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EARLY-LIFE LOCAL LABOR MARKET CONDITIONS AND OLD-AGE MALE MORTALITY: EVIDENCE FROM HISTORICAL DEINDUSTRIALIZATION OF THE NEW ENGLAND TEXTILE SECTOR

Hamid Noghanibehambari
Jason Fletcher

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ABSTRACT

Previous studies document the potential links between early-life insults and life-cycle outcomes. However, fewer studies examine the effects of local labor market shocks during early-life on old-age male mortality. This article empirically investigates this link using a large-scale deindustrialization as a source of shocks to local labor markets: the decline in the New England's textile industry during the 1920s and 1930s. Consistent with prior studies, we find small impacts on migration and changes in sociodemographic composition of counties post-deindustrialization. Using Social Security Administration death records linked with historical censuses 1900-1940 and difference-in-difference event studies, we find reductions in longevity for those born in highly-exposed counties whose families are categorized as non-migrants and those residing in non-urban areas. The results suggest intent-to-treat effects of about 3.3 months while the treatment-on-treated calculations suggest reductions of about 4 years in longevity of children of affected families. Using 1950-1960 census data, we find that those born in highly-exposed counties post-deindustrialization reveal large reductions in schooling, decreases in high school completion, and significant decreases in measures of socioeconomic standing. We further discuss the policy implication of these findings.

Hamid Noghanibehambari
Austin Peay State University
Marion St
Clarksville, TN 37040-5164
noghanih@apsu.edu

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

1. Introduction

A growing body of empirical studies examine the sources of health disparities at old age and provide evidence that exposures during in-utero, early-life, and early childhood have significant influence in explaining the variations in old-age health (Almond et al., 2018; Almond & Currie, 2011; Barker, 1994, 1995, 1997). Disruptions during the critical period of health capital development and skill formation may change the trajectory of individuals across the life cycle (Cunha & Heckman, 2007). The path dependent nature of health and human capital formation, in the absence of policy interventions, may appear in adverse outcomes during old age.

One important contributor to the health endowment during early life is parental job prospects and local labor market conditions. Parental income and general economic conditions provide a set of health-related inputs, such as better health insurance and healthcare affordability[4], and non-health-related inputs, such as improved nutrition and housing access (Rosenzweig & Schultz, 1983). Empirical studies link local labor market conditions and business cycles to infants' and children's health. In the long-run, those who experienced adverse economic conditions during early life reveal reductions in several measures of health and human capital during adulthood and old age (De Cao et al., 2022; Lindo, 2011; Nikolova & Nikolaev, 2021; Regmi & J. Henderson, 2019; Scholte et al., 2015; Van Den Berg et al., 2010). Specifically, a narrow literature links economic conditions in early life and old age mortality and longevity (Lindeboom et al., 2010; Schmitz & Duque, 2022; Van Den Berg et al., 2006, 2009, 2011, 2015).

This paper joins this literature by evaluating the effects of local labor market conditions during early life on old-age longevity. Specifically, we focus on a large-scale deindustrialization that took place in the textile sector in New England region during the early decades of the 20th century. Textile manufacturing was a highly prosperous sector in New England during the 19th and early 20th century, covering about 15 percent of the total labor force by 1920. However, starting from 1920, the industry faced large contractions in size. From 1920 to 1940, the industry shed about 7 percentage-points of its labor force. Several factors contributed to the overall decline in the region's textile industry, including outdated machinery of factories combined with a slower

[4] Although the literature to examine this channel in early decades of the 20th century is limited, there is evidence that healthcare access was an important Contributor to health outcomes during this period. Hoehn-Velasco (2018) examines the effects of county health departments on mortality rates during the period of 1908-1933. She finds advantages in increases in access to child-oriented health services through the opening of county health departments for infant mortality rates.

pace of adaptation, increased foreign competition and outsourcing from European countries with relatively cheaper labor, changes in demand size and preferences post World War I, and specifically increased competition with southern states who had more abundant raw materials and cheaper labor.

We exploit variations in timing and location of this deindustrialization to examine the effects of being born under adverse economic conditions on old-age longevity. In so doing, we employ death records data from Social Security Administration linked with the full-count 1940 census. We use cross-census linking rules to establish a linking between historical censuses 1900-1930 and 1940 in order to infer individuals' county-of-birth. This information is essential to evaluate the effects of local conditions. We implement two-way fixed effect models and event studies that compares outcomes of individuals born in different pre-and-post deindustrialization years in localities with higher versus lower initial reliance on textile industries in their labor force. We find null effects among the general population. However, we find relatively larger effects among people living in non-urban areas and those who do not migrate from their county-of-birth in years leading to 1940. Among these non-urban non-migrants of high exposure counties, we find a reduction of about 3.3 months in longevity. We provide empirical evidence that the exposure to deindustrialization does not result in changes in population composition based on several observable individual and family characteristics. Moreover, we find considerably larger impacts among those with low-educated fathers and low socioeconomic status families. Further, we use 1950 and 1960 censuses to evaluate potential mechanisms. We find that individuals born in higher exposure counties reveal significant reductions in education and their measures of occupational standing and occupational education score.

The current study is motivated by and adds to two strands of empirical research on the health effects of economic shocks. First, since the New England textile market fall was partly driven by cross-state and international import competition, this study relates to recent line of research that examine the health impacts of trade liberalization and increases in import competition. For instance, Pierce & Schott (2020) show that localities in the US with higher exposure to trade liberalization with China experienced increases in adult mortality rate. Fan et al. (2020) use data from China and show that trade expansions post-2000 caused increased working hours and reduced self-reported health measures among working age population. However, Feng et al. (2021) show that export expansion resulted in increases in earnings among Chinese workers and improved their general health. Fernández Guerrico (2021) employs data from Mexico and show that localities with higher reductions in manufacturing employment after trade liberalization with China experienced increased mortality rates and obesity. Lang et al. (2019) show that individuals in localities with higher exposure to import completion from China reveal reductions in mental and physical health. Noghanibehambari (2023) exploits the variations in local exposure to the North American Free Trade Agreement (NAFTA) to examine the effects of local labor market conditions on birth outcomes in the US. He finds significant negative impacts on a wide array of infants' health. Olper et al. (2018) employs cross-country data over the years 1960-2010 and show that trade liberalization reduced child mortality rates. Our study joins this literature and evaluates long-term impacts of such trade-induced shocks on longevity and mortality outcomes.

Second, the current study also relates to the limited empirical evidence of early-life economic conditions and later-life old-age longevity, specifically in the case of the US. Van Den Berg et al. (2006) examine this association using historical data in Netherlands using national business cycle as a proxy for local economic conditions. They find that being born during an economic boom (versus a recession) is associated with about 1.6 years longer lives. Lindeboom et al. (2010) show that cohorts born during the potato famine of the mid-19th century in Netherlands lived between 2.5-4 years shorter lives during adulthood. Cutler et al. (2007) examine the effects of early-life exposure to the Dust Bowl and the Great Depression on later-life health using census-region-level macroeconomic data as a proxy for local economic conditions. They find no significant effects on a wide range of health outcomes including mortality. Schmitz & Duque (2022) explore the effects of local unemployment during the Great Depression on later-life outcomes using birth-state variations in wage index as a proxy for general local economic conditions. They employ Health and Retirement Study (HRS) and examine the effects on epigenetic aging signature, a predictor of mortality risks. They find significant associations that are localized for in-utero exposures as opposed to pre-conception and childhood exposures. Noghanibehambari et al. (2022) use historical county-level bank deposits data to proxy for local economic conditions. They employ Social Security death records linked with the full-count 1940 census and show significant association between per capita deposits at the county-year-of-birth and later-life longevity.

Our paper contributes to the literature in three ways. First, although the decline in New England's textile industry was partly caused by a post-war decline in labor demand, cross-state competition and import competition were also important factors. Therefore, our paper can also contribute to the recently developed and ongoing literature and policy debates regarding the effects of international trade competition (Autor et al., 2019; Autor et al., 2013; Batistich & Bond, 2023; Hakobyan & McLaren, 2016, 2017; Pierce & Schott, 2020). This literature usually finds that individuals residing in localities with higher reliance in industries that are affected by trade policy changes or labor demand shocks reveal reductions in job prospects, earnings, and experience adverse health outcomes. Our paper adds to this literature and documents a heterogenous impact on long-run mortality for a deindustrialization that is partly trade-induced. In addition, there were fewer social insurance during the period of study to insulate the negative effects of worsening economic conditions (Modrek et al., 2022; Noghanibehambari & Engelman, 2022). Although limited small-scaled unemployment insurance existed specifically for the New Deal period of post-1930, they were not designed or targeted based on industry-specific job losses. Second, we provide evidence of a large-scale deindustrialization with differential impacts among localities which lends to the exogeneity assumption of our method. This is in contrast with previous works that directly test for the associations using regional-level, state-level, or county-level proxies for economic conditions (Cutler et al., 2007; Noghanibehambari et al., 2022; Schmitz & Duque, 2022). Our event study design allows for a flexible cross-cohort change in the effects which are detached from any time-varying proxies that are arguably contaminated with other determinants of health and later-life longevity. Moreover, we show that the effects are driven by specific subpopulations, i.e., non-urban and non-migrant families. This fact suggests that barriers to migration and cross-occupation movements might play a role, which should be the target of policies to alleviate the long-run consequences of deindustrialization. Third, we also contribute to the literature on early-life exposures and later-life outcomes, specifically mortality (Aizer et al., 2016; Almond, 2006; J. M. Fletcher, 2018; J. M. Fletcher et al., 2010; Hayward & Gorman, 2004; Montez & Hayward, 2011, 2014). We focus on longevity for two reasons. First, although it is an extreme measure of old age health, it is an accurate measure which alleviates the concerns over other subjective measures. Second, it is a summary measure of general health and is correlated with a wide range of other individual measures, including human capital and economic success (Buchman et al., 2012; Chetty et al., 2016; Lubitz et al., 2003; Mathers et al., 2001).

2. Historical Background

New England's abundant water ways and rivers provided unique geographic advantages to exploit hydraulic power necessary to run early textile mills in the late 18th and early 19th century. Therefore, it became a center for textile production in the US. During the 19th century, the industry was successful in incorporating new technological advancements, such as the spinning jenny and power loom to expand production. The primary focus of the region was producing coarse textiles, such as cotton, wool, and flax.

This hub of industrial powerhouses started to contract, specifically during the 1920s and 1930s. the New England states (i.e., Rhode Island, Maine, Connecticut, Massachusetts, New Hampshire, and Vermont) experienced large-scale plant closures in textile industries (Choi, 2022). Between 1920 to 1940, textile industry of the region reduced about 7 percentage-points employment of the labor force, off an initial baseline of 15 percent.

Several factors contributed to this large-scale deindustrialization. First, expansions in manufacturing industries in countries with cheaper labor, such as Germany and Japan, during the early 20th century and specifically after World War I increased import competition. Second, southern US states started expansions in the textile industry during this period. Availability of raw materials, e.g., cotton, and lower costs of labor combined with better climate for cotton production resulted in more competitive prices relative to productions from New England (Koistinen, 2002). Third, during this period, there was a change in consumer preference towards lighter and more fashionable products, such as synthetic fibers, while New England textile focused on more coarse products, such as wool (Rosenbloom, 1999). Fourth, in the face of demand reductions and import competition, factories started to lower the costs which reflected in labor benefits. Labor unions started labor strikes to raise the pay. This labor unrest contributed to the general production decline in the industry (Greenlees, 2019).

Manufacturers in New England textile industries started political lobbying for "retrenchment" policies (to cut local and state taxes to lower production costs) and "federal interventions" acts (to absorb federal aids to save the region's industry). Both set of actions resulted in limited effects (Koistinen, 2000). Although several firms could relocate to southern states to continue production, many others went bankrupt and were shut down (Koistinen, 2002). However, there is evidence that other manufacturing sectors and jobs in public sectors grew in size which mitigated the negative effects on local economies (Rosenbloom, 1999). Choi (2022) uses historical census data and show that individuals in highly affected towns in New England states were less likely to migrate, switched to agricultural sector, and faced decreases in their occupational scores.

3. Empirical Method

Our empirical method rests on the assumption that individuals in localities with higher versus lower exposure reveal similar trends in the outcomes in the absence of industrial decline. Later in the paper, we employ a series of event-studies and balancing tests to lend to the exogeneity assumption of our method. The econometric method employs a difference-in-difference strategy that compares longevity of cohorts who are born in counties at different terciles of 1900 textile share of labor force (first difference) and born after 1920 (as the starting year of deindustrialization) versus those born before (second difference). Specifically, we utilize this comparison using the following ordinary least square regressions:

$$y_{icb} = \alpha_0 + \alpha_1 Q^3_{c,1900} Post_b + \alpha_2 Q^2_{c,1900} Post_b + \alpha_3 X_i + \alpha_4 Z_{cb} + \zeta_c + \xi_b + \varepsilon_{icb} \quad (1)$$

Where the outcome is age-at-death of individual i who was born in county c and year b. Dummy variables Q^3 and Q^2 indicate whether birth-county c is at the third and second tercile of share of textile employment in the labor force in 1900, respectively. In X, we include individual and family controls. Individual controls include dummies for race and ethnicity. Family controls include dummies for maternal education, paternal literacy, and paternal socioeconomic index. The matrix Z, includes a series of county covariates that are extracted from the full-count censuses 1880-1940 and interpolated for inter-decennial years. These county controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score. County fixed effects (ζ) absorbs all time-invariant county features that influence later-life health and mortality. Birth cohort fixed effects (ξ) absorb secular changes in cohorts' health and life expectancy that evolves over time. Finally, ε is a disturbance term. We cluster standard errors by birth-county and birth-year to account for both spatial and serial correlation in error terms.

To show the effects across different birth cohorts, we employ event-studies of the following form:

$$y_{icb} = \alpha_0 + \sum_{k=\underline{T}}^{1918} \beta_k I(b = k) \times Q^3_{c,1900} + \sum_{j=1921}^{\overline{T}} \theta_j I(b = k) \times Q^3_{c,1900} + \alpha_1 X_i + \alpha_2 Z_{cb} + \zeta_c + \xi_b + \varepsilon_{icb} \quad (2)$$

The set of coefficients β_k represents pre-trend coefficients which capture the difference in longevity of individuals in high exposure counties in years prior to 1920s. Since the main results point to significant and large effects for the top-tercile counties, we define high exposure as counties in top-terciles of 1900 textile employment share in the labor force (Q^3). The parameters θ_j capture the effects on longevity of individuals in high exposure counties in years after 1920. We group event-time coefficients into two-year increments, removing the coefficients of 1919-1920 cohorts to set them as the reference group. All other parameters are as in equation 1.

4. Data and Sample Selection

We employ Social Security Administration Death Master Files (DMF) and Numerical Identification (Numident) records as the primary source of data extracted from the CenSoc Project (Goldstein et al., 2021). The DMF data reports deaths to male individuals between the years 1975-2005 and Numident data reports deaths that occurred between 1988-2005 to both males and females. There are three advantages in using DMF-Numident data. First, the CenSoc-extracts of DMF-Numident provides links at the individual level to the full-count 1940 census.[5] It allows us to observe individual and family characteristics as observed in 1940. Second, we can use the cross-census linking rules to locate individuals in historical censuses and deduce their county-of-birth, an extremely scarce identifier. Third, the DMF-Numident death sample contains an initial sample of millions of observations which leaves room to have restrictions to specific cohorts and geographic regions and still have enough observations with sufficient statistical power. These aspects of the data make it superior to many alternative datasets to study mortality outcomes, such as National Longitudinal Mortality Study or Health and Retirement Study.[6]

[5] The linkage is based on "ABE fully automated" method based on first name, last name, and age (Abramitzky et al., 2012, 2014, 2017).
[6] The HRS data also provides links to 1940 census for about 9K observations. Restricting that linked HRS data to cohorts and states of the final sample of this paper leaves less than a few hundred observations in the final sample, while the DMF-Numident results in about 170K individuals.

The 1940 census and other full-count historical censuses are extracted from IPUMS Project (Ruggles et al., 2020). We use cross-census linking rules provided by the Census Linking Project (Abramitzky et al., 2020).[7] This cross-census linking is essential for two reasons. First, since the focus is local county-level economic conditions, we need to have a reliable proxy for county-of-birth. Second, the industrial decline, as we will argue later, increased migration, specifically for non-urban populations. Therefore, we also need to build measures of migration from birth-county to the 1940 county. To deduce the birth-county, we split the sample into 11-year cohort bins. We then use the first census they could appear as their "original census". For instance, for cohorts of 1900-1910, the original census is 1910 census. We link 1940 census to 1910 census and use the county-of-residence in 1910 as county-of-birth. This procedure goes on until 1930. For cohort of 1930-1935, we use "county-of-residence in 1935" information as reported in the 1940 census. For cohorts of 1936-1940, we use county-of-residence in 1940 as county-of-birth.

Since linking between historical censuses is based on names and women change their names over time, the linking rules are for males only. Therefore, the main sample restriction is that we focus on male mortality in this paper. We restrict the sample to cohorts of 1900-1940. Moreover, as the industrial decline was restricted to New England, we focus on individuals who were original born in New England states (Rhode Island, Maine, Connecticut, Massachusetts, New Hampshire, and Vermont). Figure 1 shows the geographic distribution of counties in the final sample based on their 1900 share of textile employment in the labor force.

Summary statistics of the final sample are reported in Table 1. The average age-at-death in the first, second, and third tercile of textile production is 75.2, 75.6, and 75.5 years, respectively. While the majority of the final sample consists of whites, there are slightly higher share of whites and lower share of blacks in high textile counties. There are fewer migrants and higher share of urban residents in high textile counties.

Parental literacy dummies and father socioeconomic status dummies are based on the family characteristics in the original census of observation.[8] Parental literacy is fairly similar across different textile-exposure counties. The dummy variables regarding maternal education are extracted from family characteristics in 1940 census as 1940 was the first census to report

[7] We use ABE exact names methods as the linking rule (Abramitzky et al., 2021).
[8] For cohorts born between 1930-1940, these variables are based on 1940 census.

educational outcomes. Therefore, they are more representative of more recent cohorts since earlier cohorts are more likely to have moved out of their original households. We observe slightly higher share of low-educated mothers in top terciles of textile counties. Finally, based on county-level covariates, higher baseline textile employment counties have slightly higher occupational income scores.

5. Results

5.1. County-Level Employment

Before starting with the main results, we explore changes across years to industry-specific employment using a county-by-decennial year panel data extracted from full-count decennial years 1880-1940. In so doing, we implement event studies that include county and census-year fixed effects in which the outcome is the share of employment in a specific industry as a share of total county labor force. We interact the census-years with top-tercile of 1900 textile share of labor force, similar to equation 2, to explore the effects on high-exposed counties in different years relative to low-exposed counties. The results are depicted in Figure 2, Figure 3, and Figure 4. We standardize outcomes (with respect to the mean and standard deviation of the full sample) to facilitate cross-figure comparisons. The top-left panel of Figure 2 suggests no pre-trend in the share of textile in high versus low exposure counties. For post-1920 census-years, we observe significant and large drops in the share of textile employment. For instance, share of 1940 textile employment is about 1 standard deviation lower than that of 1920. The top-right panel implies an overall increase in the share of agriculture, forestry, and fisheries employment, although none of the coefficients are statistically significant. We do not observe a clear pattern of change in the mining sector (bottom-left panel of Figure 2). We observe overall reductions in construction sector prior to 1920, although it reached back to the initial levels by 1940 (bottom-right panel of Figure 2).

Moreover, we do not observe a clear pattern of change for other non-textile manufacturing industries (top panels of Figure 3). Transportation sector reveals slightly higher share by 1940, though the effects of 1890-1910 and 1930 are almost identical to those of 1920 (bottom-left panel of Figure 3). The employment in utility sector seems to be fairly stable as a share of labor force in pre and post years (bottom-right panel of Figure 3). Wholesale trade, retail trade, finance, insurance, and real estate, and public administration sectors also do not reveal a discernible pattern of change (top panels and bottom-right panel of Figure 4). For jobs related to private and public sector, we observe an overall increasing trend, although both pre-trend and post-trend coefficients are statistically insignificant.

The overall pattern of employment in other sectors do not point to a contemporaneous change in sharp growth or drop in other industries. However, to the extent that sectors such as services and agriculture grow post-1920, the negative effects of worsening economic conditions and plant closures are potentially mitigated and hence our results underestimate the true impacts.

5.2. Effects on Individual Migration

We start our analysis by examining the effects on the likelihood of individuals' migration. We use the full sample and equation 1 in which the outcome of interest is a dummy indicating if the person' birth-county is different than county-of-residence in 1940. The results are reported in column 1 of Table 2. While both coefficients are positive, they are statistically insignificant and small relative to the outcome mean. However, we observe significant impacts for the non-urban population. Those born in the third and second terciles of 1900 textile are 1.5 and 1.8 percentage-points more likely to migrate, off a mean of 0.58. Among urban populations, we observe small and insignificant effects of exposure on migration.

5.3. Effects on Longevity

The main results of equation 1 are reported in column 1 of Table 3. We observe negative but small and insignificant coefficients. Among non-migrants, the coefficients considerably rise in magnitude although remain statistically insignificant. Among migrants, the top-tercile exposure coefficient becomes positive, suggesting some benefits for longevity. This is in line with several studies that suggest improvements in outcomes for those who migrate during times of economic difficulties (Chyn, 2018; Derenoncourt, 2022; Ludwig et al., 2013). In columns 4 and 5, we examine the heterogenous impacts among nonurban (i.e., rural) versus urban-born individuals. We find a sharp rise in the negative effects among non-urban-born people. The third-tercile county effect is now statistically significant, suggesting an average of two months reduction in longevity.

Columns 2 and 4 suggest much larger impacts among non-urban non-migrant people. In column 6, we focus on this subsample. We find that those born in top-tercile of 1900 textile counties reveal 3.3 months reductions in longevity. The effects on those at the second tercile is close to zero and statistically insignificant.

We complement this table by examining the effects across birth cohorts using event-study estimates of equation 2. The results are depicted in four panels of Figure 5. Among the four subpopulations, we do not observe a significant and robust pattern of pre-trend. We observe drops in post-trend coefficients among non-migrants and those born in rural areas (bottom panels).

In Figure 6, we focus on the subsample of non-urban non-migrants and replicate the event studies. From 1900-1920, there is no significant evidence of a difference in longevity for those in high exposure versus low exposure counties. However, post-trend coefficients reveal a sharp drop among those born in high exposure counties relative to those in low exposure counties. The effects last until 1940 and there is no sign of revival.

The fact that we observe negative and significant effects of high exposed counties among non-migrants and non-urban residents suggests that the negative impacts are primarily concentrated among people and places with fewer alternative job prospects. However, we should note that these effects are intent-to-treat which measure the potential impacts among all people in the data. Therefore, we expect to observe larger effects as we focus on the subpopulations who are more likely to be affected. For instance, there is evidence that low educated individuals are less likely to switch occupations (Sicherman & Galor, 1990). There is also evidence of heterogenous impacts of early-life shocks on later-life outcomes by parental education and family socioeconomic status (Almond et al., 2018; Currie, 2009). We examine these heterogeneities in Table 4. In column 1 and 2, we show the effects among children with illiterate and literate fathers, respectively. We find about 10.7 and 4.6 months reductions in longevity for the illiterate-father subsample in third and second tercile counties, respectively. Although these estimates are statistically insignificant due to very small sample size, they point to much larger impacts relative to literate-fathers subsample.

Further, we observe a reduction of about 4.9 months in longevity of those with low-socioeconomic index fathers in top-tercile counties while this effect is 1.9 months for those with high-socioeconomic index fathers (column 3 versus 4). In column 5 and 6, we ignore occupational mobility post-deindustrialization and explore the effects among those whose fathers' occupation is in textile and non-textile industries, respectively. We infer father's occupation using historical censuses 1900-1930. However, for those observations were not matched to historical censuses, we use father's occupation (if available) in 1940. We observe considerably larger effects among those with fathers working in textile sector, though the small sample sizes limit statistical power.

6. Robustness Checks

6.1. Balancing Tests

One concern in interpreting the results is the endogenous survival of individuals into death records which changes the composition of the final sample based on characteristics that are correlated with their early-life exposure to the deindustrialization. For instance, if there are more individuals with higher educated parents in the final sample who are also more exposed to the industry decline, then our results probably underestimate the true effects as parental education is positively associated with later-life longevity (Huebener, 2019; Noghanibehambari & Fletcher, 2022). This endogenous selection cannot simply be captured by including parental education controls as there are also unobservable factors associated with the influence of education on health. We can empirically test this issue by regressing a series of observable characteristics on the exposure measures introduced in equation 1, conditional on county and birth-year fixed effects. The results of this exercise are reported in Table 5. We do not observe any significant associations with the likelihood of being white, black, other races, and Hispanic. We observe statistical association between father being literate and second tercile of exposure (0.7% relative to the outcome mean) but not for the third tercile (0.09% relative to the outcome mean) (column 5).

We also observe small but statistical associations with the likelihood of mother being literate. However, there are three reasons that these estimates are not concerning. First, the implied change with respect to the outcome means are very small. Second, there is no clear pattern of effects consistent across outcomes. Third, to the extent that maternal education increases longevity, these results suggest underestimations in negative deindustrialization-longevity associations. Finally, since we do not observe a consistent and robust pattern of associations with observable characteristics, we can rule out the correlations with unobservable features (Altonji et al., 2005; J. Fletcher et al., 2021).

6.2. Endogenous Merging

Another concern in interpreting the main results is the selection into the final sample based on Numident-DMF links to the census and cross-census linking rules. The data linking could be endogenous if it is correlated with other determinants of longevity. For instance, nonwhites, immigrants, and people of lower socioeconomic status (who have, on average, lower longevity) are usually underrepresented in the linked samples due to higher errors in enumerations. If the successful linking is correlated with our exposure measures, then the estimates partly reflect this endogenous compositional change. We empirically investigate this concern by examining the associations between successful merging and the exposure measures as in equation 1. In so doing, we use the original male cohorts of 1900-1940 in New England who are categorized as non-urban non-migrants using information in 1940.[9] We then merge this sample with the final sample of non-migrant non-urban individuals and generate a dummy variable indicating successful merging. We then regress this dummy variable on the right-hand side variables of equation 1. The results are reported in Table 6. We observe small and insignificant associations between exposure measures and successful merging in the full sample (column 1), sample of low-educated mothers (column 2), and non-homeowners as a proxy for wealth (column 3). These tests rule out the concerns of endogenous merging across censuses and Numident-DMF death records.

6.3. Alternative Specifications

In Table 7, we further examine the robustness of the results to alternative specifications and functional forms. For comparison, we report the results of column 6 of Table 3 in the first column. In column 2, we add birth-month and death-month fixed effects to account for the influence of seasonality of birth and death month in determining longevity (Seretakis et al., 1997; Vaiserman, 2021). In column 3, we interact county fixed effects with individual race dummies and parental education and socioeconomic status dummies to allow for time-invariant effects of counties vary for different subpopulations. In both columns, we observe quite robust estimates.

In columns 4 and 5, we replace the outcome with log of age at death. Column 4 replicates the ordinary least square and column 5 impalements an Accelerated Failure Time (AFT) model to examine the robustness (Aizer et al., 2016). We observe almost identical coefficients in both columns. High exposure is associated with 0.38 percent reduction in longevity. Using the mean age-at-death of top-tercile counties from Table 1, this effect is equivalent to about 3.4 months which is very similar to that of column 1. In columns 6-7, we replace the outcome with dummy variables indicating survival beyond 65 and 75 years. We observe similar pattern as column 1.

[9] Since this sample is not linked to historical censuses (as we are evaluating the endogenous merging between censuses as well as Numident-DMF data), we use county-of-1940 and county-of-1935 to detect non-migrants. We also use county-of-1940 as a proxy for county-of-birth.

Individuals born in high exposed counties are 1.2 and 1.9 percentage-points less likely to reach ages 65 and 75, off a mean of 0.85 and 0.48, respective.

Our sample covers various cohorts with very different life expectancy observed in a limited death window. One concern about this selection is over/under-representation of different cohorts in death records. The CenSoc-extracts of Numident-DMF provides a weighting variable which employs data from Human Mortality Database to correct for this differential representation of cohorts. We use this weight in our regressions and replicate the results in column 8. We observe quite comparable effects to those of column 1.

In column 9, we show that standard errors are quite robust when we employ county-level clustering. Another concern in our analysis is the overlap of the Great Depression with a portion of our sample. Several studies document the potential influence of the Great Depression and the introduction of social programs under the New Deal on short-run and long-run health outcomes (Cutler et al., 2007; Fishback et al., 2007; Modrek et al., 2022; Noghanibehambari & Engelman, 2022; Schmitz & Duque, 2022). To examine the sensitivity of the results, we remove cohorts of 1930s and replicate the results. We find almost identical coefficients compared to column 1.

7. Mechanisms

Education and general socioeconomic status are among the potential mediators between early-life shocks and old-age outcomes (Almond et al., 2018; Currie, 2009). Further, several studies point to the potential benefits of education and income for longevity (Chetty et al., 2016; J. M. Fletcher, 2015; Lleras-Muney, 2005; Meghir et al., 2018; Salm, 2011). The census provides information on education and various measures of socioeconomic standing, which we could use to explore mechanism channels between early-life shocks and later-life longevity. However, many of post-1920 cohorts have not finished their education or not in the labor market by 1940. Therefore, we move to later censuses to examine these potential impacts. One drawback is that the public-use censuses of 1950-onwards do not report county. However, IPUMS extracts de-identify county based on other available geographic variables. We use 1950 and 1960 censuses and the limited de-identified counties to examine the mechanisms.[10] we limit the sample to individuals aged at least 22 and whose state-of-birth is the same as state-of-residence in the census to mitigate

[10] While the final sample of the main analyses has 67 counties, the sample of 1950-1960 census contains 30 identified counties.

the influence of migrants. We implement regressions similar to equation 1 and report the results in Table 8. Among top-tercile of exposure counties, we observe significant increases in years of schooling. We do not observe an effect among those at the second tercile, a pattern that is consistent with the results of longevity in Table 3. We also observe significant increases in the likelihood of being low educated (columns 2-3) and reductions in having a college education (column 4). We also observe significant and relatively large reductions in socioeconomic measures. High exposure (top-tercile of 1900 textile) is associated with 2.3 and 2 units reductions in socioeconomic index and occupational educational score, off a mean of 37.1 and 19.4, respectively.

We can compare these estimates with other studies that examine determinants of mortality to understand what portion of effects could pass through these channels. For instance, Halpern-Manners et al. (2020) employs Numident data, implements twin fixed effect strategy, and documents that an additional year of schooling is associated with about 4 months higher age at death. Combining this estimate with column 1 of Table 8, one can deduce a longevity reduction of 1.2 months through decreases in education, which is about 38 percent of the reduced-form of Table 3.

8. Discussion

The results suggest that high exposure to the deindustrialization have a long-lasting effect among non-migrants and individuals born in rural areas. The estimated effects reveal an intent-to-treat (ITT) effects of about 3.3 months. To understand the economic magnitude of this effect, we compare it with other studies of mortality and other early-life shocks. Fletcher & Noghanibehambari (2021) examine the impacts of college openings on college education and longevity using Numident data. They find treatment-on-treated (TOT) effects of 1 year of additional life for college-educated individuals. Therefore, high exposure to the industrial decline in early-life can offset about 28 percent of benefits of college education for longevity. Aizer et al. (2016) examine the long-term effects of a social program in the early 20th century on old-age mortality. They study Mothers Pension (MP) program which was designed to pay cash benefits to poor single mothers for a period of three years. The payments accounted for about 30-40 percent of pre-transfer maternal income. They show that children of mothers whose applications were accepted for MP live about 12 months additional lives. Therefore, our ITT effects are about 28 percent of the TOT benefits of a relatively large cash transfers which lasted three years. Chetty et al. (2016) examine the income-longevity relationship using individual-level tax returns and mortality database over the years 1999-2014 in the US. They find that each additional income percentile (about $8K change from the mean in 2020 dollars) is associated with about 1.9 months higher longevity. Therefore, early-life exposure to the severe deindustrialization of textile industry is equivalent to the longevity effects of a reduction of about $14K in household income.

Although we find considerable heterogeneity in Table 4, these estimates are still ITT effects. One way to convert them into TOT effects is to use the first-stage effects of exposure on textile employment using a similar regression to equation 1 and a county-census-year data. We find that high-exposed counties (top-tercile of 1900 textile) reveal about 7 percentage-points drop in textile employment. Using the ITT effect of 3.3 and deflating by the first-stage effect of 0.07, a back-of-an-envelope calculation suggests a TOT impact of about 4 years among non-urban non-migrants whose fathers lost their job due to deindustrialization.

Life expectancy in the US increased from about 48.2 years in 1900 to 62.1 in 1940. Our results suggest that longevity reductions due to a high exposure to the New England textile decline is equivalent to about 2 percent of overall life expectancy difference across cohorts in the final sample.

Using the original 1940 census, non-migrant cohorts in high exposure counties born after 1920 count to about 101.6K individuals. Assigning the ITT effects of column 6 of Table 3, one can calculate roughly 335K life-years lost due to early-life exposure to deindustrialization.

9. Conclusion

Changes in local labor market conditions may have spillover impacts on short-run and long-run health outcomes. The studies that examine the link between economic conditions and health report mixed evidence which differ by the outcome, subpopulation, and setting. While health outcomes of adults, and specifically working age populations, usually deteriorate during economic booms (Ruhm, 2000, 2015, 2016), infants' and children's health outcomes, on the other hand, reveal a procyclical behavior (Baird et al., 2011; Page et al., 2019; Waldmann, 1992). One important and policy-relevant question is that to what extent these negative impacts last. A strand of literature examine this question for a wide range of life-cycle outcomes (Almond et al., 2018; Almond & Currie, 2011). However, little is known about the link between early-life economic shocks and old-age longevity. This article explores this question using a large-scale deindustrialization case: the decline in the New England textile industry during the 1920s and 1930s.

We show that worsening demand and increasing competition resulted in large reductions in textile employment in New England post-1920, specifically for counties with higher initial reliance on textile. The effects on migration is small and mainly concentrated among non-urban individuals, a finding consistent with previous studies (Choi, 2022). We then implement event-studies and difference-in-difference regressions to compare old-age longevity of individuals born in counties with higher versus lower exposure to the deindustrialization after 1920s versus before. We find reductions in longevity of about 3.3 months for exposed cohorts who reside in non-urban areas and whose family did not migrate from their county-of-birth until 1940. We also find substantial heterogeneity in the effects with the largest impacts among illiterate fathers, low socioeconomic status fathers, and those whose fathers' reported occupation is in textile industry. Further, we show that during early adulthood, exposed individuals have lower educational attainment and significantly lower measures of socioeconomic standing.

We should note that this study relates to a period with limited social insurance programs. Although one should exercise caution in interpreting the results for other setting, it has the advantage to help us better isolate the effects of economic shocks not confounded by health benefits of social programs. The fact that we find long-lasting effects for early-life economic shocks suggests the possibility that policies that focus on families with infants and children could have high returns. Furthermore, we find that the negative effects are concentrated among non-urban population, those with lower alternatives in the labor market, and non-migrants. Therefore, our results point to benefits of contexts and environments that facilitate migration and occupational mobility during demand shocks, for instance, increased foreign competition after trade policy changes.

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Tables

Table 1 - Summary Statistics

	First Tercile of 1900 Textile		Second Tercile of 1900 Textile		Third Tercile of 1900 Textile	
	Mean	SD	Mean	SD	Mean	SD
Death Age (Month)	905.68181	117.88879	907.73021	116.69032	907.27198	117.36972
Birth Year	1919.0186	10.34837	1918.9376	10.17406	1918.7877	10.31607
Death Year	1994.4964	7.78776	1994.5857	7.74135	1994.3993	7.82917
White	.98822	.1079	.99133	.09268	.99315	.08246
Black	.01022	.1006	.00827	.09054	.00652	.08051
Other	.00156	.03943	.0004	.01997	.00032	.01795
Hispanic	.00399	.06303	.00329	.0573	.00399	.06306
Non-migrant	.3464	.47582	.39958	.48981	.39586	.48903
Nonurban	.4254	.4944	.47721	.49948	.3443	.47514
Father Literate	.9415	.2347	.9479	.22223	.94284	.23214
Father Literate Missing	.00905	.0947	.00329	.05725	.00502	.07067
Mother Literate	.93679	.24334	.93035	.25455	.92688	.26033
Mother Literate Missing	.00923	.0956	.003	.05465	.00497	.07031
Father Socioeconomic Index 1st Quartile	.34311	.47475	.2593	.43825	.22893	.42014
Father Socioeconomic Index 2nd Quartile	.14307	.35015	.18153	.38546	.21743	.4125
Father Socioeconomic Index 3rd Quartile	.18104	.38506	.19833	.39874	.19025	.3925
Father Socioeconomic Index 4th Quartile	.19134	.39336	.21299	.40942	.21235	.40897
Father Socioeconomic Index Missing	.14144	.34847	.14784	.35494	.15104	.35808
Mother Education < High School	.35576	.47875	.39306	.48843	.41523	.49276
Mother Education High School	.22364	.41668	.21046	.40764	.19203	.3939
Mother Education College	.02784	.16453	.03304	.17874	.02965	.16961
Mother Education Missing	.39275	.48836	.36344	.48099	.36309	.48089
County Covariates:						
Share of Textile Employment	.01765	.00913	.06438	.02895	.22282	.09981
Population	429592.82	384490.42	221672.01	143734.75	400790.01	249857.15
Share of Population Aged 0-4	.09751	.01634	.09512	.01175	.09522	.00999
Share of Population Aged 5-10	.11168	.01555	.10991	.00797	.11036	.00663
Share of Population Aged 11-18	.13843	.01545	.13638	.00799	.13952	.00649
Share of Population Aged 19-25	.1206	.01421	.12008	.01121	.12338	.01025
Share of Population Aged 26-55	.40244	.04114	.40836	.01535	.40734	.01339
Share of Population Aged 56-more	.12934	.04088	.13015	.03038	.12419	.0219
Share of Females	.49774	.01353	.49904	.01141	.51054	.00734
Share of Families with Children < 5	.38841	.09987	.37063	.04895	.37475	.04257
Share of Whites	.98574	.0124	.98769	.0085	.9903	.00557
Share of Blacks	.01251	.01109	.01162	.00859	.00909	.00537
Share of Other Races	.00175	.00288	.0007	.00078	.0006	.00036
Share of Hispanics	.00223	.00347	.00186	.00108	.00133	.00086
Share of First-Generation Immigrants	.23641	.10572	.22403	.0672	.27073	.05217
Share of Second-Generation Immigrants	.30603	.1143	.31094	.08633	.358	.05815
Share of Literate People	.89357	.17677	.89913	.17063	.88953	.16778
Share of Married	.55713	.05121	.57543	.02589	.55684	.01853
Average Family Size	4.25489	.38962	4.15709	.18664	4.34468	.15894
Average Occupation Income Score	24.36417	3.3335	25.72637	2.04917	26.34033	1.37115
Observations	177233		320824		524208	

Table 2 - The Effects of Exposure to Deindustrialization on Migration

	Outcome: Migrant, Subsamples:		
	Full Sample	Non-Urban	Urban
	(1)	(2)	(3)
3rd Tercile of 1900 Textile ×	.01799***	.01266*	.00951*
$I(Birth\ Year > 1920)$	(.00514)	(.00758)	(.0055)
2nd Tercile of 1900 Textile ×	.01439***	.0175***	.00215
$I(Birth\ Year > 1920)$	(.00496)	(.00668)	(.0058)
Observations	1022265	408981	613284
R-squared	.11025	.13386	.11021
Mean DV	0.612	0.592	0.625
County FE	✓	✓	✓
Birth Year FE	✓	✓	✓
Controls	✓	✓	✓

Notes. Standard errors, two-way clustered on county and birth-year, are in parentheses. Controls include individual, family, and county covariates. Individual controls include dummies for race and ethnicity. Family controls include dummies for maternal education, paternal literacy, and paternal socioeconomic index. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.
*** p<0.01, ** p<0.05, * p<0.1

Table 3 - The Effects of Deindustrialization in Local Labor Market at Birth Year on Old-Age Longevity

	Outcome: Age at Death (Months), Subsamples:					
	Full-Sample	Non-Migrants	Migrants	Non-Urban	Urban	Non-Urban Non-Migrants
	(1)	(2)	(3)	(4)	(5)	(6)
3rd Tercile of 1900 Textile	-.02926	-1.43081	1.05161	-2.00512*	1.12322	-3.59698**
$\times I(Birth\ Year > 1920)$	(.65271)	(.98809)	(.83755)	(1.04887)	(.87567)	(1.68271)
2nd Tercile of 1900 Textile	-.30775	-.50661	-.15384	-1.19346	.15026	-.40289
$\times I(Birth\ Year > 1920)$	(.64206)	(1.01084)	(.82613)	(.98802)	(.8968)	(1.60949)
Observations	1022265	397100	625165	408981	613284	166920
R-squared	.5111	.4519	.54255	.52113	.50437	.46301
Mean DV	907.140	906.997	907.231	905.316	908.357	905.200
County FE	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes. Standard errors, two-way clustered on county and birth-year, are in parentheses. Controls include individual, family, and county covariates. Individual controls include dummies for race and ethnicity. Family controls include dummies for maternal education, paternal literacy, and paternal socioeconomic index. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.
*** p<0.01, ** p<0.05, * p<0.1

Table 4 - Exploring Heterogeneity across Subsamples of Non-Migrant Non-Urban Population

	Outcome: Age at Death (Months), Subsamples:					
	Father Illiterate	Father Literate	Father Low Socioeconomic Index	Father High Socioeconomic Index	Father's Occupation: Textile	Father's Occupation: Non-Textile
	(1)	(2)	(3)	(4)	(5)	(6)
3rd Tercile of 1900 Textile × $I(Birth\ Year > 1920)$	-14.06235*	-3.32726*	-6.0959***	.06516	-11.19208*	-3.56146*
	(8.25994)	(1.72854)	(2.23069)	(2.59174)	(6.39344)	(1.81847)
2nd Tercile of 1900 Textile × $I(Birth\ Year > 1920)$	-12.46773	-.0377	-1.81441	1.99001	-8.15832	-.07772
	(8.52252)	(1.6372)	(2.03129)	(2.49611)	(6.26562)	(1.71795)
Observations	9791	157126	85170	81749	21255	145662
R-squared	.39071	.46796	.48588	.43434	.37434	.47489
Mean DV	904.619	905.234	910.752	899.412	902.914	905.529
County FE	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes. Standard errors, two-way clustered on county and birth-year, are in parentheses. Controls include individual, family, and county covariates. Individual controls include dummies for race and ethnicity. Family controls include dummies for maternal education, paternal literacy, and paternal socioeconomic index. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.
*** p<0.01, ** p<0.05, * p<0.1

Table 5 - Balancing Tests of Non-Urban Non-Migrant Sample

	Outcomes:							
	White	Black	Other	Hispanic	Father Literate	Father Literate Missing	Mother Literate	Mother Literate Missing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
3rd Tercile of 1900 Textile ×	.00161	-.00086	-.00075	.00095	.00125	.00654***	.01594***	.00549***
$I(Birth\ Year > 1920)$	(.00147)	(.00071)	(.00132)	(.00093)	(.00441)	(.0015)	(.004)	(.00149)
2nd Tercile of 1900 Textile ×	-.00076	-.00069	.00145	.0001	.00529	.00138	.01675***	.00104
$I(Birth\ Year > 1920)$	(.00153)	(.00071)	(.00138)	(.00096)	(.00413)	(.00089)	(.00382)	(.00093)
Observations	166920	166920	166920	166920	166920	166920	166920	166920
R-squared	.02733	.02328	.02354	.00485	.03028	.01719	.03078	.0158
Mean DV	0.994	0.001	0.005	0.003	0.941	0.004	0.934	0.004
County FE	✓	✓	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes. Standard errors, two-way clustered on county and birth-year, are in parentheses. Controls include individual, family, and county covariates. Individual controls include dummies for race and ethnicity. Family controls include dummies for maternal education, paternal literacy, and paternal socioeconomic index. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.
*** p<0.01, ** p<0.05, * p<0.1

Table 6 - Exploring the Association between Exposure to Deindustrialization and Census-Final-Sample Successful Merging

	Outcome: Successful Merging between Original Cohorts of 1940 Census and Final Sample, Subsamples:		
	Full Sample	Mother Education < High School	Family non-Homeowner
	(1)	(2)	(3)
3rd Tercile of 1900 Textile ×	.00877	-.00653	.01111
$I(Birth\ Year > 1920)$	(.00975)	(.00899)	(.01365)
2nd Tercile of 1900 Textile ×	.00308	.0023	.00036
$I(Birth\ Year > 1920)$	(.00747)	(.00886)	(.01089)
Observations	535274	190914	153845
R-squared	.09226	.04262	.07681
Mean DV	0.247	0.316	0.243
County FE	✓	✓	✓
Birth Year FE	✓	✓	✓
Controls	✓	✓	✓

Notes. Standard errors, two-way clustered on county and birth-year, are in parentheses. Controls include individual, family, and county covariates. Individual controls include dummies for race and ethnicity. Family controls include dummies for maternal education, paternal literacy, and paternal socioeconomic index. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.
*** p<0.01, ** p<0.05, * p<0.1

Table 7 - Robustness Checks

	Column 6 Table 3	Adding Birth-Month and Death-Month FE	Interacting County FE with Covariate Dummies	Outcome: Log Age at Death	Outcome: Log Age at Death, Method: Accelerated Failure Time (AFT)
	(1)	(2)	(3)	(4)	(5)
3rd Tercile of 1900 Textile	-3.59698**	-3.56517**	-3.57803**	-.004**	-.00396**
$\times I(Birth\ Year > 1920)$	(1.68271)	(1.6841)	(1.6911)	(.00194)	(.00166)
2nd Tercile of 1900 Textile	-.40289	-.37321	-.55868	-.00036	-.00035
$\times I(Birth\ Year > 1920)$	(1.60949)	(1.60906)	(1.61628)	(.00185)	(.00156)
Observations	166920	166920	166912	166920	166920
R-squared	.46301	.4641	.46398	.44075	--
Mean DV	905.200	905.200	905.202	6.800	905.200
	Outcome: Age at Death > 65	Outcome: Age at Death > 75	Weighting by CenSoc weights	Clustering SE on County	Restricting to Birth Cohorts < 1930
	(6)	(7)	(8)	(9)	(10)
3rd Tercile of 1900 Textile	-.01192*	-.01811**	-3.3671**	-3.59698**	-3.5347**
$\times I(Birth\ Year > 1920)$	(.00665)	(.00893)	(1.56125)	(1.5204)	(1.7621)
2nd Tercile of 1900 Textile	-.00884	-.01034	-.46433	-.40289	-.4316
$\times I(Birth\ Year > 1920)$	(.00633)	(.00823)	(1.50203)	(1.41313)	(1.68635)
Observations	166920	166920	143145	166920	151322
R-squared	.1727	.34127	.45216	.46301	.39642
Mean DV	0.853	0.480	938.906	905.200	918.336

Notes. Standard errors, two-way clustered on county and birth-year (except for column 9), are in parentheses. All regressions include controls include individual, family, and county covariates. Individual controls include dummies for race and ethnicity. Family controls include dummies for maternal education, paternal literacy, and paternal socioeconomic index. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score. *** p<0.01, ** p<0.05, * p<0.1

Table 8 - Exploring Mechanisms Using 1950-1960 Census

	Outcome: Age at Death (Months), Subsamples:					
	Years of Schooling	Years of Schooling < 9	Years of Schooling < 12	Education: College-More	Socioeconomic Index	Occupational Education Score
	(1)	(2)	(3)	(4)	(5)	(6)
3rd Tercile of 1900 Textile	-.31241***	.04085***	.03212***	-.03212***	-2.28168***	-2.01358***
$\times I(Birth\ Year > 1920)$	(.05165)	(.00804)	(.00428)	(.00428)	(.33112)	(.26295)
2nd Tercile of 1900 Textile	-.04943	.01734***	.00536	-.00536	-.06541	-.32431
$\times I(Birth\ Year > 1920)$	(.03231)	(.00521)	(.00444)	(.00444)	(.27973)	(.24462)
Observations	207577	207577	207577	207577	161003	160455
R-squared	.24469	.16865	.0243	.0243	.02908	.01855
Mean DV	7.285	0.315	0.842	0.158	37.065	19.390
County FE	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓
Census Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes. Standard errors, two-way clustered on county and birth-year, are in parentheses. Controls include individual and county covariates. Individual controls include dummies for race and ethnicity. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.
*** p<0.01, ** p<0.05, * p<0.1

Figures

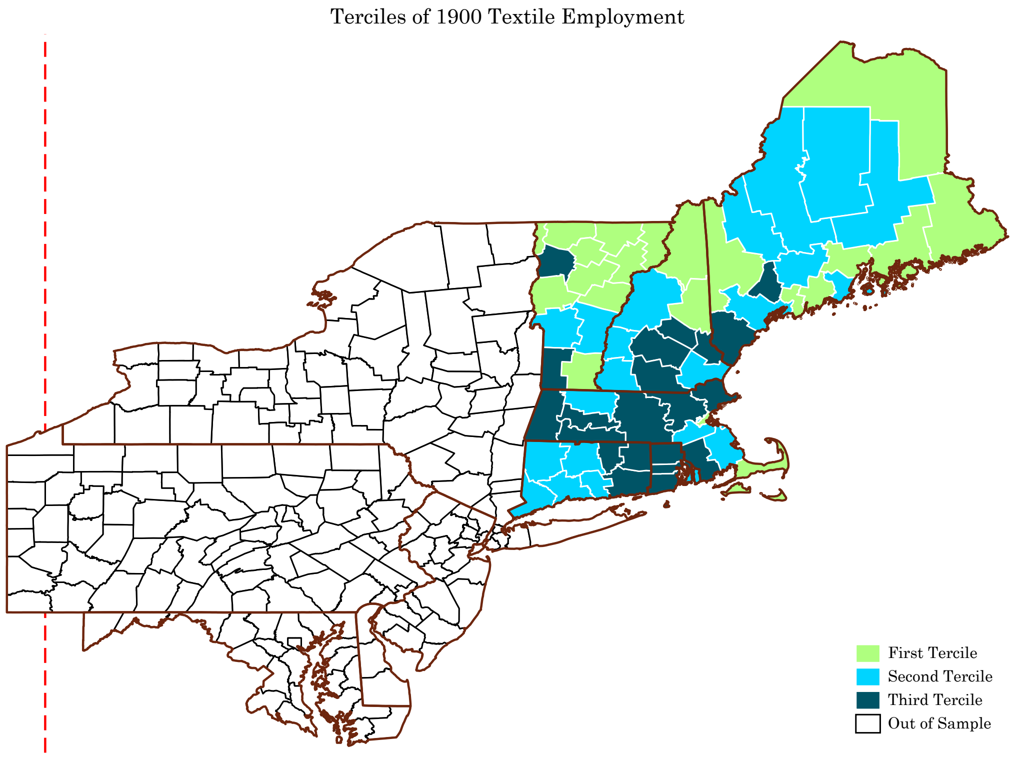

Figure 1 - Geographic Distribution of Counties based on 1900 Share of Textile Employment in the Labor Force

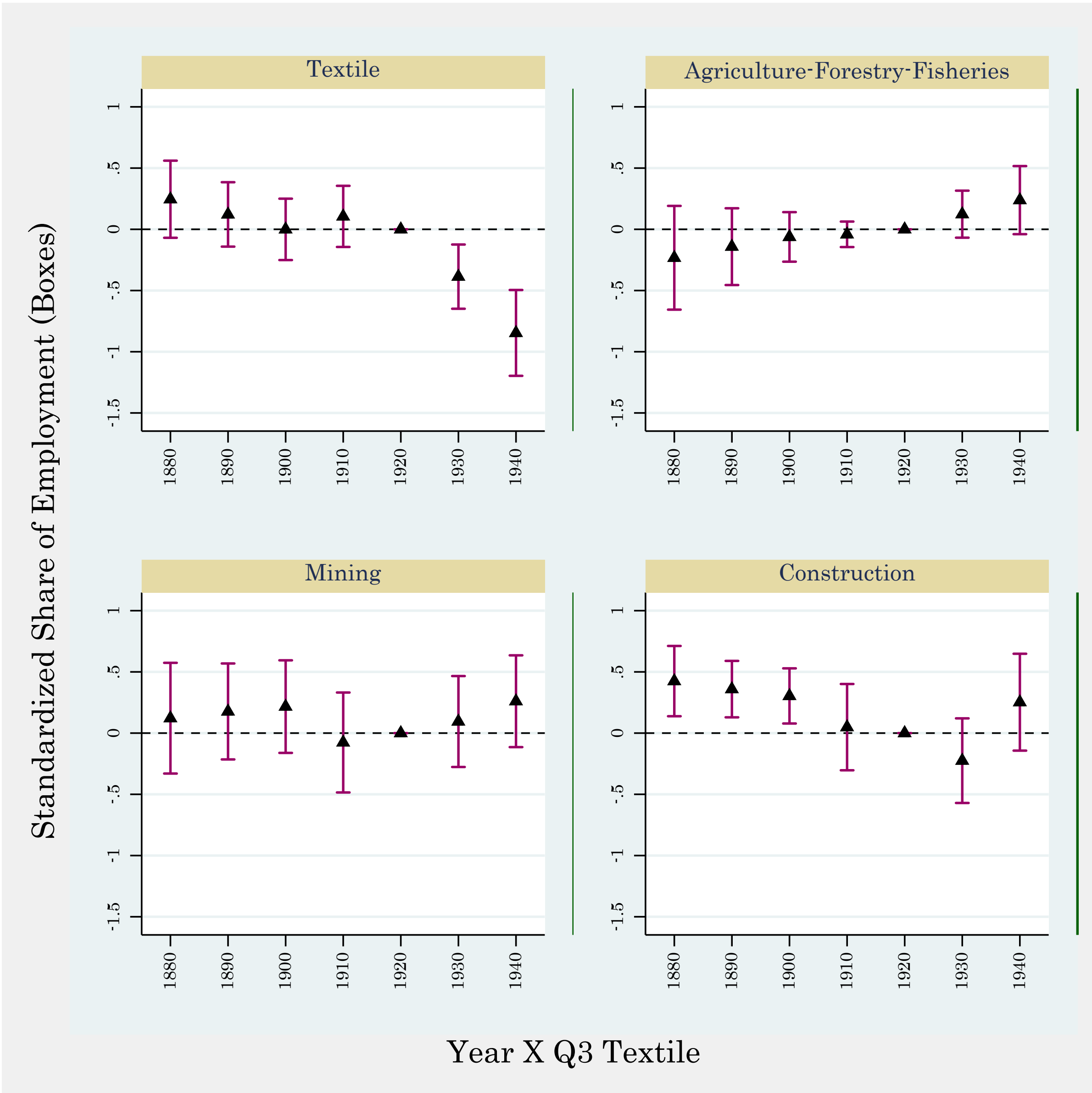

Figure 2 - Event Study to Examine the Changes across Employment in Different Sectors over the Census Years in Counties at the Top-Tercile versus Bottom-Terciles of 1900 Textile Employment

Notes. Point estimates and 90 percent standard errors are depicted. Standard errors are two-way clustered on county and birth-year. Regressions include county fixed effects, birth year fixed effects, and controls. Controls include individual and county covariates. Individual controls include dummies for race and ethnicity. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.

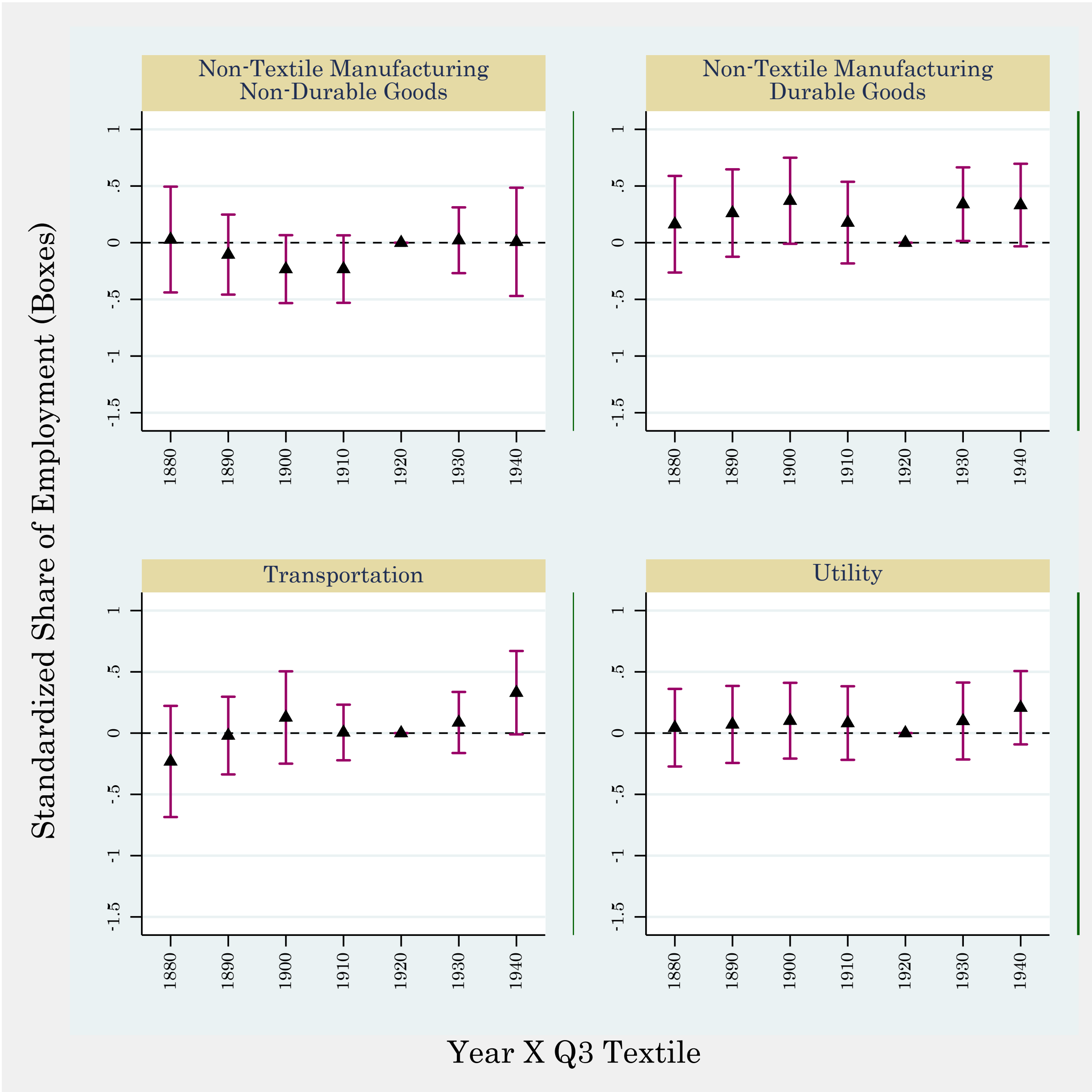

Figure 3 - Event Study to Examine the Changes across Employment in Different Sectors over the Census Years in Counties at the Top-Tercile versus Bottom-Terciles of 1900 Textile Employment

Notes. Point estimates and 90 percent standard errors are depicted. Standard errors are two-way clustered on county and birth-year. Regressions include county fixed effects, birth year fixed effects, and controls. Controls include individual and county covariates. Individual controls include dummies for race and ethnicity. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.

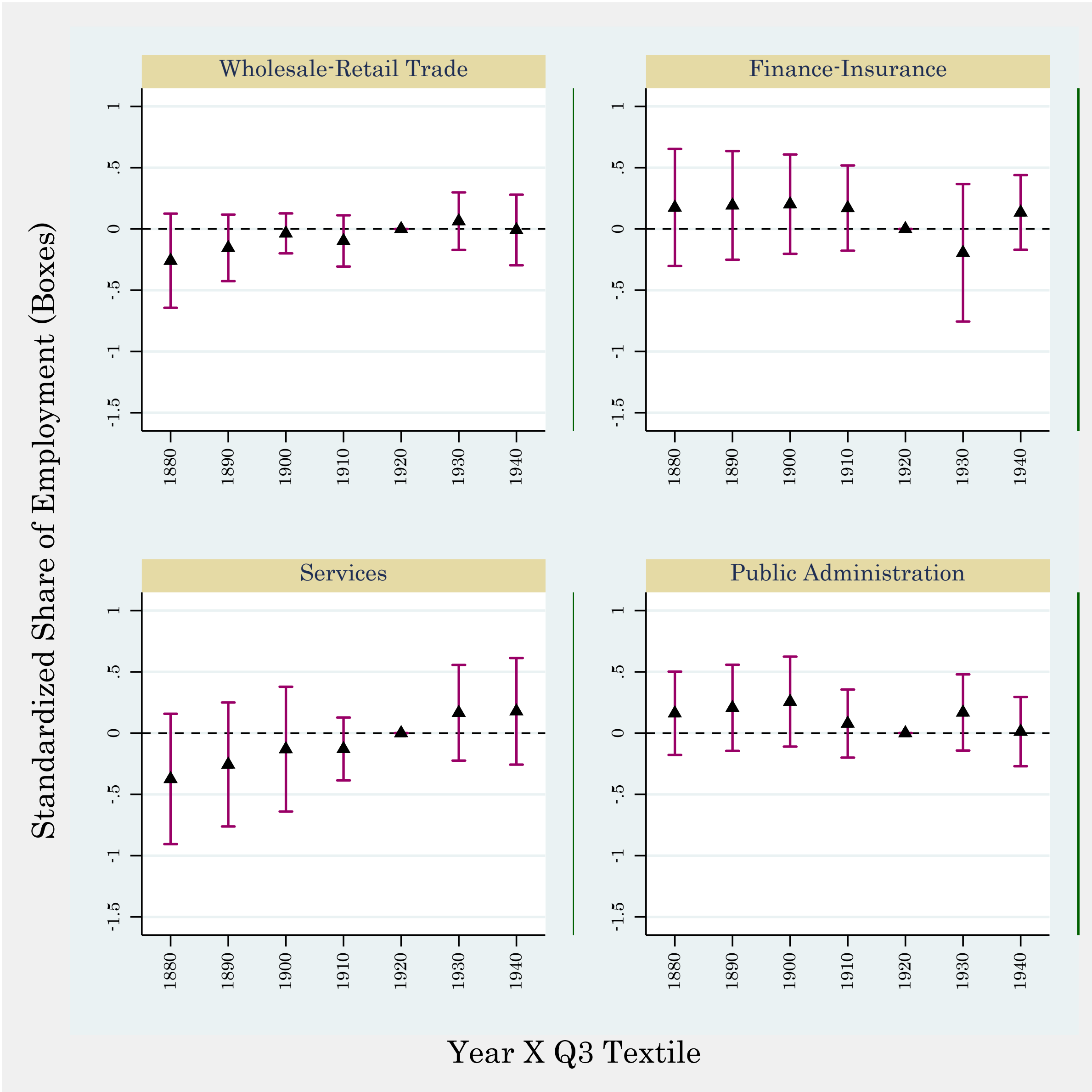

Figure 4 - Event Study to Examine the Changes across Employment in Different Sectors over the Census Years in Counties at the Top-Tercile versus Bottom-Terciles of 1900 Textile Employment

Notes. Point estimates and 90 percent standard errors are depicted. Standard errors are two-way clustered on county and birth-year. Regressions include county fixed effects, birth year fixed effects, and controls. Controls include individual and county covariates. Individual controls include dummies for race and ethnicity. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.

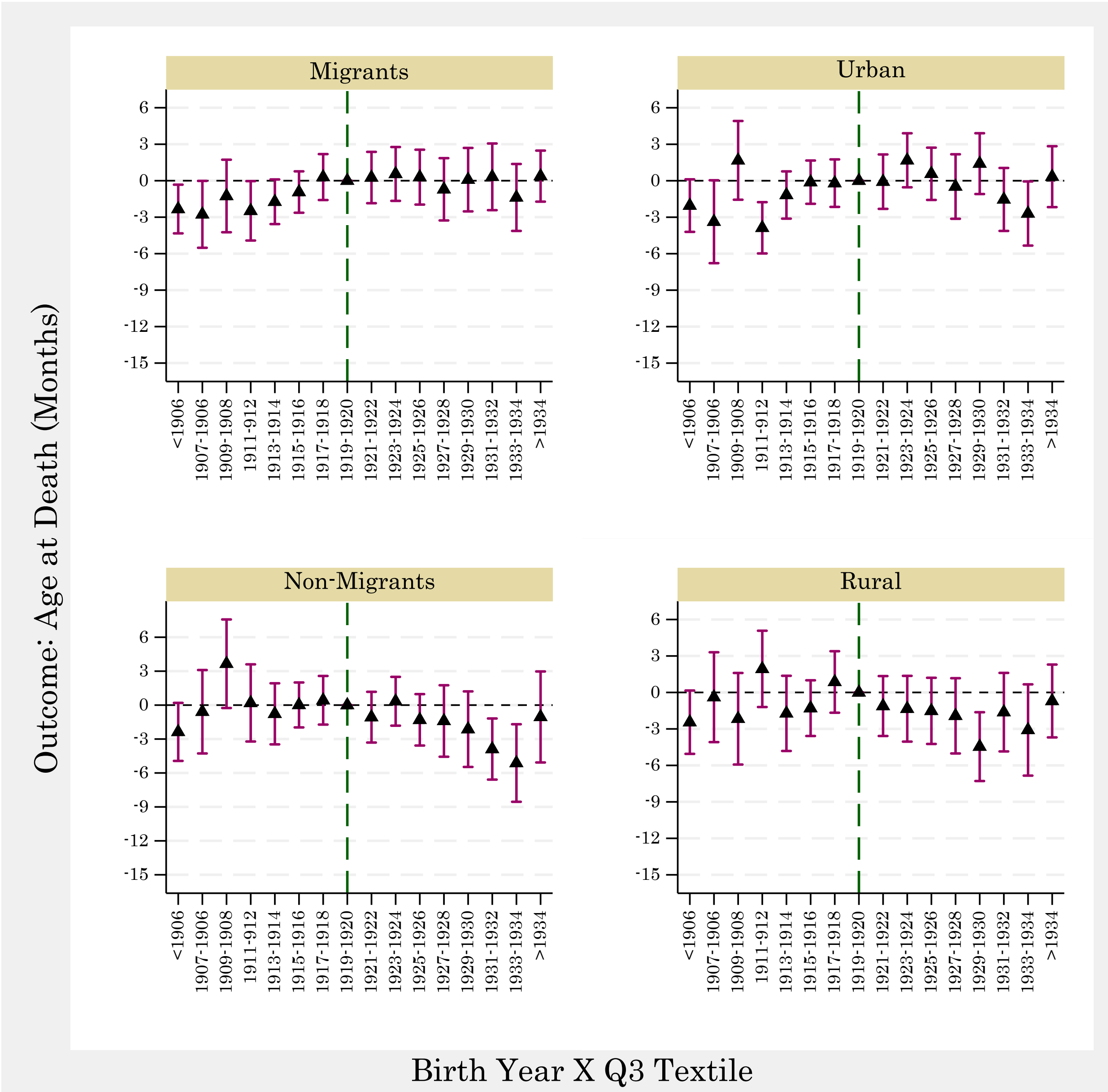

Figure 5 - Event Study to Examine the Effects of Early-Life Exposure to Deindustrialization across Birth Cohorts and Subpopulations on Old-Age Longevity

Notes. Point estimates and 90 percent standard errors are depicted. Standard errors are two-way clustered on county and birth-year. Regressions include county fixed effects, birth year fixed effects, and controls. Controls include individual and county covariates. Individual controls include dummies for race and ethnicity. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.

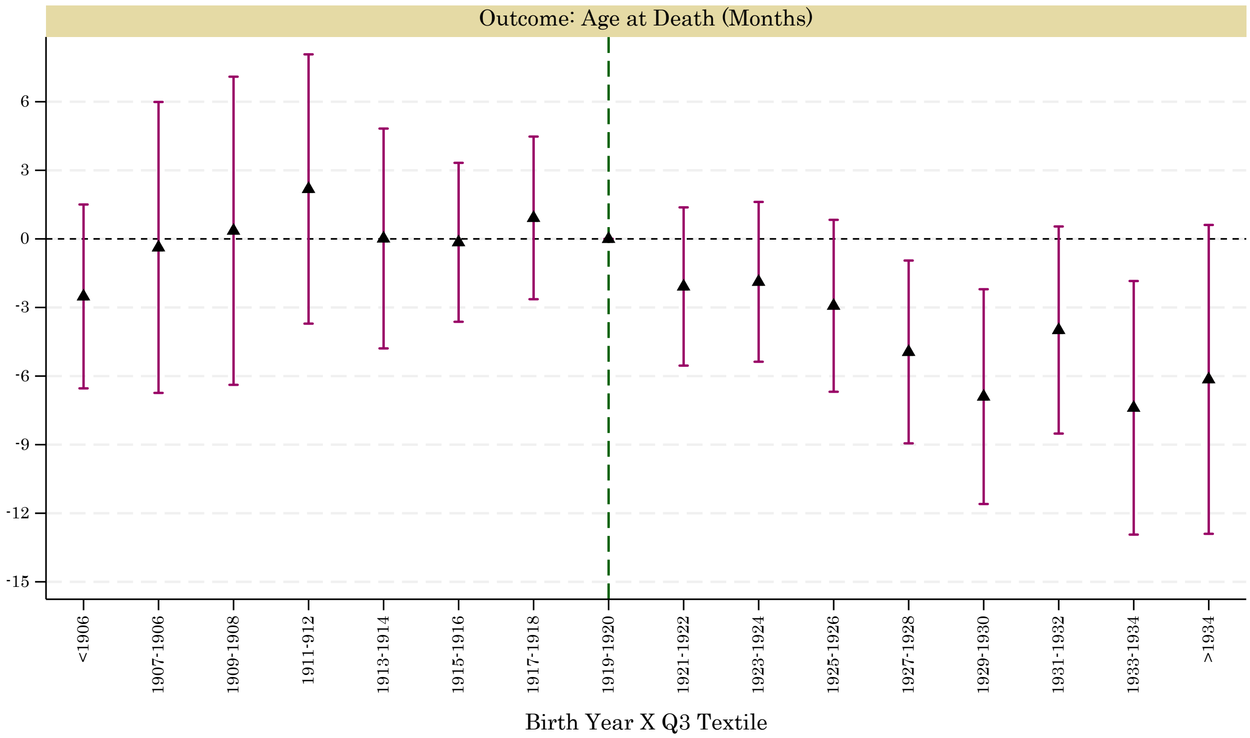

Figure 6 - Event Study to Examine the Effects of Early-Life Exposure to Deindustrialization across Birth Cohorts of Non-Urban Non-Migrant Population on Old-Age Longevity

Notes. Point estimates and 90 percent standard errors are depicted. Standard errors are two-way clustered on county and birth-year. Regressions include county fixed effects, birth year fixed effects, and controls. Controls include individual and county covariates. Individual controls include dummies for race and ethnicity. County controls include average population, the share of population in different age groups, share of population in different race groups, share of immigrants, share of parried individuals, average family size, and average occupational income score.