flexner Report was a central part of this broader reform movement. While these changes improved scientific training, they also reduced the number of medical schools, including several institutions that had trained Black physicians. As a result, the overall supply of physicians became more concentrated geographically and demographically, and racial disparities in access to medical care persisted or widened in many areas (Bailey, 2017; Clay et al., 2025; Harley, 2006; Hunt, 1993; Webb-Detiege, 2024).

3. Data and Sample

The primary source of data is the Social Security Administration's Numerical Identification (Numident) and Death Master File (DMF) death records, obtained through the CenSoc project (Breen et al., 2023; Breen & Osborne, 2022; Goldstein et al., 2021, 2023). The DMF covers deaths for males from 1975 to 2023, while the Numident includes both genders for deaths occurring between 1988 and 2005. These data include exact birth and death dates, which we use to calculate longevity, i.e., age at death. The CenSoc-linked versions of Numident and DMF offer distinct advantages that make them especially suitable for our research setting. Most importantly, the death data are linked at the individual level to the full-count 1940 Census, allowing us to observe early-life family characteristics and a wide range of adulthood outcomes for cohorts born in the early 20th century. Moreover, the original dataset includes roughly 17 million death records, providing a sufficiently large sample such that leaves us with ample statistical power even after applying sample restrictions to specific cohorts and geographic areas. This is especially useful in our setting where the treated group (Black individuals) constitutes a small share of the population. This stands in sharp contrast to more commonly used sources like the Health and Retirement Study, which, even after linkage to the 1940 Census, yield only a few thousand observations, and even fewer treated individuals after sample restrictions.

Additionally, recent developments in cross-census linkage methods allow us to trace many individuals across historical censuses (specifically, 1910–1930), enabling us to recover information on their county of birth or early-life residence (details below). This is a crucial advantage in our context, as early-life geographic identifiers are rarely available in datasets that also contain information on later-life health and mortality.

Since our main treatment (i.e., physician availability shocks) is measured at the county level and assigned during early life, it is essential to obtain a reliable proxy for the individual's county of birth. We implement three strategies to identify this. First, the Numident data include county of birth as reported to the Social Security Administration, enabling us to recover birth county for approximately 45% of the sample. Second, we employ cross-census linkage techniques developed by Price et al. (2021) to link individuals from the Numident/DMF-1940-Census dataset to historical censuses from 1910 to 1930 (extracted from Ruggles et al. (2024)). We then use the county of residence from these earlier censuses as a proxy for birth county: 8.6% are linked to 1910, 11.7% to 1920, and 13.4% to 1930. The 1940 Census also reports the county of residence in 1935, which we use as an early-life proxy for an additional 1.5% of individuals. For the remaining unlinked individuals, we use their 1940 county of residence, covering 19.8% of the sample.[5] We apply similar linkage methods to obtain measures of maternal literacy and paternal socioeconomic status, which we include as control variables in our regressions.

Data on city/county-level physicians are derived from historical American Medical Directories. We aggregate physician counts at the county level and define a physician loss as any instance in which a county had one or more physicians in a given year and no physician in the following year, as well as cases in which such losses persisted in subsequent years. Because physician listings were near-universal, such losses are unlikely to reflect measurement error. Instead, they likely capture events such as physician migration, retirement, or death. We interpret these events as physician availability shocks, or simply physician loss. In the segregated context of the early 20th century, Black individuals were generally not served by white physicians and vice versa. As such, Black physician loss plausibly affected Black individuals disproportionately. This context creates a unique setting to study the research question. Consequently, we focus on the inventory of Black physicians and their impact on Black individuals. Notably, white individuals

[5] In Appendix Table A-1, we demonstrate that our results remain robust when using the 1940 county of residence as a proxy for individual's county of birth.

had more viable substitutes when white physicians exited the area, whereas Black individuals faced more constrained options due to both the limited number and geographic dispersion of Black physicians. This asymmetry helps mitigate concerns about treatment spillovers and enhances the validity of our identification strategy.

We merge the Black physician inventory with the Numident/DMF dataset using county and year of birth. We focus on year of birth to isolate the effects of in utero and early-life exposure to physician shocks, consistent with prior literature highlighting the lasting effects of early-life conditions on later-life outcomes. Event studies and placebo analyses confirm that the primary effects occur during early life, with minimal influence from exposure at later ages. We restrict the sample to birth cohorts from 1906 to 1929[6] and to counties that experienced a Black physician loss. Figure 1 illustrates the geographic distribution of these counties. Finally, to avoid double-counting and over-weighting individuals who appear in both the Numident and DMF, we remove duplicate records.

Summary Statistics – Summary statistics of the final sample are presented in Table 1, with separate panels for Black and white individuals. The final sample includes approximately 2.46 million observations, of which 8.4% are Black. The average age at death is around 78.6 and 79.1 years for Blacks and whites, respectively. The main treatment variable, "exposure," is a binary indicator equal to one if the individual was born after the county experienced a Black physician loss. The average exposure is 0.78 among Black individuals and 0.72 among white individuals. The share of females is approximately 39% for Black subsample and 35% for white subsample. The relatively low female representation reflects the fact that only the Numident data include both genders, while the DMF data (which we pool with Numident) cover male individuals only. In terms of education, 86% of Black individuals and 57% of white individuals in the sample have less than 12 years of schooling.

[6] The physician inventory data from the American Medical Directories begin in 1906, which determines the earliest cohort we can observe. We also limit the sample to cohorts born in 1929 or earlier to avoid confounding influences from the Great Depression. The economic downturn likely affected both local physician supply and demand, and it may also have directly influenced later-life outcomes, including mortality, for individuals exposed to the shock in utero or during early childhood (Noghanibehambari et al., 2024; Noghanibehambari & Fletcher, 2024b; Schmitz & Duque, 2022). Limiting the sample to cohorts born no later than 1929 helps isolate the effects of physician availability from these broader macroeconomic shocks.

4. Econometric Method

Our identification strategy exploits three sources of variation: spatial and temporal variation in Black physician loss across counties and years and the disproportionate impact of healthcare access shocks on Black individuals due to the segregated healthcare environment of the early 20th century (Brown, 2024; Byrd & Clayton, 2012; Collins & Thomasson, 2002). We implement this identification strategy using the following specification:

$$y_{icst} = \alpha_0 + \alpha_1 Black_i \times Exposure_{cst} + \alpha_2 Exposure_{cst} + \alpha_3 X_i + \gamma_{st} + \lambda_c + \varepsilon_{icst} \quad (1)$$

The outcome is age-at-death of individual i was born in county c in state s in year t. The variable $Exposure$ indicates being born in the county that experienced a black physician loss. In X, we include individual and family characteristics, including dummies for gender, race, ethnicity, dummies for paternal socioeconomic status, and dummies for maternal literacy. To account for all other state-level shocks that affect long-run outcomes, we include state-by-year fixed effects (represented by γ). Therefore, the variation comes from comparing the same cohorts born in the same state but in counties with differential exposure. To control for all unobserved time-invariant county characteristics, we include county fixed effects (represented by λ). Finally, ε is a disturbance term. Standard errors are clustered on county.

In these regressions, the coefficient α_1 measures the difference in longevity between Black individuals and non-Black individuals with an exposure of one versus zero. The coefficient α_2 captures the effect of exposure on non-Black individuals' longevity and serves as a placebo test. To further limit the influence of time-varying county-level confounders, our preferred specification includes birth county by birth year fixed effects. In these regressions, we compare the longevity of Black and non-Black individuals exposed to Black physician death who were born in the same county in the same year.

To complement this analysis, we implement event studies to observe the evolution of longevity by race across counties that experience a black physician loss, as follows:

$$y_{icst} = \alpha_0 + Black_i \times \sum_{j \neq -1} \Phi_j I(t - t^*_{cs} = j) + \sum_{j \neq -1} \Theta_j \times I(t - t^*_{cs} = j) + \alpha_1 X_i + \gamma_{st} + \lambda_c + \varepsilon_{icst} \quad (2)$$

In this specification, the function $I(.)$ is a unit indicator function that turns on if the inside argument is true. As in Equation 1, t indexes birth cohort, and t^* represents the county-specific year of a Black physician loss. The parameters Φ and Θ represent event-time coefficients for Black and non-Black individuals, respectively. These sets of coefficients show the evolution of effects across different years before and after a Black physician death and provide transparent and dynamic evidence for the timing and progression of these effects.

Since the staggered nature of this specification raises concerns regarding the endogeneity of ordinary least squares estimates, we employ the interaction-weighted estimator developed by Sun & Abraham (2021) to estimate these equations. Additionally, we show that the results are robust to alternative estimators, including De Chaisemartin & d'Haultfoeuille (2024), and Borusyak et al. (2024).

5. Results

5.1. Event Study

The event study results of Equation 2 are reported in Figure 2. The top and bottom panels display the evolution of effects among Black versus non-Black individuals, and the main effects among non-Black individuals, respectively. Among non-Black individuals, we do not observe a discernible or statistically significant set of coefficients in the years before or after the event. The point estimates are small in magnitude and statistically insignificant, as expected, given that the loss of a Black physician should not affect the longevity of non-Black (mostly white) individuals. In the top panel, the pre-event point estimates are similarly small and insignificant, suggesting limited concerns about pre-existing trends in longevity across counties. However, following the event, the longevity of Black individuals begins to decline. The post-event point estimates increase in magnitude and become statistically significant, indicating reductions in the longevity of Black individuals relative to white individuals after the loss of a Black physician.

5.2. Main Difference-in-Difference Results

The main results of Equation 1 are reported in Table 2. Column 1 presents regressions that include only county and birth year fixed effects along with baseline controls. Column 2 adds state-by-birth-year fixed effects. In Column 3, we include county-by-birth year fixed effects. The main effect of exposure remains small in magnitude and statistically insignificant across all

specifications, suggesting minimal effects among non-Black individuals. The difference-in-differences coefficient on the interaction term is fairly stable across specifications. The fully parameterized regression in Column 3 indicates that exposure to a Black physician loss in the year of birth is associated with a 4.2-month reduction in longevity later in life during old age.

5.3. Heterogeneity Analysis

Several studies suggest that early-life shocks have differential impacts on later-life outcomes depending on gender and socioeconomic status (Almond, 2006; Bleakley, 2007; Hoynes et al., 2016). This theoretical background motivates the examination of heterogeneous effects in our setting. To explore this, we replicate the main analysis separately for females, males, and subsamples defined by low and high maternal education, based on information from the 1940 census. The results are presented in Columns 1–4 of Table 3. We observe larger effects among males compared to females. The effects are also larger for those from families with lower maternal education.

The exposure variable captures any decline in the number of physicians in a county, regardless of the initial physician inventory. It is plausible that the loss of two physicians has a more detrimental impact than the loss of a single physician, as the former likely served a larger number of individuals. In Columns 5–6, we observe larger reductions in longevity for counties that experienced greater drops in physician numbers.

Another interesting source of heterogeneity arises from the variation in the share of Black residents across counties. Counties with a higher share of Black residents may experience the loss of a Black physician differently for two reasons. First, the presence of more treated individuals in these counties means that physician loss affects a larger population segment—individuals who otherwise could have accessed physician services. Second, stronger community support in counties with higher shares of Black residents may help substitute for lost healthcare access, potentially mitigating the adverse effects. In columns 7–8, we find that the effects are larger in counties with a higher share of Black residents.

Additionally, we explore heterogeneity by county population. This analysis is motivated by the notion that the impact of physician loss may differ across counties with varying population densities, for at least two reasons. First, more populous counties typically have a larger absolute number of residents per physician, so the loss of even a single provider may disrupt care for a

larger number of individuals, especially in underserved populations. Second, larger counties may differ in their baseline healthcare infrastructure, such as the availability of alternative providers, hospitals, or informal care networks, which could either amplify or buffer the effects of physician loss. The results, reported in columns 9–10, suggest slightly larger coefficients in more populous counties.

6. Robustness Checks

6.1. Contemporaneous Changes in County Characteristics

The first concern in interpreting the main results is that the shock to the physician inventory may reflect contemporaneous changes in a county's socioeconomic or sociodemographic landscape, and that the estimated effects may capture these underlying changes rather than the causal impact of physician loss. To address this concern, we empirically test for such associations using county characteristics from the 1900–1920 decennial censuses in regressions that include county fixed effects and state-by-year fixed effects. The exposure variable in these regressions is defined similarly—as a binary indicator for years following a physician loss. These results are reported in Table 4. In Column 1, we observe a significant reduction in the number of physicians, corresponding to a drop of roughly 69% relative to the mean, consistent with our main treatment definition. However, when we examine the associations with other county-level characteristics, the coefficients are generally small and statistically insignificant. For instance, although the coefficient on the share of Black residents is negative and significant at the 10% level, the point estimate suggests only a 0.6 percentage point change relative to the mean. More importantly, the coefficients on key measures of socioeconomic status, reported in Columns 8–9, are extremely small in magnitude, suggesting that our exposure variable is not systematically correlated with concurrent changes in counties' socioeconomic composition.

6.2. Balancing Tests

Another concern in interpreting the results is the possibility of endogenous changes in the composition of individuals in the final sample. For example, selective survival or differential fertility (i.e., changes in who becomes a parent following the event) could alter the characteristics of individuals observed in the death records. If such selection is correlated with health or longevity, it may bias our estimates. For instance, if the event leads to increased selection of white individuals or those from higher socioeconomic backgrounds (groups that tend to have higher longevity for

reasons not fully captured by our control variables) this would introduce a downward bias in the estimated effect (i.e., attenuate the true impact).

To investigate this concern, we regress a range of observable characteristics in the final sample on the exposure measure, conditional on county and state-by-year fixed effects. These results are reported in Table 5. Across a wide array of observable traits, we do not find significant or discernible coefficients. For instance, the coefficient of maternal literacy indicates a change of only 0.8% relative to the mean. Overall, the results in Table 5 suggest no systematic relationship between exposure and observable individual characteristics. This lack of association with observables provides support for the claim that there is likely no significant association with unobservable characteristics either (Altonji et al., 2005).

6.3. Alternative Specifications

We further examine the robustness of our findings to alternative specifications in Table 6. In Column 1, we replicate the main results from Column 3 of Table 2 to serve as a benchmark for comparison. In Column 2, we interact county fixed effects with dummies for individual race, gender, and parental characteristics. This comprehensive set of fixed effects allows for unobserved, time-invariant county characteristics to have differential impacts on longevity across subpopulations. For instance, exposure to place-based diseases such as malaria has been shown to affect racial groups differently, especially in the segregated environment of the early 20th century (Bleakley, 2010; Elman et al., 2019; Hong, 2013; Quinn et al., 2011). These fixed effects help account for such heterogeneity. The resulting coefficient is approximately 29% larger than in the benchmark, suggesting that accounting for these subgroup-specific fixed effects increases the estimated impact.

In Column 3, we include fixed effects for ZIP code of residence at the time of death to account for contemporaneous influences on mortality. These fixed effects are estimated using a considerably smaller subset of observations for which ZIP code information is available. The estimated coefficient decreases slightly in magnitude but remains comparable to the main result.

In Columns 4–8, we assess robustness to alternative functional forms. The coefficient on the log of age at death in Column 4 closely matches the percentage change relative to the mean outcome reported in Column 1. The coefficients on binary outcomes indicating survival beyond

ages 70 through 85, shown in Columns 5–8, reveal stronger effects at older ages. This pattern suggests a latent influence of early-life physician access on old-age survival.

In Column 9, we weight the regressions by the share of Black residents and find a point estimate that is comparable to the main results.

In Columns 10–14, we implement alternative estimators. Column 10 reports the coefficient from a standard ordinary least squares (OLS) regression. In Column 11, we estimate an Accelerated Failure Time (AFT) model with an exponential distribution, a method more suited for survival analysis (Aizer et al., 2016). The estimated coefficient is even larger than that from the log outcome specification in Column 4.

One concern in interpreting the results is the potential endogeneity in linking individuals between the death records and the 1940 Census. Specifically, if certain subpopulations (who may be more vulnerable to physician availability shocks) are over- or underrepresented in the linked data compared to the original population, this could introduce selection bias into our regressions. We address this concern empirically in two ways.

First, we directly examine the relationship between exposure and the likelihood of being linked. To do so, we link individuals in our final sample to the full-count 1940 Census records for those born in the same counties and years (i.e., original population). We then construct a dummy variable indicating successful linkage and regress this indicator on the exposure measure, controlling for the fixed effects included in Equation 1. The results are reported in Appendix Table A-2. We find that exposed Black individuals are somewhat less likely to appear in the linked data. Although the point estimate suggests a relatively modest change (roughly 10%), it is statistically significant. This finding is not unexpected, as exposure may increase the likelihood of earlier death (prior to the start of our death record window in 1975) thereby reducing the probability of appearing in the linked dataset.

Second, to evaluate whether the selection patterns observed in Appendix Table A-2 are problematic for our main estimates, we implement the Heckman (1979) two-step selection correction model. In the first stage, the probability of successful matching is calculated using observable characteristics, from which the inverse Mills ratio (IMR) is computed. In the second stage, we include the IMR as an additional control in the longevity regression to adjust for potential

selection bias. The resulting coefficient (Column 12, Table 6) remains comparable in magnitude to the baseline result reported in Column 1, suggesting that selection into the linked data does not meaningfully bias our main findings.

Finally, in Columns 13–14, we document that our results are robust to recently developed alternative difference-in-differences estimators, specifically those proposed by De Chaisemartin & d'Haultfoeuille (2024) and Borusyak et al. (2024).

6.4. Placebo Tests

To further complement the robustness analysis and validate the main findings, we implement a series of placebo tests designed to ensure that the results are not driven by spatial-temporal trends in health outcomes that may later be reflected in longevity. Specifically, we test whether the observed effects are unique to early-life exposure by assigning placebo exposure dates corresponding to physician loss events occurring at later ages during childhood and adolescence. This approach is motivated by theoretical and empirical evidence suggesting that early childhood is a particularly sensitive period for shaping long-term health and mortality outcomes, relative to later stages such as adolescence or early adulthood (Almond & Currie, 2011; Barker, 1994, 1995, 1997; Cunha & Heckman, 2007).

The results of these placebo tests are reported in Appendix Table A-3. In Panels A, B, and C, we define placebo exposure at ages 10, 15, and 20, respectively. Across all three specifications, we find no statistically significant or sizable associations between the placebo exposure measures and longevity. The point estimates are small in magnitude and statistically insignificant, further supporting the interpretation that the main results are indeed driven by early-life exposure to physician loss, and not by unrelated spatial-temporal factors.

6.5. Truncation of Death Window

The truncated nature of the Numident/DMF data may introduce bias if it systematically excludes individuals with shorter or longer lifespans due to differential censoring based on treatment status. For example, if exposed Black children were more likely to die at younger ages (prior to the beginning of death window in 1975) our estimates would underestimate the true effect of early-life physician loss. To assess this concern, we compare the main results with those obtained using the Berkeley Unified Numident Mortality Database (BUNMD), a dataset extracted from the CenSoc project (Goldstein et al., 2021). The key advantage of BUNMD is its inclusion

of death records prior to 1975, although the coverage for earlier deaths is limited. Another benefit is that BUNMD includes county of birth as reported in Social Security records. However, it does not include linkage to the 1940 Census, which prevents us from controlling for parental characteristics in the regressions.

The results are reported in Appendix Table A-4. To facilitate comparison, we first replicate the main results using our primary dataset with different death-year restrictions. Column 1 reports results for deaths occurring between 1975 and 2023. In Column 2, we restrict the death window to 1975–2007 to align with the end of the death coverage window in BUNMD. We observe a slight decline in the coefficient, suggesting that right truncation (missing later deaths) may introduce a modest downward bias in our estimates.

In Column 3, we report the BUNMD-based estimate for death years 1975–2007. The estimated effect is substantially larger, approximately 6.9 months of reduced longevity. When we expand the death window to include deaths from 1960–2007 in BUNMD, we observe a similar effect size of 6.7 months (Column 4). This similarity suggests that including earlier deaths does not significantly alter the estimated effect, indicating that left truncation is unlikely to introduce meaningful bias into our main regressions.

7. Mechanisms

In the main regressions, we intentionally avoid including individual-level characteristics (aside from predetermined attributes such as gender and race) since many of these variables may themselves be shaped by early-life exposure. Indeed, characteristics such as education and labor market outcomes are likely mediators in the pathway between early-life shocks and later-life longevity. Conditioning on such post-treatment variables could attenuate the estimated effects of early-life exposure. For example, both educational attainment and labor force participation are well-documented predictors of adult mortality (Chetty et al., 2016; Fletcher, 2015; Halpern-Manners et al., 2020; Lleras-Muney, 2005; Lleras-Muney et al., 2020). Therefore, we examine these intermediary outcomes as potential mechanisms through which early-life exposure to physician loss affects long-term mortality.

Using individual-level information from the 1940 Census, we examine impacts on education and labor market outcomes in Columns 1–4 of Table 7. While we do not find a statistically significant effect on years of schooling, we observe a significant increase in the

likelihood of having less than 12 years of education (Columns 1–2). Specifically, exposure is associated with a 9.3% increase in the probability of not completing high school. We also find evidence of reduced labor force participation (Column 3), and a substantial reduction in wage income, suggesting a 45% decline (Column 4).

Migration represents another plausible mechanism. By enabling access to improved environments and opportunities, migration may influence long-term health and mortality (Aaronson et al., 2021; Baran et al., 2024; Derenoncourt, 2022; Katz et al., 2001; Raj Chetty et al., 2016). In Column 5, we show that exposed individuals are less likely to migrate out of their birth state by 1940, suggesting that reduced geographic mobility may be one channel through which early-life shocks affect life-course outcomes. In Appendix Table A-5, we evaluate this hypothesis by examining heterogeneity in the main effects based on individuals' migration status. We find that the estimated effect is larger among non-migrant individuals compared to migrants, providing further support for the interpretation that migration may buffer the adverse effects of early-life physician loss.

Finally, The Numident/DMF data is linked to the World War II enlistment records, which include anthropometric indicators such as height and weight. These measures allow us to calculate Body Mass Index (BMI). These anthropometric outcomes are shown to correlate with health and longevity (Crimmins & Finch, 2006; Petursson et al., 2011; Sunder, 2005). Among linked individuals with non-missing anthropometric data, we assess the effect of early-life exposure on these physical health markers. Although we do not detect a significant effect on height (Column 6), we do observe an increase in the share of individuals classified as overweight (BMI > 24) in Column 7. This finding is consistent with the interpretation that early-life healthcare disruptions may adversely affect physical health trajectories.

8. Discussion on the Magnitude of the Effect

The results suggest a reduction of 4.1 months in the longevity of Black individuals exposed to early-life physician loss. We put this estimate into perspective in three ways. First, we compare our estimated effect with those of similar early-life exposures on later-life longevity in studies using comparable historical settings. For example, Hoehn-Velasco (2021) shows that early-life exposure to the establishment of county health departments in the rural South during the early 20th century is associated with an improvement in longevity of approximately 2.2 months. Similarly,

Fletcher and Noghanibehambari (2025) find that early-life exposure to the opening of birth control clinics across U.S. cities between 1920 and 1940 is associated with gains in longevity ranging from 1 to 5 months. Relative to these studies, our estimated effect lies at the upper end of the magnitude range, highlighting the significance of early access to healthcare providers.

Second, we extend the comparison to include other forms of early-life exposure. Aizer et al. (2016) examine the effects of the Mothers' Pension (MP) program—a targeted cash transfer to poor, unmarried mothers that lasts for up to three years and accounts for approximately 30% of pre-transfer income. They show that early exposure to the program increases children's later-life longevity by up to 11 months. Vu et al. (2023) analyze the effects of in utero exposure to lynchings of African Americans in the early 20th century and find a reduction in longevity of about 3 months. Noghanibehambari & Fletcher (2024b) examine the impact of in utero and early-life exposure to the Dust Bowl and document a reduction in old-age longevity of approximately 1 month. The fact that our estimated effect is comparable to those found in the context of large-scale early-life shocks (such as large-scale cash transfers, environmental disasters, or racially targeted violence) highlights the relevance of early-life physician access as a critical determinant of long-term health.

Third, we conduct a back-of-the-envelope calculation to monetize the value of life years lost due to early-life exposure to physician loss. Using the number of Black individuals in the final sample (206,664) and the point estimate from the main results (4.2), we estimate approximately 72,332 life years lost. Based on Value of Statistical Life (VSL) of $10 million (Kniesner & Viscusi, 2019) and assuming an average lifespan of 79 years (Table 1), the implied value of one year of life is approximately $126,582. Applying this valuation, the total life years lost due to early-life exposure to physician loss in our sample is equivalent to approximately $9.16 billion. This figure highlights the substantial long-term societal cost of disruptions in early-life access to medical care.

9. Conclusion

Recent research increasingly highlights the importance of in utero and early-life conditions in shaping outcomes in later life, particularly regarding health and mortality in old age. This body of work emphasizes that disparities in longevity today may be partially rooted in exposures and experiences during early developmental periods. This research is particularly relevant in the context of a rapidly aging U.S. population and the projected growth of the elderly demographic in the coming decades (Mather & Scommegna, 2024; Rice & Fineman, 2004). Our study builds on

this growing literature by presenting new evidence on the lasting impact of early-life disruptions in physician availability on long-term mortality outcomes.

This study provided new evidence on the long-term consequences of early-life physician availability on old-age mortality among Black Americans in the early 20th century US. Using Social Security Administration death records linked to the 1940 Census, we examined the effects of early-life exposure to the loss of Black physicians at the county level. Our identification strategy exploited spatial and temporal variation in physician losses and relied on a difference-in-differences framework comparing outcomes for Black and non-Black individuals. Given the racially segregated nature of the medical system during this period, we assumed that shocks to Black physician availability would have minimal, if any, effect on non-Black populations. We found that early-life exposure was associated with a 4.2-month reduction in longevity. Our estimates proved robust to a wide range of specifications, including alternative estimators, placebo exposure timings, and additional controls. We further provided empirical evidence to rule out concerns related to changes in county-level characteristics, sample composition, and selection bias from linkage procedures. Additionally, we showed that the effects were larger among males, individuals from lower socioeconomic backgrounds, non-migrants, and those born in more populous counties with a higher share of Black residents. Finally, we provided evidence that deteriorations in educational attainment, income, labor force participation, and anthropometric outcomes during early adulthood likely served as mediating pathways.

Life expectancy at age 55 in the U.S. increased from 17.69 years to 18.5 years for birth cohorts between 1900 and 1920—an overall gain of approximately 9.7 months. Thus, our estimated effect of early-life exposure to physician availability shock accounts for roughly 43% of the total increase in life expectancy at age 55 across these cohorts.

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Tables

Table 1 - Summary Statistics

	Blacks		Whites	
	Mean	**SD**	**Mean**	**SD**
Death age (month)	942.877	96.81	948.977	97.581
Birth year	1915.47	4.83	1915.063	4.749
Death year	1994.051	7.64	1994.155	7.994
Year of physician death/retirement in the county of birth	1910.582	4.549	1911.186	4.29
Exposure	.778	.416	.722	.448
Female	.389	.488	.35	.477
Hispanic	.002	.05	.012	.11
Father SEI below median	.515	.5	.396	.489
Father SEI above median	.059	.235	.376	.484
Father sei missing	.426	.495	.228	.419
Mother literate	.463	.499	.721	.448
Mother literacy missing	.43	.495	.226	.418
Years of schooling	6.761	3.449	10.125	3.032
Education < 12 years	.856	.352	.572	.495
In labor force	.675	.468	.69	.463
Log wage income	.219	9.532	1.05	9.316
Height	68.319	2.627	68.378	2.629
Overweight	.126	.332	.168	.374
County characteristics				
Share of females	.496	.02	.487	.024
Share of under-5 children	.585	.144	.487	.154
Share of whites	.594	.235	.904	.148
Share of blacks	.404	.235	.094	.147
Share of other races	.002	.011	.003	.011
Share of Hispanics	.004	.021	.007	.034
Share of first-generation immigrants	.044	.099	.198	.171
Share of second-generation immigrants	.047	.089	.188	.142
Share of white-collar workers	.02	.008	.029	.01
Share of farmers	.241	.117	.115	.127
Share of literate	.827	.099	.929	.066
Share of married	.608	.029	.595	.039
Average socioeconomic index	19.393	5.201	26.108	6.167
Average occupational income score	17.699	3.532	22.434	3.941
Share of homeowners	.42	.137	.442	.139
Share of population in the labor force	.567	.064	.552	.049
Observations	206,664		2,250,648	

Table 2 - Effect of Early-Life Exposure to Black Physician Loss in County of Birth on Later-Life Old-Age Longevity

	Outcome: Age-at-Death (Months)		
	(1)	(2)	(3)
Black × Exposure	-4.41***	-4.38***	-4.18***
	(.63)	(.59)	(.59)
Exposure	.11	-.19	
	(.34)	(.28)	
Observations	2457305	2457305	2457305
R-squared	.19	.19	.2
Mean DV	948.464	948.464	948.464
Birth-County FE	✓	✓	✓
Birth-Year FE	✓	✓	✓
Controls	✓	✓	✓
Birth-State-by-Birth-Year FE		✓	
Birth-County-by-Birth-Year FE			✓

Notes. Standard errors, clustered on county, are in parentheses. Controls include individual, family, and county-level covariates. Individual controls include dummies for gender, race, and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies. County controls include the share of females, under-5 children, whites, Blacks, other races, Hispanics, first-generation immigrants, second-generation immigrants, white-collar workers, farmers, literate individuals, and married individuals, as well as average socioeconomic index, average occupational income score, share of homeowners, and share of the population in the labor force. Regressions cover birth cohorts of 1906 – 1918 who died between the years 1975 – 2023.

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

Table 3 - Heterogeneity Analysis

	Outcome: Age-at-Death (Months), Subsamples:				
	Females	Males	Mother Education < High School	Mother Education ≥ High School	Number of Physicians Pre-Loss Bottom-Quartile
	(1)	(2)	(3)	(4)	(5)
Black × Exposure	-3.16***	-4.21***	-4.76***	-3.71***	-6.18***
	(.66)	(.8)	(1.3)	(.65)	(1.47)
Observations	868571	1588670	904579	1552492	620582
R-squared	.39	.09	.17	.19	.17
Mean DV	982.483	929.863	928.434	960.131	950.731
	Number of Physicians Pre-Loss Top-Three Quartiles	County Share of Blacks Top-Three Quartiles	County Share of Blacks Bottom-Quartile	County Population Above Median	County Population Below Median
	(6)	(7)	(8)	(9)	(10)
Black × Exposure	-4.03***	-4.25***	-2.48	-6.53***	-4.12***
	(.64)	(.61)	(3.46)	(1.67)	(.61)
Observations	1836723	1842729	614576	1041566	1415739
R-squared	.2	.2	.17	.17	.21
Mean DV	947.698	946.757	953.582	948.293	948.590

Notes. Standard errors, clustered on county, are in parentheses. Regressions include individual/family controls and birth-county-by-birth-year fixed effects. Individual controls include dummies for gender, race, and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies Regressions cover birth cohorts of 1906 – 1918 who died between the years 1975 – 2023.
**** p<0.01, ** p<0.05, * p<0.1*

Table 4 - Association Between Black Physician Loss and Changes in County-Level Characteristics

	Outcomes:						
	Number of black physicians	Share under-5 children	Share of females	Share of whites	Share of Blacks	Share of Hispanics	Share of immigrants
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exposure	-.2914***	.00037	-.00007	-.00136*	.00133*	.00041**	.00121*
	(.06939)	(.00143)	(.0004)	(.00079)	(.00076)	(.0002)	(.00068)
Observations	17857	17857	17857	17857	17857	17857	17857
R-squared	.42185	.98287	.934	.99663	.99718	.99204	.99688
Mean DV	0.680	0.576	0.488	0.772	0.225	0.007	0.123
	Socioeconomic index	Occupational income score	Share of literate	Share of married	Share of homeowners	Share white-collar occupation	Share of farmers
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Exposure	-.03252	-.0463*	-.00046	.00015	-.00103	-.0001	.0012
	(.0303)	(.02564)	(.00071)	(.0005)	(.00129)	(.00011)	(.00105)
Observations	17857	17857	17857	17857	17857	17857	17857
R-squared	.98913	.98488	.98348	.94187	.98222	.96494	.97742
Mean DV	20.657	18.554	0.886	0.615	0.513	0.022	0.232

Notes. Standard errors, clustered on county, are in parentheses. Regressions include county fixed effects and state by year fixed effects. The data covers the years 1910 – 1940.
**** p<0.01, ** p<0.05, * p<0.1*

Table 5 - Association Between Black Physician Loss and Changes in Sociodemographic Composition of the Final Sample

	Outcomes:				
	Female	Black	White	Other Races	Hispanic
	(1)	(2)	(3)	(4)	(5)
Exposure	.00006	.001	-.00088	-.00011	-.00027
	(.00185)	(.00088)	(.00089)	(.00012)	(.0003)
Observations	2457308	2457308	2457308	2457308	2457308
R-squared	.01593	.24134	.235	.03513	.19915
Mean DV	0.353	0.084	0.913	0.003	0.011
	Father Socioeconomic Status Below Median	Father Socioeconomic Status Above Median	Father Socioeconomic Status Missing	Mother Literate	Mother Literacy Missing
	(6)	(7)	(8)	(9)	(10)
Exposure	.00205	.00318	-.00523	.0053	-.00358
	(.00236)	(.00225)	(.0036)	(.0033)	(.00322)
Observations	2457308	2457308	2457308	2457308	2457308
R-squared	.14574	.14499	.04501	.04539	.03415
Mean DV	0.406	0.349	0.244	0.699	0.243

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

Table 6 - Robustness to Alternative Specifications

	Outcome: Age-at-Death (Months), Specifications:						
	Main Results	Adding County by Individual/Family Covariates FE	Adding ZIP Code of Death FE	Outcome: Log Age at Death	Outcome: Age at Death > 70	Outcome: Age at Death > 75	Outcome: Age at Death > 80
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black × Exposure	-4.185***	-5.435***	-3.21***	-.004***	-.004	-.021***	-.023***
	(.586)	(.639)	(.667)	(.001)	(.002)	(.003)	(.003)
Observations	2457305	2457286	1052874	2457305	2457305	2457305	2457305
R-squared	.196	.197	.427	.195	.102	.119	.121
Mean DV	948.464	948.464	975.860	6.849	0.832	0.663	0.442
	Outcome: Age at Death > 85	Weighting by Share of Blacks	OLS Regressions	Accelerated Failure Time (AFT) Model	Two-Stage Heckman Model	De Chaisemartin & D'haultfoeuille (2020) Estimator	(Borusyak et al. (2024) Estimator
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Black × Exposure	-.014***	-4.51***	-6.15***	-.006***	-3.89***	-3.65**	-7***
	(.003)	(.67)	(.59)	(.001)	(.64)	(1.69)	(2.69)
Observations	2457305	2450109	2457305	2457305	9786252	2457305	2457305
R-squared	.12	.22	.19	---	---	---	---
Mean DV	0.202	947.535	948.464	948.464	948.464	948.464	948.464

Notes. Standard errors, clustered on county, are in parentheses. Regressions include individual/family controls and birth-county-by-birth-year fixed effects. Individual controls include dummies for gender, race, and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies Regressions cover birth cohorts of 1906 – 1918 who died between the years 1975 – 2023.

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

Table 7 - Mechanism Channels

	Outcomes:						
	Years of Schooling	Education < 12 Years	In Labor Force	Log Wage Income	Cross-State Migrant	Height	Overweight
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black × Exposure	-.041	.055***	-.015***	-.445***	-.026***	-.104	.036***
	(.07)	(.007)	(.003)	(.079)	(.01)	(.087)	(.012)
Observations	2410427	2457305	2457305	2457305	2457305	222992	222609
R-squared	.285	.163	.364	.162	.094	.103	.069
Mean DV	9.844	0.596	0.688	0.980	0.239	68.374	0.166

Notes. Standard errors, clustered on county, are in parentheses. Regressions include individual/family controls and birth-county-by-birth-year fixed effects. Individual controls include dummies for gender, race, and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies Regressions cover birth cohorts of 1906 – 1918 who died between the years 1975 – 2023.
**** p<0.01, ** p<0.05, * p<0.1*

Figures

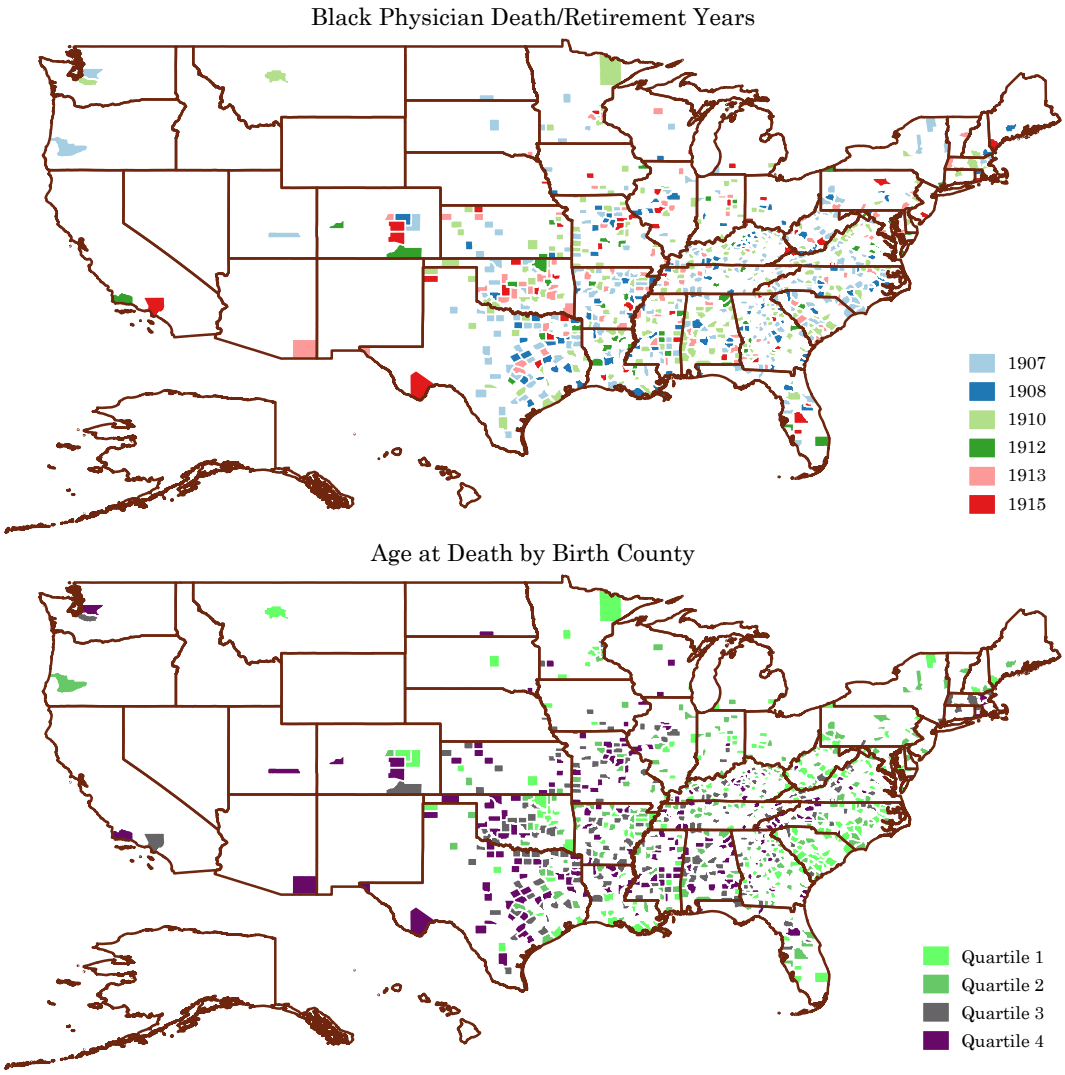


Figure 1 - Geographic Distribution of Counties in the Final Sample

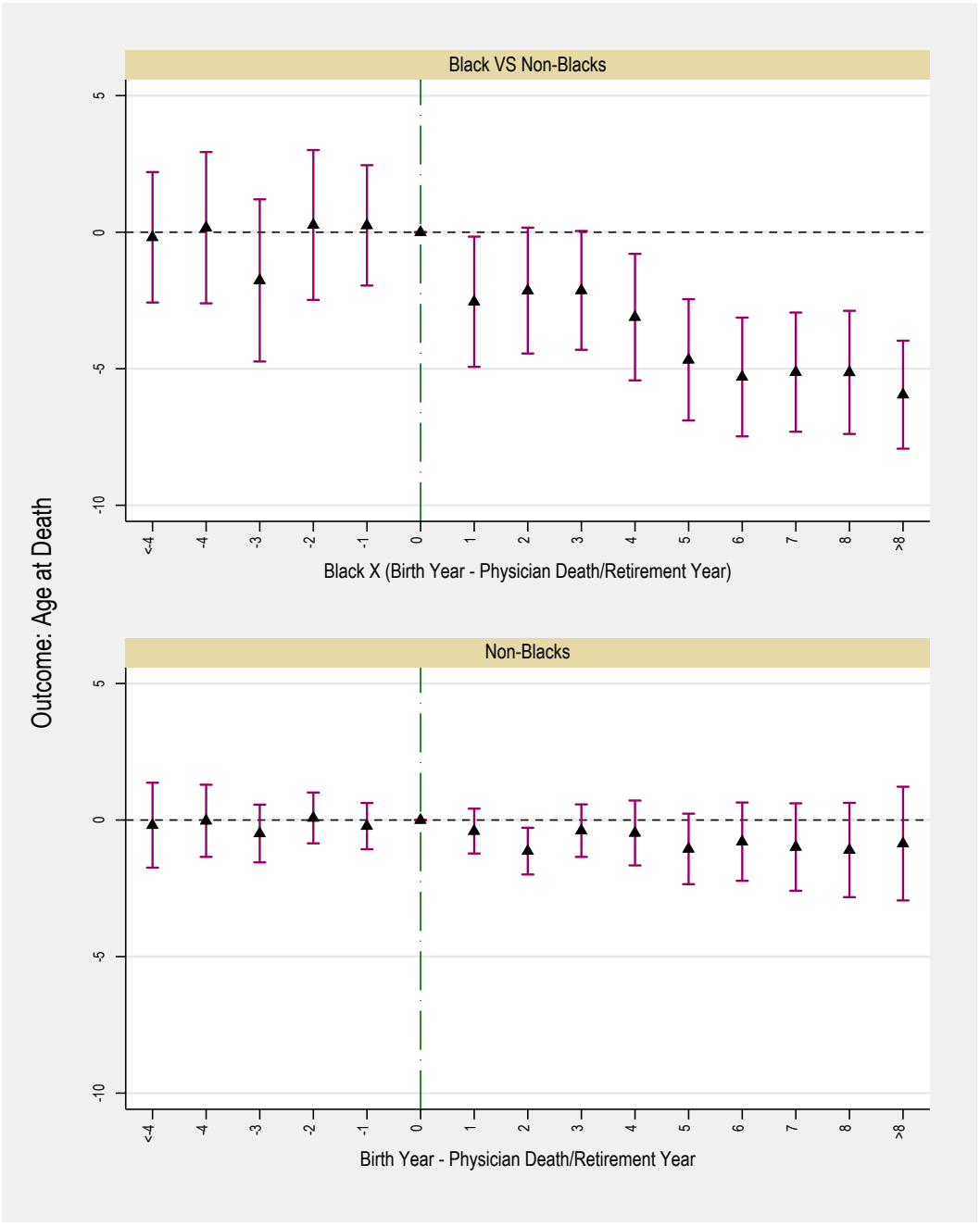


Figure 2 - Event Study Analysis to Examine the Effects of Black Physician Loss on Later Life Longevity of Black Children

Notes. Point estimates and 95% confidence intervals are reported. Standard errors are clustered on birth county. Regressions include individual/family controls and birth-county-by-birth-year fixed effects. Individual controls include dummies for gender, race, and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies Regressions cover birth cohorts of 1906 – 1918 who died between the years 1975 – 2023.

Appendix A

Appendix Table A-1 - Replicating the Main Results Using County of Residence in 1940 Census as a Proxy for County of Birth

	Outcome: Age-at-Death (Months)		
	(1)	(2)	(3)
Black × Exposure	-5.81***	-4.33***	-4.93***
	(.72)	(.65)	(.74)
Exposure	.21	.02	
	(.47)	(.26)	
Observations	2451331	2451293	2451328
R-squared	.19	.19	.19
Mean DV	948.915	948.917	948.915
Birth-SEA FE	✓	✓	✓
Birth-Year FE	✓	✓	✓
Controls	✓	✓	✓
Birth-State-by-Birth-Year FE		✓	
Birth-SEA-by-Birth-Year FE			✓

Notes. Standard errors, clustered on county, are in parentheses. Controls include individual, family, and county-level covariates. Individual controls include dummies for gender, race, and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies. County controls include the share of females, under-5 children, whites, Blacks, other races, Hispanics, first-generation immigrants, second-generation immigrants, white-collar workers, farmers, literate individuals, and married individuals, as well as average socioeconomic index, average occupational income score, share of homeowners, and share of the population in the labor force. Regressions cover birth cohorts of 1906 – 1918 who died between the years 1975 – 2023.

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

Appendix Table A-2 - Association Between Black Physician Loss and Endogenous Merging between Death Records and 1940 Census

	Outcome: Successful Merging Between SSA Death Records and 1940 Census	
	(1)	(2)
Black × Exposure	-.0143***	-.0158***
	(.0015)	(.0017)
Exposure	.002***	
	(.0007)	
Observations	9786252	9786252
R-squared	.0567	.0577
Mean DV	0.154	0.154
Birth-County FE	✓	✓
Birth-Year FE	✓	✓
Controls	✓	✓
Birth-State-by-Birth-Year FE	✓	
Birth-County-by-Birth-Year FE		✓

Notes. Standard errors, clustered on county, are in parentheses. Controls include individual, family, and county-level covariates. Individual controls include dummies for gender, race, and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies. County controls include the share of females, under-5 children, whites, Blacks, other races, Hispanics, first-generation immigrants, second-generation immigrants, white-collar workers, farmers, literate individuals, and married individuals, as well as average socioeconomic index, average occupational income score, share of homeowners, and share of the population in the labor force. Regressions cover birth cohorts of 1906 – 1918.

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

Appendix Table A-3 – Placebo Tests: Exposure Assignment at Later Years of Childhood/Early Adulthood

	Outcome: Age-at-Death (Months)	
	(1)	(2)
Panel A. Assignment of exposure at age 10		
Black × Exposure	-.23	-.38
	(.79)	(.82)
Exposure	-.09	
	(.34)	
Observations	1323477	1323446
R-squared	.29	.29
Mean DV	1001.697	1001.696
Panel B. Assignment of exposure at age 15		
Black × Exposure	.21	.96
	(.97)	(1.06)
Exposure	-.45	
	(.37)	
Observations	570047	569773
R-squared	.41	.42
Mean DV	1051.613	1051.588
Panel C. Assignment of exposure at age 20		
Black × Exposure	-.1	.3
	(1.34)	(1.58)
Exposure	.26	
	(.46)	
Observations	307916	306766
R-squared	.45	.47
Mean DV	1073.922	1073.686
Birth-County FE	✓	✓
Birth-Year FE	✓	✓
Controls	✓	✓
Birth-State-by-Birth-Year FE	✓	
Birth-County-by-Birth-Year FE		✓

Notes. Standard errors, clustered on county, are in parentheses. Controls include individual, family, and county-level covariates. Individual controls include dummies for gender, race, and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies. County controls include the share of females, under-5 children, whites, Blacks, other races, Hispanics, first-generation immigrants, second-generation immigrants, white-collar workers, farmers, literate individuals, and married individuals, as well as average socioeconomic index, average occupational income score, share of homeowners, and share of the population in the labor force. Regressions cover birth cohorts of 1896-1908, 1891-1903, and 1886-1898 for panels A, B, and C, respectively.

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

Appendix Table A-4 - Examining Truncation Bias Using Berkeley Unified Numident Mortality Database (BUNMD) Data

	Outcome: Age-at-Death (Months), Data and Sample:			
	Numident/DMF, Death Years 1975 – 2023	Numident/DMF, Death Years 1975 – 2007	BUNMD, Death Years 1975 – 2007	BUNMD, Death Years 1960 – 2007
	(1)	(2)	(3)	(4)
Black × Exposure	-4.23***	-3.84***	-4.82***	-4.7***
	(.59)	(.57)	(.55)	(.56)
Observations	2457305	2421210	3851660	3867105
R-squared	.2	.23	.26	.25
Mean DV	948.464	946.153	960.562	959.380

Notes. Standard errors, clustered on county, are in parentheses. Regressions include individual controls and birth-county-by-birth-year fixed effects. Individual controls include dummies for gender, race, and ethnicity. Regressions cover birth cohorts of 1906 – 1918 who died between the years 1975 – 2023.
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table A-5 - Heterogeneity by Migration Status between Birth County and County of Residence in 1940

	Outcome: Age-at-Death (Months)			
	Cross-County Migrants (between Birth and 1940)	Non-Migrants (between Birth and 1940)	Cross-County Migrants (between 1940 and Death)	Cross-County Migrants (between 1940 and Death)
	(1)	(2)		
Black × Exposure	-3.26***	-4.8***	-3.14***	-3.53***
	(.8)	(.82)	(.88)	(1.19)
Observations	981913	1475329	650825	396198
R-squared	.21	.19	.4	.39
Mean DV	956.419	943.169	976.403	975.946
Birth-County FE	✓	✓	✓	✓
Birth-Year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Birth-State-by-Birth-Year FE	✓	✓	✓	✓
Birth-County-by-Birth-Year FE	✓	✓	✓	✓

Notes. Standard errors, clustered on county, are in parentheses. Regressions include individual/family controls and birth-county-by-birth-year fixed effects. Individual controls include dummies for gender, race, and ethnicity. Family controls include dummies for paternal socioeconomic status and maternal literacy dummies Regressions cover birth cohorts of 1906 – 1918 who died between the years 1975 – 2023.
**** p<0.01, ** p<0.05, * p<0.1*