

Is life expectancy really falling for low SES groups? Lagged selection bias and artifactual trends in mortality

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Abstract

Recent public health studies have made headlines by reporting that for some disadvantaged subpopulations in the U.S, recent period mortality rates are higher and life expectancy lower today compared to period rates from the past. In misleading syntax, this pattern has been described as a “fall” or “decline” in life expectancy. We illustrate how this important syntactic choice is akin to comparing average temperatures for the entire U.S in 1990 to only those for Alaska in 2010, and describing the difference as a “decline.” We explicate dynamics underlying what we call “Lagged Selection Bias” (LSB), and illustrate why comparing the same measure from two different periods in the presence of LSB does not actually identify a trend. If there are large and stable social disparities in health, 2010 life expectancy in a more disadvantaged group can be lower than 1990 life expectancy—even though *everyone lives longer* in the later period. We argue that it is long past time to examine an alternative—and arguably more plausible—interpretation of patterns in period mortality and period life expectancy. We urge more explicit attention to LSB and its dynamics among researchers interpreting changes in period measures stratified by subgroups whose composition is changing, and discuss approaches that have been used to manage it.

Recent public health studies have made headlines (Cowley, 2013; Tavernise, 2012, 2013), reporting that for some subpopulations in the United States, mortality rates have been higher and life expectancies lower for recent compared to earlier time periods (Kindig & Cheng, 2013; J. K. Montez, Hummer, Hayward, Woo, & Rogers, 2011; Jennifer Karas Montez & Zajacova, 2013b; Olshansky et al., 2012). These patterns have been described in both popular and academic discourse as a “rise” in mortality and a “decline” in life expectancy. We suggest that it is long past time to admit an alternative—and arguably more plausible—interpretation of these patterns. The fact that a measure was computed for two different periods does not, by itself, make the difference between them a *trend*. Imagine if researchers measured the average temperature for the whole United States a decade ago, and then for only Alaska this year, and found the former number to be lower than the latter. Would it be appropriate to say that average temperatures had “declined” over the decade? We argue that it would not, and that it is likewise not appropriate to be describing many of the observed differences in subgroup life expectancy or mortality as “trends.”

Nevertheless, scholars and journalists alike have quickly adopted this “trend” conclusion and given short shrift to an alternative interpretation that we find far more plausible. Here, we make the case that this alternative explanation should be the subject of serious empirical investigation and discussion in scholarly and public discourse, rather than getting the treatment it gets now—which is a cursory dismissal relegated to the “limitations” sections of academic papers. We take seriously the reality of health disparities in the U.S. and the research and policy attention that they demand; understanding and redressing these disparities requires that trends be accurately characterized.

Our concern is that **stable differences between noncomparable subgroups are being mistaken for time trends in a broader group**—a phenomenon we term **Lagged Selection Bias (LSB)**. LSB generally occurs when 1) there is a temporal lag between when individuals are selected into their exposure group and when the outcome manifests, and 2) the dynamics of selection into the exposure group were changing over the period spanned by the lag. Due to this temporal lag, the *implications* of changing selection dynamics only become manifest long after the changes have happened. When these conditions apply, it is impossible to tell from the data alone whether contemporary differences in the outcome indicate contemporary trends, or changes from a long time ago in the exposure selection process, or some complicated *combination* of these two. A proper interpretation, therefore, must be explicitly based on the history of the selection process itself.

To take a very specific example, we focus on recent provocative papers (Kinsey, Jemal, Liff, Ward, & Thun, 2008; Meara, Richards, & Cutler, 2008; Jennifer Karas Montez & Berkman, 2014; J. K. Montez et al., 2011; Jennifer Karas Montez & Zajacova, 2013b; Olshansky et al., 2012) that have analyzed life expectancy or age-standardized mortality over time, stratified by high school completion. These papers have explicitly interpreted differences as “trends.” Other papers have built on this “trend” interpretation, and jumped to conclusions about health impacts of contemporaneous trends in employment, health behaviors, or social policies (Jennifer Karas Montez & Berkman, 2014; Jennifer Karas Montez & Zajacova, 2013a).

These “trend” interpretations gloss over important differences between demographic constructs like period life expectancy and real-world outcomes like the average length of life. They also give short shrift to two critical facts of the social history of the United States:

1.) Lag between exposures and outcome. High school completion in the U.S. has historically been determined by the time a person is in his or her early 20s (Cameron & Heckman, 1991), long before the ages when most mortality is observed.

2.) Secular change in the dynamics of selection into the exposure group. Access to high school increased dramatically over the 20th century in the U.S. (Figure 1). The average white girl born at the end of the First World War stood about a 50% chance of finishing high school by her 20th birthday; born at the end of the Lyndon Johnson administration, a 90% chance. Much of this expansion in access was driven by changes in the opportunity costs facing working class families who wanted to send their children to high school (Horan & Hargis, 1991; Walters & Briggs, 1993; Walters & O'Connell, 1988); as access expanded, it likely became more equitable.

When one computes mortality rates or life expectancy for the year 1990 for white female high school non-completers, one ascribes to those reaching age 70 the risk of dying that was faced by a woman who was excluded from high school around the end of the Great Depression. By contrast, the same exercise in 2010 involves ascribing mortality risk at age 70 based on the experience of a woman excluded from high school in the late 1950s/early 1960s—which means, she was left behind during a period of unprecedented expansion in access to secondary education.

The high school completion status of those dying in a given year is like light from a distant star—it reflects social conditions that prevailed decades prior. In terms of mortality risk, those excluded from high school in

the early part of the 20th century are not comparable to those excluded from high school a generation later. Those left behind by the 1960s likely had childhoods that were more disadvantaged along many dimensions, and so were at higher mortality risk all along. Life expectancy among high school non-completers in 1990 will largely be determined by the mortality experience of the relatively lower-risk subgroup; in 2010, it will be determined by the mortality experience of the higher-risk subgroup. Describing differences between these two subgroups as a “decline” in the life expectancy of high school non-completers simply because they were born at different times almost certainly reflects LSB.

In recent discourse, others have acknowledged the problem of LSB while retaining the “trend” interpretation by noting that some or all of the observed “declines in life expectancy” among high school non-completers may have been “caused” or “explained” by the fact that the non-completer group became “increasingly disadvantaged” over the period of analysis (Olshansky, 2013). The words “decline,” “cause,” and “increasingly” in this context are simultaneously true and misleading, in the same way that it would be simultaneously true and misleading to compare average temperatures in Alaska this year to the whole United States a decade ago, and then acknowledge that some or all of the observed “decline” in temperatures may have been “caused” by the fact that the land area under consideration grew “increasingly” distant from the Equator.

Lagged selection bias is always a concern that should be in the background when two different period demographic constructs are compared. But owing to the historical specifics in this case, it is likely to be especially important. To illustrate the dynamics more clearly, we have used U.S. population data and forecasts to recreate the mortality experience of 141 birth cohorts of women from 1880 to 2020. The overall risk of dying at each age for women in each cohort is based on actual and forecasted data from the U.S. Social Security Administration (F. C. Bell & Miller, 2005). They report overall risk of dying at each age for American women (of all races) born 1900, 1910, 1920, and so on up to 2000, and also make internal actuarial forecasts up to 2020; those are the risks of dying that our average simulated cohort members face. We use simple linear interpolation to fill in the years between reports, and also project backward to 1880. We also capture the social *gradients* around these average risks by randomly assigning each simulated person to one of 1001 early life socioeconomic status (EL-SES) categories and assigning those who are more (less) disadvantaged in terms of EL-SES to a higher (lower) than average risk of dying at any age. These disparities are based on patterns observed in the United States (Hayward & Gorman, 2004; Jennifer Karas Montez & Hayward, 2011); the specific gradient that we used is shown in figure 2.

We also incorporate an EL-SES gradient in access to high school reflecting historical patterns (Walters & O'Connell, 1988). In cohorts for whom high school completion is rare, it is only the most advantaged young adults who achieve it; as access expands, it also becomes more equitable. Our example rules out an increase in actual mortality risk for *anyone*—every individual stands a lower chance of dying at every age if they are born later, than they would have stood if they had been born earlier but had the exact same characteristics. For example, in our simulation the average person in the most disadvantaged quintile in 1990 dies at age 69.5 years; in 2010, at age 72.2 years (Figure 3).

One key point of this exercise is to illustrate how **a universal increase in real-world longevity like the one shown in Figure 3 does not preclude a “decline” in period life expectancy and parallel “increase” in period mortality.** This is illustrated in figures 4 and 5. Life expectancies at age 25 for the bottom quintile of the EL-SES distribution as well as for high school non-completers are shown in figure 4. When we identify disadvantage based on the stable EL-SES characteristic, period life expectancies at age 25 are 43.8 years in 1980, 44.2 years in 1990, 44.4 in 2000, and 44.7 in 2010. When we rely on people's access to high school to identify their exposure, the corresponding values are 48 years, 47.3 years, 45.3 years, and 43.4 years. Age standardized mortality rates, shown in figure 5, move inversely with period life expectancies; this reflects the fact that the two demographic constructs are computed in very similar ways.

Even though, as figure 3 illustrates, mortality disparities are *actually* large and stable in this example, figures 4 and 5 depict facts from the *same* simulated history, and seem to indicate that disparities are changing. This difference arises not because social policies are becoming *less* equitable but rather because of *improvements* in equity of access to education over the 20th century. As a result of that policy success, people who were already vulnerable to shorter lifespan because of conditions in their early lives nonetheless had access to high school, whereas those exposed to similar conditions from an earlier birth cohort had no such access. As a result, averages for high school non-completers in earlier years include the less vulnerable, as well as the very vulnerable; in the later years, the less vulnerable were simply reclassified to the high-school completer group, leaving behind only the very vulnerable. **It is that reclassification that drives the artifactual “decline” in life expectancy a half century later, not any change in anyone's actual risk of dying.**

More than simply illustrating the important distinction between real-world health outcomes like longevity and demographic constructs like period life expectancy, the exercise underlying figures 2-5 helps pin down the *timing* when we would *expect* to start seeing artifactual “trends” in the demographic constructs, given the

history of educational expansion in the United States over the 20th century. The exercise suggests that, if health disparities have been large and stable over the past century, and given trends in overall mortality risk and educational expansion over the past century, one would *expect* to see an artifactual “rise” in period age-standardized mortality rates among the least educated underway by the 1990s, and continuing for about a generation. This matches the patterns that have been reported.

Finally, figure 6 illustrates the sense in which the LSB problem is not simply a matter of *period* measures being used where *cohort* measures would be more apt. We plot average age at death for those without a high school diploma, *by birth cohort*. Those born in 1940 who did not finish high school had a date of death assigned, on average, in 1999 (at age 59); among those born in 1965, the average death was assigned for 2017 (at age 52). Parallel with the artifactual decline in *period* life expectancy is an artifactual decline in *cohort* life expectancy, which is driven entirely by the fact that for the earlier cohorts, the early life experiences of those represented in the green versus the blue dotted lines are more similar than for the later cohorts. This illustrates the point that constructing cohort life tables instead of period life tables will not solve the LSB problem, as long as the two key conditions apply (lag between exposure and outcome, and secular change in selection dynamics).

This example further allows us highlight two important forms of harm that LSB can do to policy.

- In this example, trends in disparities are mischaracterized. Claims that these patterns “indicate that increasing high school graduation rates and redesigning work-family policies may improve longevity” (Jennifer Karas Montez & Zajacova, 2013a) are inappropriate, in that they encourage advocates and policymakers to investigate the wrong time span in search of social or policy changes to be remedied. The period represented in figures 4 and 5 might look like an age of worsening disparities, when in fact it is marked by the same large, stable disparities that have prevailed for generations.
- Perhaps even more troubling, as these dynamics play out, if analysts were to continue to stratify on high school completion, disparities would appear to solve themselves. Life expectancy is about 2 years shorter using high school non-completer mortality risks from 1999 compared with 1990; by the 2010-2020 period, this “rate of decline” is only half as steep. Even if underlying health disparities remain unaddressed as the cohorts that lived through the mid 20th century die out, the “light from a distant star” will dim. This “dimming light” reflects only the stabilization of schooling access in the last quarter of the 20th century; it should not be interpreted as an indication of improvements in health equity in the first quarter of the 21st.

Lagged Selection Bias in Other Applications

There are many other applications beyond the one we chose for our detailed example where LSB can generate artifactual “trends.” For example, recent work has compared age-specific mortality rates for different periods in individual U.S. counties, and found “increases” in that risk for some counties. Kindig, et al remark, “(a)lthough we are accustomed to seeing varying rates of mortality reduction in states and nations, it is striking and discouraging to *find female mortality rates on the rise in 42.8 percent of US counties, despite increasing medical care expenditures and public health efforts.*” Migration between urban and rural areas has ebbed and flowed— in both directions— over the past 60 years, with different social patterning for different birth cohorts (Frey, 1995). Departing a rural county to move to the urban core in the late 1970s and early 1980s, for those born in the post-1955 baby boomer cohorts, may have been a sign of socioeconomic advantage; in the cohorts before and after, the opposite may be true (Fuguitt & Beale, 1996; Long & DeAre, 1988; Richter, 1985). In that sense, comparing mortality risks among those who were born in rural areas in the 1930s and aged in place to risks among those who had the same experience but were born 20 years later may generate an artifactual “trend.” What it *means* to age in place would have been different for these different cohorts. It is impossible to interpret the differences across period demographic measures, therefore, without explicitly accounting for the underlying social history of migration patterns in the United States over the 20th century.

In another application, Thun et al examined the age-specific risk of mortality for smokers compared to non-smokers in three time periods (1959-1965, 1982-1988, and 2000-2010). They found that the relative risk of mortality was higher in the later periods; they refer to that difference as an “increase” in mortality risk due to smoking, and try to identify what changed during the period of analysis to explain it. They offer plausible biological mechanisms that could explain an “increase” (for example, rises in smoking intensity), but some or all of the “trend” they report may also be an artifact of LSB. The largest fraction of habitual smokers report that they had their first cigarette somewhere between their mid-teens and early 20s—though this age at initiation has also varied by birth cohort, making the dynamics even more complicated (Anderson, Burns, Major, Vaughn, & Shanks, 2001). Furthermore, the socioeconomic patterning of smoking flipped in the 1960s and 1970s. For those born before that social transformation (and dying by the 1980s), smoking was an indication of early life socioeconomic advantage; for those born later (and dying in more recent years), it is an indication of early life *disadvantage* (Maralani, 2013). Comparing smokers dying today to smokers dying decades ago also involves comparing two groups with very different early life socioeconomic status on

average. This sort of shifting socioeconomic patterning occurs for many health risk factors as they first begin to emerge (Phelan, 2010); when it does and for generations afterward, LSB must be a central concern in any analysis of later-life outcomes.

Conclusions and Recommendations

In some circumstances where there is high risk of LSB, analysts have recognized the problem and tried to manage it. Meara et al (Meara et al., 2008), for example, aim to neutralize the problem by randomly assigning some individuals with more than high school education to the less than high school category, such that the fraction of the population labeled disadvantaged remains constant for all years where they compute life expectancies. We support this kind of approach, but as our discussion has highlighted, the risk of LSB arises not simply from changes in social patterning in the period when life expectancy is computed, but rather from changes over time that happened about a half-century prior. In order to neutralize the bias, it is not necessarily sufficient to equalize the prevalence of disadvantage from one *period* to the next in the analysis. Rather, one must ensure that the standard applied, for example, to the cohort born in 1920 when analyzing 1990 mortality is substantively the same as the one applied to the cohort born in 1940 when analyzing 2010 mortality. This is a very difficult adjustment to make properly, in part because it requires assumptions about the very thing the analysis is meant to estimate—the relationship between disadvantage and mortality. (This relates closely to problems discussed in the long and controversial literature in demography on how to disentangle age, period, and cohort effects (A. Bell & Jones; Luo, 2013; Ryder, 1965; Yang, Schulhofer-Wohl, Fu, & Land, 2008).

Ezzati et al (Ezzati, Friedman, Kulkarni, & Murray, 2008) recognize that the population composition of counties has changed over time; they try to neutralize this effect by averaging computed life expectancy for a county together with life expectancies in the counties from which recent in-migrants have arrived. This approach can address *some* important sources of migration-driven selection bias—especially for places that tend to receive people of relatively high mortality risk (e.g., places with tertiary care facilities or hospice services). It does not, however, help at all with the problem of *lagged* selection bias. In order to use an approach like theirs to manage LSB, it would be necessary to find those who moved away *many decades ago*, and aggregate their experience back with their county of origin.

Finally, Thun, et al statistically control for educational attainment in their smoking analysis, but since the meaning of a high school education is different for the cohorts at high risk of dying in 1959 compared with those dying in 2000, this adjustment is unlikely to do much to reduce the risk of LSB.

These approaches are laudable steps toward proper accounting for the risk of LSB, but none is sufficient—in part, because none takes proper explicit account of the social history of selection into the exposure groups. Nonetheless, public health research has largely ignored this important empirical issue. In the case of education especially, it is premature to close the case with a claim that “research has found that compositional changes contributed little to diverging trends in well-being and mortality” (Jennifer Karas Montez & Zajacova, 2013b); rather, we agree that “more research is needed” on this question (Jennifer Karas Montez & Berkman, 2014). This need is urgent, given the field’s rush to judgment on the meaning of observed “trends.”

In the meantime, what can scholars interested in mortality trends do? We offer three recommendations:

1. Whenever possible, identify trends by tracking the same population. For example, Preston and Wang transformed data on mortality and smoking in order to track individual birth cohorts of men and women as they aged (Preston, 2006). In doing so, they were able to identify and compare real sex-specific trends in mortality, thereby shedding light on the long-term effects of smoking in early life.
2. When different populations must be compared, stratify on characteristics that have been stable for at least a human lifespan, or that are not effectively fixed at an early point in the life course. It is methodologically possible to compute measures of life expectancy over time for subgroups like high school non-completers or residents of small geographic areas in the same way that one might compute them for men or women. However, when doing so, it is nearly impossible to interpret observed differences.
3. Stratify with *explicit* reference to social history and with a clear, articulated grounding in both social and biological theory. When in the life course were the stratification criteria most likely to have been determined? How does that translate to calendar time, and to relevant social history? Did the timing or social patterning shift across birth cohorts? Given what is known about the biology of the outcome, how might this affect estimation results, and in what calendar years would these effects be observed? Drawing on what is known in biology and social science, it may be possible to evaluate to what extent we might be observing “light from a distant star”—that is, the long reach of social conditions that prevailed decades previous. If the relevant social and demographic dynamics are well documented (as in the case of education), then a simulation along the lines that we have used here to generate our example might help to shed light on the extent and nature of the risk of LSB.

In general, our objective has been to urge caution in interpretation of analyses that are at high risk of LSB. If an outcome is measured on non-comparable populations, then the mere fact that the measures are

computed for two different periods does not make the difference between them a “trend,” and the blithe misuse of words like “fall” or “decline” is likely to do more harm than good for public health.

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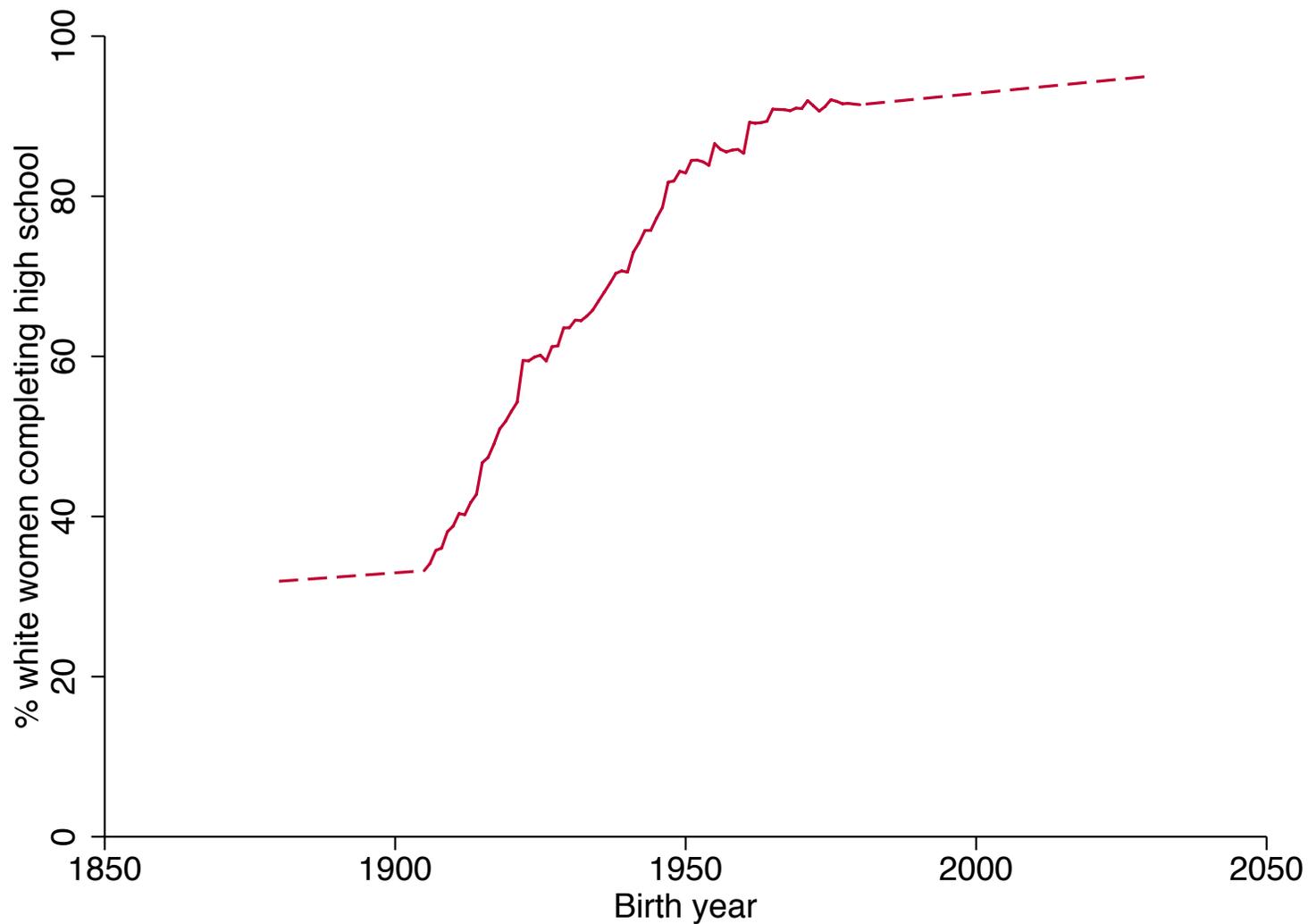
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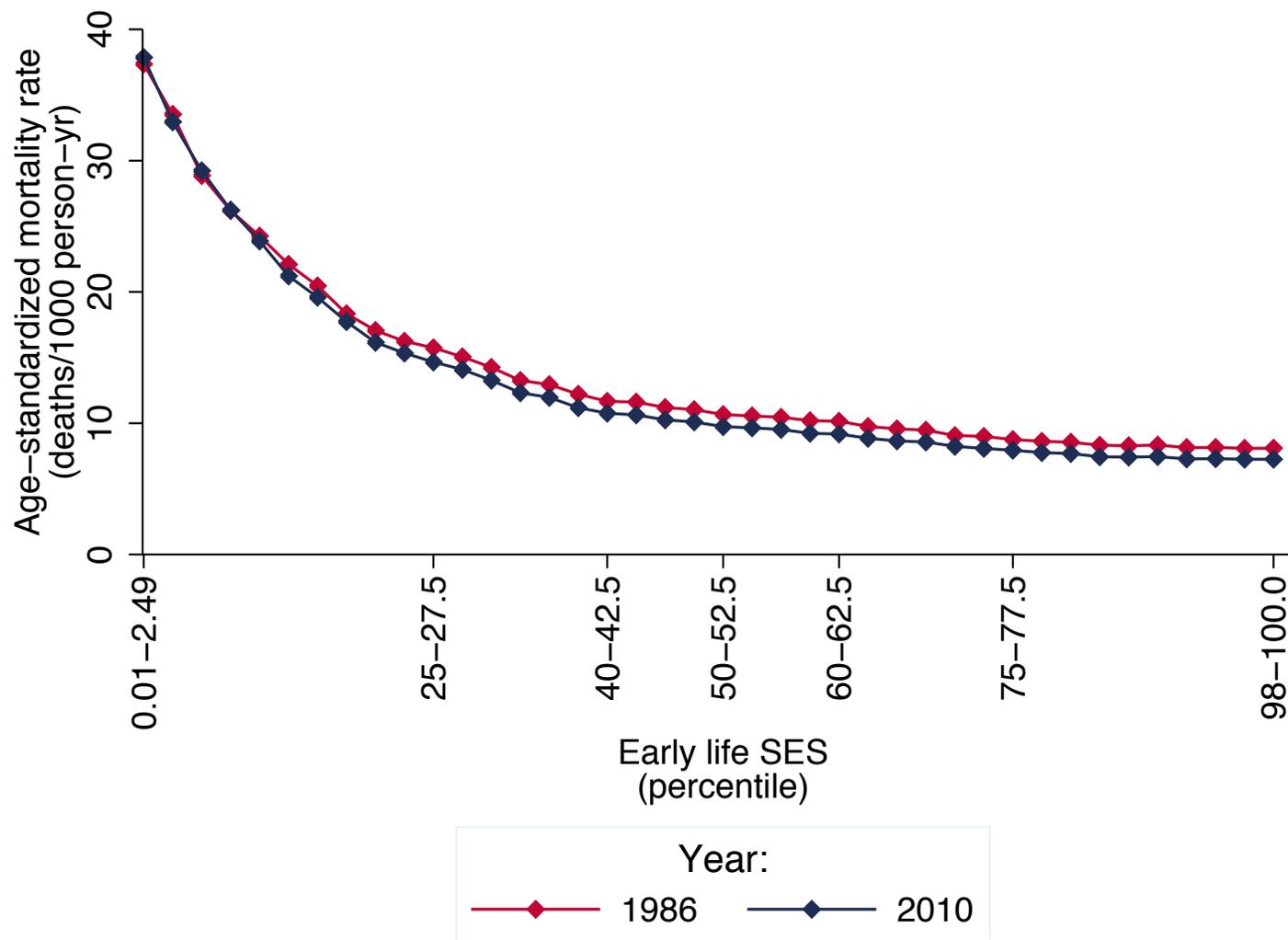
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Figure 1. High School Completion Prevalence: White women, by birth cohort



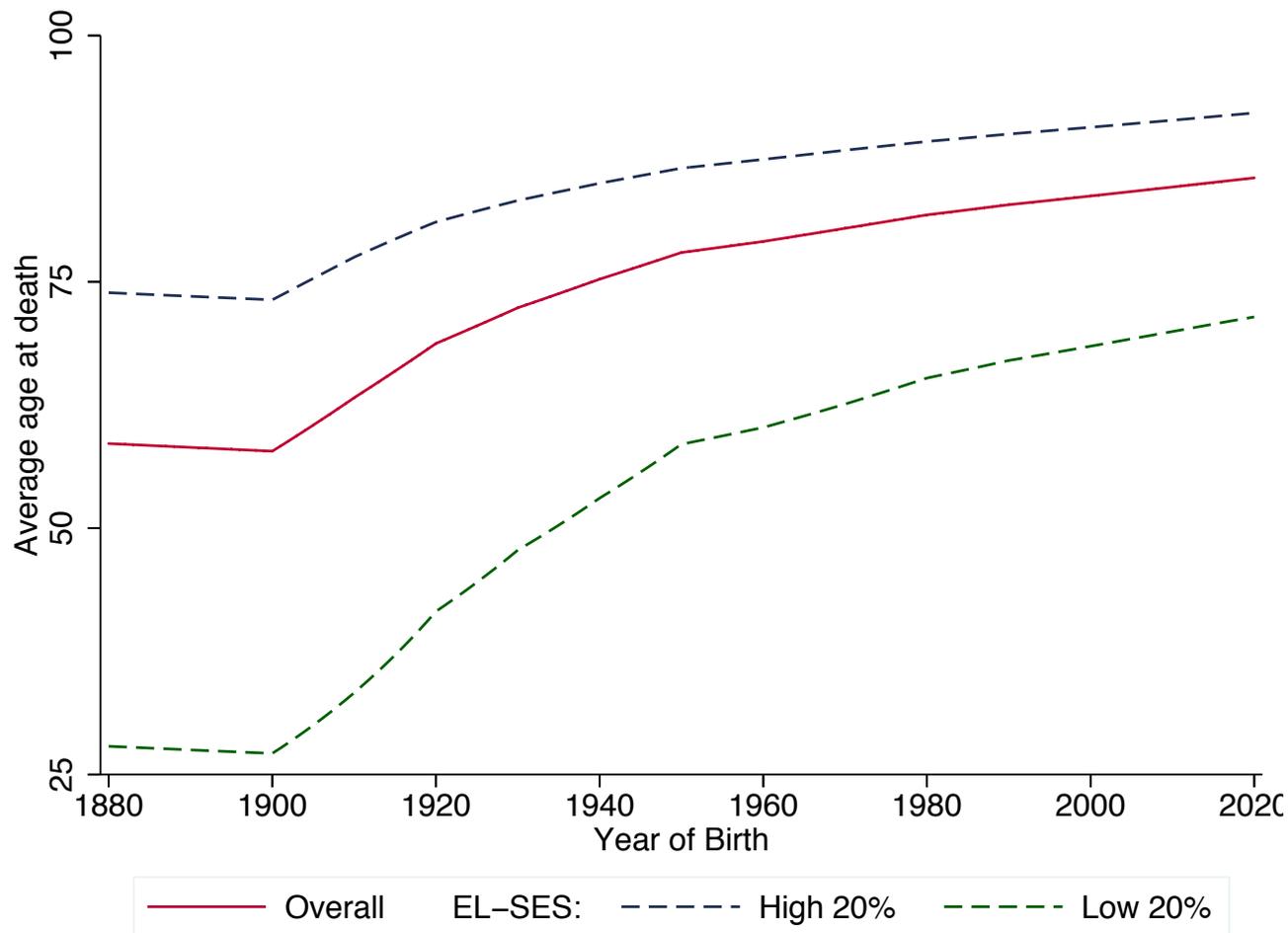
Notes for figure 1: Data source: U.S. Census, Integrated Public Use Microsamples, 1% samples for 1940, 1950, 1960, 1970 censuses, and 5% samples for 1980, 1990, and 2000. Dotted lines indicate birth cohorts for which census data is not available; we used assumptions to fill in those probabilities.

Figure 2. Simulation: Mortality gradients across early life socioeconomic status



Notes for figure 2: We simulated the educational and mortality experience of 141 birth cohorts (1880 to 2020); each cohort consists of 1 million hypothetical people. Overall mortality risk for each cohort at each age were based on observed real-world patterns. The socioeconomic patterning in that risk was set based on observed disparities in mortality risk. These curves illustrate the gradients in mortality that operate in our example; age-standardized mortality rates are about 1.8 times higher for those who were exposed to lower-than-median socioeconomic status in early life, compared with those who were exposed to higher-than-median socioeconomic status.

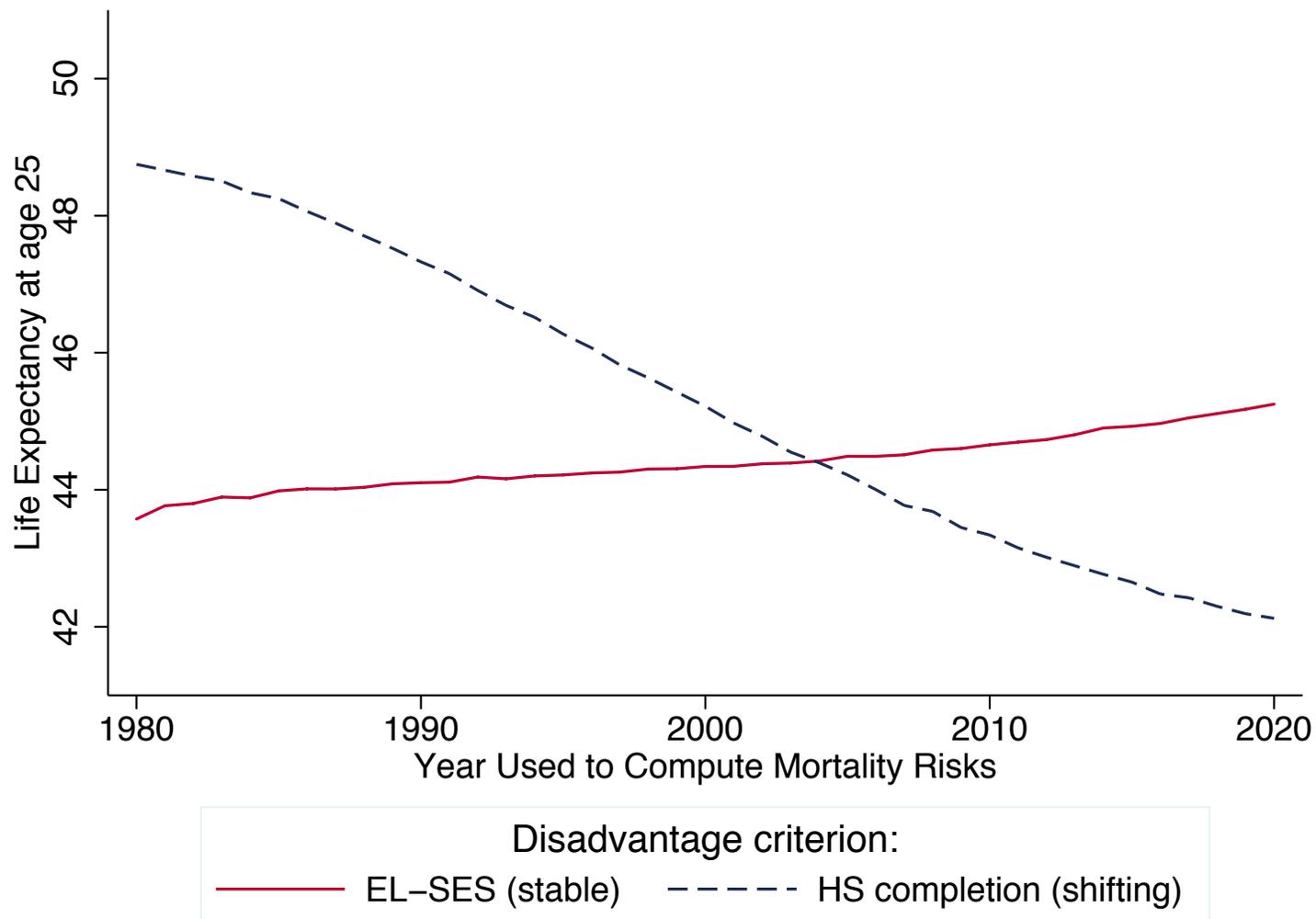
Figure 3. Simulation: Cohort life expectancy, by birth year and early life socioeconomic status



Notes for figure 3: We simulated the educational and mortality experience of 141 birth cohorts (1880 to 2020); each cohort consists of 1 million hypothetical people. Overall mortality risk for each cohort at each age were based on observed real-world patterns. Those parameters determine the placement and shape of the solid red line. The socioeconomic patterning in that risk was set based on observed disparities in mortality risk (see figure 2); those parameters determine the placement of the dotted lines relative to the solid red line.

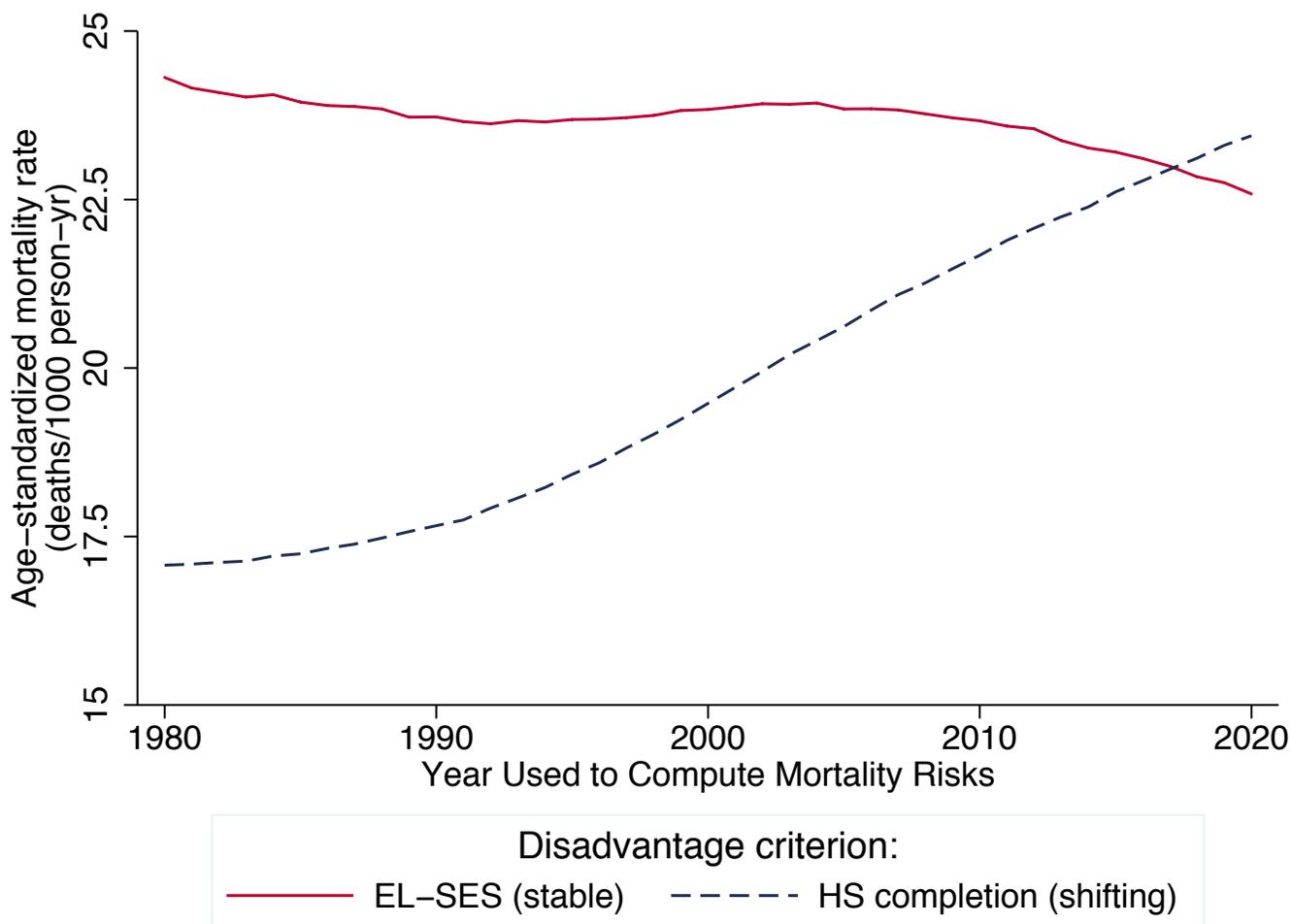
EL-SES: “early life socioeconomic status”

Figure 4. Simulation: Period life expectancy at age 25 among disadvantaged subgroups



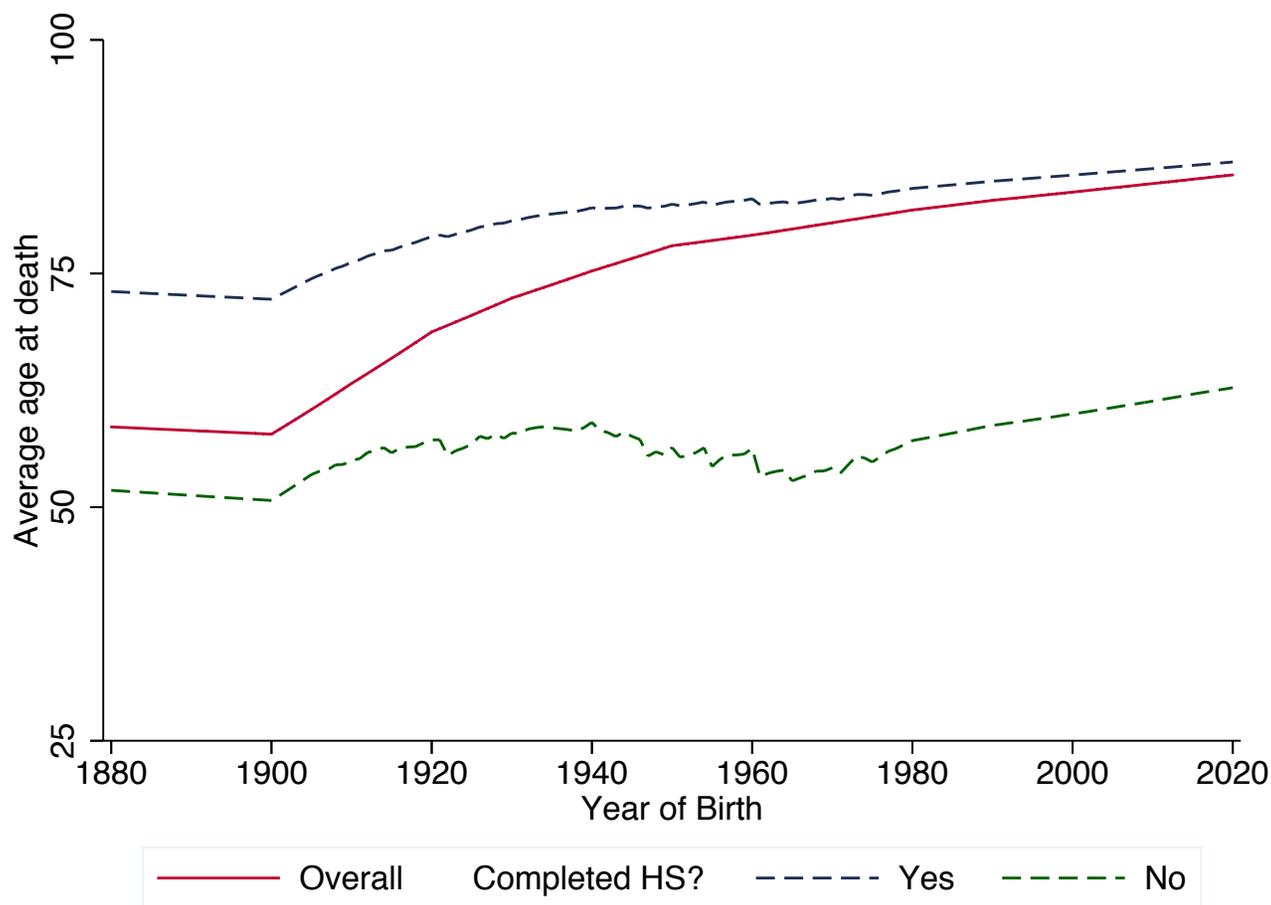
Notes for figure 4: We decomposed the population of our simulation example in two different ways—first, by early life socioeconomic status (EL-SES), and then in a separate analysis, by high school completion status. In our example, there are large, stable EL-SES gradients in mortality rates (see figure 2), but high school completion status has no independent relationship with mortality risk. The “decline” in period life expectancy reflected by the downward slope of the blue dotted line, therefore, is entirely an artifact of increasing equity in access to high school over the mid-20th century.

Figure 5. Simulation: Period age-standardized mortality rates among disadvantaged subgroups



Notes for figure 5: We decomposed the population of our simulation example in two different ways—first, by early life socioeconomic status (EL-SES), and then in a separate analysis, by high school completion status. In our example, there are large, stable EL-SES gradients in mortality rates (see figure 2), but high school completion status has no independent relationship with mortality risk. The “increase” in period mortality rates reflected by the upward slope of the blue dotted line, therefore, is entirely an artifact of increasing equity in access to high school. Period age-standardized mortality rates are computed in the traditional way—which is, by taking a weighted average of age-specific mortality rates, where weights are determined by a “standard” age distribution, which is held constant in every year for each subgroup. In this case, the “standard” distribution is the age distribution of the full 2010 population.

Figure 6. Simulation: Cohort life expectancy, by birth year and high school completion status



Notes for figure 6: We simulated the educational and mortality experience of 141 birth cohorts (1880 to 2020); each cohort consists of 1 million hypothetical people. Overall mortality risk for each cohort at each age were based on observed real-world patterns. Those parameters determine the placement and shape of the solid red line. The socioeconomic patterning in that risk, and in access to high school, was set based on observed patterns. Jointly, those two socioeconomic patterns determine the placement of the dotted lines relative to the solid red line. In our example, high school completion status has no independent with mortality risk. The downward slope of the green-dotted line for the cohorts born in the second quarter of the 20th century, therefore, is entirely an artifact of increasing equity in access to high school as these cohorts reached their adolescence.

HS: “high school”