

The Broad Decline in Health and Human Capital of Americans Born after 1947

Nicholas Reynolds
University of Essex *

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Abstract

I present evidence of a cross-cohort decline in the health and human capital of Americans, beginning with those born after 1947 and continuing until those born in the mid-1960s. Education, men's wages, women's maternal health (proxied by their infants' birthweight), and mortality all exhibit trend breaks near the 1947 cohort, such that each outcome worsens for subsequent cohorts relative to prior trend. The decline is large enough to drive: i) educational declines in the 1960s, ii) increases in low birthweight in the 1980s, iii) mortality increases since 1999, and to contribute substantially to iv) wage stagnation since the 1970s.

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Health and human capital improved enormously across American cohorts born in the first half of the twentieth century. Many argue that the key driver of these improvements were gains in early life health.¹ I present evidence that, in a number of dimensions, this progress in adult health and human capital hit a sudden stop and reversed trend for cohorts born after 1947. Instead, the underlying health and human capital of Americans born in each year between 1947 and the mid-1960s has been worse than that of those born a year earlier. I also argue this decline likely originated in adolescence or earlier.

My cohort-based theory provides a partially unified explanation of puzzling societal declines occurring at distinct points over the last 60 years. As post-1947 cohorts have aged their depressed health and human capital have manifested as lower test scores, educational attainment, wages, and maternal health, and increased mortality rates — contributing to well-known aggregate declines in these outcomes. In the 1960s and 1970s, these cohorts drove the sharp declines in achievement tests, high school graduation rates, and college enrollment rates (National Commission on Excellence in Education, 1983; Card and Lemieux, 2001b). The depressed labor market ability of men in these cohorts then led them to have lower wages, contributing to wage stagnation since the 1970s, particularly for those without a college degree (Gould, 2014; Katz and Murphy, 1992; Autor et al., 2008). Depressed health as mothers led the women in these cohorts to give birth to less healthy infants, driving the upturn in the low birth weight rate in the 1980s (Centers for Disease Control, 1994; Currie and Gruber, 1996). Ultimately, the poor health of these cohorts has increased their likelihood of untimely death, contributing to recent mortality increases at midlife (Case and Deaton, 2015, 2017; National Academies of Sciences et al., 2021).

Figure 1 shows that the trend across cohorts in age-adjusted educational attainment, wages, maternal health (proxied by the birth weight of infants), and mortality all exhibit trend breaks near the 1947 cohort, such that each outcome declines for subsequent cohorts relative to the prior trend. These simultaneous trend breaks, while striking, could in principle reflect differences in external factors which these cohorts were exposed to, rather than underlying differences in health and human capital. That is, cohorts born after 1947 may have been otherwise similar to earlier cohorts, but were merely unlucky to have experienced bad conditions throughout their lifetime.

I therefore provide evidence, under increasingly weak assumptions about the nature of external factors, that these patterns reflect a decline in the underlying health and human capital of cohorts born after 1947, relative to the prior trend. First, I estimate traditional age-period-cohort models, which assume cohort, age, and year factors are each additively separable. Second, I estimate models with a trend break of unknown location in cohort effects while allowing for a separate polynomial in age in each year, adapting methods from the structural break literature. In compan-

¹See for example Fogel (1986); Fogel and Costa (1997); Costa and Steckel (1997); Floud et al. (2011); Fogel (2012); Costa (2015).

ion work, I present evidence from a method which allows identification and estimation of a relative cohort decline in a general nonseparable framework (Reynolds, 2023b).

These methods reveal strong evidence of a trend break at the 1947 or 1948 cohort in underlying health and human capital evident in each of the above outcomes. The remaining threat to validity would be changes in the impact of age across years which cannot be approximated by smooth polynomials, and repeatedly “hit” the same cohorts. Alternative explanations in the literature do not have this feature. For example, the effects of shifts in supply or demand will be smooth as long as individuals who are close in age are substitutable (Card and Lemieux, 2001a).

The cohort-based decline is large enough to have contributed substantially to the year-over-year declines described above. This suggests that previously independent searches for the causes of educational declines of the 1970s, increases in the low birth weight rate in the 1980s, increases in midlife mortality since 1999, and to a lesser extent wage stagnation over the last 50 years, can be at least partially unified.

Why did things go so deeply wrong for Americans born after 1947? My investigation so far yields no smoking gun. It appears the decline likely originated in adolescence or earlier. It seems plausible that the initial poor start for these cohorts was exacerbated by period-based factors, such as labor demand and opioid supply, and future research should examine these interactions. The decline is also remarkably widespread across racial groups and geography, among native-born Americans.

1. Literature review

Few papers have noted that multiple outcomes have declined for post-1947 cohorts. An exception is Borella et al. (2020), who compare the outcomes for white Americans without a college degrees born in the 1940s to those born in the 1960s across a number of outcomes. My study differs in: i) studying average outcomes for all Americans in each cohort born between 1930 and 1970, ii) focusing on whether the declines in these (and other) outcomes are driven by “cohort effects” — long-standing differences in health and human capital — or external factors which have hit these cohorts at different points in their lifetime, iii) documenting a sharp trend break in outcomes near the 1947 cohort.

The declines in educational attainment for cohorts born after the late-1940s is well known but the cause is not. See for example, Card and Lemieux (2001b); Heckman and LaFontaine (2010); Acemoglu and Autor (2012); Goldin and Katz (2007); Handy and Shester (2019).

Studies on wage declines in the 1980s for lower educated men, considered but largely dismissed the role of a cross cohort decline in “earnings ability” (eg. Bound and Johnson, 1992; Katz et al., 1999). An earlier paper, Bishop (1989), estimated that the decline in cognitive ability, as measured by test scores, had a large effect on wage rates for these cohorts and would continue to

contribute substantially to productivity decline. Guvenen et al. (2022) document a decline in men’s median lifetime earnings of more than 10 percent between the 1942 and 1960 birth cohorts. My paper builds on theirs by explicitly confronting the challenge of identifying whether these (and other) declines for these cohorts are driven by long-standing differences in health and human capital.

Case and Deaton (2015) documented a shocking increase in the all-cause mortality rate of non-Hispanic white Americans and noted a proximate cause was increases in so-called “deaths of despair” — drug and alcohol poisonings, suicide, and chronic liver diseases and cirrhosis. Subsequent research has examined mortality across racial groups and suggested that cohort differences in health and disadvantage which predated the 1990s are likely important as well (Case and Deaton, 2017; Lleras-Muney, 2017; Masters et al., 2017; Zang et al., 2018; Acosta et al., 2020; Reynolds, 2023b; National Academies of Sciences et al., 2021). With the exception of my earlier paper focused on white mortality (Reynolds, 2023b), these papers do not focus on the precise timing of the cohort decline.

Since I began work on this project a series of papers by Hui Zheng and coauthors have shown declines in cognitive ability and various health outcomes for approximately the same cohorts (Zheng, 2021; Zheng and Echave, 2021; Zheng et al., 2022). Interpretation is complicated by the fact that all results come from a “mixed-model” in which age effects are modelled as fixed effects and cohort and period effects as random effects; and there is no focus on the precise timing of the decline.

Adult height also suddenly stopped increasing for white Americans born between approximately 1955 and 1974, while the height of Europeans continued to grow rapidly (Komlos and Lauderdale, 2007a,b; Komlos, 2010).

2. Data

A. Maternal health

I use the 1968 to 1995 Birth Data Files from the National Center for Health Statistics. They provide detailed information derived from birth certificates, from a 50 percent sample of all births in 1968, and progressively larger samples until 1985, after which they include the universe of births.

I consider the health of infants as a proxy for maternal health and also to provide evidence of an intergenerational effect of the apparent decline in cohort health. Under the common assumption that a mother’s health “endowment” enters the infant health production function (Rosenzweig and Schultz, 1983; Grossman and Joyce, 1990), a decline in the health of infants by *mother’s birth cohort* may be viewed as evidence of a decline in maternal health for these cohorts.²

²Almond and Chay (2006) use this data similarly to study black-white gaps in health.

Mothers' year of birth is not recorded explicitly. I therefore calculate the approximate birth year of each mother as the infant birth year minus the mother's age. I restrict my analysis to births to mothers born between 1930 and 1970. I use the years 1968 to 1995 and births to mothers ages 18 to 40. This results in a sample of more than 75 million births.

I study birth weight as a continuous measure, and the low birth weight percentage, the percentage of infants weighing less than 2500 grams. For all analysis I calculate these measures in cells by year, single age, and birth year using sampling weights; and then conduct weighted regressions and other analysis on these cell means.

B. Wages and educational attainment

I use the Current Population Survey, Merged Outgoing Rotation Group (CPS-MORG), from 1979 to 1993, which has been used extensively in studies of wage inequality and trends, and has some notable advantages over other sources.³

I focus on the wages of prime-age men age 25 to 54. This sidesteps difficulties with large changes in selection for employed women in this period (Goldin, 2006). I again calculate the approximate birth year as the survey year minus the respondent's age. I restrict my analysis to cohorts born between 1930 and 1965. I end the analysis in 1993 to avoid having to address issues related to the substantial redesign of the CPS earnings questions in 1994 (Autor et al. (2005)). These restrictions lead to a sample of 970,479 men with non-missing earnings used in the main analysis.

I address the problem of topcoding of earnings, by focusing on the median and other quantiles. I calculate the sample median and other quantiles separately for age-year-sex cells, using the survey weights. I follow Chamberlain (1994) and Chetverikov et al. (2016) and use a two step procedure: first estimating cell quantiles, and second estimating models on the cell quantiles. I adjust earnings using the CPI-U-RS.

To study educational attainment I also use the CPS-MORG data, 1979-2016, and include men and women aged 25-75, who were born from 1930 to 1965. I calculate approximate average years of schooling for each cohort based on the 16 schooling categories in the CPS, and calculate the share of each cohort who have achieved different levels of educational attainment.

C. Mortality

I use data from the Human Mortality Database (HMD) on number of deaths and population counts by year and age. These data are derived from official vital statistics and census estimates. I again define cohort as year minus age. I restrict my analysis to the years 1975-2019, ages 25 to 85, and cohorts born between 1930 and 1965.

³See for example DiNardo et al. (1996); Lemieux (2006); Autor et al. (2008); Acemoglu and Autor (2011); and Gould (2014). I use the CEPR Uniform Extracts (for Economic and Research", 2020) and use their earnings definition, which is designed to follow NBER's recommendation and similar to past research.

3. Evidence of cohort decline from age-period-cohort models

I first present evidence of cohort declines in health and human capital from additively separable age-period-cohort models, common in economics and demography⁴:

$$Y_{apc} = \gamma_c + \phi_p + \alpha_a + \epsilon_{apc} \quad (1)$$

The objects of interest are the sequence of cohort effects, γ_c . ϕ_p and α_a are full sets of fixed effects in year and age, respectively. ϵ_{apc} is an orthogonal error.

Conceptually, the cohort effects reflect the impact of underlying, fixed differences between individuals born in different years. To identify the cohort effects, the above model assumes that the impact of external factors can be decomposed as additively separable year and age components. It is unrestricted with respect to the dynamics of year-over-year changes impacting each outcome, and with respect to the *shape* of age effects. However, it does not allow age-by-year interactions.

Under these assumptions, the cohort effects answer meaningful counterfactual questions. The difference between the cohort effect for the 1947 cohort, γ_{1947} , and that of the 1960 cohort, γ_{1960} , reveals how the outcomes of individuals born in these two years would have differed — holding all external factors fixed.

This model is not identified due to the exact collinearity of age, period, and cohort (Hall, 1968; Deaton, 1997). Identification can be achieved by imposing one additional linear restriction. I take two approaches. First, I estimate “detrended cohort effects”, which will reflect the true cohort effect, minus some unknown long-run trend in cohort effects. Second, I will consider the implications for the sequence of cohort effects of imposing additional assumptions on either the age or period effects.

Denote the first cohort included in the model as 1 and the last as C, the first year as 1 and the last as P, and the first age included in the model as 1 and the last as A. Define linear “trends” in each as follows:

$$\beta_c \equiv \frac{\gamma_C - \gamma_1}{C}; \quad \beta_p \equiv \frac{\phi_P - \phi_1}{P}; \quad \beta_a \equiv \frac{\alpha_A - \alpha_1}{A}$$

Then define “detrended” cohort, age, and year effects as:

$$\tilde{\gamma}_c = \begin{cases} 0 & \text{if } c = 1 \\ \gamma_c - \beta_c \cdot c & \text{if } c \in (2, C] \end{cases} \quad \tilde{\alpha}_a = \begin{cases} 0 & \text{if } a = 1 \\ \alpha_a - \beta_a \cdot a & \text{if } a \in (2, A] \end{cases} \quad \tilde{\phi}_p = \begin{cases} 0 & \text{if } p = 1 \\ \phi_p - \beta_p \cdot p & \text{if } p \in (2, P] \end{cases}$$

And finally define two composite linear trends:

$$\tilde{\beta}_{pc} = \beta_p + \beta_c; \quad \tilde{\beta}_{ac} = \beta_a - \beta_c$$

⁴Eg. Deaton (1997); Aguiar and Hurst (2013); Lagakos et al. (2018); Yang (2008); Masters et al. (2014, 2017); Zang et al. (2018).

One can then rewrite the model in Equation 1 as:

$$Y_{apc} = \tilde{\gamma}_a + \tilde{\phi}_p + \tilde{\delta}_c + \tilde{\beta}_{ac} \cdot a + \tilde{\beta}_{pc} \cdot p + \epsilon_{apc} \quad (2)$$

This reparameterized model is identified. One can identify detrended age, period, and cohort effects, as well as two composite linear trends which represent i) the sum of the period and cohort trends, and ii) the age trend minus the cohort trend. Under additional assumptions on age or period trends one can identify or bound the cohort trend, and therefore the full set of cohort effects.

A. Detrended cohort effects

Figure 2 shows regression estimates of the detrended cohort effects for six outcomes: men’s median log wage, the low birth weight percentage by mother’s birth cohort, the log mortality of men and women, and the average years of schooling of men and women. The estimated cohort effects for each of the different outcomes each have an approximately piecewise linear shape with a large trend break located at or near the 1947 cohort, and declines for cohorts born after that year. These patterns are consistent with a large decline in cohort health and human capital, relative to trend, which was broad enough to impact outcomes as disparate as maternal health, wages, and mortality.

Panel A shows results for the median hourly wage of employed men. The estimated cohort effects exhibit a clear piecewise linear shape, with a large trend break precisely at the 1947 cohort. The cohort effects increase from a normalized 0 in 1930 to .14 by 1947, before suddenly changing slope and declining for subsequent cohorts. This pattern suggests a large break in the cross-cohort trend in labor market ability at the 1947 cohort, such that each cohort born after this year has declining ability relative to the trend for prior cohorts. The results imply that the 1965 cohort had a median wage nearly 29 log points lower, ie. 33 percent lower, than they would have had the trend in labor market ability for the 1930 to 1947 cohorts continued.

Panel B shows results for the low birth weight rate of infants by their mother’s birth cohort. Recall that age and cohort in these models refer to the age and year of birth of the *mother*. The estimated cohort effects again exhibit a piecewise linear shape: declining rapidly until the 1947 cohort, sharply changing slope after that cohort, and increasing nearly linearly until the 1965 cohort. The cohort effects decline from the normalized 0 in 1935 to a minimum of -.87 for the 1947 cohort, before reversing trend. The results imply a large break in the cross-cohort maternal health trend at the 1947 cohort, such that each cohort born after this year has declining health relative to the trend for prior cohorts. The size of the trend break suggests that the 1965 cohort would have had a low birth weight rate approximately 2.2 percentage points lower had the cohort health decline not occurred.

Panels C and D show similar results for the log mortality rate of men and women. The shape

of the estimated cohort effects are not as near piecewise linear as those for the labor market and maternal health outcomes. However, they exhibit clear changes in trend near the late 1940s cohorts, consistent with elevated mortality and declining health for subsequent cohorts. For men the cohort effects decline — not precisely linearly — from 0 to below $-.1$ by the 1947 cohort, then suddenly reverse trend and increase rapidly until reaching above $.05$ by the late 1950s cohorts. They then flatten and decline slightly for subsequent cohorts. This pattern suggests that men born in 1960 had mortality near $.25$ log points higher than they would have had health improvements continued at the same rate as for the 1930 to 1947 cohorts. For women the cohort effects exhibit two smaller trend breaks at the 1947 and 1951 cohorts, but still show evidence of a decline in health after the late 1940s relative to the prior trend. The cohort effects decline from 0 to below $-.11$ by the 1947 cohort, they then change trend and are nearly flat until the 1951 cohort. They then change trend *again* after the 1951 cohort and increase nearly linearly to around 0 by the 1960 cohort. The size of the two trend breaks imply that the 1965 cohort has had nearly $.25$ log points higher than it would have had the health improvements for the 1930 to 1947 cohorts continued at the same rate for later cohorts.

Panels E and F shows results for the average years of schooling of men and women. Again, the estimated cohort effects appear approximately piecewise linear in shape, with a large trend break precisely at the 1947 cohort. For men, the cohort effects increase from a normalized 0 in 1930 to $.83$ by 1947, before suddenly changing trend and declining for subsequent cohorts. For women the pattern is similar though the differences in cohort effects are smaller in magnitude; and the 1947 trend break is smaller in magnitude but followed by a second trend break in the early 1950s (similar to the pattern for women’s log mortality). Appendix Figure 2 shows results separately for the share of each cohort with a high school degree, bachelor’s degree, and advanced degree. Consistent with the prior literature reviewed above, they all show evidence of a clear, large trend break at or near the 1947 cohort.

B. Cohort effects under additional assumptions

Restrictions on period trend

For low birth weight and log mortality I consider the implications for cohort effects of different restrictions on the period trend. Lagakos et al. (2018) call this the “Deaton-Hall” approach in reference to Hall (1968) and Deaton (1997). By restricting the trend in period effects in different ways, one can pin down the trend in cohort effects based on the composite period/cohort trend defined above.⁵

I consider three benchmark cases: i) assuming that the period trend, β_p , is equal to 0, ii) assuming that the cohort trend, β_c , is equal to 0, and an intermediate case iii) assuming that β_p and β_c

⁵See also Fosse and Winship (2019).

are equal.⁶ Many other cases are possible, but examining what these assumptions imply about the sequence of cohort effects can help to make the mechanics of the model and its underidentification clear.

It is arguably natural to think of case i) as an upper bound on the period trend, and therefore a lower bound on the cohort trend. For example, for low birth weight assuming $\beta_p \leq 0$ is assuming that average growth in period effects between 1968 and 1995 is non-positive, in other words that over this period there was at a minimum not a worsening of external factors which impact the low birth weight rate. For mortality analogously, it would be assuming that between 1975 and 2019 the external environment impacting mortality rates did not worsen. Therefore we can view the sequence of cohort effects in case i) as a “best case” scenario for cohort effects, in the sense that they represent the most that cohort effects impacting low birth weight or mortality could have declined between the 1930 and 1965 cohort (or any two cohorts for that matter). Under this assumption the true sequence of cohort effects must lie above the case i) sequence, with the case ii) and iii) sequences representing two other possible sequences above the lower bound. Though note case ii) is likely not a natural upper bound on the cohort trend.

Figure 3, Panel B shows cohort effects under the different scenarios for the low birth weight percentage. The bottom series shows cohort effects under case i), assuming that β_p is equal to 0. Even under this assumption, which could be viewed as the “best case” for the long run trend in cohort effects, the cohort effects increase between the 1947 and the 1960 cohort by more than .25. This would imply that maternal health declined across these cohorts enough to increase the low birth weight percentage for these mother’s by more than .25 percentile points. The series based on scenarios ii) and iii) imply much *worse* declines in maternal health between the 1947 and 1965 cohorts.

Panels C and D show results for men’s and women’s log mortality. The bottom series shows cohort effects under case i), assuming that β_p is equal to 0. The “best case” for the long run trend in cohort effects implies a small set of cohorts across which cohort effects were worsening in absolute terms. For men they would imply that the impact of cohort health on log mortality was worsening between the 1947 and 1952 cohort, before beginning to improve again. For women, they would imply that the impact of cohort health on log mortality was worsening in absolute terms only between the 1951 and 1957 cohorts. The series based on cases ii) and iii) imply much larger increases in cohort effects after the 1947 cohort, and a larger range of cohorts across which cohort health was worsening.

No growth in wage age effects after 50

It seems hard to argue that external factors impacting wages have definitely not improved

⁶Case ii) will yield the “detrended” cohort effects presented above.

over time, ie. that the trend in period effects for wages is less than 0. I therefore make assumptions on the age effects, adopting what Lagakos et al. (2018) call the Heckman-Lochner-Taber approach in reference to Heckman et al. (1998). The idea behind this approach is to make the theory-guided assumption of no growth in age effects in the last few years of a workers career (or a slight decline due to eg. human capital depreciation).

I implement this approach by first estimating a restricted version of the age-period-cohort model in Equation 2. I restrict the age effects from ages 50 to 54 to be equal to 0. This represents a first benchmark case i) of no growth in age effects between ages 50 and 54. I then consider two additional cases in which human capital depreciation (or other factors) lead age effects between age 50 to 54 to i) decline by .005 per single-year-of-age, and ii) decline by .01 per single-year-of-age; corresponding to human capital depreciation rates of .5 % and 1 % respectively.

The cohort effects implied by these three scenarios are shown in Panel A of Figure 3. The bottom series shows cohort effects under the assumption that age effects are constant between ages 50 and 54. This series would imply that labor market skill declined between the 1947 and 1965 cohorts enough to reduce log wages by approximately .16. The middle series shows cohort effects under the assumption that age effects decline .005 per year for ages 50-54, which imply that labor market skill declined between the 1947 and 1965 cohorts enough to reduce log wages by around .11. Finally, the top series shows cohort effects under the assumption that age effects decline .01 per year for ages 50-54. This assumption implies an absolute decline in cohort effects between the 1947 and 1965 cohorts of around .02.

4. Evidence of cohort decline allowing for smooth age-by-year interactions

The above results from the age-period-cohort models are striking, but could be biased by external factors which disproportionately impact individuals of particular ages in particular years.

Therefore, in this section I present evidence of a decline in cohort health and human capital using a novel methodology, which allows me to identify a trend break in cohort effects while allowing for smooth changes in external, age-specific factors across years.

Consider the following model:

$$Y_{apc} = \underbrace{\beta \cdot c}_{\text{long-run trend in cohort effects}} + \underbrace{\mathbb{1}_{c \geq \lambda} \cdot \delta \cdot (c - \lambda)}_{\text{trend break in cohort effects}} + \underbrace{f^p(a)}_{\text{year-specific impact of age}} + \epsilon_{apc} \quad (3)$$

The first two terms on the right-hand-side specify the cohort effects as piecewise linear with a single, *unknown* trend break. β represents a long-run trend in cohort effects. The second term introduces a trend break in cohort effects. δ represents the size of this trend break, and λ represents

the unknown cohort at which the break occurs. This specification of the shape of cohort effects is similar to that seen visually in the detrended cohort effects estimated above. I assume that the changing external age-by-year factors, $f^p(a)$, take the form of a polynomial of known order in each year. ϵ_{apc} is an orthogonal error.

The location and size of the trend break are still identified with the introduction of separate polynomials in age *in each year*. The sharp trend break in cohort effects is orthogonal to the smooth, polynomial age-by-year interactions.

I estimate the model by least squares, following the structural break methodology in Hansen (1999, 2000). The location of the trend break of these piecewise linear cohort effects is treated as an unknown parameter to be estimated. My baseline specification includes age fixed effects, year fixed effects, and a separate quadratic-in-age in each year as controls. I also probe the robustness of the estimated cohort trend break to including increasingly higher order polynomials in age in each year.⁷

Intuitively, I allow external factors which impact individuals of different ages to change but restrict them to do so smoothly. Most alternative explanations I want to rule out would take such a smooth form. For example, changes in the supply or demand for workers of different experience levels will have a smooth impact across ages, as long as individuals of nearby ages are sufficiently close substitutes. If the biological aging process is smooth then changes in the disease environment may disproportionately increase the mortality of young adults, but they will do so smoothly.

Table 1 shows the results. The estimated location of the cohort break are centered at the 1947 and 1948 cohorts. For both of the maternal health outcomes, infant mean birth weight and share low birth weight, the trend break is estimated to occur at the 1948 cohort. For both of these outcomes, the 99 percent confidence interval includes *only* a single cohort. For the men's median log wage it is estimated to occur at the 1947 cohort, with a confidence region including only 2 cohorts — 1946 and 1947. Cohort effects in models of the log mortality of men and women are estimated to have a trend break at the 1947 and 1949 cohorts respectively. The estimated cohort break location for years of schooling are 1948 and 1950 respectively. The estimated break locations for mortality and education are very precisely estimated — with the 99 percent confidence interval including only a single cohort.

The estimated trend breaks in cohort effects for all outcomes are large in magnitude and precisely estimated. The estimated size of the trend break, δ for the median log wage is -.016. This implies that the median man in the 1960 cohort has a wage roughly 23 percent lower than they would have had the cohort effects followed the pre-1947 cohort trend. For mean birth weight the

⁷For educational attainment I only include age fixed effects as external year factors and age-by-year factors do not seem likely to drive educational attainment after age 25. I restrict the location of the break to not be one of the 5 youngest or oldest cohorts.

size estimate of -6.35 grams implies that the 1960 cohort has given birth on average to 76.2 grams lighter infants, than they would have if the pre-1948 cohort trend had continued. More strikingly, the share low birth weight estimated trend break size of .23 implies that the 1960 cohort had a low birth weight rate near 2.8 percentage points higher than they would had the earlier cohort trend continued. The low birth weight rate nationally in 1975 was only 7.4 percentage points. Therefore this effect of the health decline by the 1960 cohort is on the order of 40 percent. For men’s log mortality the estimated size of the trend break is .028 and for women’s log mortality it is .029, implying the single-year mortality risk for the 1960 cohort was roughly *1.5 times* what it would have been had the pre-1947/1949 trend continued.

For all six outcomes, I fail to reject null hypothesis of no break in the cohort trend at a very low significance level, using the bootstrap based test described in Hansen (1996, 2000).

Appendix Table 1 shows that these estimates are generally quite robust to different specifications of the age-by-year control function.

5. Role of cohort decline in year-over-year declines

I use the cohort trend break estimates from the previous section to quantify the role of the cohort decline in driving year-over-year declines. The results are dependent on the model, and therefore do not allow for (potentially plausible) interactions between cohort human capital and period-specific factors such as labor demand or opioid supply.

Using the parameter estimates from the cohort trend break models above, I create a counterfactual dataset, where each observation takes the following form: $\tilde{Y}_{apc} \equiv Y_{apc} - \mathbb{1}_{c \geq \hat{\lambda}} \cdot \hat{\delta} \cdot (c - \hat{\lambda})$. I calculate summary measures of each outcome by year using the raw data, Y_{apc} , and the transformed data, \tilde{Y}_{apc} , in which the estimated cohort trend break has been removed.

Figure 4, Panel A shows results for the low birth weight rate. The raw series shows that while the low birth weight rate declined from above 8 percent in 1968 to 6.7 percent by 1985, it then reversed trend and increased to near 7.3 percent by 1995 (Centers for Disease Control, 1994). In contrast, the transformed series in which the cohort trend break has been removed exhibits no similar increase. The series implies that absent the cohort health decline, the low birth weight rate would have declined much more rapidly until 1985 — falling to 5.7 percent by that year. These improvements would have slowed after the mid-1980s — but the low birth weight rate would have continued to decline slowly until 1995 to just below 5.5 percent.

Panel B shows similar results for men’s wages. In particular it shows the average across men age 25 to 54 of median wages by single age-bins.⁸ This measure of wages declines in real terms from 1979 to 1993, from 3.14 to below 3.04. By this measure real wages declined from 23 to

⁸If log wages are assumed to take a symmetric distribution then this provides an estimate of age-adjusted mean log wages.

below 21 dollars an hour, in constant 2014 dollars. In contrast the transformed log wage series, in which the cohort trend break is removed, increases very slightly from 3.16 in 1979 to 3.17 in 1993.

Panel C and D show analogous results for the age-adjusted mortality rates of men and women age 45 to 54, the ages focused on in Case and Deaton (2015). The raw data show declines in men’s and women’s midlife mortality between 1975 and 1990. A “bump” is then evident during the AIDS epidemic, larger for men. Both series then exhibit a clear change in trend near 1999 and slowing improvements in the mortality rate after that year. For men the mortality rate even increases between 1999 and the mid-2000s. The change in trend is completely absent in the transformed series in which the cohort trend break in log mortality is removed. That is, the decline in cohort health can completely explain the stagnation — and for men slight increase — in mortality after 1999. Panels E and F show the mortality rates of older men and women, aged 55 to 64. Mortality for both sexes declines between 1975 and around 2010, before changing trend and increasing. Again the change in trend is absent in the counterfactual series in which the cohort trend break is removed — suggesting the mortality increases for this group are also plausibly driven by the cohort health decline.

6. Towards a cause

A number of results suggest the cohort decline likely originated in childhood or adolescence. High school completion declined for these cohorts (shown in Appendix Figures 1 and 2). Standardized test scores taken as early as 16 declined for the same cohorts — part of a widely noted decline in standardized test scores beginning in the late 1960s (eg. National Commission on Excellence in Education, 1983; Koretz, 1987). Appendix Figure 3 shows that SAT scores decline beginning with approximately the 1947 cohort. Similar declines occurred in other achievement tests, including nearly universal tests in select states (Harnischfeger and Wiley, 1975). Appendix Figure 4 and Appendix Tables 2 and 3 show evidence that the cohort decline in health and human capital is concentrated among the native born and not evident for foreign-born US residents. In Reynolds (2023a) I show that the onset of puberty appears to have been delayed for approximately the same cohorts.

The cohort decline was widespread across demographic groups born in the US. Appendix Tables 4 and 5 show for most outcomes it is evident across racial groups and across Census Regions (the one exception being black men’s mortality). Reynolds (2023a) shows further evidence of the decline’s wide geographic reach.

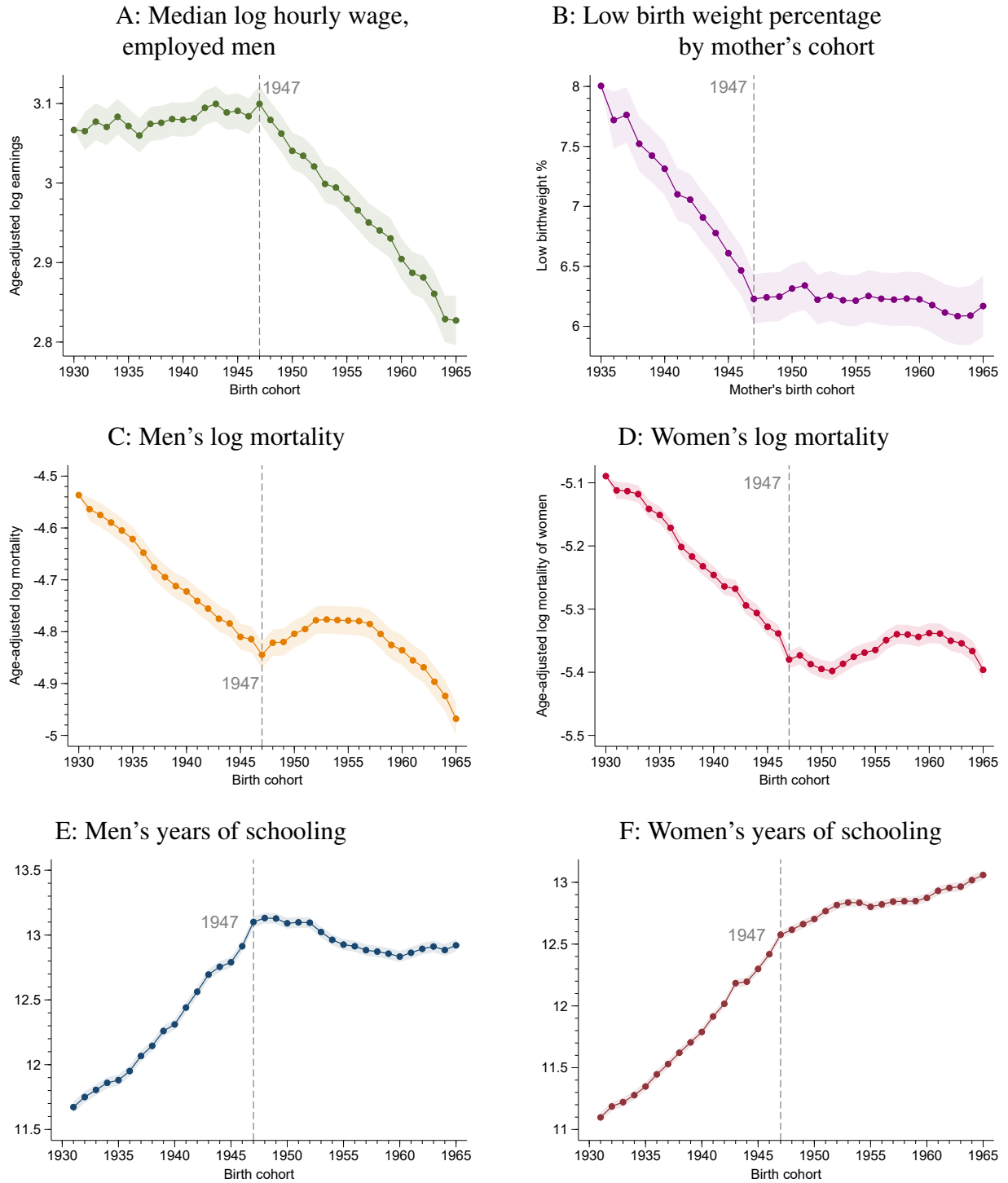
A number of ex ante plausible hypotheses appear unlikely. These cohorts observable family background and childhood residence locations did not change in ways that appear likely detrimental. Prior improvements in infant mortality, suddenly slowed in the 1950s, making a single-index mortality selection explanation appear unlikely. Changes in maternal smoking and exposure to a

number of common pollutants also appear unlikely causes. See Reynolds (2023a,b).

Easterlin (1987) famously predicted that Baby Boomers would be negatively affected by their large cohort size. The simplest version of such a theory cannot explain the cohort decline. Cohort size increased dramatically in 1946 and 1947, but these cohorts health and human capital was still *improving*. However, if cohort size has more complex a lagged effect, such a theory could be potentially relevant. Average birth order began to increase after 1946, but initial calculations suggest the effect of this change would be too small to explain the cohort decline Reynolds (2023b).

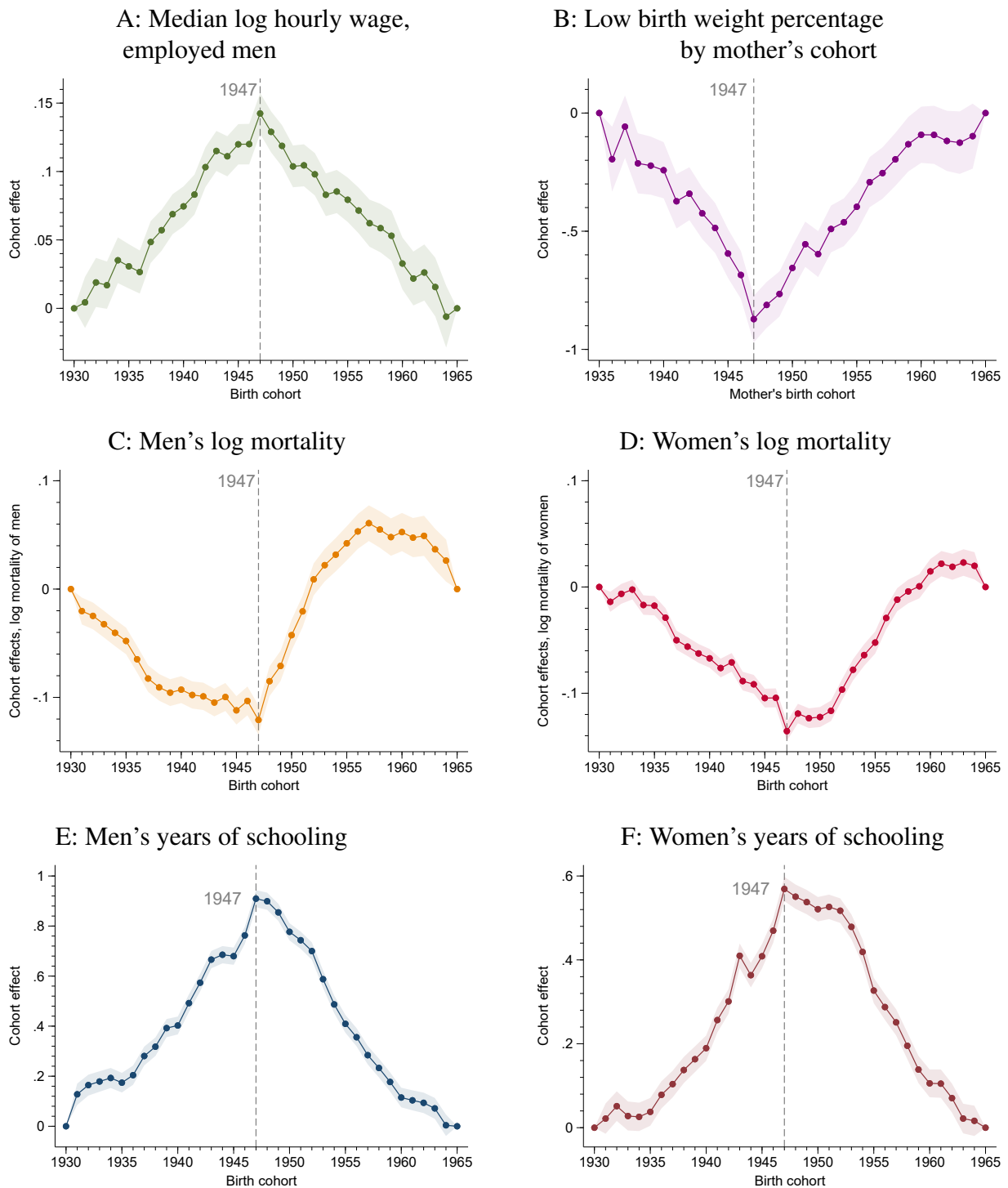
One potentially promising candidate cause is lead pollution from motor vehicle exhaust. Motor vehicle use increased rapidly after 1945 and McFarland et al. (2022) estimate that the share of children with blood lead content above the 2015 threshold for “clinical concern” increased from 50 percent for the 1940-45 cohorts to 100 percent for the 1966-75 cohorts.

Figure 1: Age-adjusted outcomes by birth cohort



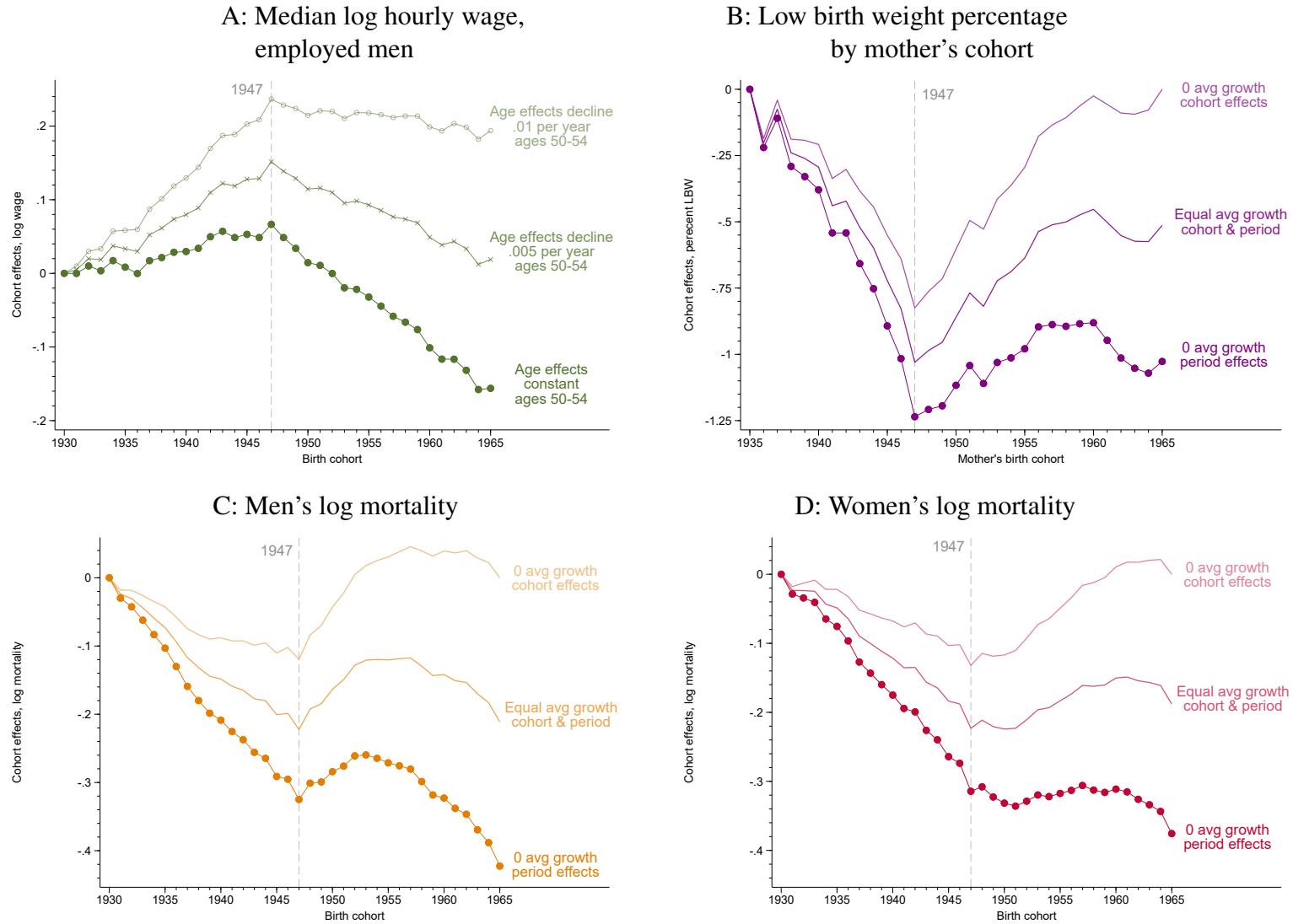
Each panel plots age-adjusted outcomes by birth cohort. Age adjustment is done by regressing the outcome on a full set of cohort fixed effects and age effects, and plotting predicted values. Panel A is based on CPS-MORG data, 1979-1993, and includes men age 25-54, who were born from 1930 to 1965. Panel B is based on vital statistics natality microdata, 1968-1990, mothers age 18-40, who were born from 1935-1965. Panel C and D are based on data from the Human Mortality Database, and include 1975-2019, ages 25-85, cohorts born from 1930 to 1965. Panels E and F are based on CPS-MORG data, 1979-2016, and includes men and women aged 25-75, who were born from 1930 to 1965.

Figure 2: Detrended cohort effects from additively separable age-period-cohort models



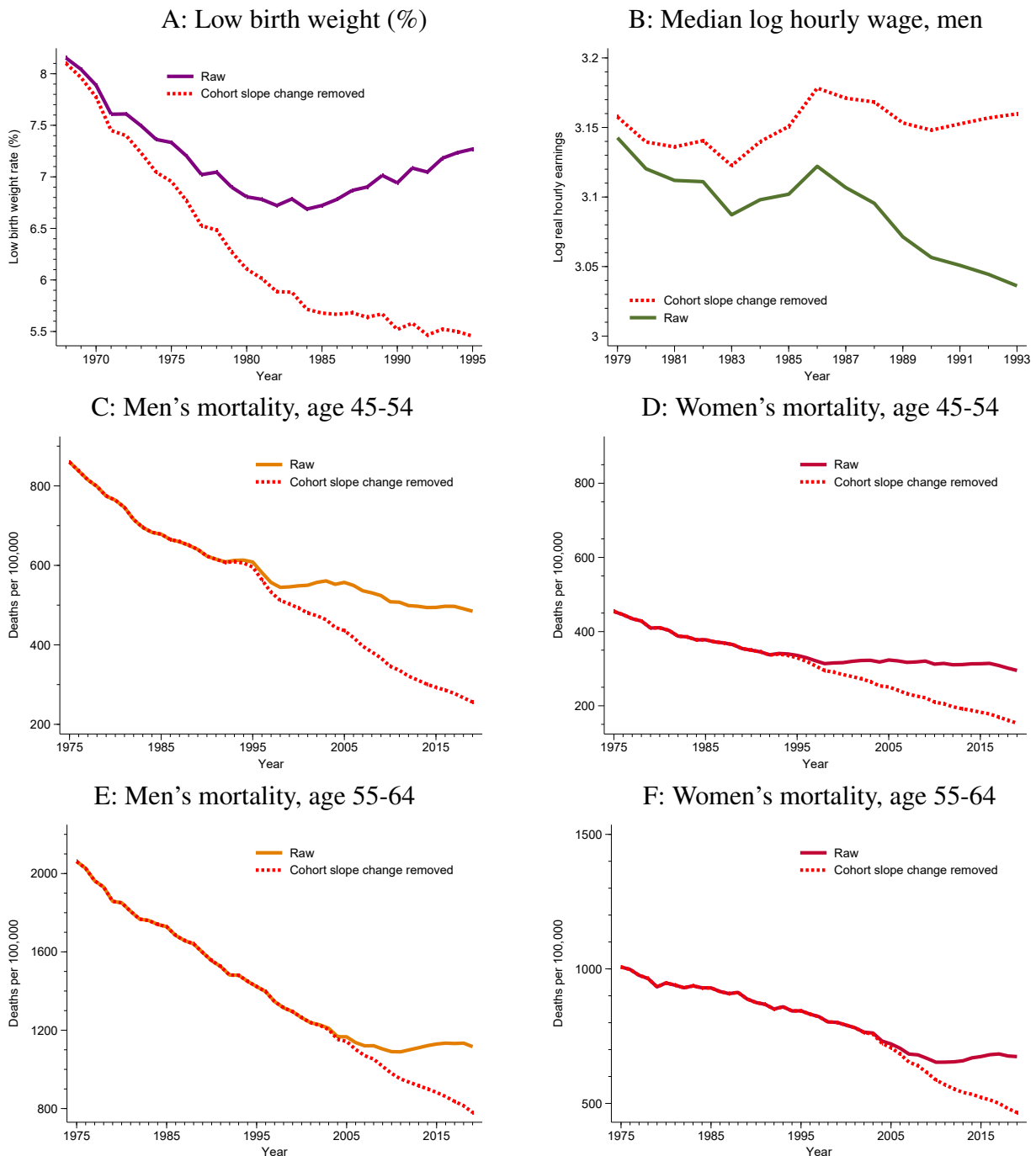
Each panel plots detrended cohort effects from estimation of age-period-cohort models based on Equation 2. Data sources and sample restrictions for each outcome are the same as in Figure 1.

Figure 3: Cohort effects under different assumptions,
additively separable age-period-cohort models



Each panel plots cohort effects from estimation of age-period-cohort models based on Equation 2. Each series shows cohort effects under different assumptions on the age effects, or the average growth in period/cohort effects. See Section 5b. Data sources and sample restrictions for each outcome are the same as in Figure 1.

Figure 4: Impact of cohort decline on year-over-year declines



This figure shows counterfactual year-over-year trends in 4 outcomes had the trend break in cohort effects not occurred, ie. if the pre-break cohort trend had continued. The trend break estimated based on the model in Equation 3 is subtracted from each observation. These transformed data are then used to calculate each of the listed outcomes, which are plotted as the dashed red line. The same outcome based on the untransformed, raw data is plotted as a solid line. Panel A shows the percent of infants born at low birth weight. Panel B shows the average across men age 25-54 of median wages for single age-bins. Panels C through F show mortality rates of men and women, age-adjusted assuming a uniform population distribution by single year of age. Data sources for each outcome are the same as for Figure 1.

Table 1: Evidence of relative cohort decline — cohort trend break models with unknown break location

controlling for year FEs, age FEs, and separate quadratic-in-age in each year

	Trend break in cohort effects		
	Size δ	Location λ	Existence p -value
<u>Intergenerational infant health</u>			
Low birth weight (%)	0.241 (0.014)	1948 [1948, 1948]	< .001
Mean birth weight (g)	-6.35 (0.35)	1948 [1948, 1948]	< .001
<u>Labor market</u>			
Median log wage, men	-0.016 (0.001)	1947 [1946, 1947]	< .001
<u>Log mortality</u>			
Men	0.028 (.001)	1947 [1947, 1947]	< .001
Women	0.029 (.001)	1949 [1949, 1949]	< .001
<u>Years of schooling</u>			
only controlling for year FEs, age FEs			
Men	-0.106 (.001)	1948 [1948, 1948]	< .001
Women	-0.074 (.001)	1950 [1950, 1950]	< .001

Each row shows the results of estimation of a model based on Equation 3, with the listed outcome as a dependent variable. All models are estimated by least squares, following the approach outlined in Hansen (1999, 2000). All models except those for years of schooling control for year fixed effects, age fixed effects, and a separate quadratic-in-age in each year. The years of schooling models only control for age FEs. The column titled “Size” reports the estimated size of the cohort break δ , with the standard error in parentheses. The column titled “Location” reports the cohort at which the trend break is estimated to occur, with a 99 % confidence interval in brackets calculated by inverting a likelihood ratio statistic. The column titled “Existence” reports a p-value from an F-type test, based on 1000 bootstrap samples, for the null hypothesis that no trend break occurs, ie. that cohort effects are linear. Intergenerational infant health results are based on vital statistics natality microdata, 1968-1995, mothers age 18-40 who were born between 1930 to 1970. Labor market results are based on CPS-MORG data, 1979-1993, and includes men age 25-54, who were born from 1930 to 1965. Log mortality results are based on data from the Human Mortality Database, and include the years 1975-2019, ages 25-85, cohorts born from 1930 to 1965. Years of schooling results are based on CPS-MORG data, 1979-2016, and includes men and women aged 25-75, who were born from 1930 to 1965.

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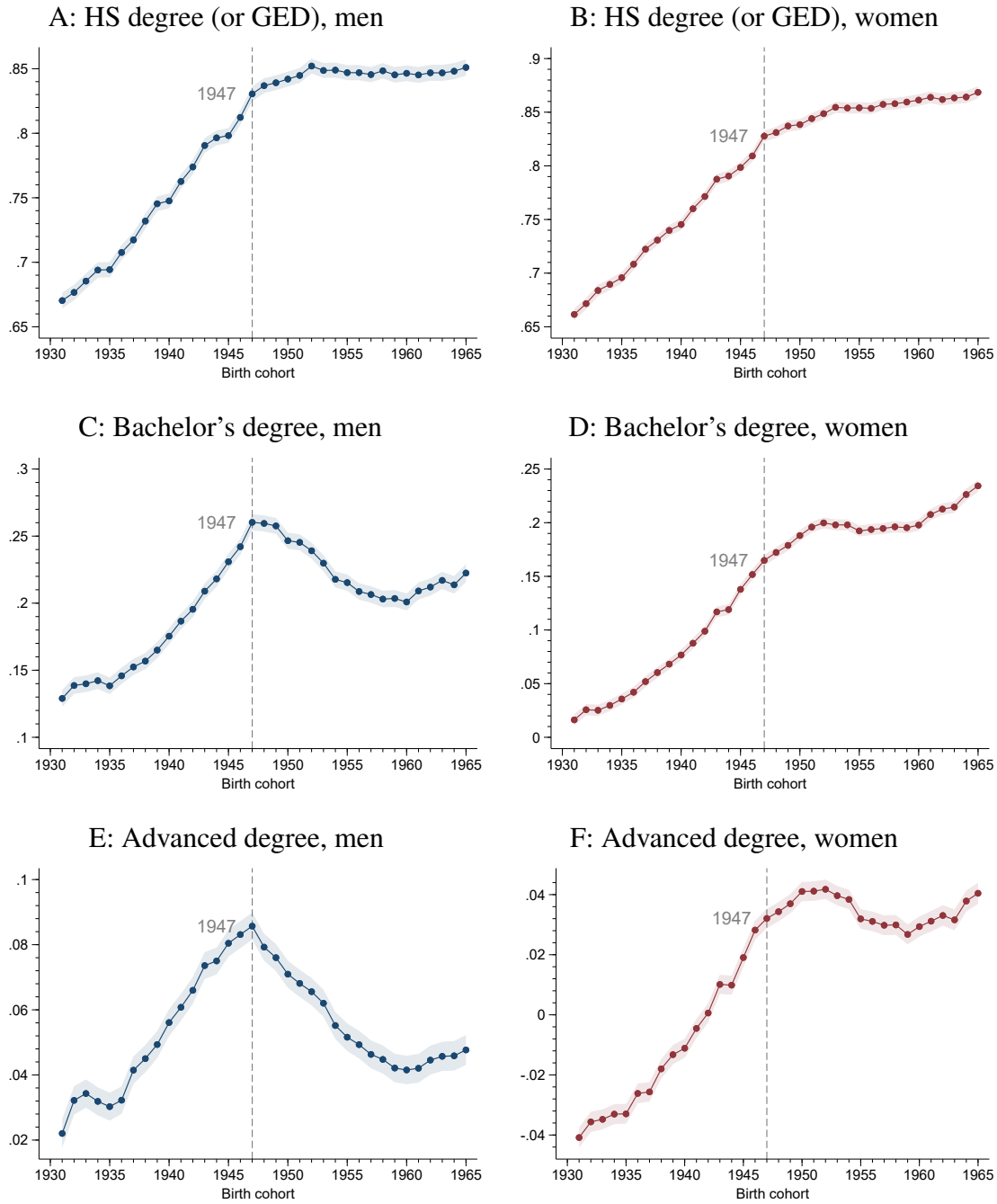
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Supplementary Appendix for:
“The Broad Decline of Health and Human Capital
of Americans Born after 1947”

by Nicholas Reynolds

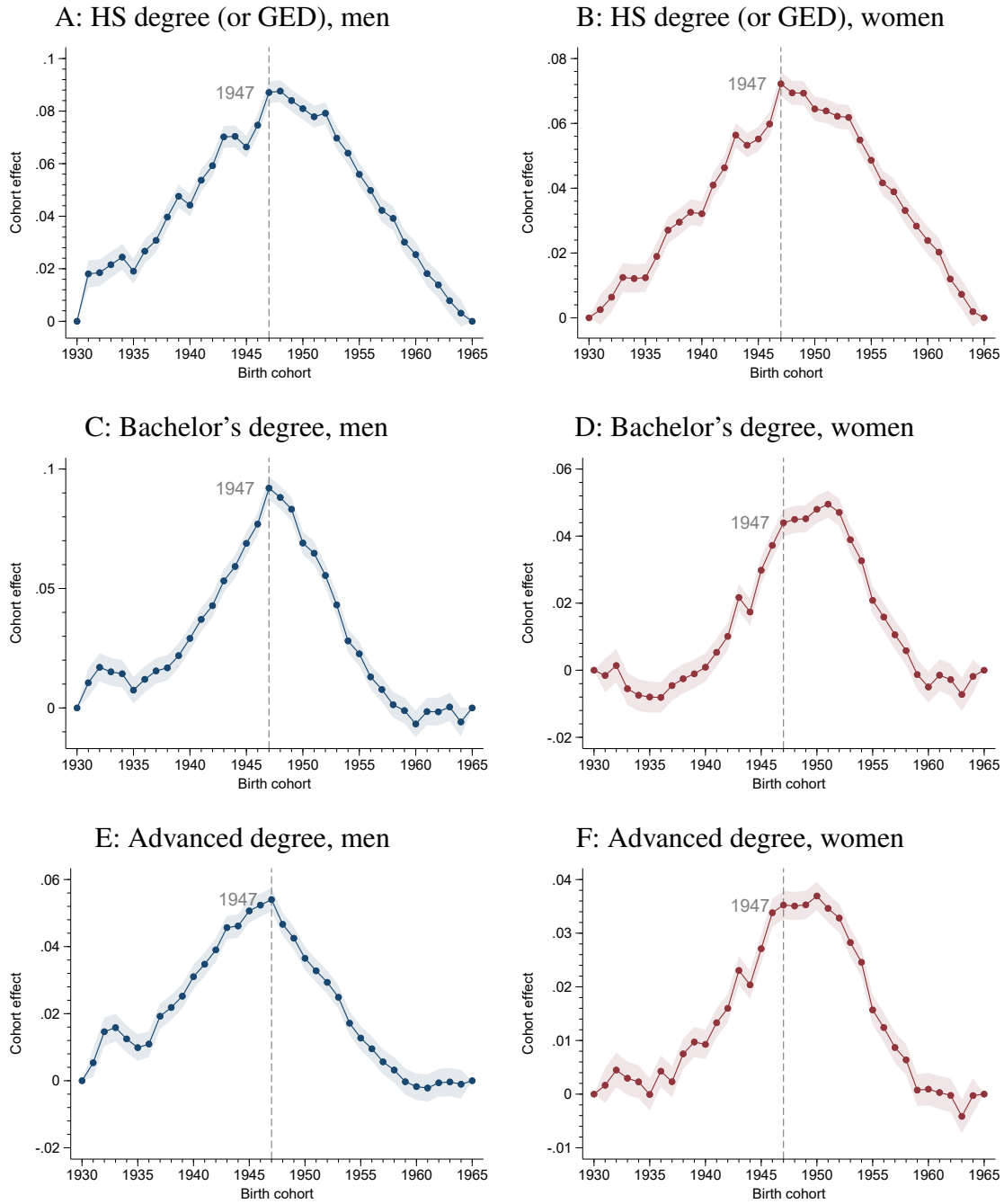
November 6, 2023

Appendix Figure 1: Age-adjusted educational attainment by birth cohort



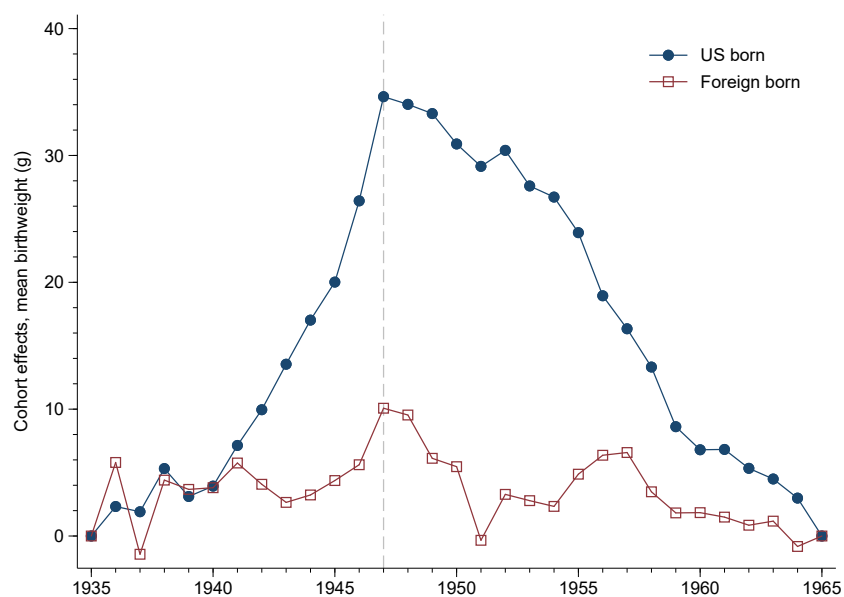
Each panel plots age-adjusted outcomes by birth cohort. Age adjustment is done by regressing the outcome on a full set of cohort fixed effects and age effects, and plotting predicted values. All panels are based on CPS-MORG data, 1979-2016, and includes men and women aged 25-75, who were born from 1930 to 1965.

Appendix Figure 2: Detrended cohort effects, educational attainment



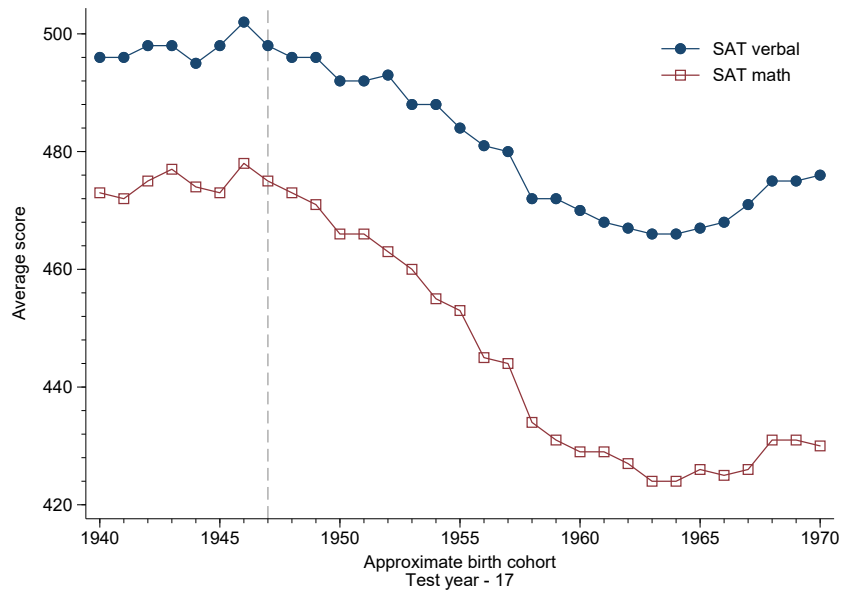
Each panel plots detrended cohort effects from estimation of age-period-cohort models based on Equation 2. All panels are based on CPS-MORG data, 1979-2016, and includes men and women aged 25-75, who were born from 1930 to 1965.

Appendix Figure 3: Detrended cohort effects, intergenerational infant birth weight, by mother's place of birth



This figure plots detrended cohort effects from estimation of age-period-cohort models based on Equation 2 — separately for mothers born in one of the 50 states or D.C., and those born outside of the US. Results are based on vital statistics natality microdata, 1970-1990, including mothers age 18-40, who were born from 1935-1965.

Appendix Figure 4: Scholastic Aptitude Test (SAT) score decline



This figure shows verbal and mathematics scores on the Scholastic Aptitude Test. Approximate birth cohort is defined as the year the test was given minus 17. Data is from Harnischfeger and Wiley (1975).

Appendix Table 1: Piecewise linear cohort effect models

robustness to varying age-by-year control function

	(1)	(2)	(3)	(4)
<u>Panel A: Mean birth weight</u>				
Size	-1.83 (0.13)	-6.35 (0.35)	-5.52 (0.54)	-6.91 (0.70)
Location	1948 [1947, 1949]	1948 [1948, 1948]	1947 [1947, 1947]	1947 [1947, 1947]
P-value for existence	< .001	< .001	< .001	< .001
<u>Panel B: Low birth weight percentage</u>				
Size	0.085 (0.003)	0.241 (0.014)	0.195 (0.022)	0.200 (0.029)
Location	1947 [1947, 1948]	1948 [1948, 1948]	1947 [1937], [1948]	1947 [1948, 1948]
P-value for existence	< .001	< .001	< .001	< .001
<u>Panel C: Median log wage</u>				
Size	-0.016 (0.0005)	-0.016 (0.001)	0.015 (0.002)	-0.017 (0.003)
Location	1947 [1947, 1947]	1947 [1946, 1947]	1953 [1946, 1947], [1952, 1953]	1947 [1947, 1947]
P-value for existence	< .001	< .001	< .001	< .001
<u>Panel D: Log mortality, men</u>				
Size	0.016 (0.001)	0.028 (0.001)	0.029 (0.001)	0.029 (0.001)
Location	1942 [1941, 1943]	1947 [1947, 1947]	1947 [1947, 1947]	1947 [1947, 1947]
P-value for existence	< .001	< .001	< .001	< .001
<u>Panel E: Log mortality, women</u>				
Size	0.017 (0.000)	0.029 (0.001)	0.021 (0.001)	-0.019 (0.002)
Location	1948 [1948, 1948]	1949 [1949, 1949]	1950 [1950, 1950]	1950 [1949, 1950]
P-value for existence	< .001	< .001	< .001	< .001
Year FEs	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes
Quadratic-age-by-year	No	Yes	No	No
Cubic-age-by-year	No	No	Yes	No
Quartic-age-by-year	No	No	No	Yes

Each column shows the results of estimation of a model based on Equation 3, with the listed outcome in single age-by-year bins as the dependent variable. All models are estimated by least squares, following the approach outlined in Hansen (2000). The row titled “Size” reports the size of the trend break in cohort effects, δ , with the standard error in parentheses. The row titled “Location” reports the estimated cohort at which a trend break occurs, with a 99 % confidence region in brackets calculated by inverting the likelihood ratio statistic. The row titled “P-value for existence” reports a p-value from an F-type test, based on 1000 bootstrap samples, for the null hypothesis that no trend break occurs, ie. that cohort effects are linear.

Appendix Table 2: Cohort decline concentrated among those born in the US — piecewise linear cohort effect models of intergenerational infant birth weight

	robustness to varying age-by-year control function			
	(1)	(2)	(3)	(4)
<u>Panel A: Mothers born in US</u>				
Size	-5.209 (0.167)	-7.292 (0.438)	-5.879 (0.650)	-7.059 (0.839)
Location	1949 [1949, 1949]	1948 [1947, 1948]	1947 [1947, 1947]	1947 [1947, 1947]
P-value for existence	< .001	< .001	< .001	< .001
<u>Panel B: Mothers born outside US</u>				
Size	1.888 (0.351)	-1.669 (0.815)	-3.213 (1.194)	6.565 (1.591)
Location	1943 [1941, 1947]	1947 [1938, 1962]	1947 [1938, 1962]	1951 [1951, 1951]
P-value for existence	< .001	.139	.096	.041
Year FEs	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes
Quadratic-age-by-year	No	Yes	No	No
Cubic-age-by-year	No	No	Yes	No
Quartic-age-by-year	No	No	No	Yes

Each column shows the results of estimation of a model based on Equation 3, with the listed outcome in single age-by-year bins as the dependent variable. All models are estimated by least squares, following the approach outlined in Hansen (2000). The row titled “Size” reports the size of the trend break of cohort effects, δ , with the standard error in parentheses. The row titled “Location” reports the estimated cohort at which a trend break occurs, with a 99 % confidence region in brackets calculated by inverting the likelihood ratio statistic. The row titled “P-value for existence” reports a p-value from an F-type test, based on 1000 bootstrap samples, for the null hypothesis that no trend break occurs, ie. that cohort effects are linear. Results are based on vital statistics natality microdata, 1970-1995, mothers age 18-40 who were born between 1930 to 1970.

Appendix Table 3: Trend break in cohort effects for share of employed men working in white-collar occupations — native-born versus foreign-born

robustness to varying age-by-year control function				
	(1)	(2)	(3)	(4)
<u>Panel A: Born in US</u>				
Size	-0.007 (0.0004)	-0.017 (0.0009)	-0.017 (0.002)	-0.014 (0.002)
Location	1946 [1944, 1946]	1946 [1946, 1946]	1946 [1946, 1946]	1946 [1946, 1947]
P-value for existence	< .001	< .001	< .001	< .001
<u>Born outside US</u>				
Size	-0.008 (0.001)	0.027 (0.005)	0.031 (0.008)	0.018 (0.006)
Location	1941 [1940, 1943]	1932 [1932, 1933], [1943, 1944]	1932 [1932, 1933]	1956 [1932], [1936, 1937], [1956, 1957]
P-value for existence	< .001	< .001	< .001	0.007
Year FEs	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes
Quadratic-age-by-year	No	Yes	No	No
Cubic-age-by-year	No	No	Yes	No
Quartic-age-by-year	No	No	No	Yes

Based on Integrated Public Use Microdata Samples from the Decennial Censuses of 1970, 1980, 1990 and 2000 (?). The share of employed men in white-collar occupations corresponds to the “managers /professionals /technicians /finance /public safety” category in ?, and I use the occupational crosswalk from these authors to obtain consistent occupational categories.

Each column shows the results of estimation of a model based on Equation 3, with the the share of employed men in white-collar occupations as the dependent variable in single age-by-year bins as the dependent variable. All models are estimated by least squares, following the approach outlined in Hansen (2000). The row titled “Size” reports the size of the trend break of cohort effects, δ , with the standard error in parentheses. The row titled “Location” reports the estimated cohort at which a trend break occurs, with a 99 % confidence region in brackets calculated by inverting the likelihood ratio statistic. The row titled “P-value for existence” reports a p-value from an F-type test, based on 1000 bootstrap samples, for the null hypothesis that no trend break occurs, ie. that cohort effects are linear.

Appendix Table 4: Evidence of cohort decline across racial groups — piecewise linear cohort effect models
controlling for age FEs and year FEs

		(1) White	(2) Black	(3) Hispanic	(4) Other races
Median log wage	<i>Size</i>	-0.0130 (0.0005)	-0.0251 (0.0018)	-0.0133 (0.0023)	-0.0143 (0.0036)
	<i>Location</i>	1946 [1945, 1947]	1948 [1947, 1949]	1949 [1946, 1953]	1949 [1936, 1953]
Low birth weight (%)	<i>Size</i>	0.10 (0.005)	0.08 (0.01)		0.07 (0.03)
	<i>Location</i>	1949 [1948, 1949]	1947 [1945, 1949]		1950 [1938, 1957]
<u>Log mortality</u>					
Men	<i>Size</i>	0.0235 (0.0003)	-0.0294 (0.0023)		.0103 (0.0012)
	<i>Location</i>	1944 [1944, 1945]	1956 [1954, 1956]		1945 [1943, 1947]
Women	<i>Size</i>	0.0218 (0.0005)	0.0125 (0.0007)		0.0131 (0.0024)
	<i>Location</i>	1950 [1950, 1950]	1945 [1944, 1947]		1953 [1948, 1956]

Each column shows the results of estimation of a model based on Equation 3, with the listed outcome as a dependent variable. All models are estimated by least squares, following the approach outlined in Hansen (2000). Intergenerational infant health results are based on vital statistics natality microdata, 1968-1995, mothers age 18-40 who were born between 1930 to 1960. Labor market results are based on CPS-MORG data, 1979-1993, and includes men age 25-54, who were born from 1930 to 1960. Log mortality results are based on data from the Human Mortality Database, and include the years 1975-2019, ages 25-85, cohorts born from 1930 to 1960. The column titled “Size” reports the estimated size of the cohort break δ , with the standard error in parentheses. The column titled “Location” reports the cohort at which the trend break is estimated to occur, with a 99 % confidence interval in brackets calculated by inverting a likelihood ratio statistic. The row titled “Existence” reports a p-value from an F-type test, based on 1000 bootstrap samples, for the null hypothesis that no trend break occurs, ie. that cohort effects are linear.

Appendix Table 5: Evidence of cohort decline across Census Regions — piecewise linear cohort effect models

		(1) Northeast	(2) Midwest	(3) South	(4) West
Median log wage	<i>Size</i>	-0.0170 (0.0009)	-0.0194 (0.0013)	-0.0139 (0.0010)	-0.0149 (0.0012)
	<i>Location</i>	1947 [1946, 1947]	1948 [1947, 1949]	1946 [1945, 1947]	1947 [1946, 1948]
Low birth weight (%)	<i>Size</i>	0.12 (.01)	0.11 (.01)	0.13 (.01)	0.09 (.01)
	<i>Location</i>	1947 [1946, 1947]	1949 [1948, 1950]	1948 [1947, 1948]	1948 [1946, 1949]
<u>Log mortality</u>					
Men	<i>Size</i>	0.0284 (0.0016)	0.0287 (0.0014)	0.0341 (0.0012)	0.0286 (0.0015)
	<i>Location</i>	1946 [1946, 1946]	1946 [1946, 1946]	1946 [1946, 1946]	1946 [1946, 1946]
Women	<i>Size</i>	0.0320 (0.0019)	0.0288 (0.0019)	0.0331 (.0016)	0.0269 (0.0028)
	<i>Location</i>	1948 [1948, 1949]	1950 [1950, 1950]	1950 [1949, 1950]	1949 [1949, 1949]

Each column shows the results of estimation of a model based on Equation 3, with the listed outcome as a dependent variable. All models are estimated by least squares, following the approach outlined in Hansen (2000). Wages and birth weight include controls for age fixed effects and year fixed effects — mortality also includes a separate quadratic-in-age in each year. The column titled “Size” reports the estimated size of the cohort break δ , with the standard error in parentheses. The column titled “Location” reports the cohort at which the trend break is estimated to occur, with a 99 % confidence interval in brackets calculated by inverting a likelihood ratio statistic. The row titled “Existence” reports a p-value from an F-type test, based on 1000 bootstrap samples, for the null hypothesis that no trend break occurs, ie. that cohort effects are linear. Intergenerational infant health results are based on vital statistics natality microdata, 1968-1995, mothers age 18-40 who were born between 1930 to 1970. Labor market results are based on CPS-MORG data, 1979-1993, and includes men age 25-54, who were born from 1930 to 1965. Log mortality results are based on data from the Human Mortality Database, and include the years 1975-2019, ages 25-85, cohorts born from 1930 to 1965.