Unlucky Cohorts: Earnings, Income, and Mortality Effects from Entering the Labor Market in a Recession

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Abstract

We study the persistent effect of initial labor market conditions for labor market entrants in the United States from 1976 to today on earnings, receipt of government support, and mortality by education, gender, and race groups. We find that adverse effects are larger for workers without a college degree and nonwhites. While these effects are partly offset by increases in the receipt of food stamps for the least advantaged, we find persistent increases in poverty. We also find moderate increases in mortality later in life, suggesting an unlucky start still has adverse effects once earnings and wage losses have faded.

JEL classifications: E32, I14, I23, I32, J22, J31

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1 Introduction

The first years after entering the labor market are typically a very productive period for young workers. During this period, young workers' wages grow rapidly and they experience frequent job changes towards better paying jobs.[2] At the same time, young workers are particularly vulnerable to adverse conditions in the labor market. For example, it is well known that young workers bear the brunt of recessions in terms of higher unemployment rates, partly because earnings tend to fall most for those workers entering new jobs. It is a long-standing concern among economists and policy makers alike that interruptions of the initial process of career progression caused by recessions can have lasting consequences on earnings and other relevant outcomes, including health insurance coverage, health effects, or family formation.

There is indeed increasing evidence from careful studies of college graduates that even temporary exposure to increased unemployment rates can lead to persistent earnings reductions. For example, using data from the National Longitudinal Study of Youth (NLSY), Kahn (2010) has shown that college graduates entering the labor market during the large recession in the early 1980s experienced reductions in earnings lasting up to fifteen years. Oyer (2008; 2006) presents evidence on persistent effect on career choice for MBAs and PhD Economists. Oreopoulos et al. (2012) show that college graduates in Canada suffer persistent earnings losses, and that these losses are substantially larger for those graduates predicted to have low earnings to begin with. Outcomes other than earnings appear to respond to initial labor market entry as well.[3] For example, based on the NLSY, Maclean (2013) finds that male entrants during the early 1980s recession experience long-lasting effects on self-reported health, consistent with findings from mature workers that labor market shocks can have long-term effects health, including mortality.[4]

These studies provide powerful evidence that the fear that recessions can have lasting repercussions for young workers is well founded. These findings give reason to concern, not least because new college graduates are typically not eligible for programs meant to buffer temporary earnings losses. By focusing on college graduates, the aforementioned papers have been able to provide empirical evidence that is highly compelling and provides important proof-of-concept results in this literature establishing the importance of persistent effects

[2] E.g., Topel and Ward (1992),Murphy and Welch (1990).

[3] E.g., Altonji et al. (2016) analyze occupational choice of college graduates, Giuliano and Spilimbergo (2014) analyze the effects on attitudes.

[4] E.g., Sullivan and Von Wachter (2009) show that mature job losers suffer long-term increases in mortality rates.

of early labor market conditions. This is partly because they can exploit high-quality longitudinal data that allow measuring the labor market in which workers enter, among others, and partly because the date of entry and typical career progression is well defined for college graduates.

However, it is well known that less advantaged groups in the labor market, such as low educated workers or minorities, experience much larger increases in unemployment during recessions. These groups are thus at risk of suffering even larger longer-term effects than the highly educated workers studied in depth in the existing literature. At the same time, in contrast to college graduates, these workers are more likely to have access to the social safety net. Indeed, recent work from European countries suggests that entry conditions have a stronger effect on a range of outcomes, including self-reported health, for lower-educated individuals (e.g., Cutler et al. (2015)), despite the wide-spread prevalence of generous social support systems in these counties. Despite the concern that less advantaged individuals could fare worse, the effect of adverse labor market entry for these groups of workers has not been studied extensively in the literature, especially for the United States where safety nets have considerably lower coverage. This is partly because typical longitudinal data does not have sufficient samples to study these groups, and partly because career progression, and hence initial conditions, are harder to measure.[5]

In order to advance the literature, in this paper we examine the persistent effects of entering the labor market in a recession on a broad range of socio-economic outcomes and a key measure of health, mortality, for all young workers who entered the labor market in the United States from 1976 to 2015. Our study includes college graduates, but also focuses on groups not typically analyzed separately, such as women, non-whites, and individuals with less than a college degree. To identify the effect of initial labor market conditions, we exploit year to year variation in unemployment rates in the state of labor market entry. To estimate these effects, we bring to bear several data set with extensive coverage over time and large sample sizes: repeated cross-sections from the Annual Social and Economic Supplement to the Current Population Survey; data from the Decennial Census and the American Community Survey; and data from Vital Statistics.

Given these data sets contain information on a large number of entry cohorts in the labor market, they allow us to obtain the first estimates of entering the labor market in a recession for a typical young labor market entrant in the United States. Another key advantage is

[5]There is a separate literature on the scarring effects of individual labor market shocks, such as unemployment spells, occuring independently of macroeconomic conditions. While identification is difficult, the effects appear to depend partly on the context (e.g., Von Wachter and Bender (2006)).

that the large sample sizes allows us to study with sufficient precision the effect for smaller groups, such as high-school drop outs. Finally, the data contain information on a range of additional outcomes that have not been studied in for young workers in the U.S. labor market. This includes information about the role of the social insurance system for young workers, such as receipt of Medicaid and food stamps and measures of poverty. In addition, the data allow us to study the effect of initial labor market conditions on a key health outcome that has received increasing attention in the context of labor market shocks, short- and long-term responses of mortality.

These substantive advantages come at a price in terms of precision of our research design imposed on us by the data. In particular, the cross-sectional data we use do not contain information on the timing and location of entry into the labor market. The former data issue is related to potential endogenous timing of labor market entry, something all studies of this kind have to deal with.[6] The latter is unique to our use of cross sectional data, since regional mobility can introduce either random measurement error or systematic bias. Given the importance of these measurement aspects, we address these issues head on in the paper using several approaches. Overall, after careful analysis, we conclude that our approach for studying the persistent effect of adverse labor market conditions based on repeated cross-sectional data is feasible and yields very similar findings to estimates that explicitly correct for mobility or endogenous labor market entry.

Based on this approach, we obtain four key findings. Our first main result is that for the full sample of labor market entrants in the U.S. from 1976 to 2015, we find that entering the labor market in times of high unemployment leads to a substantial initial effect of earnings. Consistent with findings in the previous literature, this effect fades gradually, but persists until ten years into the labor market. Our findings imply that for a moderate recession raising unemployment rates by three points, the loss on cumulated earnings is predicted to be on the order of 60% of a year of earnings. These effects are substantial, and very robust to controls for selective migration or endogenous entry into the labor market. Further analysis suggests that the initial effect is due to both employment and wage reductions, whereas the longer-term effect is mainly due to persistent declines in wages.

Our second main finding is that the effect on earnings varies considerably in the population. While all groups we studied experience persistent effects from adverse initial labor market conditions – including women, high-school graduates, and those with some college – the effects are particularly large for two groups: nonwhites and high-school drop outs. Al-

[6]However, our analysis of lower education groups has to deal with the concern that we pick up individuals whose education is in progress, something we discuss explicitly in the paper.

though due to the smaller samples precision tends to be somewhat lower for these groups, the earnings losses are substantial. In examining the sources, we find these differences are partly driven by greater losses in employment, measured in terms of number of weeks worked in the past year, for non-whites and high-school dropouts.

Third, we find that the U.S. social insurance provides a buffer for unlucky labor market entrants and that these effects are largest for those who suffer the greatest earnings losses. We find precisely estimated temporary but persistent increases in the incidence of receipt of Supplemental Nutrition Program Assistance (SNAP, formerly known as food stamps) for the full sample. The effects are present for both men and women and whites and non-whites, and are driven by a rise in receipt among those with a high-school degree and high-school dropouts. As result, the effect on household income is lower than the effect on annual earnings. However, the insurance provided is imperfect, and we find temporary but persistent effects on poverty among all groups but those with at least some college or more.

We also find that adverse initial labor market conditions rise the receipt of Medicaid for all groups with exception of those with at least some college or more. Again, the effects are particularly strong for non-whites, and high-school dropouts. For those with a high-school degree, a rise in Medicaid receipt appears to buffer a temporary, employment-related loss in private health insurance coverage. Only those with some college experience a temporary reduction in private health insurance coverage that leads to a net decline in any health insurance receipt, albeit the effect is short lived. College graduates do not experience a reduction in health insurance coverage.

Finally, we used Vital Statistics data to estimate the effect on mortality. We find that unfavorable economic condition around graduation have a negative effect on mortality in the short-run, no impact in the medium run, and increasingly positive effects on death rates, starting around 20 years after labor market entry when cohorts enter their forties. The negative short-run effects have little to do with population health as they are driven by car accidents (in line with findings in Ruhm (2000); Stevens et al. (2015)). The null results during young adulthood are plausible, given that mortality is an extreme outcome that is likely triggered by a reduction in latent health at the lower end of the health distribution, which is usually reached only at older ages. A notable exception were African-Americans during the HIV/AIDS epidemic, who suffered elevated mortality rates at that age during the peak of the epidemic. Indeed, for this group we find positive effects already in the 20s and 30s. Together with the positive long-run effects for the overall sample which are driven by disease-related mortality, our results suggest that economic conditions at labor market entry do not only

have a strong impact on economic outcomes but also on individuals' long-term health. This is consistent with papers that have studied the effect of adverse labor market entry on other measures health outcomes (e.g., Maclean (2013)).

Extrapolating the positive effects observed between age 40 and 50 into old-age, suggests that a 3 percentage point increase in the graduation year unemployment rate decreases life expectancy by about 3 months. This is a relatively limited effect, given that life expectancy has been increasing by two months annually over the past decades. But it is in a similar range as the effects of other studies that have analyzed the effects of business cycles on U.S. mortality in the short- and long-term (Ruhm 2000; Stevens et al. 2015; Coile et al. 2014). As expected, the effects we find are substantially smaller than findings based on job losers in Sullivan and Von Wachter (2009), but in a similar ballpark if we scale by the reduction in lifetime earnings. As in other cases, it is important to keep in mind that our long-term mortality analysis is based on a limited number of cohorts which might not be fully representative of future or past generations.

These findings extend the literature on persistent effects of temporary labor market conditions along several dimensions. The foregoing literature, especially on U.S. youth, had concentrated mainly on college graduates. While there are some findings based on broader samples, this is the first study to comprehensively address and compare differences in the persistent effect of initial labor market conditions for labor market entrants in the United States. Our finding of an important education gradient in the short- and long-term effect of initial labor market conditions on earnings and mortality complements similar findings in recent work by Cutler et al. (2015) based on national-level employment conditions in broad range of European countries.

Our results also provide useful information for the importance and effect of the social insurance system in buffering cyclical employment effects. Most of the focus in this area is on mature workers, in particularly what sources of income are available for the long-term unemployed (e.g., Rothstein and Valletta (2014)). But little is known about how the social insurance system helps labor market entrants weather weak economic conditions. Young workers are typically not covered by unemployment insurance, and are usually single, excluding them from typical welfare programs.

Moreover, our results provides a useful practical contribution to the literature analyzing persistent effects of the local environment during youth and early adulthood. Among others, these effects have received recent attention of studies of inter-generational transmission of income and the role of neighborhood effects based on rich longitudinal data (e.g., Chetty et

al. 2016b; Chetty and Hendren 2016). Our findings suggest that labor market mobility may be sufficiently low and non-systematic that characteristics of initial location at the time of entry into labor market can be well approximated by current state of residence (or state of birth). These findings confirm erlier results of Card and Krueger (1992) that find mobility adjustments do not affect results of the earnings effects of school characteristics, as well as evidence of Autor et al. (2014) that local trade shocks do not lead to significant migration to less affected areas.

Our mortality results suggest that short-term economic fluctuations can have lasting impacts on a cohort's mortality profile, complementing previous studies that find persistent mortality effects for economic conditions around birth (Van den Berg et al., 2006), adolescence (Cutler et al., 2016) and before retirement (Coile et al., 2014), as well as papers that have analyzed the effects of graduating during a recession on health outcomes (Cutler et al. 2015; Maclean 2013). At a broader level, our results also contribute to an ongoing, highly publicized debate about mortality trends and inequality in mortality. Overall, mortality has declined tremendously over the past decades, with particular improvements for African-Americans (Currie and Schwandt, 2016b). But poor, non-hispanic whites in middle age, on the contrary, have faced stagnating or even raising mortality rates (Case and Deaton, 2015).[7] Autor et al. (2017) suggest that this development might be linked to long-term reductions in manufacturing jobs induced by globalization and import competition. Our results suggest that the time when a cohort enters the labor market might play a particularly important role in mitigating these effects of economic conditions on mortality.

The remainder of the paper is structured as follows. Section 2 describes how our empirical approach, our data, and how we assess whether the cross-sectional data can be successfully used to estimate the long-term effect of initial conditions. Section 3 summarizes the effect of initial unemployment rates on the socio-economic outcomes we study, including annual earnings, hourly wages, employment, program receipt, and health insurance coverage. Section 4 presents our findings on the long-term effects of adverse initial conditions on mortality, and a final section concludes.

[7] Moreover, a broad number of studies has found that the mortality gap between rich and poor is diverging at older ages (e.g. Bosworth and Burke (2014); Chetty et al. (2016a); Goldring et al. (2016); Pijoan-Mas and Ríos-Rull (2014); Singh and Siahpush (2014); Waldron (2013); Wilmoth et al. (2011)), while Currie and Schwandt 2016a,b recently documented a strong convergence for infants, children, and adolescents.

2 Empirical Approach and Data

We seek to extend the existing literature focusing on college graduates to include the study of the effect of entering the labor market in a recession for more disadvantaged groups in the labor market. To do so, we use data from repeated cross-sections in the March Current Population Survey, Decennial Census, American Community Survey, and Vital Statistics. This approach has several advantages in our context. It allows us to work with much larger samples and hence enables us to study the responses of smaller subgroups, such as nonwhites or low-educated wokers. The data cover a longer time period, allowing us to analyze the effects of entering the labor market for all graduating cohorts from 1976 to 2015. This is the first paper to do so in the United States, and this is only possible due to the use of cross-sectional data. In addition, information in March CPS data allow us to analyze additional outcomes that are particularly relevant for lower income workers. Finally, our approach allows us to study mortality, a rare outcome in the age range relevant for our study.

However, working with cross-sectional data has some drawbacks as well, all of which we try to address directly. In particular, we do not know the actual state of entry into the labor market. Similarly, we do know the exact timing of entry into the labor market. Hence, in our main specification we use the unemployment rate prevailing in the current state of residence at the time the individual was implied to have entered the labor market based on the completed years of schooling. In Section 2.1 we describe our baseline specifications for socio-economic outcomes taking these choices as given. In Section 2.2 we discuss the potential effects of migration and endogenous entry into the labor market and how we deal with them. Section 2.3 extends the baseline specification to the analysis of mortality. Sections 2.4 and 2.5 provide additional detail on the data sources and our sample restrictions.

2.1 Basic Approach for Estimating Effects on Socio-Economic Outcomes

In our main specifications, we proxy the key variable of interest – the state unemployment rate in the year of labor market entry – by the unemployment rate prevailing in the implied year of entry based on completed years of education in the current state year of residence. For ease of reference, in some cases we will refer to the implied year of entry as a graduation cohort (even though in some cases individuals do not literally graduate), and to the unemployment rate in the implied year of entry as 'graduation unemployment rate'. Similarly, if we refer to 'state' without further clarification, the state of current residence is intended.

Since our main independent variable varies only across states and year of labor market

entry, when analyzing socio-economic outcomes we collapse the individual-level data at the level of current state-of-residence, year of labor market entry, calendar year, and education groups.[8] Thereby, we use an individual's reported highest level of education to create four standard, non-overlapping education groups: less than highschool; exactly a highschool degree; some college; a four year college degree or more. All regressions are weighted by the corresponding cell sizes. Standard errors are clustered at the level of graduation cohort x state of current residence to account for cohort-specific serial correlation in labor market outcomes.

As in Oreopoulos, von Wachter, and Heisz (henceforth OWH) and others, we use the following specification with the actual graduation year:

$$\bar{y}_{g,s,t,s} = \alpha + \beta_e u^G_{g,s} + \gamma_e + \delta_g + \lambda_s + \theta_t + \pi_s + \epsilon_{g,s,t,s} \tag{1}$$

The indices g, s, t, e, and s refer to the graduation cohort, state, calendar year, years of potential experience (years since graduation) and four groups of schoolings s. $\bar{y}$ are different socio-economic outcomes collapsed at the level of graduation year, state and calendar year. While our main outcomes is log annual earnings, we also implement this model for additional outcomes, which are discussed in Section 2.4. γ, δ, λ and θ are the coefficients on unrestricted experience, graduation cohort, state and calendar year fixed effects (excluding one additional year fixed effect), respectively. The coefficient vector of interest, β_e, contains the coefficients on the interaction of the unemployment rate at the year of graduation ($u^G_{g,s}$) with dummies for the individual years since graduation. This means the effects of the graduation unemployment rate is allowed to vary for every year following graduation.

The different fixed effects control for the typical experience profile, for nation-wide cohort effects, for state-specific time-constant effects as well as nation-wide contemporaneous shocks. Therefore, the coefficient vector β_e captures deviations from the typical experience profiles in the different outcomes that are uncorrelated with contemporaneous nation-wide shocks and related to cohort-state specific variation in the unemployment rate at labor market entry. Since we do not include the current state unemployment rate, β_e captures the average change in y from graduating in a recession, given the regular subsequent evolution of the local labor market conditions (see OWH for a more detailed discussion).

A key step in our analysis is to reestimate equation (1) separately by education groups,

[8] As discussed below, in some specifications we use the predicted unemployment rate, using the average migration and education shares across cohorts defined by year and state of birth. For these specifcations we collapse the data by state-of-birth x year-of-birth x calendar year.

gender, and race groups. To estimate an aggregate benchmark effect, in contrast to previous work that usually focuses on only one education group, in our baseline specification we pool multiple education groups. State-cohort-level variation in educational attainment could be a confounding factor. For this reason, we also collapse by education group and control directly for education dummies. This effectively yields state-cohort-level controls, akin to the unrestricted nation-wide cohort effects. As in other studies, our results could in principle be affected by endogeneity of the year of graduation and hence endogenity with respect to the initial unemployment rate, something that is further discussed below.

2.2 Corrections for Interstate Mobility and Selective Labor Market Entry

This section discusses the different biases arising from interstate migration and selective labor market entry, followed by a description of how we correct for these potential biases.

2.2.1 Endogenous and Random Migration Out of State of Graduation

In our baseline specification we use the current state of residence to match economic conditions at labor market entry to outcomes. This approach has two potential problems. First, in response to a local recession around the time of labor market entry people might migrate into other states that are less affected. The implied mismatch would lead to an attenuation bias, as the migrants from poorly performing states are systematically mismatched to the better economic conditions in their new state of residence.[9] If there is selection in who tends to leave, the bias could go either way. We can test for such selection effects with balancing regressions that use the racial or gender composition as a dependent variable.

Second, people might migrate independently of local labor market circumstances ("random" migration). The implied geographical mismatch would lead to attenuation bias, too, though it would be less strong than in the case of endogenous migration. In both cases the bias aggravates over time as the share of migrants accumulates within graduation cohorts.

Fortunately, the Census data allows to correct for the biases arising both from endogenous and random migration. The Census reports people's state of birth, a characteristic that is fixed at birth and therefore unaffected by either kind of migration. But simply matching people to the graduation year unemployment rate in their state of birth implies a mismatch for those who migrated *before* graduation, leading once again to attenuation bias. For example, in the

[9] If there was no cost of migration, our approach based on local shocks would not be able to pick up any effects.

2000 Census, about 20% of 18 year-olds live outside their state of birth.[10]

To adjust for the mismatch of birth and graduation state, we use the Census data to compute the average dispersion of cohorts born in one state across the different states in the U.S. around age 18. We construct the weighted average unemployment rate a cohort born in one state faced at different graduation ages, using the average migration shares as weights. The resulting migration-adjusted graduation year unemployment rate corrects for migration *before* graduation, and it is independent of endogenous as well as random migration *after* graduation.

2.2.2 Endogenous Timing of Entry Into the Labor Market

Our baseline specification treats the time of labor market entry (proxied by years of education plus 6) as exogenous. But people might prolong their educational attainment in order to avoid unfavorable conditions at entry, or end their education prematurely in order to benefit from good labor market conditions. Such endogenous timing of labor market entry attenuates our estimates towards zero if it is uniformly distributed in the society. If there is selection into timing, the bias can go either way. For example, if those with higher potential earnings are better in timing their labor market entry then we would tend to overstate effects.[11]

In order to correct for this bias in our regressions of interest, we predict the shares in each cohort graduating at different ages and then construct the weighted average unemployment rate at graduation age, using the graduation shares as weights. Importantly, we predict the graduation shares based on state fixed effects and nationwide cohort fixed effects, both of which are independent of local labor market shocks at the time of implied graduation.

Because this timing-adjusted unemployment rate is predicted at the cohort level, this approach requires collapsing the data by year of birth instead of year of graduation (and tracking effects over cohorts' age rather than experience profiles).

2.2.3 Jointly Adjusting for Migration and Timing of Labor Market Entry

The adjustments for migration and for timing of labor market entry described above are both based on predictions at the birth cohort level. This allows us to combine the two approaches.

[10] This attenuation is likely to be more dramatic than the one caused by random migration *after* graduation, because migration rates are much higher during the first two decades of people's lives than during the following two.

[11] We can test for the presence of endogenous timing with regressions of a cohorts' share of high school and college graduates on the unemployment rate at age 18. Selection into timing can additionally be explored by looking at the racial or gender composition of these graduation cohorts.

To do so, we first construct the location-adjusted unemployment rate $u_{c,b}^{locA}$ which the birth cohort c born in state b faces at different ages A=16, 18, ...

$$u_{c,b}^{loc16} = u_1^{c+16} m_{c,b,1} + u_2^{c+16} m_{c,b,2} + ...$$

$$u_{c,b}^{loc18} = u_1^{c+18} m_{c,b,1} + u_2^{c+18} m_{c,b,2} + ...$$

...

With u_1^{c+A} indicating the unemployment rate in year in state 1 in year c+A, and $m_{c,b,1}$ the share of cohort c with birth state b living in state 1 around age 18 (using age-specific migration rates does not affect our estimates). In a second step, we use the predicted shares graduating at different ages to collapse the location adjusted unemployment rates at different graduation ages into a single weighted average graduation unemployment rate $u_{c,b}^{pred}$:

$$u_{c,b}^{pred} = u_{c,b}^{loc16} e_{c,b}^{16} + u_{c,b}^{loc18} e_{c,b}^{18} + ...$$

With $e_{c,s}^{18}$ indicating the predicted share of cohort c in birth state b with 18 years of education. We refer to the resulting migration and timing adjusted unemployment rate $u_{c,s}^{pred}$ as "predicted unemployment rate".

Note that this adjustment procedure reduces the amount of variation available for the estimation of our effects of interest, as we are averaging both across locations and graduation years. As a consequence, estimates based on the predicted unemployment rate will be less precisely. More importantly, however, such estimates will be free of potential bias due to migration and endogenous timing of labor market entry.

2.3 Estimating Effects on Mortality

Mortality rates are constructed at the level of state of birth x year of birth x age and regressed on the predicted unemployment rate using the following specification

$$mort_{c,b,a} = \alpha + \beta_a u_{c,b}^{pred} + \gamma_a + \delta_c + \lambda_{b,c10} + \theta_t + \epsilon_{c,b,a} \tag{2}$$

The indices c, b, a, and t, refer to the birth cohort, birth state, age, and calendar year. $mort$ are annual deaths per 10,000 persons, in levels or in logs. γ, δ, λ and θ are the coefficients on unrestricted age, birth cohort, birth state and calendar year fixed effects (excluding one

additional year fixed effect, as in the baseline specification), respectively. The birth state effects are addtionally interacted with dummies for 10-year birth cohorts ($\lambda_{b,c10}$), to allow for flexible trends across broader cohort groups within a given birth state. The coefficient vector of interest, β_a, contains the coefficients on the interaction of the average unemployment rate a cohort is predicted to face in the year of graduation with dummies for the individual years of age. This means the effects of the predicted average graduation unemployment rate is allowed to vary for every year of age.

2.4 Data on Socio-Economic and Mortality Outcomes

The data used in this paper come from three main sources: the Annual Social and Economic Supplement of the March Current Population Survey (CPS), which provides socio-economic characteristics for repeated cross-sections for large samples of individuals, the US Vital Statistics (Vital Stats), which provides information on every single death in the US starting in 1968, and the Decennial Census of Population (Census) in combination with American Community Survey (ACS), which provide the population denomiator for the construction of annual mortality rates.[12] We also use the Census/ACS data to construct migration and timing adjusted graduation unemployment rates.

In order to relate socio-economic outcomes and deaths from these two data sets to the unemployment rate at labor market entry, information on the year of entry into the labor market and the state of graduation are required. The CPS reports the number of years of education until 1993 and the highest completed degree thereafter. This allows to calculate the respondent's year of graduation as the year of birth plus 6 plus years of education. The state of current residence is included in all waves. As discussed, this variable is used as a proxy for the state of graduation in our baseline specification.

The death files from the Vital Stats include the state, date, and the cause of death along with core demographic characteristics such as age, sex, race, and state of birth. The same characteristics are reported in the Census/ACS, which allows us to construct death rates for the subgroups defined by these demographics. After 1989, the mortality files additionally include information on the decedent's education, thus in principle we could construct mortality rates by education group and graduation year for the years following 1989. However, constructing mortality rates by education group is problematic, due missing information and coding mismatches between the Vital Stats and the Census (Currie and Schwandt 2016a,b).

[12] Annual mortality rates require annual population estimates. The ACS provides annual estimates for 2000-2014. For the Census years 1980, 1990, and 2010, we construct population estimates by state of birth and year of birth and apply a linear interpoliation for the intercensus years.

2.5 Sample Restrictions

State-level unemployment rates are available from the Bureau of Labor Statistics only since 1976. Therefore we exclude individuals who graduated before 1976 when using the actual graduation year and individuals who were born before 1960, i.e. of age 16 before 1976, when using the predicted unemployment rate (which is based on the unemployment rates cohorts face at age 16 and above). Further we restrict the main analysis to start in 1979 when the state of birth becomes available in the Vital Statistics. The CPS analysis include year up to 2016, while the mortality is available until 2014. To ensure our estimates are based on a sufficient number of entry cohorts, we make two additional restrictions. We confine the CPS analysis to individuals between age 16 and 40. In addition, we limit the analysis to individuals with at most 15 years of potential experience (when using the actual graduation unemployment rate) or are at most of age 33 (when using the predicted unemployment rate). Mortality effects are analyzed at different age ranges (up to age 48) and in each case only those cohorts are included that are observed across the entire respective age range.

Table 1 presents summary statistics for our main sample and lists the variables we analyze. In addition to studying the effect of entering the labor market in a recession on log annual earnings and mortality rates, we also examine the effect of a range of other factors related to the social insurance system. In particular, we study the receipt of support from programs such as Supplemental Nutrition Assistance Program (SNAP, formerly food stamps), unemployment insurance, welfare, as well as the incidence of Medicaid and receipt of health insurance more generally. These outcomes are of particular relevance for less advantaged workers, and might provide mediating factors for the effects on mortality.

3 The Effect of Entering the Labor Market in a Recession on Socio-Economic Outcomes

3.1 The Effect on Earnings for the Full Sample: Baseline Estimates and Sensitivity

3.1.1 Baseline Estimates

Figure 1 shows the effects of the initial unemployment rate on log annual earnings in the first 15 years in the labor market. The figure displays the coefficients β_e on the interaction of dummies for potential experience with the unemployment obtained from estimating equation 1 for the entire sample of labor market entrants. The point estimates with standard errors for five experience groups are shown in Table 2. The resuls cleary show, as expected, that

earnings at labor market entry fall when the local unemployment rate rises. The effects are substantial; for a three-point rise in the unemployment rate - roughly the typical increase from peak to through of the business cycle - the point estimates in Table 2 suggest an initial reduction of earnings by approximately 11%. This effects only slowly declines with time spent in the labor market. The reduction is still significanly different from zero ten years after graduation (a reduction of 2.6% for a three-point rise in initial unemployment rates), but then fades to zero.

These estimates confirm previous findings mostly based on college graduates and for more narrow time periods. Our results tend to be somewhat larger than previous studies. While the estimates are difficult to compare because of differences in cohorts and time periods, the study by OWH is most comparable, since it also include a broad number of cohorts covering multiple recession. Focusing on college graduates, OWH find an initial earnings loss of 2% that fades over time. For U.S. college graduates during the severe 1980s recession, Kahn (2010) finds somewhat larger estimates. According to our estimates, a one-point rise in the initial unemployment rate reduces cumulated earnings by approximately 20% of an average annual earnings in the sample (summing the coefficients in Table 2 and scaling for the experience years they represent). Hence, a recession – rougly corresponding to a three-point rise in the unemployment rates – would lead to a reduction of cumulated earnings by approximately 60% of average annual earnings, a substantial effect. Relative to total earnings in the first ten years of the labor market, the effect is approximately 6%.[13] The fact that our findings tend to be somewhat larger is likely due to the fact that we include in our analysis more vulnerable groups than are typically studied, something we return to in Section 3.3. Before, we first show that these effects are not due to the fact that we are using cross-sectional data.

3.1.2 Correcting for Interstate Mobility and Endogenous Labor Market Entry

In Section 2.2 we describe the biases that can arise from inter-state migration and graduation timing, and how they can be corrected using Census data. Figure 2 shows how these corrections affect our baseline estimates. The red triangles show the CPS estimates for annual income as in Figure 1. The solid black line without markers shows that the baseline specification results in very similar estimates in the Census/ACS data.

For the third set of estimates in Figure 2, the blue squares, we use respondents' state of

[13]These numbers are upper bounds, since the fact that experience profiles are increasing implies that percentage losses earlier in a career receive a lower weight in an appropriately weighted total.

birth instead of state of residence as a proxy for the state of graduation. As explained above, this specification is not affected by any endogenous or random migration *after* graduation but there is likely attenuation due to migration *before* graduation. The resulting estimates are attenuated by about 20% in the first years after graduation in comparison with the baseline specification, as one would expect given a pre-graduation migration rate of about 20%. The difference between estimates fades at higher experience years, as accumulative migration after graduation is attenuating the baseline estimates but not those based on the state of birth.

The hollow green markers, finally, show estimates based on the predicted unemployment rate which corrects for migration (both before and after graduation) as well as for endogenous labor market entry. The effect profile is shown over age instead of experience, as the predicted unemployment rate is constructed at the level of year and state of birth. The adjusted effects seem a bit more noisy, likely due to the adjustment-induced reduction in identifying variation. But despite the loss in precision, effects are very similar to the baseline specification in the first years of age / experience plotted in the figure. And in line with a slight attenuation of the baseline specification due to accumulative random migration after graduation, the adjusted effects are somewhat stronger in later years. Overall, these results suggest that in our baseline specification any bias due to endogenous timing of labor market entry and interstate migration is very limited.

3.2 The Effect on Other Outcomes for the Full Sample

3.2.1 Effects on Employment and Wages

The CPS data allows us to decompose the earnings effect into an effect stemming from a reduction in the number of annual weeks worked, a reduction in usual hours worked per week, and a reduction in hourly wages (calculated by dividing total annual earnings by approximate total annual hours). The result, shown in Figure 3, indicates some interesting patterns. First, exposure to high unemployment rates at labor market entry leads to a precisely estimated persistent reduction in hourly wages lasting all 15 experience years included in the analysis. While the effect after ten years in the labor market is small, clearly an unlucky initial start depresses earnings even for those individuals obtaining a job. Given these estimates are based on a potentially positively selected group of individuals that found a job, they may understate the true reduction in earnings capacity for unlucky entrants. Second, we find non-negligible effects on weeks worked that are conentrated in the first five years after labor market entry. Finally, we find smaller but surprisingly persistent reduction in usual hours worked. An examination of the point estimates shown in Table 2 show that about two thirds of the effect on

annual earnings we find in the first three years is driven by a reduction in total hours worked (weeks worked times usual hours). This drops to 50% in experience years 4 to 5. In contrast, two thirds of the longer-term effect of adverse initial labor market entry on annual earnings is driven by a reduction in hourly wages.

3.2.2 Effects on Social Welfare and Total Income

Figure 1 shows the effect of a high initial unemployment rates on the log of family income. There is a clearly visible negative and persistent effect of intial labor market conditions that is precisely estimated even ten years into the labor market. However, the impact is smaller than for annual earnings, especially in the first years after labor market entry. Cumulating the coefficients in Table 2 imply that the life-time effect of three-point rise in initial unemployment rate is roughly 13%, about half the effect of earnings.

One source of difference are transfers from the social insurance system, such as SNAP (food stamps) or welfare payments. We exploited the available information in ASEC to directly assess the effect of initial unemployment rates on receipt of transfer income. The results are shown in Figure 3 and Table 2. We find a persistent rise in the receipt of food stamps, but no effect for other outcomes, such as unemployment insurance, receipt of Earned Income Tax Credits, or welfare receipt (not shown). This confirms that unemployment insurance in particular does little to smooth adverse initial conditions in the labor market for young workers, partly because of lack of eligibility, partly because a substantial portion of the effect goes through persistent reductions in wages.

While the point estimates of the effect on food stamps appear small, they are precisely estimated even up to ten years after labor market entry. Relative to the average fraction of the sample receiving food stamps, the effect is non-negligible. For a three-point rise in unemployment rates, the initial effect is about a rise in 1.5% and the cumulated effect is approximately 3.7%. Given that about 10% of individuals receive food stamps, and 23% of Americans report to have received food stamps at some point, these are substantial effects. We also analyzed the response in the magnitude of log income received food stamps (not shown). We find that in the first four to five years after entry income from food stamps rose by a precisely estimated 2-3%. This can explain a substantial part of the difference between labor earnings and household income shown in Figure 1.[14]

[14]Given these magnitudes line up well, we did not separately analyze the potential insurance stemming from a rise in spousal earnings.

3.2.3 Effects on Health Insurance

Since health insurance receipt was chiefly tied to employment throughout most of the period under study, the employment effects we find can imply a loss in private health insurance coverage. Based on the ACES, we can analyze receipt of private health insurance, availability of any health insurance, and receipt of medicaid as separate outcomes. As expected, we see in Table 2 that exposure to a high initial unemployment rate leads to a reduction in the incidence in private health insurance. Consistent with our findings on the reduction in employment, effect fades after about four to five years in the labor market. In contrast, the losses in overall access to health insurance are concentrated in the first couple of years after labor market entry. The difference is explained by a persistent rise in the incidence of receiving Medicaid, shown in Figure 3. As shown in Table 2, this effect is significantly different from zero for about 7 years after labor market entry. The point estimates are small, but have to be compared against an average 10% of Medicaid receipt in our sample (see Table 1). Scaling the coefficient again by three, suggests labor market entry in recessions raises Medicaid receipt by about 12% relative to baseline.

We have also analyzed the effect on family outcomes, such as marital status, child bearing, single parenthood, or living with parents. In contrast with other studies suggesting increasing unemployment rates delay household formation, we find no effect of initial unemployment rate on marriage or child bearing. However, preliminary results suggest there is an increase in the incidence of individuals living with their parents. This rise only lasts for four to five years after entry, suggesting that individuals move into a place of their own once they have found stable employment.

3.3 The Effect on Earnings, Employment, and Wages by Education, Race, and Gender

The effects of initial unemployment rates on annual earnings and family income by gender and by whites vs. non-whites are shown in Figure 5 and Table 2. The results for men and women are qualitatively quite similar, with the exception of the effects in the first years after labor market entry. Initially, men experience both larger losses in earnings and in family income. Turning to race, the losses in annual earnings for non-whites are substantially larger than for whites, especially in the first five years in the labor market. As shown in Figure 7, this difference is mainly driven by greater employment losses for non-whites. In contrast, the effect on household income is quite comparable, suggesting that social insurance mechanisms help to buffer the bigger effects for non-whites, something which we return to

below.

Considering Figure 7, it is remarkable how, despite some differences in the initial effect, all groups considered experienced persistent losses in hourly wages. In contrast, the temporary losses in employment (as measured by weeks worked last year) we see are somewhat more disparate across groups. In particular, men appear to experience a more persistent reduction in employment, and non-whites experience the largest losses. Yet, after five years in the labor market these effects have completely faded.

Figure 6 shows that there are substantial differences in the effect of adverse initial conditions by education groups. Middle educated workers - those with high school or some college - experience patterns comparable to that for the full sample shown in Figure 1. In contrast, college graduates show markedly smaller and shorter-lived effects on annual earnings. The size of the effect is about half of that in the full sample, and more similar in magnitude to effects found for college graduates in Canada by OWH; they are somewhat smaller than findings by Kahn (2010) for college graduates entering during the severe 1982 recession. The largest effects we find are the initial losses in annual earnings experienced by high-school dropouts. These average a reduction of 5% over the first three years, and then converge in a similar fashion as the effect for those with a high-school degree. The results are substantial losses in cumulated earnings for this group. Not surprisingly, it is for high-school drop outs that government transfers play a particularly important role in delivering a more muted impact on income, something we return to below.

As shown in Figure 8 the differences in earnings losses result to an important degree from differences in employment losses. High-school drop outs experience substantial employment reductions, whereas college graduates essentially experience no significant employment reduction. In contrast, again all groups experience reductions in hourly wages lasting at least ten years into the career. As shown in Table 5, even for workers with at least a college degree or with some college the reduction is statistically significantly different from zero ten years after labor market entry. Interestingly, while reduced weekly hours is a phenomenon relevant for all demographic and education groups, there is little noticeable variance in the effect of adverse initial labor market conditions.

3.4 The Effect on Other Outcomes by Education, Race, and Gender

3.4.1 Welfare Effects by Demographic Groups

Figure 9 shows the impact of initial unemployment rates on our two key variables capturing the role of the social insurance system -- receipt of food stamps and medicaid -- by

demographic groups. Women have a slightly higher rate of initial receipt of food stamps, and this may contribute to their lower income losses. However, the difference is relatively small. However, we see a large difference in the propensity to receive food stamps by race groups. In the first five years, non-whites experience a rise in receipt of food stamps of one percentage point (Table 2). At a mean receipt of 18% in our population, that 3-point rise in unemployment rates leads to a 15% increase relative to the mean. For whites, the effect is 0.4 and 0.3 points in years 1-3 and 2-5 respectively. For a 3-point recession, this implies a approximately an 11% effect relative to the mean (8%). Interestingly, for all demographic groups, conditional on receiving food stamps the increase in the amount is relatively similar (not shown).

We also assessed the effect on other potential sources of transfer income. While we did not find precisely estimated effects for either men or both race groups, we found a non-negligible rise in the amount of UI income received for women.

Overall, despite the rise in food stamps and its effect on income, entering the labor market during slack labor markets has a significant effect on poverty rates. Figure 11 shows that poverty raise rose persistently for the first five years after labor market entry -- and in some cases even up to nine years after -- for all demographic groups, an effect that was largest for blacks. The effects are non-negligible: relative to the mean poverty rates for the different demographic groups in our age ranges shown in Table 1, the effects are in the range of 10-15%.

Figure 9 also shows the rise in the incidence of receipt of Medicaid by demographic groups. There are little discernible differences by gender, with relatively short-lived effects. The same is true for whites. In contrast, for non-whites, the figure and table show precisely estimated larger increases lasting ten years into the labor market. A 3-point rise in the initial unemployment rate is predicted to trigger a rise in approximately 1.5 points initially (an effect of about 10% relative to the mean, see Table 1) and a rise in approximately one point thereafter.

The Medicaid results relate to the coverage by health insurance more generally. Table 3 shows that there is a steep loss in private health insurance that is concentrated in the first two years in the labor market for whites and both genders (with men suffering slightly larger losses) and in the first four years for non-whites. Due to the rise in Medicaid receipt, the effect on having any health insurance shown in the Appendix is substantially muted for women and nonwhites, and reduced for men and whites as a whole. Hence, it appears Medicaid is successful at providing a partial buffer against the temporary loss in employer-provided

health insurance.

3.4.2 Welfare Effects by Four Education Groups

Figure 10 and Table 1 show the effect of initial unemployment rates on receipt of food stamps and Medicaid by education groups. The results are quite clear. Labor market entrants with some college or more do not benefit noticeably from a rise in these social insurance programs. Labor market entrants with 12 years of education experiences a precisely estimated but moderate rise lasting up to seven years after labor market entry. In contrast, high school drop outs experience substantial increases in food stamp receipt that, albeit declining some, last up to 15 years into the labor market. This suggests that the effects for broader groups discussed so far are mainly driven by responses for lower skilled workers. When considering the amounts received, these are higher initially and then decline somewhat for college drop outs, but the changes with experience are not precisely estimated (see the Appendix).

When considering receipt from unemployment insurance benefits, we found relatively imprecisely estimated effects for high school drop outs in the first couple of years after labor market entry, but no effects for any of the other groups.

Taken together, the results on food stamps and household income suggest that social insurance mechanisms do buffer the effect of adverse conditions at initial labor market entry. However, again the insurance is imperfect. As shown in Figure 12, poverty rates rise persistently for both high school graduates and high school drop outs. Given typical poverty rates in our sample of a bit over 20% (Table 1), the estimates imply a rise of approximately 15% for a moderate recession. It would be 25% in a larger downturn with a five point rise in unemployment rates, such as the Great Recession.

Figure 10 and Table 1 also show the effect on Medicaid. Again, as expected, there is no increase in the receipt of Medicaid by labor market entrants with some college or more. In contrast, less educated labor market entrants see a persistent increase in their Medicaid receipt following an adverse labor market entry. Relative to mean Medicaid receipt for the lowest education group (20.5%, Table 1), the effect is approximately 5-10%.

Again, these pattern are connected to health insurance availability more generally (results shown in the Appendix). Interestingly, temporary losses in employer-provided health insurance are concentrated among high-school graduates and those entrants with some college degree — in contrast, neither high-school dropouts nor college graduates experience a reduction. As a result, high-school graduates can partly rely on Medicaid to help buffer the temporary losses in employer-provided health insurance, whereas those with some college

cannot.

Overall, then, it appears that temporary but persistent increases in the receipt of food stamps and Medicaid help those young labor market entrants that are most affected by adverse initial labor market conditions — non-whites and high school dropouts, and to some degree high-school graduates. The buffer appears most successful for access to any health insurance, which appears to decline only in the immediate years after graduation when employment losses are most severe. In contrast, the rise in transfer payments cannot prevent a temporary but persistent rise in poverty, which reflect the large and persistent earnings losses that these less-advantage groups experience upon graduating in recessions.

4 The Effect of Entering the Labor Market in a Recession on Mortality

4.1 Nationwide Mortality Trends

Figure 13 shows mortality rates for various cohorts at age 18 and above between 1979 and 2014 (Table 6 reports death rates for the analyzed cohorts and demographic subgroups). To avoid clutter only a few ages are marked next to the graphs. The typical exponential growth of the mortality rate over age is visible for all cohorts, but there are also differences between the individual profiles. Cohorts born around 1965, for example, were most affected by the HIV/AIDS epidemic and experienced a distinctive mortality hump in their early thirties, when the epidemic peaked in the U.S. Another important development are the overall declines in mortality rates over time which are apparent when drawing a line through the markers of the same age.

On the right y-axis we plot the national unemployment rate, which fluctuates around 4 and 10 percent, with peaks during the recessions in 1981/82, 1990/91, 2001, and 2008/9. We categorize the mortality profiles into boom cohorts (green triangles) and bust cohorts (blue circles), in reference to the unemployment rate in the year when each cohort turns 18. This allows us to roughly assess the relationship of the unemployment rate around a cohorts' modal year of graduation with later mortality.

Comparing the mortality at age 40 of boom and bust cohorts suggests that a higher unemployment rate at age 18 is associated with increased mortality two decades later. Despite the downward trend in mortality at age 40, busts cohorts have higher mortality than earlier born boom cohorts (another way to see this is to draw a line through the age-40 markers of the boom cohorts; the corresponding markers for the busts cohorts lie above that line). At the same time, it is clear that a pattern based on four observations at the national level should not

be overinterpreted. The case of the HIV/AIDS mortality bump shows that there are strong nationwide shocks to individual cohorts that have little to do with the unemployment rate in a particular year. The state-cohort level analysis in the following section intents to control for such society-wide shocks. An important caveat for such analysis, visible in Figure 13, is that most cohorts are observed only at relatively young ages when mortality is low.

4.2 Baseline Mortality Estimates

Figure 14 shows the baseline mortality results, based on equation (2). Regression coefficients for this and the following mortality figures are summarized in Tables 7 to 10. Panels (A) to (C) show the effect on log mortality rates across different age ranges. Each regression includes only those cohorts that are observed over the entire respective age range. As explanatory variable we use the age-interacted predicted average unemployment rate that a cohort faces at graduation. This predicted unemployment rate accounts both for endogenous timing and migration, and is matched to cohorts by their year and state of birth (see Section 2.2) . Figure 14 (A) shows that between age 19 and 33 there is not a lot of an effect on mortality. At age 19 to 22 point estimates are negative at around half a percent but the 95% confidence intervals include zero. For the rest of the twenties and the early thirties the estimated effects move closely around zero.

A similar pattern is observed for that age range in Figure 14 (B) with a negative point estimate at age 19 and effects around zero in the twenties and early thirties. However, estimates seem to increase somewhat in the late thirties and they become significantly positive around age 40. This positive trend continues at higher ages as shown in Figure 14 (C), with point estimates reaching almost 0.025 in the mid- and late forties.

Figure 14 (D) shows effects on mortality in levels, resulting in a similar effect pattern as in panel (C). The increase of the estimated effect is somewhat steeper and continuous at higher ages, not surprisingly, given the exponentially increasing baseline mortality at that age range. Further notice, that the negative effect at age 19 is visibile as well, with the 95% confidence interval just excluding zero. As 19 is about the average year of graduation in the analyzed cohorts. The effect at that age refers more or less to the effect of the contemporaneous unemployment rate and is in line with the literature on the negative short-term effects of recessions on mortality (Ruhm 2000; Stevens et al. 2015; Miller et al. 2009; Cutler et al. 2016).

4.3 Mortality Effect by Gender, Race, and Cause

Figure 15 shows effects on mortality levels for males, females, whites, and nonwhites, for the 1960-1966 cohorts that are observed over the maximum age span. As panel (A) shows, the negative effect on mortality at age 19 (and age 20) seems to occur only among males. For females the point estimate is very close to and not significantly different from zero at that age. The increasing effect on mortality at older ages, on the other hand, is similar across gender. A more dramatic difference is visible across race. Effects are substantially larger for non-whites than for white and a positive mortality impact seems to occur already in the late twenties. At the same time, the estimates for nonwhites are also less precise, due to the smaller population base and the unequal distribution across states which further lowers the effective amount of identifying variation.

Figure 16 explores different causes of death. Panel (A) and (B) split up all causes into violent and disease related deaths. Interestingly, the negative effect at age 19 is entirely driven by violent deaths, while the increasingly positive effect at higher ages occurs only among disease related deaths. Panel (C) zooms in on violent deaths and shows that the negative effect is driven by accidents (largely car accidents). This finding is in line with studies that have analyzed the effects of the contemporaneous unemployment on mortality by cause and age groups (Ruhm 2000; Stevens et al. 2015). Figure 16 (D) shows the effect on disease related mortality separately for males and females. The positive effects for males are somewhat stronger but the overall pattern is very similar.

A central development affecting the mortality profiles of the cohorts included in the long-run analysis is the HIV/AIDS epidemic (see Figure 13), which could be driving our long-run mortality estimates. In Figure 17 we repeat the mortality regressions by demographic groups, excluding deaths due to HIV/AIDS. For males, there is a slight downward shift in the effect pattern, indicating that a minor part of the positive impact of the graduation year unemployment rate on male mortality could be driven by HIV/AIDS deaths. Among females and whites, who did not belong to risk groups during the epidemic, the exclusion of this cause of death has no impact. A strong difference, however, appears for nonwhites in panel (D), who were also most strongly affected by HIS/AIDS in terms of overall death rates. Excluding HIV/AIDS deaths shifts the pattern strongly downwards, making the remaining effect pattern similar to that for males in the overall regressions. In particular, all of the positive effects on overall mortality observed for nonwhites in their late 20s and throughout the 30s disappears when this HIV/AIDS deaths are excluded.

4.4 Size of Mortality Effects

How large are these long-term effects that we find in the overall sample? The cumulative impact of a 3 p.p. increase in the unemployment rate around graduation on mortality between age 19 and 48 amounts to 13.8 additional deaths per 10,000. As a comparison, there have been 508 deaths per 10,000 in the 1965 birth cohort between age 19 and 48. In other words, a strong recession would increase mortality in the most affected cohorts by about 2.7% in this age range. Appendix Figure **??** (A) shows the additional deaths added to the typical mortality profile. In terms of life expectancy, this effect alone would be negligible. For example, life expectancy at birth in 2000 would decrease from 77.51 to 77.47 years, by about 16 days.

What if the mortality effects continue to increase into old age? Extrapolating the increasing effect pattern estimated between age 30 and 48 up to age 80 (shown in Appendix Figures **??** (B) and **??**), would imply a decrease in life expectancy by 94 days, or about three months. This effect is substantially smaller than the reduction in life expectancy by 1 to 1.5 years that is associated with indiviudal-level job loss (Sullivan and Von Wachter 2009), but relatively large if we rescale by the reduction of life-time earnings. At the same time, the three month reduction appears modest if compared to the overall increasing trend in U.S. life expectancy, which has been growing annually by about two months over the past decades. This means you will enjoy a higher life expectancy than your two-year older sibling even if you are hit by a recession at the time of your graduation.

Another important benchmark are estimates from the literature on the impact of economic conditions on contemporaneous mortality (Ruhm, 2000). Stevens et al. (2015) provide estimates by age groups and find that a 3 p.p. increase in mortality implies 2,163 fewer deaths at age 15 to 49 for the overall U.S. (based on 2006 death rates). The cumulative effects of a 3 p.p. increase in the graduation year unemplomyent rate that I find are more than twice as large over that age range, with 5,190 additional deaths between age 19 and 48 (for the 1988 birth cohort which is 18 in 2006). In other words, if we care about the contemporaneous effects of economic conditions on adult mortality, then the long-run effects on graduation cohorts should be at least as relevant.

Another related paper is Coile et al. (2014) who use a similar strategy as the one in this paper to investigate the effects of business cycle fluctuations in preretirement years on subsequent mortality. The authors show that higher unemployment before retirement age increases survival in the short run, but the overall long-run effects on longevity are negative. They find strongest effects for recessions hitting individual is in their late 50s. A 3 p.p. increase in the unemployment rate at that age leads to 15 additional deaths per 10,000 during

the following ten years (up to age 70). This is close to the 13.8 additional deaths that we estimate for a similar increase in the graduation year unemplomyent rate over a time of about 30 years, though these numbers are not directly comparable given the different age ranges that are studied.

Overall, the results of the mortality analysis suggest that the effects of economic conditions around the time of graduation on mortality can be divided into three parts. In the short term, higher unemployment rates decrease deaths due to accidents, perhaps due to lower traffic density during economic downturns. In the medium term, from the mid twenties to the late thirties there is no detectable impact on death rates. In the long-run, starting around age 40, effects turns positive and become stronger towards the late forties. This long-term effect is driven by disease-related causes and similar between males and females. The effect size of these long-term effects is not very large even if extrapolated into old-age, but they are in a range that appears relevant to economists in related contexts. In terms of generalizability, it is important to keep in mind that the estimated long-term effects are based on a limited number of cohorts and that impacts beyond age 48 are extrapolated using the effects estimated at age 30 to 48.

5 Conclusion

A long-standing concern of economists and policy makers alike has been whether young workers entering the labor market during a recession suffer permanent consequences from their initial bad luck. While this question has been studied extensively for male college graduates, less advantaged workers such as lower educated workers, non-whites, or women have received lower attention. In this paper, we have used large samples of data from the Current Population Survey, Decennial Census, and Vital Statistics spanning over forty decades to study the effect of adverse initial conditions for multiple groups of workers. Our data did not only allow us to to study workers that are at higher risk of lasting adverse consequences. The data also allowed us to analyze whether for these less advantaged workers the adverse effects on earnings are buffered by the social insurance system. Finally, we studied whether earnings reductions have an impact on mortality even once the initial earnings effects are faded. A key step in the analysis was demonstrating the feasibility of using cross-sectional data to study the long-term effect of initial labor market conditions.

We confirm all labor market entrants experience persistent reduction in earnings, employment, and wages from entering the labor market in a recession that last at least ten years. We show these effects are substantially larger for less advantaged workers, in particular high-

school dropouts and nonwhites, but also for high-school graduates. The losses in earnings we find are partly offset by increases in the receipt of food stamps for the least advantaged groups, reducing the impact on the reduction in household income. Nevertheless, our results imply that entering the labor market leads to persistent increases in poverty. Finally, we show that these results lead to moderate increases in mortality later in life. As documented by others, we find that unfavorable economic condition around graduation initially lower mortality. However, starting around 20 years after labor market entry when cohorts enter their forties mortality rates increase. This is consistent with findings that initial labor market conditions can reduce health outcomes in middle age (e.g., Maclean, 2013). While the resulting predicted reduction in life-expectancy is small, it rests on a linear extrapolation. Studies with longer range of data will be needed to better understand the effect of initial conditions on mortality at older ages.

Overall, these findings help to complete the picture of persistent consequences of cyclical conditions for young workers. It becomes increasingly apparent that adverse early labor market conditions affect all groups in the population and influence many aspects of individual workers socio-economic outcomes. These findings highlight several important and as of yet open questions. We know relatively little so far as to the sources of the persistent reduction in employment and wages we and others documented. An important source of wage losses for college graduates appear reduction in employer quality (Oreopoulos et al., 2012). This is consistent with the fact that employment fluctuations are more pronounced at higher paying employers (e.g, Kahn and McEntarfer, 2014), leading to cyclical downgrading of labor (e.g., McLaughlin and Bils, 2001). Similar forces are likely to be present for lower-skilled labor, who are at the bottom of the job ladder. Another important question is what the longer-term consequences of adverse initial labor market conditions are. Gibbons and Waldman (2006) for example hypothesize that worse occupational outcomes and human capital accumulation makes these workers more vulnerable to future economic shocks. In contrast, findings by Schmieder and Von Wachter (2010) suggest that below-average wages as a results of adverse initial conditions may reduce the chance of future layoff. Vulnerability or resilience may also be present in long-term health outcomes. The study of these and other questions awaits data with additional information on career outcomes and data with longer time ranges.

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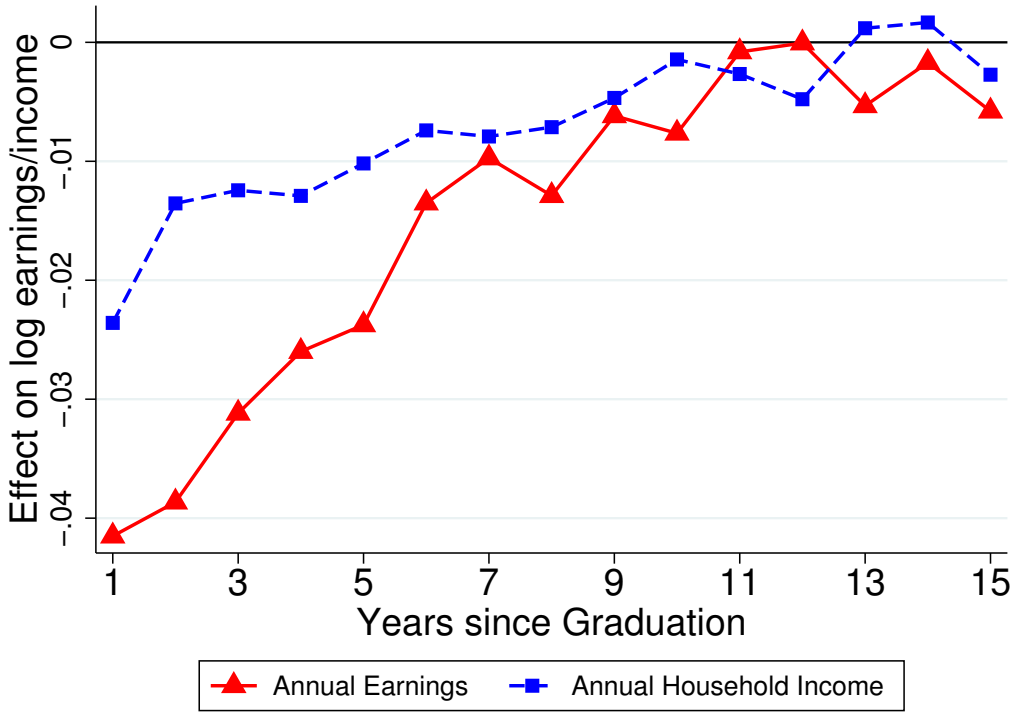
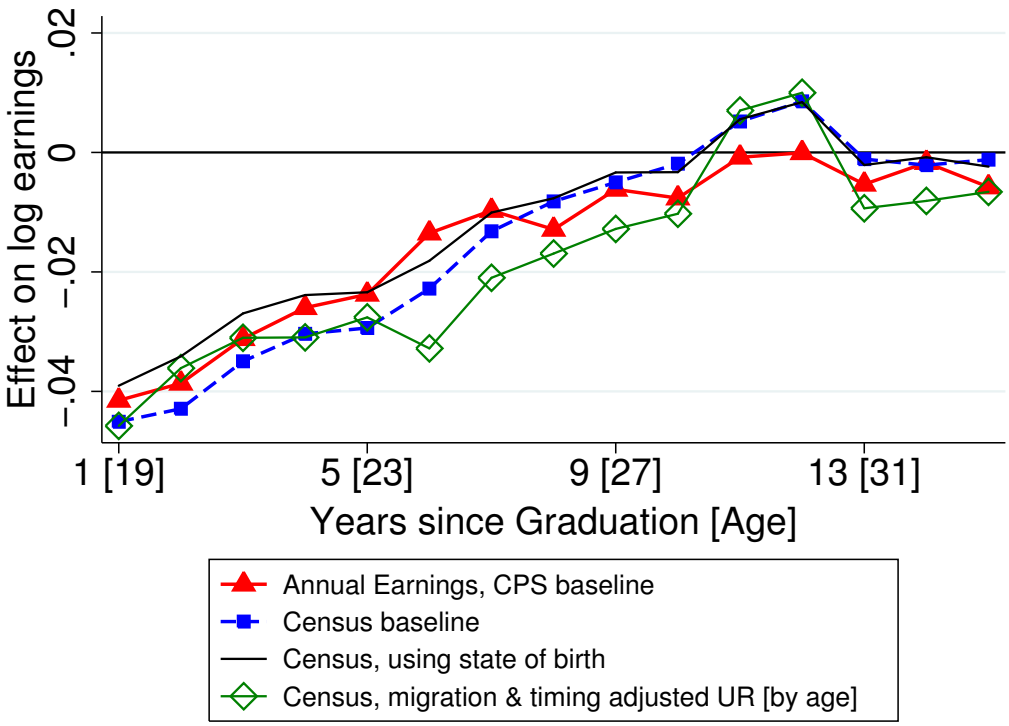
Figure 1: Effect of State Unemployment Rate at Labor Market Entry on Log Annual Earnings for Full Sample



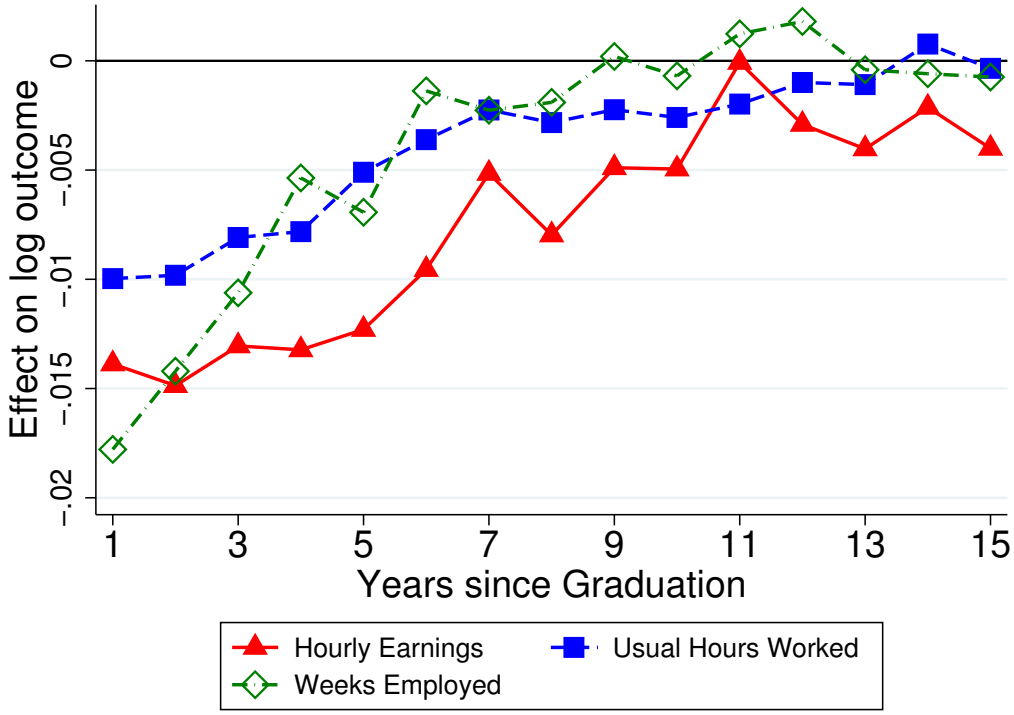
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 2: Adjusting for Migration and Graduation Timing in Census Data



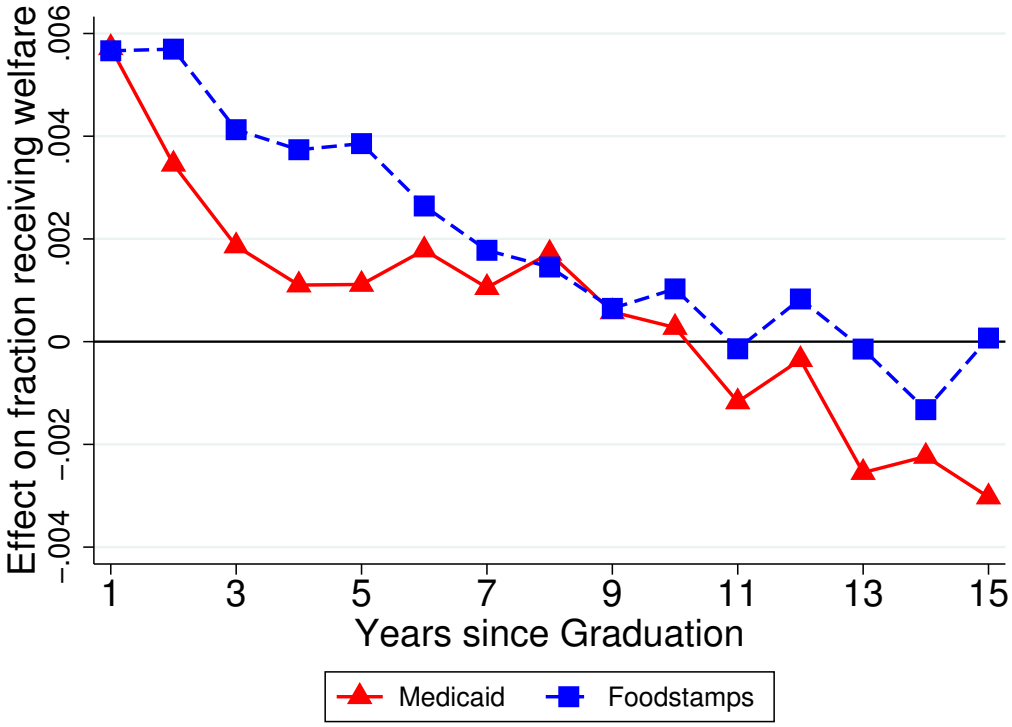
Notes: The CPS baseline results are based on ASEC Supplement to CPS from 1976 to 2016. The Census results are based on the 1980/1990/2000 Census and the ACS from 2001 to 2015. The migration and timing adjusted unemployment rate refers to the "predicted unemployment rate" described in Section 2.2.

Figure 3: Effect of State Unemployment Rate at Labor Market Entry on Employment and Wages



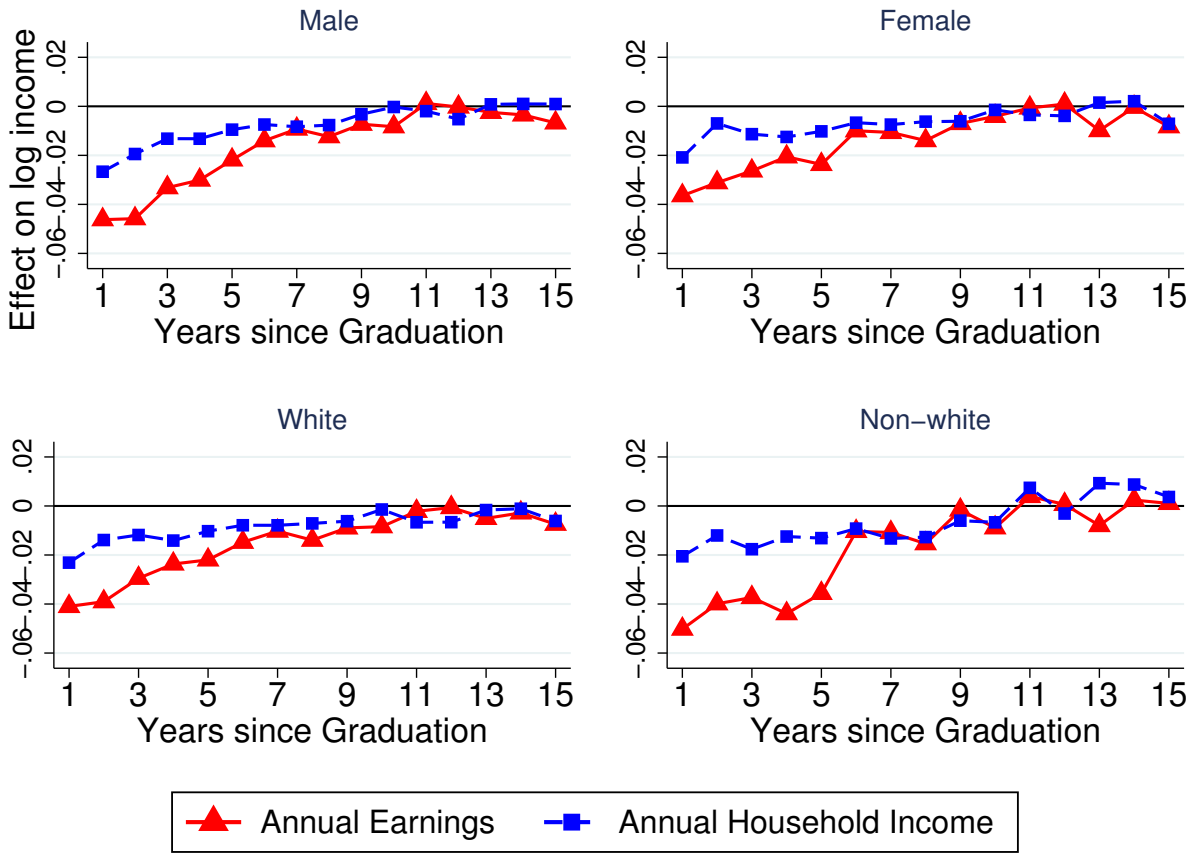
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 4: Effect of State Unemployment Rate at Labor Market Entry on Food Stamps and Medicaid for Full Sample



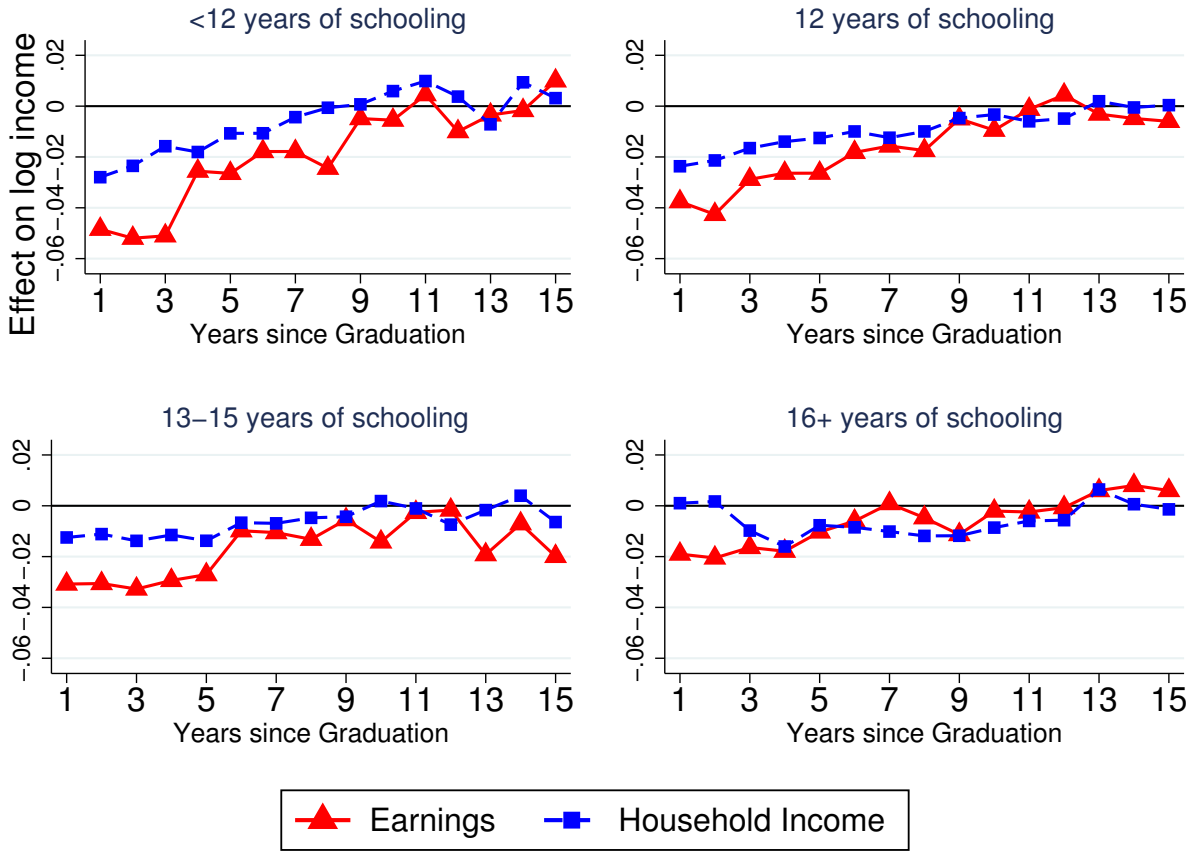
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 5: Effect of State Unemployment Rate at Labor Market Entry on Earnings and Income by Demographic Groups



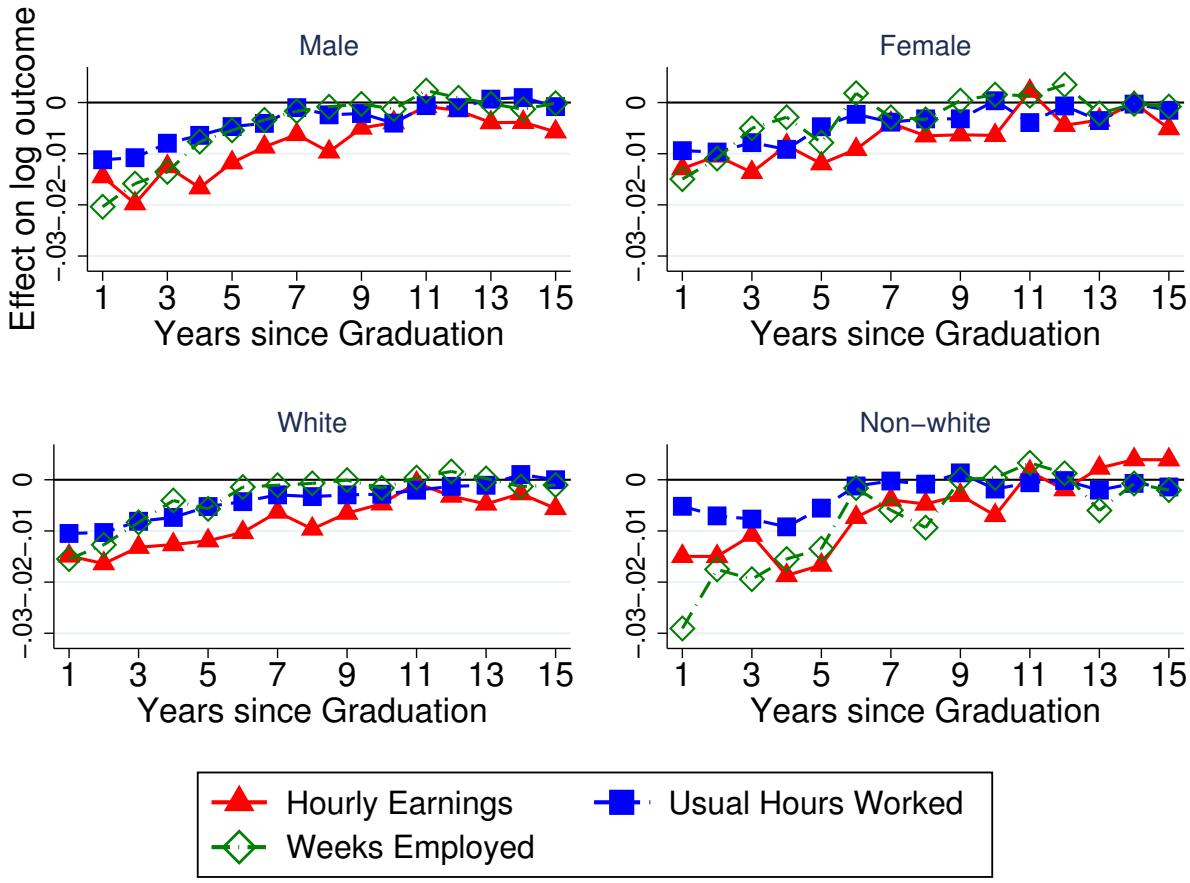
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 6: Effect of State Unemployment Rate at Labor Market Entry on on Earnings and Income by Education Groups



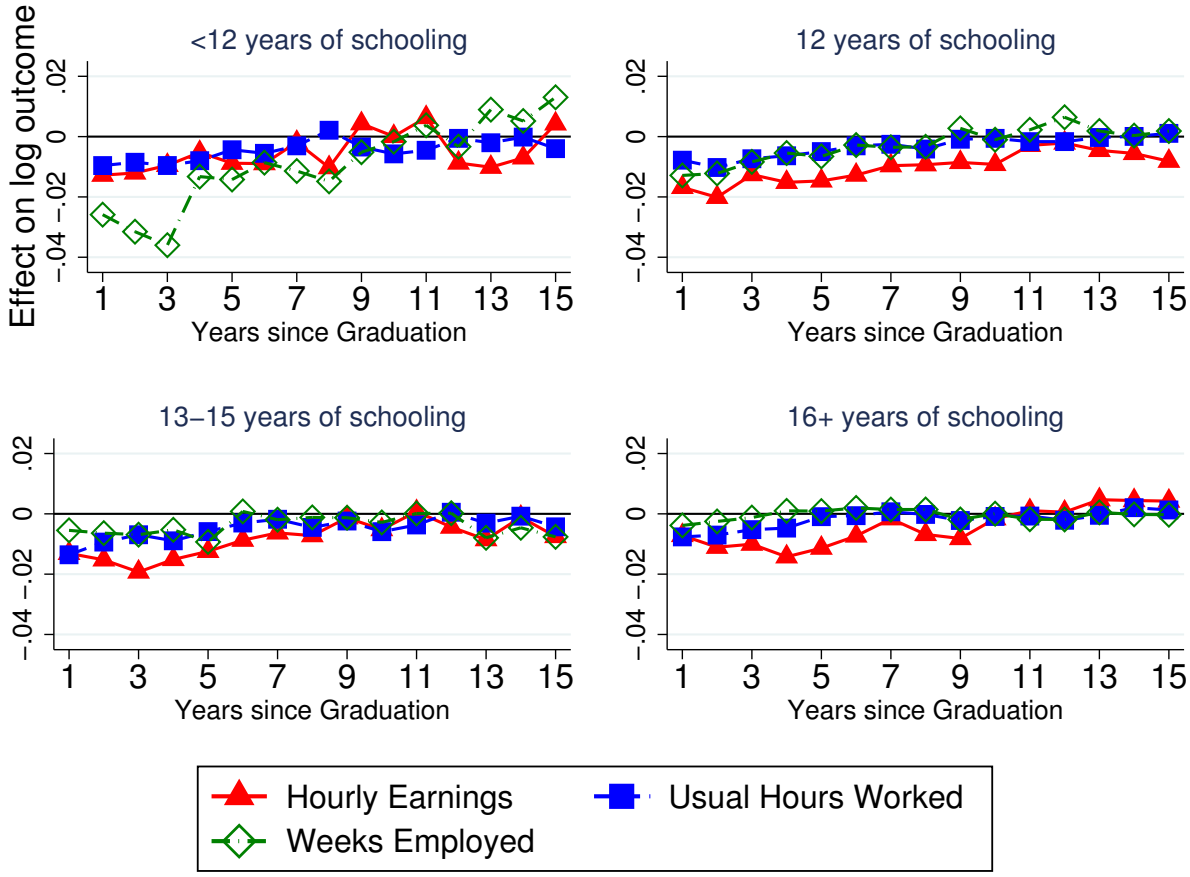
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 7: Effect of State Unemployment Rate at Labor Market Entry on Employment and Wages by Demographic Group



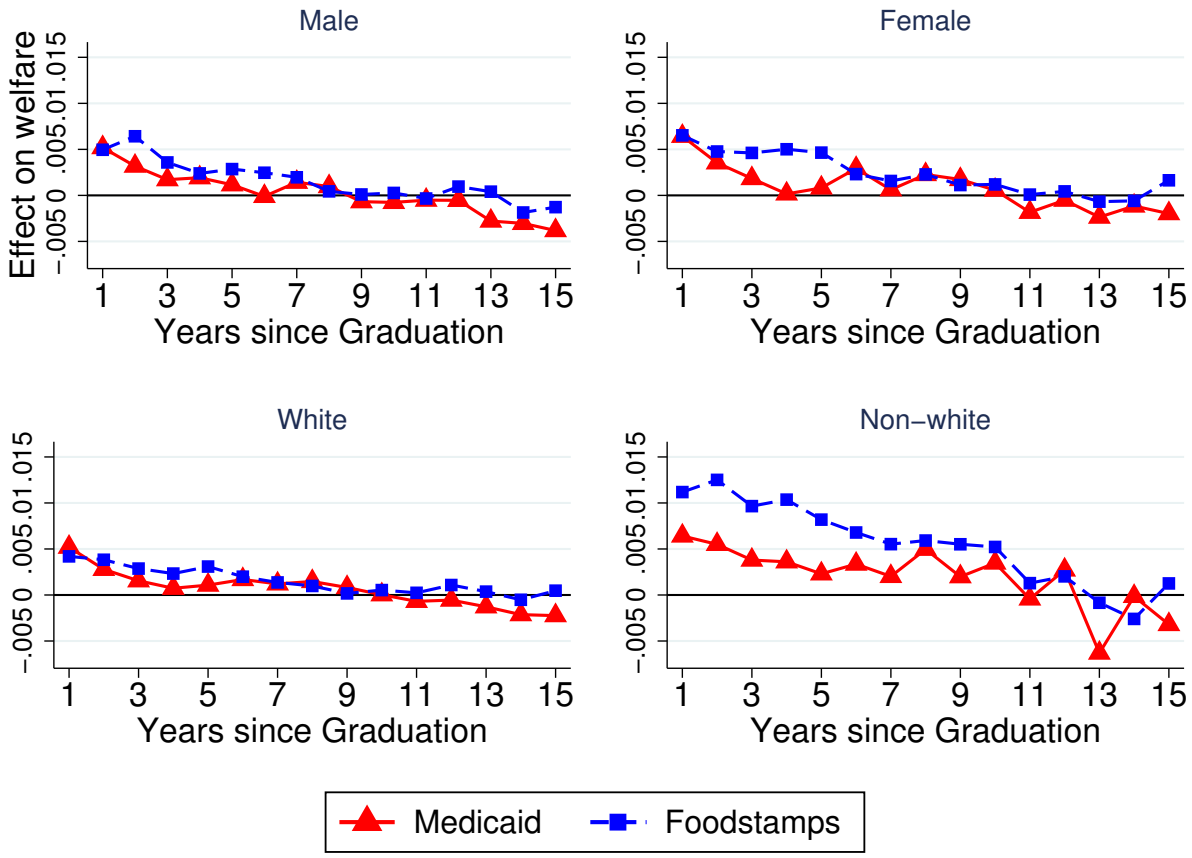
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 8: Effect of State Unemployment Rate at Labor Market Entry on Employment and Wages by Education Groups



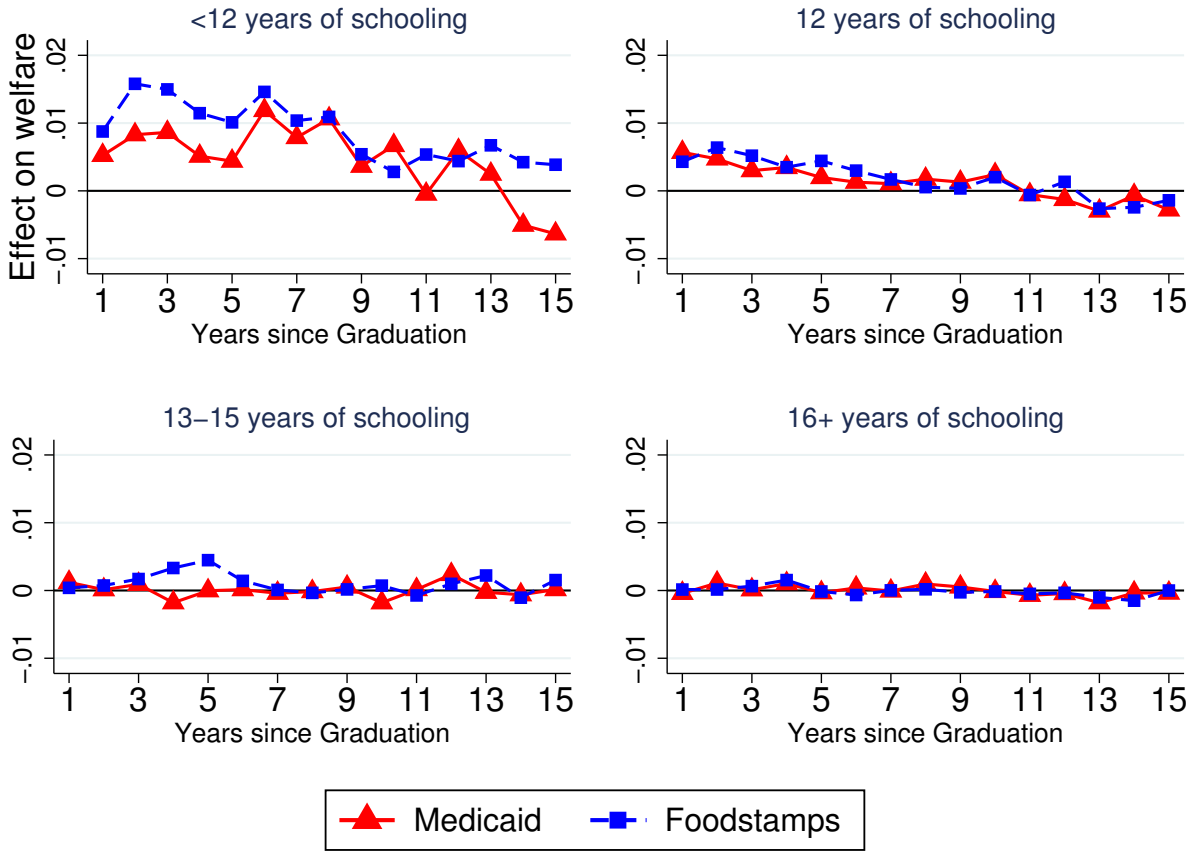
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 9: Effect of State Unemployment Rate at Labor Market Entry on Food Stamps and Medicaid by Demographic Groups



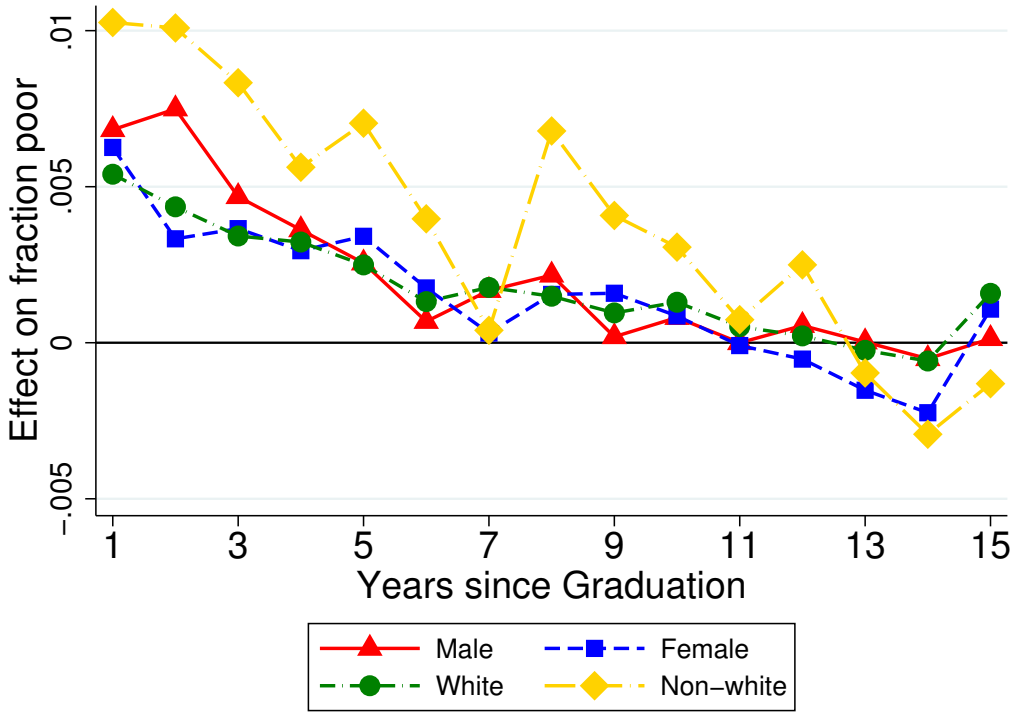
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 10: Effect of State Unemployment Rate at Labor Market Entry on Food Stamps and Medicaid by Education Groups



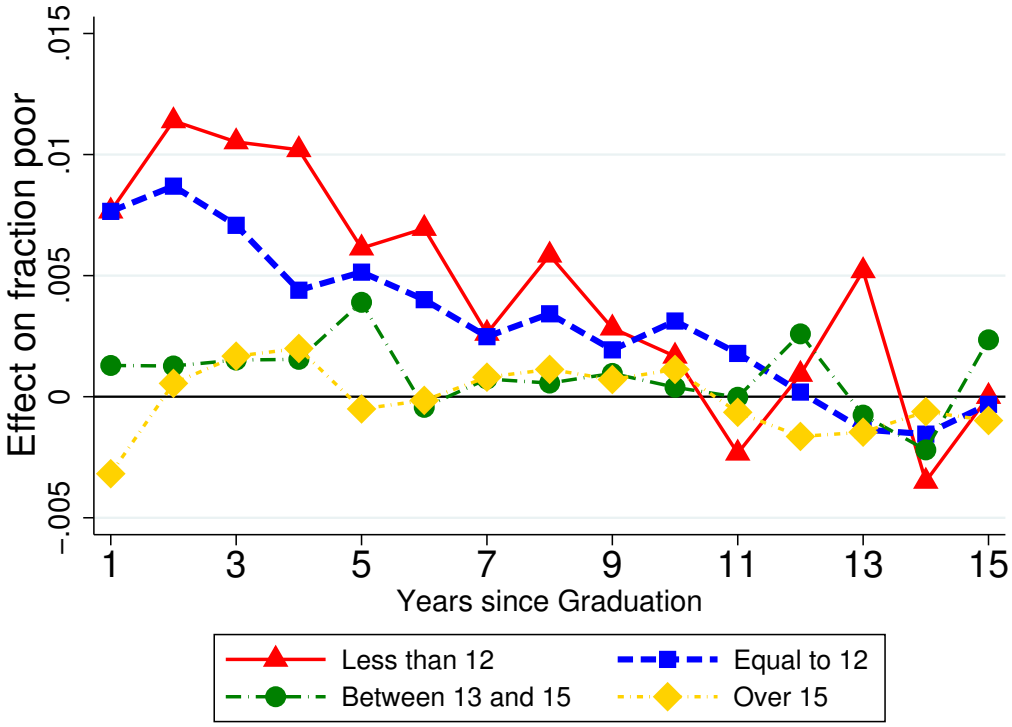
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 11: Effect of State Unemployment Rate at Labor Market Entry on Incidence of Poverty by Demographic Groups



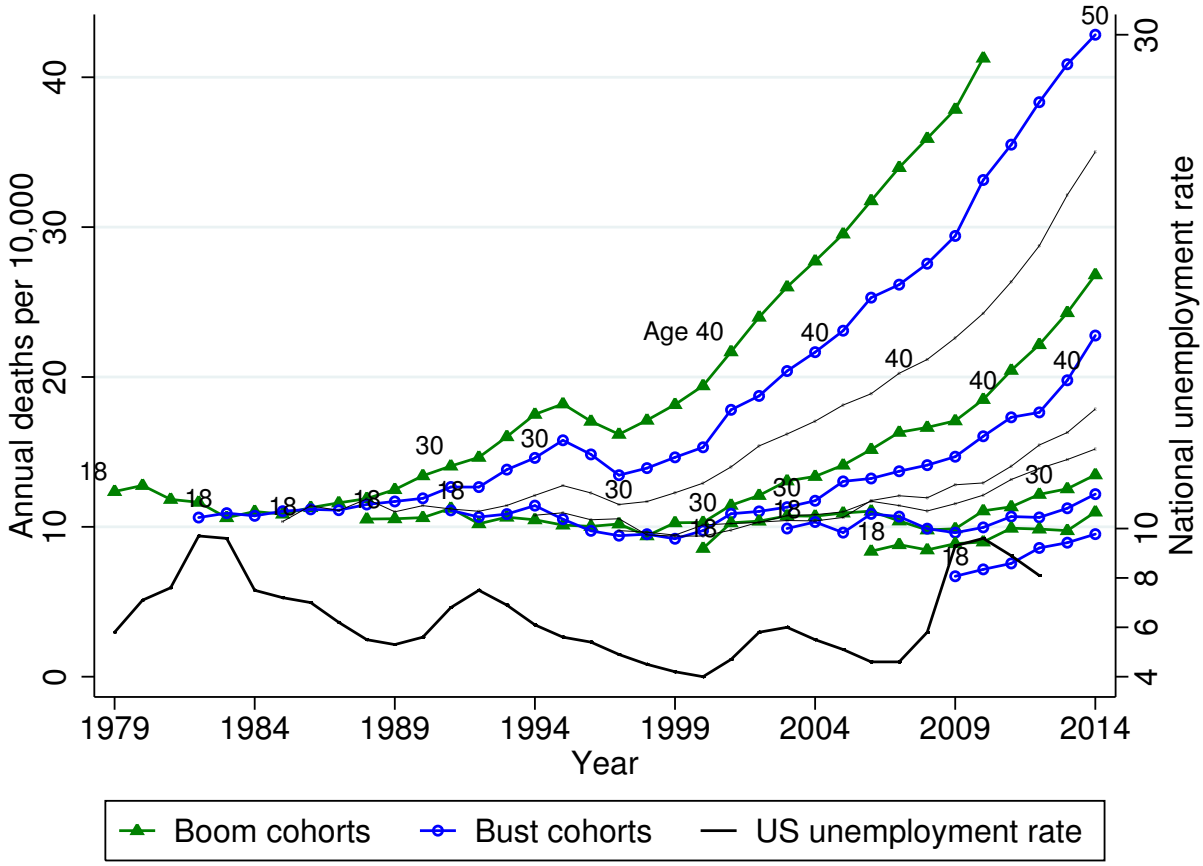
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 12: Effect of State Unemployment Rate at Labor Market Entry on Incidence of Poverty by Demographic Groups



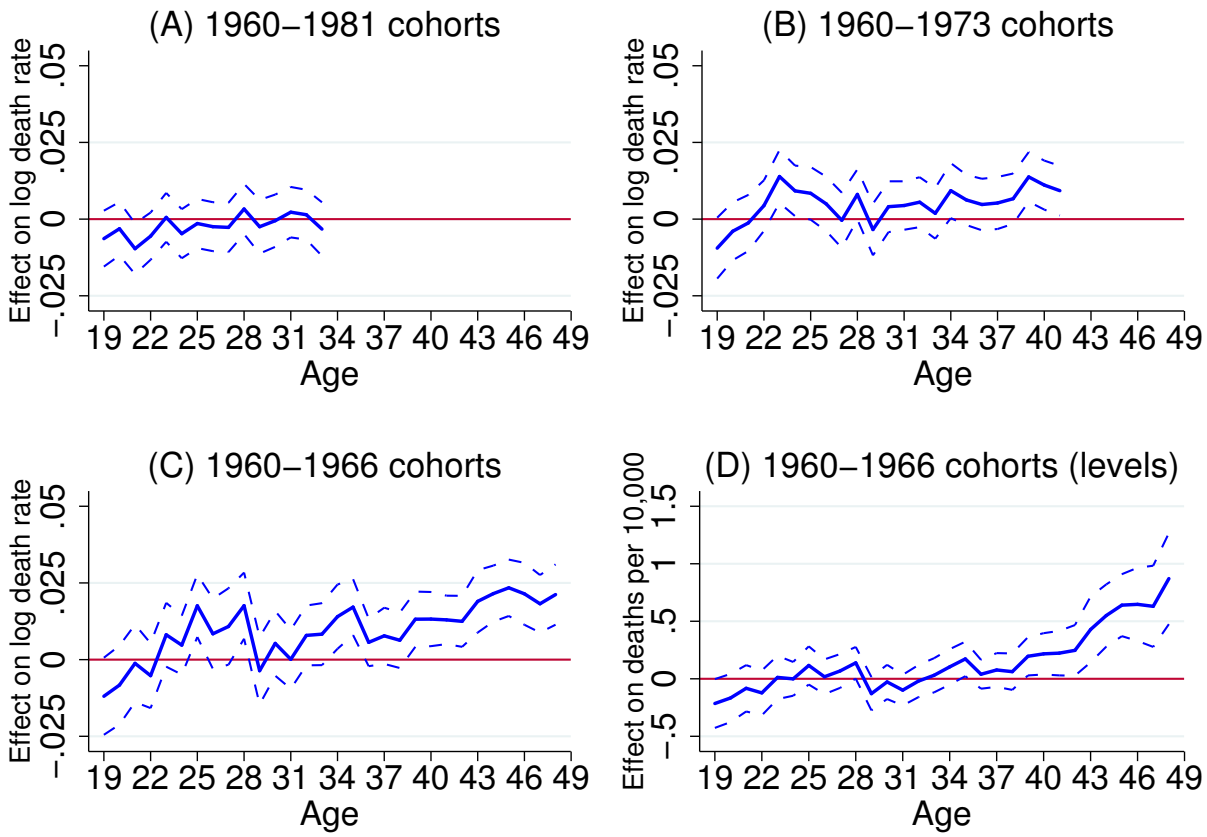
Notes: Results are based on ASEC Supplement to CPS from 1976 to 2016.

Figure 13: Mortality profiles by cohort and U.S. wide unemployment rate 1979-2014



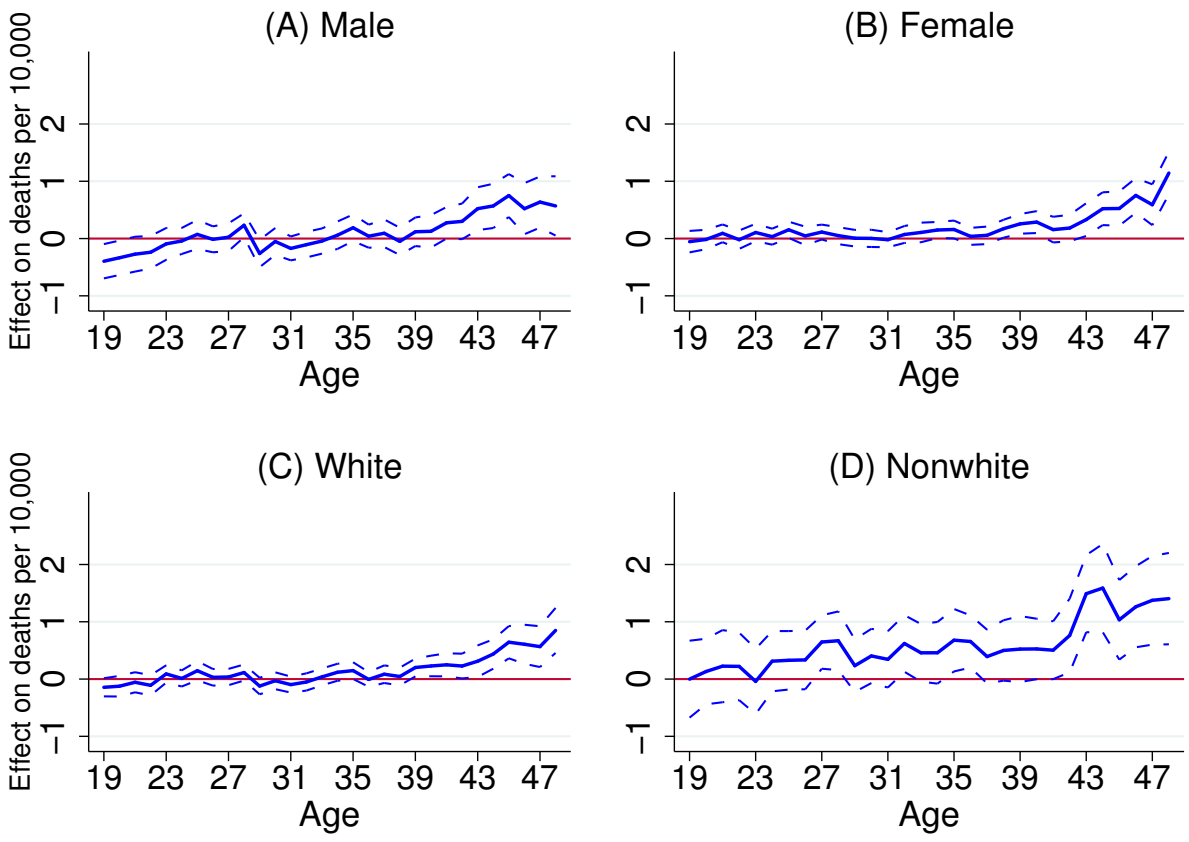
Notes: Raw mortality rates for U.S. born cohorts are plotted over time. Boom and bust cohorts are assigned in reference to the nationwide unemployment rate at age 18.

Figure 14: Effect of predicted graduation year unemployment rate on mortality



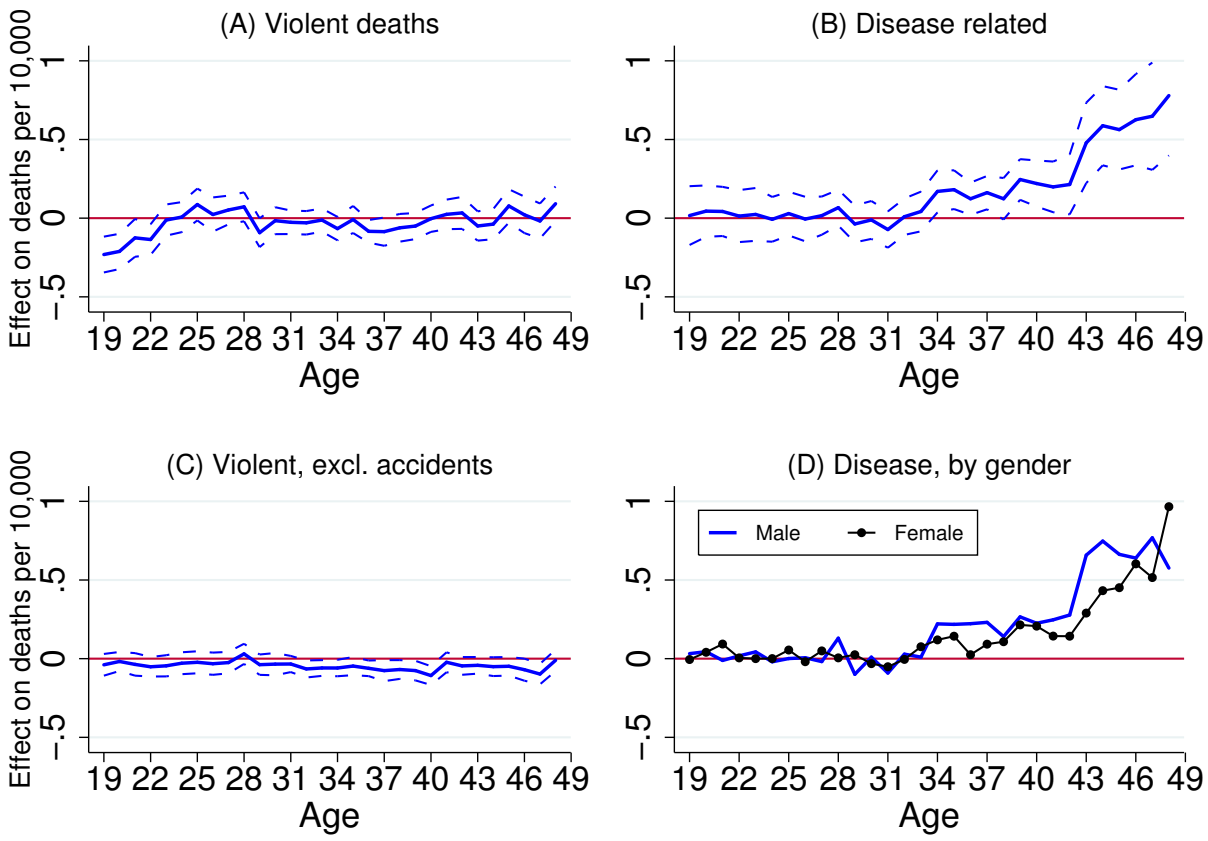
Notes: Effects of the predicted graduation year unemployment rate on mortality at different ages are plotted. The predicted unemployment rate corrects for migration and endogenous timing of labor market entry and is described in Section 2.2. The different panels include only those birth cohorts that are observed across the entire respective age range.

Figure 15: Effect of predicted graduation year unemployment rate on mortality, by gender and race



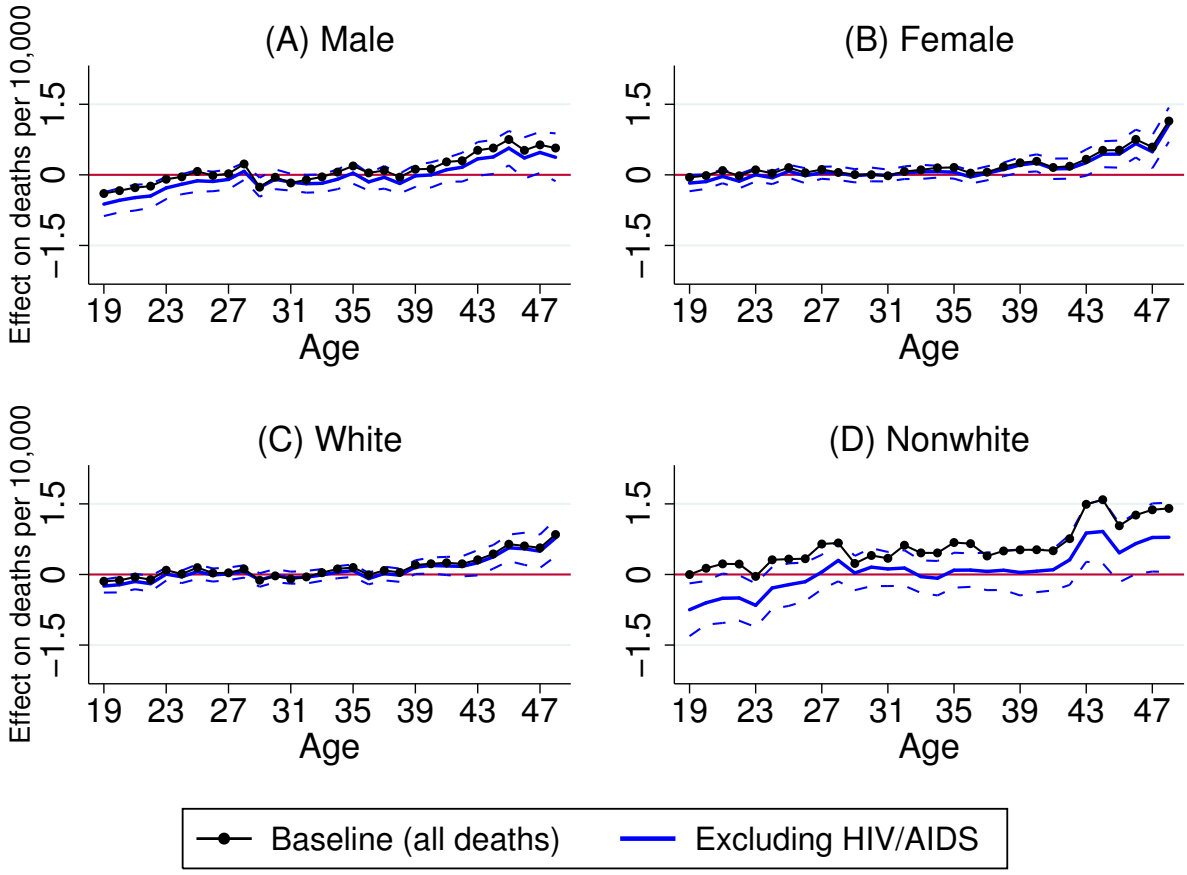
Notes: Effects of the predicted graduation year unemployment rate on mortality at different ages are plotted. The predicted unemployment rate corrects for migration and endogenous timing of labor market entry and is described in Section 2.2. The analyzed sample includes cohorts born from 1960 to 1966.

Figure 16: Effect of predicted graduation year unemployment rate on mortality, by cause



Notes: Effects of the predicted graduation year unemployment rate on mortality at different ages are plotted. The predicted unemployment rate corrects for migration and endogenous timing of labor market entry and is described in Section 2.2. The analyzed sample includes cohorts born from 1960 to 1966.

Figure 17: Effect of predicted graduation year unemployment rate on mortality, by cause



Notes: Effects of the predicted graduation year unemployment rate on mortality at different ages are plotted. The predicted unemployment rate corrects for migration and endogenous timing of labor market entry and is described in Section 2.2. The analyzed sample includes cohorts born from 1960 to 1966.