Unlucky Cohorts: Income, Health Insurance and AIDS Mortality of Recession Graduates

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Abstract

Recent studies have found that the unemployment rate graduates face when entering the labor market has a strongly negative and persistent effect on subsequent income. In this paper I investigate whether this arguably exogenous variation in income and socio-economic status is related to health insurance coverage and subsequent mortality. Using data from the CPS, I first show that graduating in a recession is not only associated with lower subsequent income but also with worse health insurance coverage. These effects are similar across gender, but much stronger for non-whites than for whites. Next, I show that results carry over when I use the unemployment rate at age 18 instead of the actual graduation age. This allows to analyze effects on mortality in the Vital Statistics, that do not report graduation age. For the baseline period, 1979 to 1991, I find strong positive effects of the unemployment rate at age 18 on mortality at ages 28-33. This effect is driven by AIDS deaths, it is homogeneous across gender but stronger for non-whites. The effect fades out when adding more years to the analysis and is close to zero in the overall period from 1979 to 2004. These results suggest that during the outbreak of a deadly epidemic recession graduates face much higher mortality rates. I argue that the negative effects on income and health insurance coverage might be a plausible mechanism underlying this finding.

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1 Introduction

A number of recent studies have shown that graduates who face a bad economic environment when entering the labor market suffer strong and persistent income losses.[1] In this paper I investigate for a sample of young adults the effects of graduating in unfavorable economic conditions on further socio-economic outcomes and on mortality. I find that unlucky recession cohorts do not only earn less. They also have worse health insurance coverage. And during the outbreak of the HIV/AIDS epidemic mortality rates have been significantly higher for these cohorts. The negative effects on income and health insurance seem the most plausible mechanisms underlying the effects on mortality.

The strong relationship of socio-economic status and health outcomes such as mortality has been documented by a broad literature in economics as well as in medicine and biology.[2] However, so far little is known about the causal mechanisms underlying this relationship. A number of studies have exploited income or wealth shocks to investigate short term effects on health outcomes.[3] Exogenous and persistent income differences that would allow to investigate gradually accumulating long-term effects of income and socioeconomic status on health are typically hard to find. Income differences due to economic conditions at graduation offer the opportunity for such an analysis.

In this paper I use data from the CPS to replicate the effects of the graduation unemployment rate on income found in the previous literature. I find that a one percentage point increase in the unemployment rate at graduation leads to an annual income loss of initially 3 % which decreases to 1.5 % over the subsequent 15 years. Further, I find strongly negative effects on the probability to be covered by private health insurance and a positive effect on Medicaid coverage. These effects on income and health insurance are stronger for non-whites than for whites but similar across gender. Next, I show that results largely carry over to a specification in which I use the unemployment rate at age 18 instead of the actual graduation age. This allows me to analyse effects on mortality in the Vital Statistics which do not report the year of graduation. For the baseline period, 1979 to 1991, I find strong positive effects of the unemployment rate at age 18 on mortality at ages 28 to 33. This effect is driven by AIDS deaths, it is homogeneous across

[1] Oreopoulos, von Wachter and Heisz 2012; Genda, Kondo and Ohta 2010; Kahn 2009; Beaudry and DiNardo 1991

[2] For reviews of the economic literature see Deaton (2003) or Cutler, Deaton and Lleras-Muney (2006), Cutler, Lleras-Muney and Vogl (2011).

[3] For example Jensen and Richter (2003), Case (2004), and Gardner and Oswald (2007)

gender but stronger for non-whites. The effect fades out when adding more years to the analysis and is not distinguishable from zero in the overall period from 1979 to 2004. These results suggest that during the outbreak of a deadly epidemic recession graduates face much higher mortality rates. The timing of the most significant periods as well as the pattern across gender and race suggest that the negative effects on income and health insurance coverage might be important mechanisms underlying these findings.

The income effects that I find are similar to those reported in the literature. Using the CPS and the PSID, Beaudry and DiNardo (1991) show that a one percentage point increase in the unemployment rate at job start leads to a subsequent income loss of 2-3 %.[4] Oreopoulos, von Wachter and Heisz (2012), hereafter OWH, use Canadian administrative data on college graduates and find an effect of -2 % on income that fades to zero within 10 years. Stronger effects they document for the bottom tercile in the skill distribution. This group suffers initial income losses of 3 % and even after ten years annual income is depressed by 2 %. These effects for the lower skilled tercile are very similar to those presented in Beaudry and DiNardo (1991). The main mechanism OWH identify is that recession graduates take jobs at poorer quality firms when entering the labor market. In subsequent years high skilled graduates move to higher quality employers. Mobility across employers and industries is increased for this group and the average quality of the employer improves over time. The lower skilled graduates, on the other hand, are stuck at low quality firms.[5] This mechanism is in line with my findings. The effect of graduating in a recession is small and vanishes within a few years. And the negative effects on income as well as private (mainly employer-provided) health insurance is suggestive that recession graduates take lower quality jobs.

My findings relate to the economic literature that analyses effects of health insurance on health outcomes. The RAND Health Insurance Experiment investigated the effects of randomized insurance co-payments for insured individuals, finding little evidence of effects on this intensive margin (Newhouse et al. 1993). The currently ongoing Oregon health care experiment randomizes at the extensive margin instead. First results show strong effects on physical and mental health (Finkelstein et al. 2012). Card et al. (2009)

[4] Further, they document a dominant effect of the minimum unemployment rate since job start. They interpret this finding as supportive evidence for a labor market model in which workers are protected from the contemporaneous unemployment rate through their employment contracts but at the same time mobile to move to other jobs or to renegotiate contracts in order to benefit from improvements in economic conditions.

[5] This mechanism of catchup through increased mobility is in line with the model developed in Beaudry and DiNardo (1991).

exploit age discontinuities in the eligibility for Medicare while Currie and Gruber (1996a, 1996b) analyze changes in eligibility rules for Medicaid. They find positive effects on the (health and) survival of elderly, newborns and children, respectively. My findings suggest that, during the outbreak of a deadly epidemic, health insurance might also matters for the survival of young adults.

Business cycle effects on mortality have been documented in existing papers. Ruhm (2000) shows that mortality is procyclical, i.e. negatively related to the contemporaneous unemployment rate. Miller at al. (2010) and Stevens et al. 2011 show that the procyclicality of mortality is driven by age groups that are not participating in the labor market: the young and in particular the very old. They argue that personal health care is cheaper in times of high unemployment, implying that the findings of Ruhm (2000) are in fact driven by a positive income effect. Van den Berg, Lindeboom and Portrait (2006) analyze the long run effect of the business cycle at the time of birth on mortality later in life. They find that those born in a recession live a few years less than those born in a boom time. Coile, Levine and McKnight (2012) use a similar strategy as the one in this paper to investigate the effects of business cycle fluctuations in pre-retirement years on subsequent mortality. The authors show that higher unemployment before retirement age reduces mortality in the short run. In the long run, however, the overall effect on longevity is negative. Reductions in health insurance coverage and health care utilization in the years following times of high unemployment seem to be important channels. Summing up, these papers provide further evidence of the business cycle's dramatic effects on people's lives.

2 The HIV/AIDS epidemic

For the interpretation of the effects on AIDS deaths found in this study, a brief overview of the HIV/AIDS epidemic is necessary. HIV is a virus that causes AIDS, a condition which progressively destroys the immune system making the body vulnerable to opportunistic infections and eventually leading to death. The main modes of exposure are male to male sex (46 % among persons reported with AIDS), followed by injection drug use (25 %) and heterosexual contact (11 %) (CDC 2001). Without treatment, AIDS is developed about 10 years after infection with HIV and leads to death in about 9 months. HIV/AIDS treatments increase survival time by about 4 to 12 years but to date there is no known cure (Tassie 2002, King et al. 2003).

In Figures A1 to A4 I illustrate overall deaths and AIDS deaths by cohorts and years for all US-born decedents. These numbers are drawn from the Vital Statistics and represent the universe of deaths in the population that is analyzed in this paper.

The first AIDS deaths occurred in 1981 but only in 1986 the epidemic started to take off (Figure A1 [b]). In the late 1980s and early 1990s the number of AIDS deaths increased steadily until 1995 whereupon it dropped abruptly and stayed at a rather constant level after 1997. 1995 to 1997 was the time that more effective antiretroviral therapies became widely available (Chiasson et al. 1999). The peak around 1995 can also be seen in overall death (panel [a]). For the most affected cohorts every 4th to 5th death in the early 1990s was caused by AIDS (panel [c]).

A major legislative development that does not directly show up in the mortality data is the Ryan White Act that has been signed in August 1990. This program provides essential medications to those infected persons who are uninsured or are unable to afford the cost of HIV related medicines. It started with a budget of $257m in 1991 increasing up to $2.3b in 2011. As of 2011, the program still serves more than half a million low-income people who are infected with HIV/AIDS. Of those 33 % are uninsured and 56 % are underinsured (Johnson 2011).

Figures A2 (a) to (f) show these numbers separately for men and women. For the included birth cohorts (i.e. in the respective age range) men die in greater numbers in all of the years (panel [a] and [b]). Still the hump around 1995 is more pronounced for men. Panels (c) and (d) show impressively that AIDS has been - at least until the late 1990s - mainly a male epidemic. During the peak years the number of male AIDS deaths is about four times higher among males for the most affected cohorts. Relative to overall deaths, AIDS death rates for these cohorts are about twice as large for men than for women. After 1997, however, male and female AIDS death rates strongly converge and equalize for the youngest cohorts in the early 2000s.

Figures A3 (a) to (f) present these graphs split up by race, showing deaths for whites and non-whites. Overall deaths (panels [a] and [b]) are higher for whites due to the larger population share of this group. About 84 % of the sample is white (see Table 1). However, during the outbreak of the epidemic AIDS deaths of whites are just about one third higher for the most affected cohorts (panels [c] and [d]) and similarly high for younger

cohorts. After 1997 there are more AIDS deaths among non-whites than among whites across all cohorts. Panels (e) and (f) show AIDS deaths relative to race-specific overall deaths. During the peak years 20 % of overall deaths are due to AIDS for whites in the most affected cohorts, while for non-whites in these cohorts almost every third death is caused by AIDS. After 1997 AIDS death fraction among white cohorts converge towards 2-3 % while non-white cohorts seem to converge to a higher level around 10 % .

There is an important distinction between the observed gender and race AIDS gap. While the gender gap is mostly driven by biological sex-specific modes of exposure the race gap reflects socio-economic differences (Chaisson 1995). In other words, the race gap in AIDS deaths implies that HIV/AIDS epidemic has dramatically increased socio-economic inequalities in mortality among young adults in the US.

In Figure A4 I illustrate this increase in the race mortality gap more directly. In this figure I plot the fraction of non-white deaths over time together with CPS estimates of the fraction of the non-white population.[6] In the absence of a race mortality gap these two graphs should lie on top of each other. Over the sample period the fraction of the non-white population increases smoothly from 14 % to 20 % due to younger cohorts with higher non-white shares that enter the sample over time. In 1979 the fraction of non-white deaths is 4 percentage points higher than the fraction of the non-white population. Thus, already before the outbreak of the AIDS epidemic non-whites died at a higher rate. In Figure A5 I plot the ratio of the non-white to white death rate that is implied by the numbers underlying Figure A4. The 4 percentage point gap in Figure A4 implies that the non-white death rate is about 30 % above the death rate of whites. In the following years up to 1985 the gap increases only slightly, from 30 % to 40 %. However, with the outbreak of the AIDS epidemic the gap increases dramatically, peaking in 1991. In this year about 16 % of the population is non-white but almost every third death comes from this minority group (Figure A4). The non-white death rate is about 110 % above the death rate of whites (Figure A5). After 1991 the race gap decreases linearly, reaching the 1985 level in 2004.

This uncovers an interesting pattern that is not evident in the previous figures. Unlike overall AIDS deaths the race gap does not peak in 1995. And it does not discontinuously

[6]Notice that in the Vital Statistics data I only include decedents born in the US while the CPS is a sample of the current population. As the non-white population is more likely to be born outside the US, the CPS estimates are likely to overstate the fraction of native non-whites. Therefore Figure A4 is likely to understate the race gap.

drop after 1995 when antiretroviral therapies became widely available. This suggests that the pattern around 1995 observed in the previous figures represent time effects that are homogeneous across race (and thus probably across socio-economic status). Instead the race gap peaks in 1991. This is the year when the White Ryan program started which was particularly directed to low-income groups and people with no or little health insurance. Figure A5 suggests that this program might have been more effective than overall AIDS deaths indicate.

The source of the observed dramatic differences in AIDS deaths across race remains an open question. Higher infection rates are likely to play an important role but even in samples of HIV infected subjects strong differences in survival rates by race and socio-economic status remain (Rothenberg et al. 1987, Chaisson 1991). Chaisson et al. (1995) find that these differences are not driven by biological factors. They show that among patients with HIV infections who received medical care from a single urban center disease progression or survival rates were not associated with race or socio-economic status (nor with sex or injection drug use). This points to a story in which access and use of medical care might play an important role.

3 Data

The data used in this paper come from two main sources: The US Vital Statistics (Vital Stats) which provides information on every single death in the US starting in 1968; and the March Supplement of the Current Population Survey (CPS), which provides a vast set of socio-economic characteristics for a repeated cross-section since 1962. About 55,000 individuals are surveyed per year in the 1960s, about 150,000 in the 1970s to 1990s and about 200,000 after 2000. In order to relate deaths and socio-economic outcomes from these two data sets to the graduation unemployment rate information on the respondent's (decedents) year and the state of graduation are required.

The death files from the Vital Stats include the date and the cause of death along with core demographic characteristics such as age, sex and race. Information on the decedent's education, which would allow to calculate the year of graduation, is included only after 1989. I use the year when the decedent was 18 as a proxy for the graduation year. From 1979 to 2004 the death certificates include the state of the decedent's birth which I use as proxy for the state of graduation. This restricts the sample to decedents born in the US.

The CPS reports the number of years of education until 1991. This allows to calculate the respondent's year of graduation as calendar year minus age plus 6 plus years of education. The state of current residence is included in all waves, which I use as a proxy for the state of graduation.

State-level unemployment rates are available from the Bureau of Labor Statistics only since 1976. Therefore I have to exclude individuals who graduated before 1976 when using the actual graduation year and individuals who were born before 1958 (i.e. of age 18 before 1976) when using age 18 as proxy for graduation age. Further I restrict my main analysis to the years 1979 to 1991 when both state of birth is available in the Vital Stats and years of education is reported in the CPS. In this sample respondents or decedents have at most 15 years of potential experience (when using the actual graduation unemployment rate) or are at most of age 33 (when using the age 18 unemployment rate). The extended sample includes the years 1979 to 2004, restricted to cohorts that graduated / were of age 18 between 1976 and 1991. Table 1 presents summary statistics for these different samples.

In order to calculate death rates I supplement the Vital Stats with population estimates from the decennial 5 % US Census 1980, 1990 and 2000. I sum up the Census estimates by year of birth and state of birth in each year and apply a linear interpolation for the intercensus years. Despite the Census' enormous sample size, population estimates by year and state of birth are noisy for the subgroup of blacks, with death rates above one in some cases. For the subgroup analysis of AIDS deaths I therefore use non-AIDS rather than Census population estimates as a proxy for cohort size.

4 Empirical Strategy

I seek to analyze the relationship of economic conditions at the year of graduation with socio-economic outcomes and mortality. For a causal interpretation we need the economic conditions to be orthogonal to other independent determinants of these outcomes.

The economic conditions at future stages in one's life are unpredictable and whether the years around one's graduation fall in a recession or a boom time can be considered to

be basically luck. Graduating in one year or the other, however, might be endogenous – e.g. those with higher potential earnings might manage to continue schooling in a recession until the job market improved. Existing studies show that endogenous graduation timing is not a big issue (OWH, Genda et al. 2010, Kahn 2009). Also, in the main specifications I am using the year when a cohort is of age 18 as predicted graduation year which determined solely by the cohort's year of birth and cannot be influenced by contemporaneous business cycle fluctuations. Further, some states may permanently have worse economic conditions than others and people in these states might be different a priori. And a recession might come along with other macro shocks that directly affect our outcomes of interest. But I control for state and cohort fixed effects to take care of such potential confounding factors. The key variables for the empirical strategy used in this paper are the year of graduation and the state of graduation.

In the CPS I proxy state of graduation by the state of current residence. This could be a problem as graduates that face high local unemployment might seek to migrate to states with more favorable labor market conditions. If those with higher potential income are better in escaping from local recessions by migrating to better states this would introduce an upward bias. We would only observe those members of a recession cohort in their state of graduation who did not make it to a better state and who would have had lower incomes in any case. OWH show that regional mobility is small and short lived for Canadian graduates. I provide evidence that such selection seems not to be relevant in the CPS either.

In the Vital Statistics data I use the year when the decedent was of age 18 as a proxy for the graduation year. The unemployment rate at age 18 is obviously only a rough proxy for the economic conditions that a cohort faces over the years it is entering the labor market.[7] Alternatively I could use a weighted average of the unemployment rates over all graduation years of a cohort, with the cohort fractions graduating in each year (estimated from the CPS) as weights. Results tend to be similar for such a specification. But using the economic conditions over a time window of several graduation years limits the number of cohorts and calendar years that can be included.[8] An advantage of this

[7] In the CPS data I show that the majority of the typical cohort in my sample graduates after 12 years of education. Given that children typically start school at age 6 the modal graduation year in my sample is at age 18. Also OWH find that those with lower (hypothetical) education suffer more from graduating in a recession. In order to capture as much of the recession effects as possible it is therefore useful to choose an age at which high school graduates rather than college graduates enter the job market.

[8] State-level unemployment rates are available only since 1976. Let's say we want to use the average unemployment rates for the years in which a cohort is of age 16-24 and only include a cohort once it

simple proxy over the actual year of graduation is that it rules out potential selection due to endogenous graduation timing.

I use the state of birth (instead of the state of residence, which is also available) as a proxy for the state of graduation. This is a conservative choice. Endogenous migration of graduates from recession to boom states would in this case attenuate estimates towards zero. The state of birth unemployment rate that I assign to a decedent would be above the unemployment rate that she actually faced when graduating. In other words, I would overstate the regressor of interest and therefore underestimate the corresponding coefficient.

My empirical strategy consists of three steps. First, I replicate OWH's specification in the CPS data, using the (reconstructed) actual year of graduation and state of residence as a proxy for state of graduation. Second, I show in the same sample that results largely carry over to a specification in which I use the unemployment rate at age 18 instead of the actual graduation year. Third, I turn to the Vital Statistics data to estimate the effect of the unemployment rate at age 18 on mortality using state of birth as proxy for state of graduation

The key variable of interest, the graduation unemployment rate, varies only across states and cohorts. I follow OWH and collapse the individual-level data at the level of state, cohort and calendar year. In the CPS specification using the actual graduation year the data are collapsed by state-of-residence x graduation year x calendar year. Using instead the age 18 unemployment rate I collapse the data by state-of-residence x year-of-birth x calendar year. The Vital Statistics data are collapsed by state-of-birth x year-of-birth x calendar year. All regressions are weighted by the corresponding cell sizes. Standard errors are clustered at the cohort x state level to account for cohort specific serial correlation.

As OWH I use the following specification with the actual graduation year:

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

turned 25. The oldest birth cohort for which state unemployment rates are available from age 16 onwards is born in 1960. And the first calendar year in which we could include this cohort is 1985. Using instead age 18 unemployment rates, the oldest included birth cohort is born 1958 and the first calendar year this cohort can be included is 1977.

The indices g, s, t and e refer to the graduation cohort, state, calendar year and years of potential experience (years since graduation). $\bar{y}$ are different socio-economic outcomes collapsed at the level of graduation year, state and calendar year. 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). 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 variations in graduation unemployment rates. Since I 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).

Using the unemployment rate at age 18 as a proxy for the graduation unemployment rate changes the regression equation slightly:

$$\bar{y}_{c,s,t} = \alpha + \beta_a u^{A18}_{c,s} + \gamma_a + \delta_c + \lambda_s + \theta_t + \epsilon_{c,s,t} \tag{2}$$

With c indicating the year of birth (instead of the year of graduation) and a years of age. $u^{A18}_{c,s}$ refers to the unemployment rate in state s in the year when birth cohort c was of age 18. I include age dummies instead of dummies for potential experience ('e' in the previous equation). In the mortality regressions $\bar{y}_{c,s,t}$ refers to the sum of deaths or to death rates at the level of year of birth x state of birth x calendar year level. Regressions are weighted by

5 The effect of the graduation unemployment rate on socio-economic outcomes

Given the large number of estimated coefficients of interest in each regression it is most convenient to present the results in a graphical way. Tables with the corresponding regression results are reported below for the core regressions.

Figures 1 - 5 illustrate the estimated effects of the unemployment rate around graduation on various socio-economic outcomes. For each outcome, three sets of specifications are estimated. The first set (a) uses the unemployment rate at the actual graduation year, which is calculated as the year of birth plus 6 plus years of education (equation 1). The second set (b) takes the year when the individual was of age 18 as proxy for the graduation year (equation 2). The third set (c) uses the same specification as (b) but also includes the CPS waves 1992 to 2004 for which education is reported in years. The effects of the graduation unemployment rate are interacted with dummies that indicate the number of years passed since the graduation date (in [a]) or the years of age (in [b] and [c]). This allows the effect of graduating in a recession to vary over time.

Figure 1I presents the effects of graduating in a recession on the natural logarithm of income. For the regression results underlying this figure see Table 2. Figure 1Ia illustrates a strongly negative and persistent effect of the unemployment rate at the actual year of graduation on annual wages. A 1 percentage point higher unemployment rate decreases wages in the first year after graduation by 3.5 %. This is a large effect. A typical recession with an increase in unemployment by 5 percentage points would be associated with an income loss of 17.5 %. This effect if strongly persistent over time. Even 15 years after leaving high school or college recession graduates face an average annual income loss of 7.5 %.

These effects are about twice as large as the estimates reported in OWH for Canadian college graduates. Importantly, however, I do not only include college graduates but also graduates from high school and even high school drop-outs. OWH show that those with lower income degrees suffer more from graduating in a recession. Their estimated effects for the bottom tercile are remarkably similar to my estimates, starting at -3 % the year after graduation and increasing to about -1.7 % after ten years (OWH, Figure 4A).

Figure 1Ib illustrates the estimated effects of the unemployment rate at the year when

the individual was 18 on wages at age 19 to 34. Until age 24 the estimates are very similar to those in Figure 1Ia when using the actual year of graduation. A cohort that faces a 1 percentage point higher unemployment rate at age 18 suffers an income decrease of about 3 % around age 20. After age 24, however, this negative effect of the age 18 unemployment rate fades out reaching zero at age 29, while this is not the case when using the actual graduation year.

This difference in estimated effects could point to a negative selection due to endogenous graduation timing that might explain part of the negative effect at higher years of potential experience in panel (a). But as shown below selection seems not to be a driving force. A more plausible explanation seems to be that at higher ages the fraction of "treated" people who actually graduated at age 18 decreases and so does the average effect of the age 18 unemployment rate.

Figure 1Ic repeats this specification including the years 1992 to 2004 for which no information on the exact years of education is available. Confidence intervals are smaller for higher ages due to larger sample sizes in these age cells. Point estimates, however, are very similar to those plotted in panel (b). This suggests that the estimated effect on income is not strongly affected by the sample period.

Figures 1IIa-c repeat these regressions for the natural logarithm of household income for the subsample of household heads and their spouses. Results are very similar to the estimates in Figures 1.

Figure 2Ia illustrates that unemployment rates are not affected in the first year after graduation, increasing to 0.032 in the second year and returning back to zero in the five subsequent years. The zero effect in the first year is surprising. Unemployment rates are positively autocorrelated and regressions do not control for the contemporaneous unemployment rate so that one would expect to see an elevated fraction of regression graduates without a job. This is the pattern shown in panels (b) and (c) for the age cohort specification. A 1 percentage point increase of the unemployment rate at age 18 increases a cohorts unemployment rate at age 19 by about 0.4 percentage points. Over the subsequent fiver years the effect goes to zero, following a similar path as in (a). Panel (II) plots the coefficients for the regression of the fraction that is not participating in the labor force. This fraction is significantly elevated in the first year after graduation in the 'exact' graduation year specification (a) which explains the zero effect on unemployment

in panel (I). Recession do not enter the labor market directly after graduation or leave it again, for example to pursue further education. It seems that high school graduates are on average less flexible than college graduates since this positive effect on non-participation is not observed in panels (IIb) and (IIc). This suggest that high school graduates might be stronger hit by a recession at their graduation year then those who complete college. Note that the income regressions in Figure 1I which are similar across specifications only include labor market participants so that the effect in panel (a) might overstate the average effect on earnings if those who postpone labor market entry have higher (potential) earnings.

Figure 3 reveals that recession graduates are also less likely to be married. Facing a 5 % higher unemployment rate at graduation reduces the likelihood that you are married in the subsequent years by about 2 percentage point. This negative effect is quite consistent across specification and only slowly fades out over time, becoming insignificant five to seven years after graduation. Only for the highest experience years / ages, effects diverge somewhat between specifications but confidence intervals are large in panels (a) and (b), indicating that these differences are not significantly different.

Figures 4I-II illustrate the effects on health insurance coverage (see also Tables 4). A 1 percentage point increase in the unemployment rate around graduation increases the likelihood to be have Medicaid coverage by about 0.3 to 0.4 percentage points. Given an average Medicaid rate of about 7.8 % in the sample, this implies that a typical recession (a 5 percentage point unemployment increase) rises Medicaid coverage among graduates by about 15 %. This strong effect only decreases slowly over time, remaining significantly positive for a whole decade after graduation. The effect on private health insurance coverage is more short-lived and about half the size in relative terms. A typical recession decreases private health insurance coverage by about 5 percentage points, which refers to a 7 % decrease given an average rate of about 70 % in the sample. These estimates indicate that recession graduates suffer an extended period of inferior health insurance coverage.

Figures 5I-II show results for regressions with race and gender as dependent variables. These characteristics are constant over time and cannot be affected by economic conditions.[9] Any significant effects in these regressions are evidence of selection that might

[9]Except through selective mortality; however, even at the height of the AIDS epidemic the mortality rate in that age range was just around 0.0015 which is too small to make a significant difference in these

point to potential biases in the previous regressions. Observations in the CPS are drawn from repeated cross-sections and grouped by the respondents year of birth (/graduation) and state of residence. Heterogeneous migration across gender or racial groups in response to the graduation unemployment rate could introduce selection. Estimates in 5Ia-b indicate that selection seems not to be an issue in the baseline sample period. None of the estimated effects in the race and gender regressions is significantly different from zero, except for age 33 in panel (Ib) which might be evidence of race specific selection in the last age group. In the extended sample (c), however, the fraction of white respondents is slightly elevated and significantly different from zero after age 24. This indicates that the identification strategy in the CPS which assumes no systematic migration might not hold for higher ages in the extended sample. However, it seems unlikely that this selection is driving the previous results which were similar across the different specifications. Further notice that selective migration would not be an issue in the mortality analysis which is based on people's state of birth that is not affected by later migration.

There are three main results from these CPS figures. First, I replicate the findings from OWH in the CPS data. Effects on income tend to be stronger but the overall pattern is similar. Second, I show that there are also effects on marital status and strong effects on health insurance coverage. The latter finding is particularly relevant when looking at health outcomes such as mortality. Third, using the unemployment rate at age 18 instead of the actual year of graduation results in very similar effects. This allows to implement this identification strategy in data sets that contain age but no information on the years of education, as the Vital Statistics.

Before turning to the mortality data it is useful to look whether different socio-economic groups are affected differently by graduating in a recession. Figures 6 to 12 present results by gender and by race (white vs. non-white). I only show results for the age 18 unemployment rate and the CPS waves 1979 to 2004 (specification [c]), as this specification delivers the most precise results. For regression tables corresponding to Figures 6, 8 and 10 see Tables 5 to 7.

Across gender estimated effects are quite homogeneous for most outcomes, with the exception of Medicaid coverage. As shown in Figures 10 (a) and (b), the positive effect on the probability to be covered by Medicaid is slightly more prolonged for women. This is not surprising as Medicaid is particularly directed to women.

regressions.

Effects are less homogeneous across race. Confidence intervals are larger for the non-white regressions due to the smaller sample size, but estimates clearly suggest a stronger effect of the age 18 unemployment rate for this subgroup. The effects on individual income, household income as well as insurance coverage are about twice to three times as large as for whites. Only the negative effect on the probability to be married at age 18 and 19 seems to be driven by whites rather than non-whites.

This heterogeneity across race is in line with OWH's finding that those with lower potential income are affected more strongly by graduating in a recession. A further explanation might be that among whites there are relatively more individuals with more than 12 years of schooling, who are therefore not "treated" by the age 18 unemployment rate. Finding that effects on income and health insurance coverage are similar across gender but stronger for non-whites than whites is useful when turning to health outcomes such as mortality. If effects on health run through these channels we should observe similar heterogeneities in health outcomes.

Figures 12 (a) - (d) test for an effect of the age 18 unemployment rate on the cohorts' race composition by gender subgroups and on the gender composition by race subgroups. The slightly positive effect on the fraction of white respondents discussed for the overall sample above seems to be driven by women (Figure 12Ib). Again, estimates for the other outcomes are very similar across gender which indicates that this slight selection seems not to be distorting results.

6 The effect of the age 18 unemployment rate on mortality

In this section I explore to which extent graduating in a recession has an effect on the graduates' subsequent mortality. As the years of education are not reported in the death certificates in many years, I use throughout the unemployment rate at age 18 as a proxy for economic conditions at graduation for all mortality regressions (equation 2). All regressions control for state of birth, cohort, calendar year and age fixed effects.

Figure 13 illustrates the effect of the age 18 unemployment rate on the number of deaths per cohort, defined by state of birth and year of birth, in the baseline sample (1979 to 1991). Until age 27 effects are virtually zero. Starting at age 28, 10 years after the age for which the unemployment rate has been taken, estimates become positive and increase linearly with every additional year of age. At age 33 the point estimate reaches 21.7 deaths (see Table 8 for numerical regression results). This implies that a 1 percentage point increase in the unemployment rate at age 18 leads to about 22 additional deaths of 33 year-olds per state and birth cohort, or 1,122 additional deaths of 33 year-olds in the overall US in a given year.

How big is this effect in terms of the living population or in terms of the average death rate? And might the effect be driven by changes in cohort sizes? The regressions control for state and for year of birth fixed effects which takes care of potential changes in cohort sizes at the national level and time-constant differences in state sizes. However, cohort sizes might not only change at the national but also at the state level. And a particularly large cohort in a given state might lead to a higher unemployment rate in this state when this cohort turns 18 and enters the labor market. At the same time a larger cohort implies a higher number of deaths and the increasing effect after age 27 might just reflect the natural increase in the death rate as cohorts become older.

Figure 14 shows on the left y-axis the effect on the death rate in terms of 10,000 cohort members using the Census population estimates. In this specification a small hump shows up already at age 25, but the overall pattern is very similar to that in Figure 13. While effects are close to zero for the early 20s, estimates linearly increase after age 26. At age 33 the effect reaches 0.5. The effect of 22 deaths displayed in Figure 13 hence refers to 5 additional deaths per 100,000 cohort members, or an increase in the death rate by 0.005 percentage points. On the right y-axis of Figure 14 the effect is displayed in terms of the natural logarithm of the death rate, approximating the percentage change in the death rate caused by a 1 percentage point increase in the unemployment rate at age 18. The effect at age 33 reaches 0.04, meaning that the 5 additional deaths per 100,000 cohort members refer to a 4 % increase in the cohort's death rate. In other words, a typical recession at age 18 - an increase of the unemployment rate by 5 percentage points - leads to a 20 % increase in death rates at age 33. This seems to be a strong effect.

How plausible is such a large mortality effect for an age range in which causes of death are not strongly driven by income or health insurance (among young adults accidents,

suicides and violence are the most common causes of death in non-epidemic times). Could this relationship be particularly strong in the analyzed time period? To answer this question I extend the analyzed time period in the next two figures. Indeed, when including the years 1979 to 1995 the effects decrease strongly (Figure 15). The estimate at age 33 decreases from 21.7 to 15.7 additional deaths (see Table 8). When including the years 1979 to 2004 in Figure 16, the effect diminishes to 3.3 and is only slightly significantly different from zero. These results suggest that the strong positive age effect displayed in Figures 13 and 14 is driven by the years prior to 1992.

The rise of the AIDS epidemic falls into this time period and AIDS deaths would be a plausible driver underlying the estimated effects, killing mostly young adults in the age range that I am analyzing. Fortunately, the cause of deaths are reported in the Vital Statistics which allows to run separate regressions for AIDS and non-AIDS deaths.

Figure 17 shows estimates from such separate regressions. As it can be seen in panel (I) the positive and increasing effect on mortality after age 27 in the baseline time period is mostly driven by AIDS deaths (Ia). Estimates for non-AIDS deaths (Ib) do also increase after age 28 but this increase is small compared to the effects in the left figure and for most years not significantly different from zero. A potential explanation for the similar, though much weaker effect pattern in panel (Ib) could be that not all AIDS deaths are diagnosed as such in the death certificate. The two figures in panel (II) show a similar pattern for the sample period 1979-1995 with most of the mortality effect being driven by AIDS deaths but a small and significant increase in non-AIDS deaths after age 30. The bottom panel shows that the mortality effect which remains in the extended sample seems to be driven in similar ways by both AIDS and non-AIDS deaths.

Overall, the results in Figure 17 indicate that the strong mortality effects documented in the baseline period are driven by AIDS deaths. But why do estimated effects decrease when including the years 1992 to 1995 (Figure 15), a period during which overall AIDS deaths were still on the rise (see Figure A2)? A possible explanation could be the Ryan White Act HIV/AIDS program that started in 1990/1 and was directed to HIV-infected people with little or no health insurance coverage. As explained above this program might not have reverted overall trends in AIDS deaths as much as the introduction of more effective antiretroviral therapies in 1995. But it might have reduced socio-economic differences in mortality rates already since 1992 (Figure A5).

In Figures 18 I analyze the effects on AIDS mortality separately for men and women as well as for whites and blacks. For such subgroup analysis effects in terms of absolute numbers of deaths would not be informative because these groups vary considerably in size. However, as mentioned above Census population estimates by state of birth x year of birth are noisy for the small subgroup of blacks. A feasible alternative is to take non-AIDS deaths as a proxy for cohort size and use the fraction of AIDS deaths divided by overall deaths for each subgroup as dependent variable. To the extend that the age 18 unemployment rate also positively affects non-AIDS deaths (the right panels in Figure 17 indicate a slightly positive effect) estimated effects on these ratios are attenuated towards zero.

The comparison of the mortality effects across gender and race in Figure 18 resembles the heterogeneity found for income and health insurance effects in the CPS. The effects of the age 18 unemployment rate on AIDS death fractions is similar for males and females, while it is 1.5 to 2 times as large for blacks compared to whites. Notice that men have higher non-AIDS mortality rates in the analyzed age range (see Figure A2) so that a similar effect on the AIDS death fraction imply that the effect in terms of AIDS death rate (i.e. per capita) is stronger for males. Across racial subgroups, blacks have higher non-AIDS mortality between age 20 and 40. This means that the white-black gap in the effect on the AIDS death rate is even more dramatic than the difference illustrated in panels (c) and (d).

7 Interpretation

I find strong positive effects of the age 18 unemployment rate on AIDS deaths at age 28 to 33 during the outbreak of the AIDS epidemic. Including years after 1991 in my analysis the estimated effects decrease. Further the effects are homogeneous across gender but stronger for the black than for the white population.

How plausible are such effects? Given the rather short time period which seems to be driving these effects, the first decade of the AIDS epidemic, one might wonder whether my specification picks up a spurious correlation of the business cycle with the dramatic increases in AIDS deaths. All regressions include cohort as well as calendar year fixed effects. This means that any variation at the national level - both at the time when

the unemployment rate is measured as well as when deaths occur - is taken out. Still, there might be a spurious relationship at the level of individual states. Possibly those states that experienced a local recession in the late 1970s just happened to be hit particularly strongly by the AIDS epidemic a few years later. Ideally one would obtain more time-series or spatial variation of economic conditions at graduation to reduce the probability that estimates pick up an accidental coincidence of local recessions and local AIDS mortality episodes. Unfortunately, there is little what could be done to increase the statistical power of the analysis in either dimension.[10] However, it is useful to think about how estimated effects would look like if they were merely driven by a coincidence of local recessions and AIDS disease spread. In this case one would expect estimates to reflect the general gender and race gaps in the AIDS epidemic as described above. For non-whites who were in general hit much more strongly by the epidemic (Table A3) I do find larger effects. However, estimated effects of the age 18 unemployment rate are very similar for male and females in terms of AIDS death fractions even though the gender gap in overall AIDS death fractions is even more pronounced than the race gap (Tables A2 vs. A3). Moreover, I find the same gender and race pattern for the effects on income and health insurance in the CPS. In other words, the pattern of mortality effects is what we should expect if these effects are driven by the same causes as the effects on income and health insurance but less so if they were due to a spurious relationship of local recessions with the outbreak of the AIDS epidemic.

Reduced form effects of economic conditions around graduation on AIDS deaths could be driven by two different mechanisms. Income losses and worse health insurance coverage among recession graduates may lead to a faster disease progression and hence a shortened survival time given an HIV infection. Alternatively, infection rates might be higher among recession graduates.

Recession graduates might be more exposed to HIV infections if they tend to be more engaged in unsafe sex or injection drug use (the main modes of exposure, see above). We could think of a story in which recession graduates have more sex partners or opportunity costs of drug use are lower due to unemployment. The effects on marital status are

[10]The overall time period of the outbreak of the HIV epidemic is constraint by history. Choosing smaller time intervals, e.g. monthly unemployment rates, would not increase statistical power because the time of graduation is not measured at the monthly level either. Disaggregating geographically, e.g. an analysis at the county level, would allow for more variation in annual economic conditions. But a county level analysis would suffer from a worse match of the decedent's place of death and place of graduation, since there is more cross county than cross state migration.

if anything weaker for blacks despite stronger mortality effects for this racial minority group. To the extent that married individuals have fewer sex partners this is evidence against the hypothesis that higher infection rates through increased sexual contacts are the main driver behind the mortality effects.

The opportunity cost story, on the other hand, seems not appealing given that the effects on unemployment are relatively weak and short-lived. As mentioned in the introduction, the weak effects on employment status are in line with the literature. The mechanism through which worse economic conditions lower the socio-economic status of graduates is that they take lower quality jobs and not that they do not find a job at all. Further, existing studies have shown that drug use such as smoking and drinking is lower during recessions (Ruhm 1994, Ruhm 2005). It is unclear, though, to which extent cyclicality findings for soft drugs can be extrapolated to hard (injection) drugs.

Finally, the weakening of the mortality effects after 1991 coincides with the introduction of the Ryan White Act which provided medical treatment to those with insufficient or no health insurance coverage. If the estimated mortality effects were solely due to higher infection rates and not due to worse health insurance coverage, the Ryan White Act should have decreased AIDS mortality rates uniformly across cohorts and not have weakened my estimates during a period in which AIDS deaths were still on the rise.

8 Conclusion

The effects of income and health insurance on health outcomes are of great interest for social scientists and public policy. In observational data such effects are difficult to identify due to the endogeneity of income and insurance. This paper seeks to exploit economic conditions around graduation as a source of exogenous variation in these two variables of interest. This empirical strategy is data demanding both in the time as well as the spatial dimension. In order to control for confounding time trends and time-fixed regional characteristics long time-series at disaggregated regional levels are required. The CPS provides a good data source to investigate the effects on income and health insurance as the state of residence is reported along with many socio-economic variables for more than three decades. The Vital Statistics mortality files, on the other hand, are a useful data source for health outcomes in that they are available for more than two decades,

they cover the entire population and provide several geographical identifiers including the decedent's state of birth. A drawback of mortality as a health measure for young adults, however, is that mortality rates are very low during that age (annually about 0.1 %). And people in that age range die predominantly of violent deaths such as car accidents, suicides and homocides which are less affected by differences in socio-economic status and health insurance coverage than disease-related causes at older ages.

The outbreak of the AIDS epidemic provides a rare and dramatic time period in which morbidity and health insurance was likely to affect mortality of young adults. In the late 1980s and early 1990s young adults in the US died in great numbers from AIDS, with every third death in the most affected cohorts being caused by AIDS during the peak years. And clinical studies suggested that survival conditional on HIV infection was lower for those who were insufficiently insured (see discussion above). However, differences in health insurance status among HIV infected people were eradicated in 1991/2 by the Ryan White Act which granted full health insurance coverage for HIV treatment for all those who were infected. This leaves a short time period for which the effects of graduating around a recession on health are especially likely to be reflected in higher AIDS mortality rates already for young adults.

For this time period this study finds strong effects on mortality which are driven almost entirely by AIDS deaths and which are stronger for blacks who are also affected most strongly in income and health insurance coverage. In the extended time period, including years up to 2004, estimated mortality effects attenuate almost entirely and are only significantly positive for the oldest age group (age 33) included in this study. As argued above, the strongly negative effects on income and health insurance coverage might be a plausible mechanism underlying the estimated effect in the baseline period. An alternative mechanism could be effects on risky sexual behavior and drug abuse (the main modes of exposure) resulting in higher infection rates and thereby increasing AIDS mortality independently of effects on health insurance coverage. The procyclicality of unhealthy behavior found in previous studies and the homogeneous effects on marital status that I find across racial subgroups are not supportive of such mechanism. However, Vital Statistics are not the optimal data source to test for effects on health behaviors.

While the outbreak of the AIDS epidemic provides a unique opportunity to study the effects of graduating in a recession on young adults' mortality, more research is needed to investigate the long term effects on other disease-related causes of death at older

ages. Including more recent years of mortality data (given the availability of restricted geographical identifiers in the 2005+ data) and historic proxies for pre-1976 state level unemployment rates will greatly expand this research along these lines.

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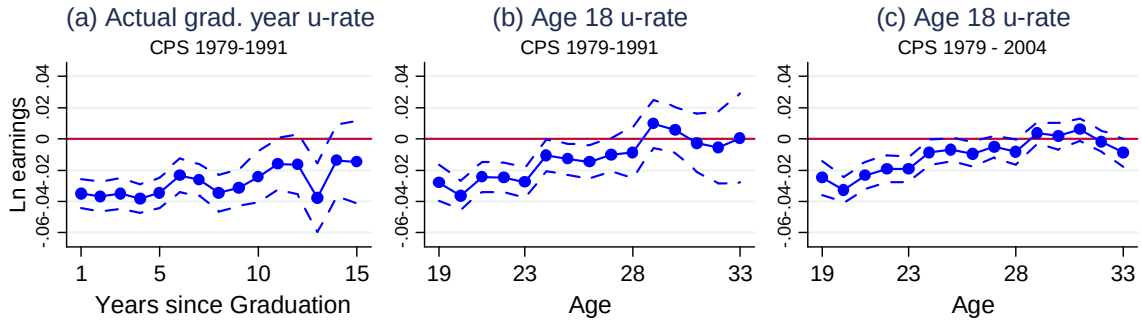
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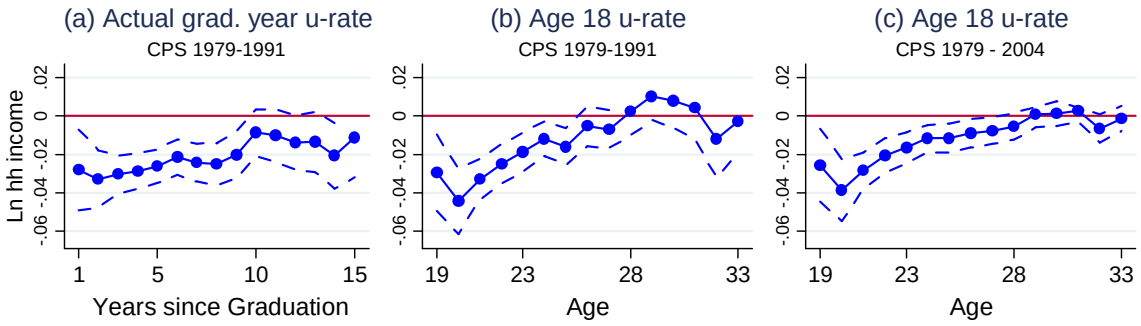
9 Figures and Tables

Figure 1 – Effect of Unemployment Rate around Graduation on: Income

(I) Ln wage



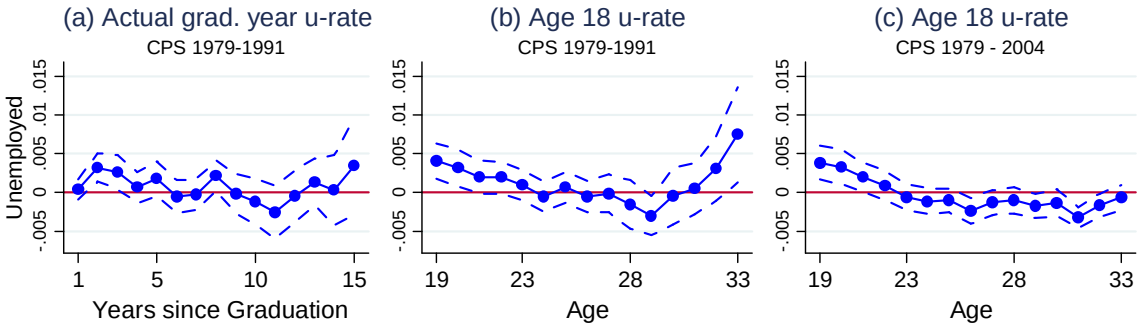
(II) Ln own household income



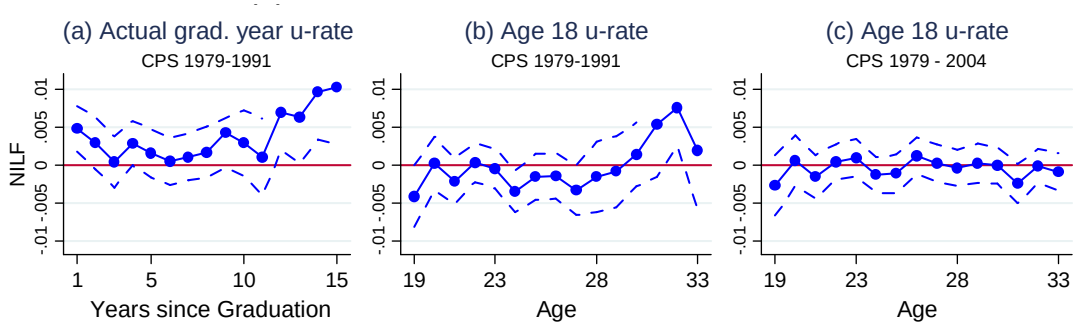
Notes: The dependent variable in panel **(I)** is the ln of individual wage and in **(II)** the ln of household income restricted to the subsample of household heads and their spouses. Subpanels **(a)** displays the coefficients of the unemployment rate at the year of graduation interacted with dummies for the years since graduation. Panels **(b)** and **(c)** displays the coefficients of the unemployment rate at age 18 interacted with dummies for age 19 to 33. Dashed lines indicate 95 % confidence intervals. Regressions control for cohort, state, calendar year, and years since graduation/age fixed effects. For regression results corresponding to panel (I) see Table 2.

Figure 2 – Effect of Unemployment Rate around Graduation on: Employment Status

(I) Fraction unemployed

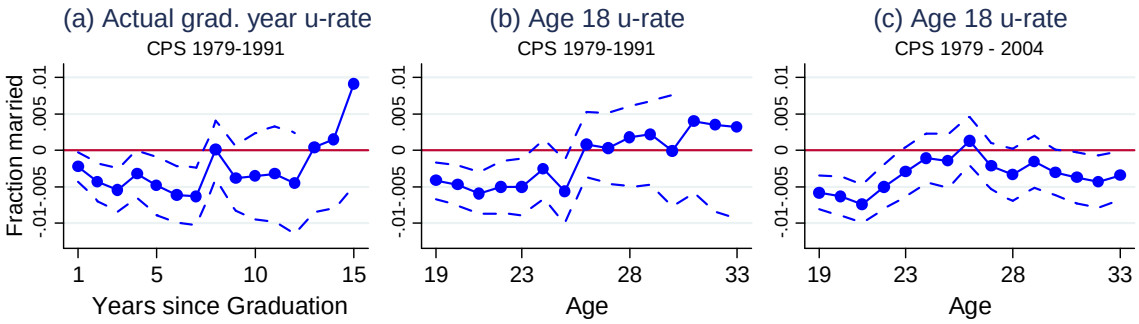


(II) Fraction 'Not In Labor Force'



Notes: The dependent variable is a dummy variable indicating whether the respondent is unemployed in panel **(I)** and whether the respondent is not in the labor force in **(II)**. Confidence intervals larger than 0.08 are suppressed for better scaling. For further explanations see the notes below Figure 1.

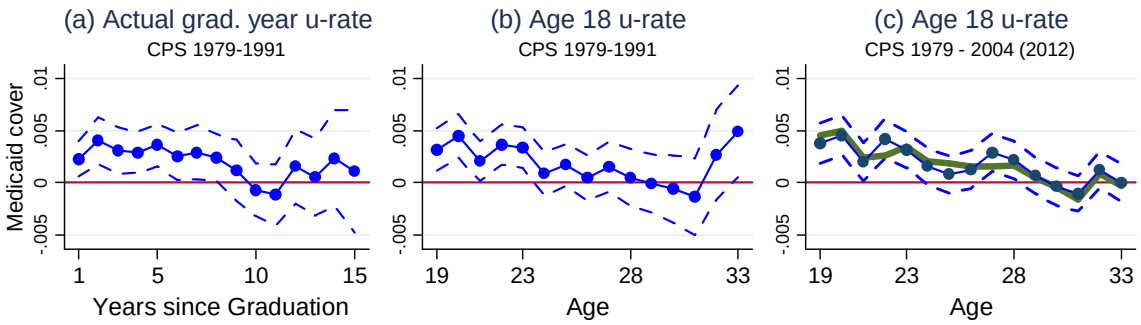
Figure 3 - Effect of Unemployment Rate around Graduation on: Fraction married



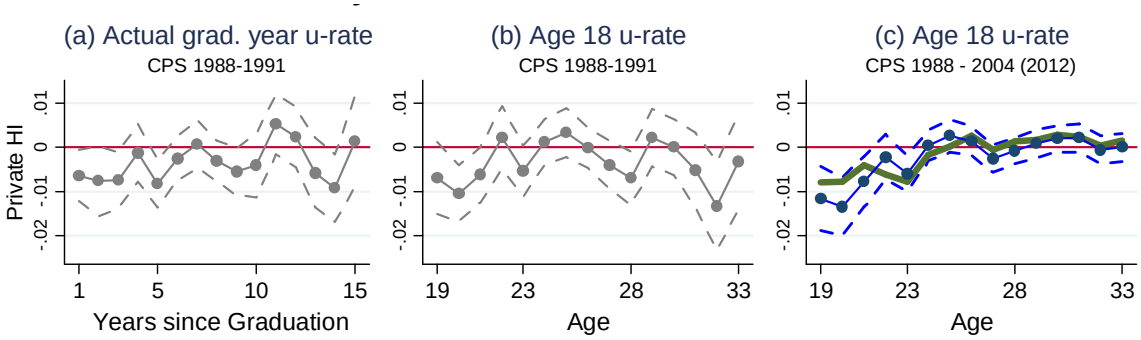
Notes: The dependent variable in all panels is a dummy indicating whether the respondent is married. For further explanations see the notes below Figure 1.

Figure 4 – Effect of Unemployment Rate around Graduation on: Health Insurance

(I) Covered by Medicaid



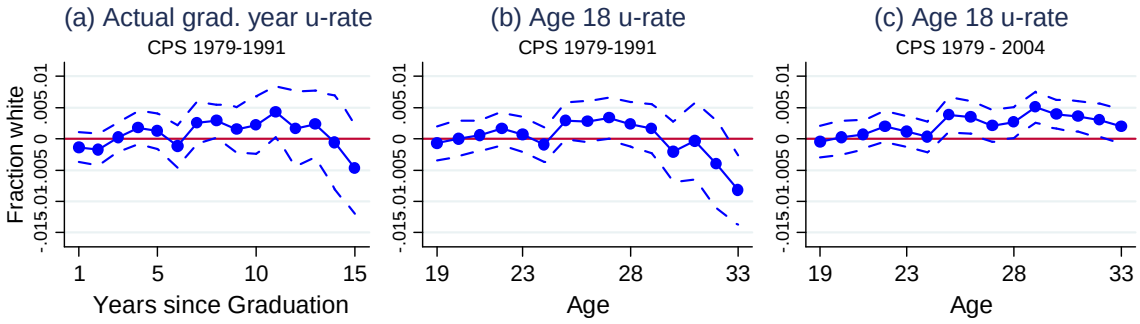
(II) Covered by any private health insurance



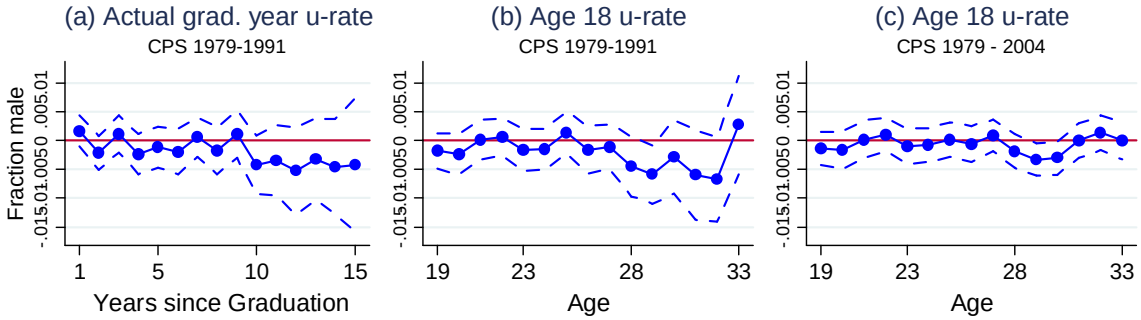
Notes: The dependent variables are dummy variables indicating coverage by **(I)** Medicaid and **(II)** any private health insurance. Lightly-colored figures in panels IIa-b and IIIa-b indicate imprecise estimates due to limited data availability (private health insurance is coded uniformly only after 1988). The solid line in the **(c)** panels represents estimates based on the years up to 1988-2012. For further explanations see the notes below Figure 1.For regression results corresponding to panel (I) see Table 3.

Figure 5 - Effect of Unemployment Rate around Graduation on: Selection Characteristics

(I) Fraction white

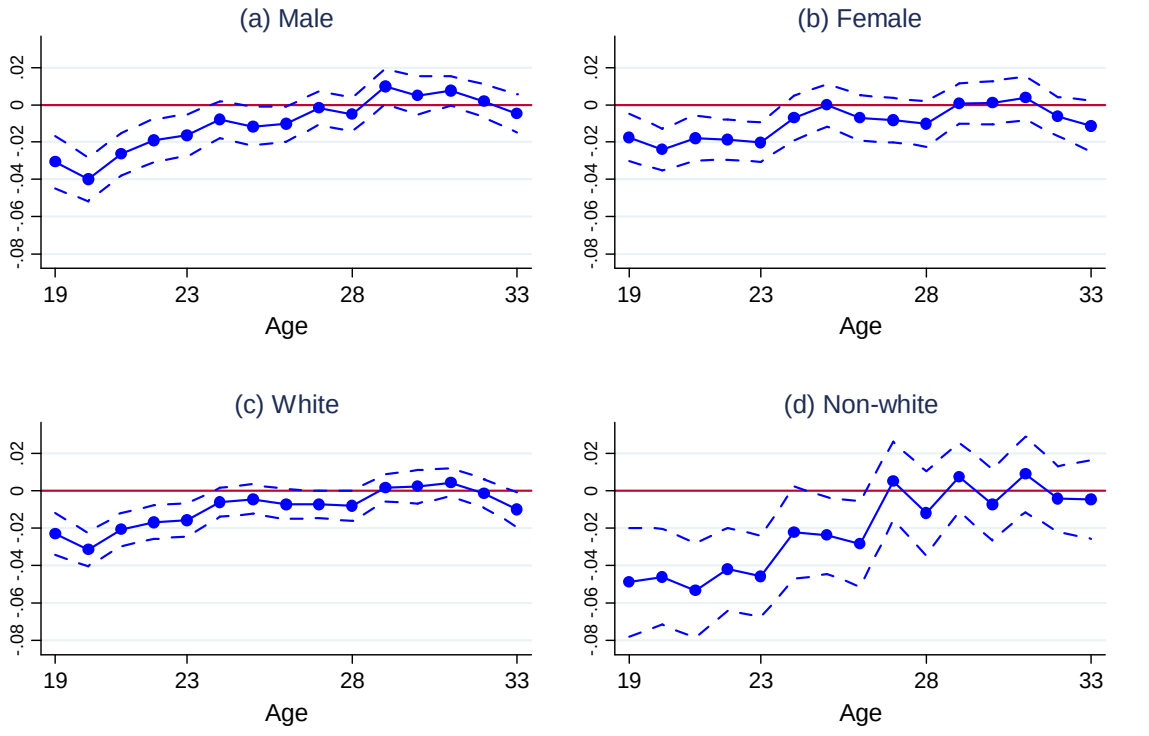


(II) Fraction male



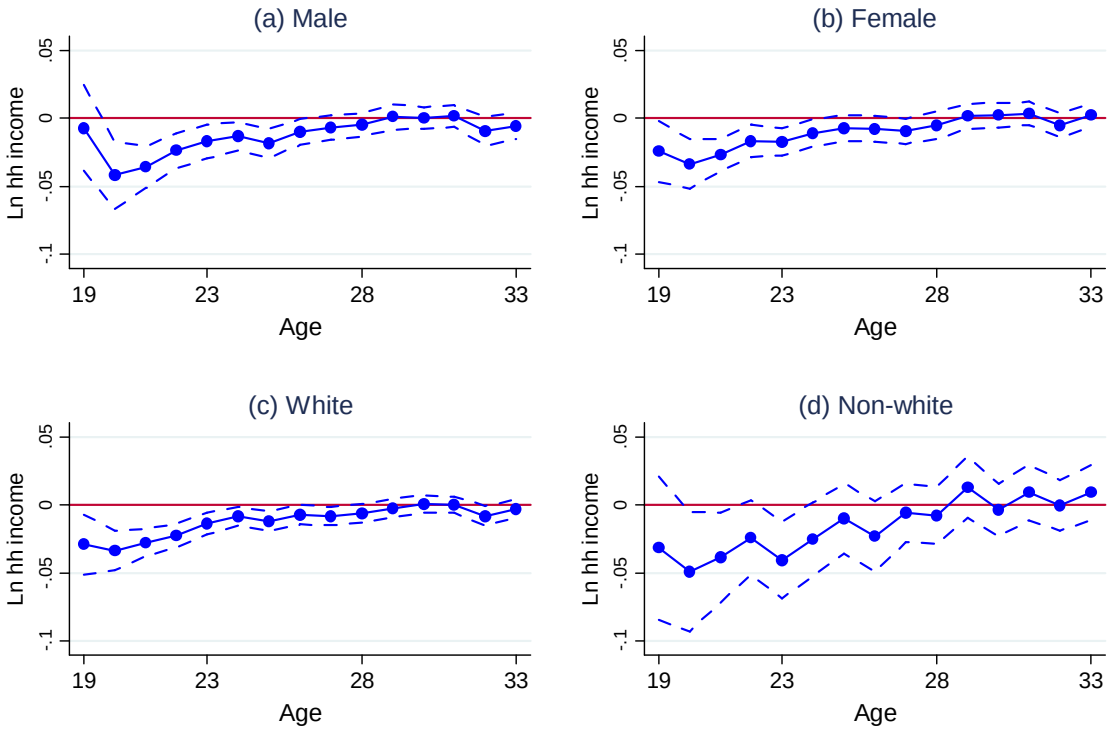
Notes: The dependent variable is a dummy variable indicating whether the respondent is white in panel **(I)** and male in **(II)**. For further explanations see the notes below Figure 1.

Figure 6 – Effect of Age 18 Unemployment Rate on: Ln Wage



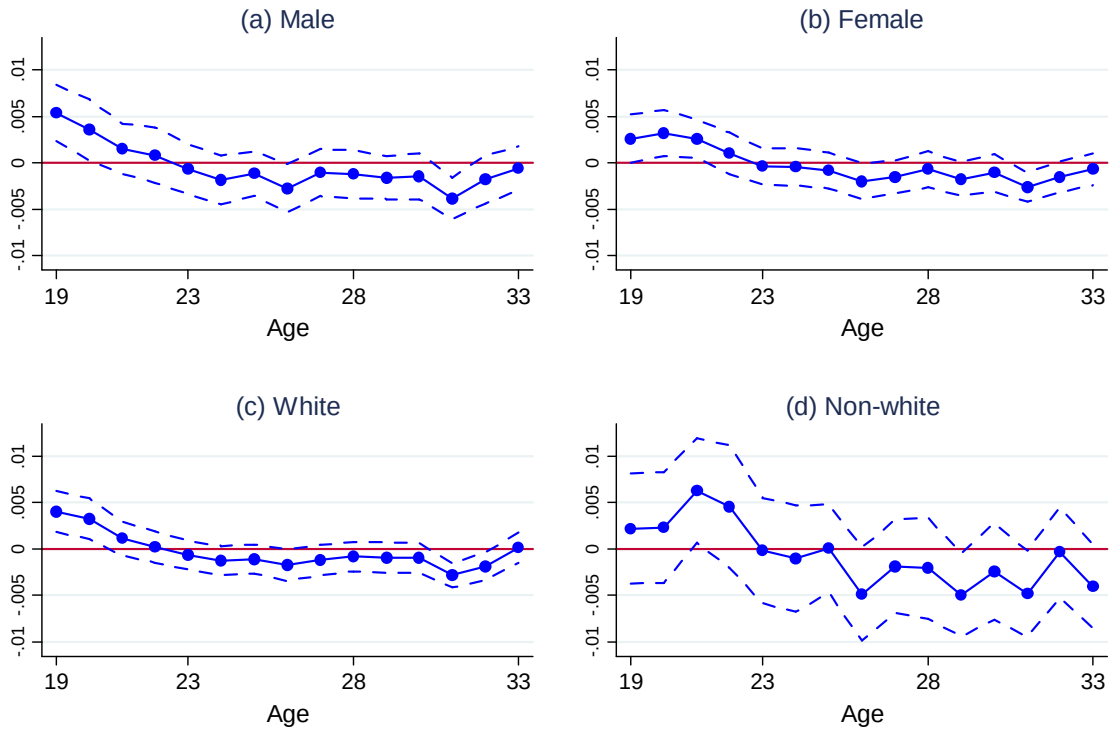
Notes: The dependent variable is the ln of individual wages. The coefficients of the unemployment rate at age 18 interacted with dummies for age 19 to 33 are displayed from separate regressions for male, female, white and non-white. Dashed lines indicate 95 % confidence intervals. Regressions control for cohort, state, calendar year, and years since graduation/age fixed effects and for the current unemployment rate. For corresponding regression results see Table 5.

Figure 7 – Effect of Age 18 Unemployment Rate on: Ln household income



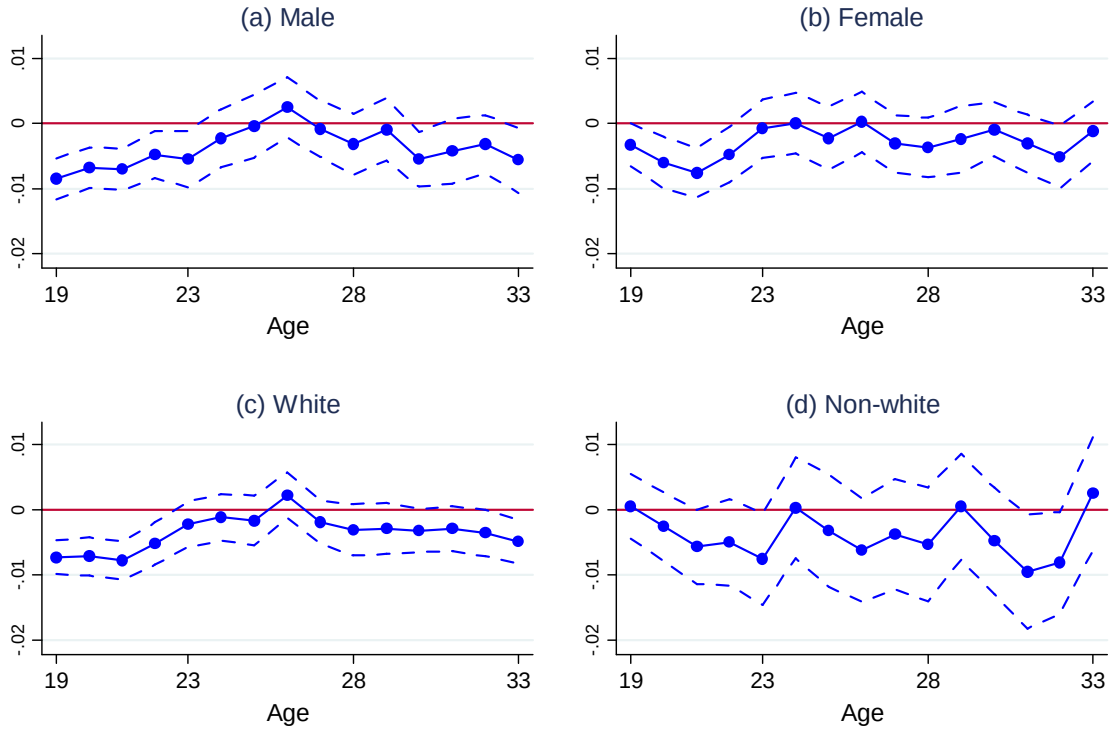
Notes: The dependent variable in all panels is the ln of household income for the subsample of household heads and their spouse. For further explanations see the notes below Figure 6.

Figure 8 – Effect of Age 18 Unemployment Rate on: Unemployed



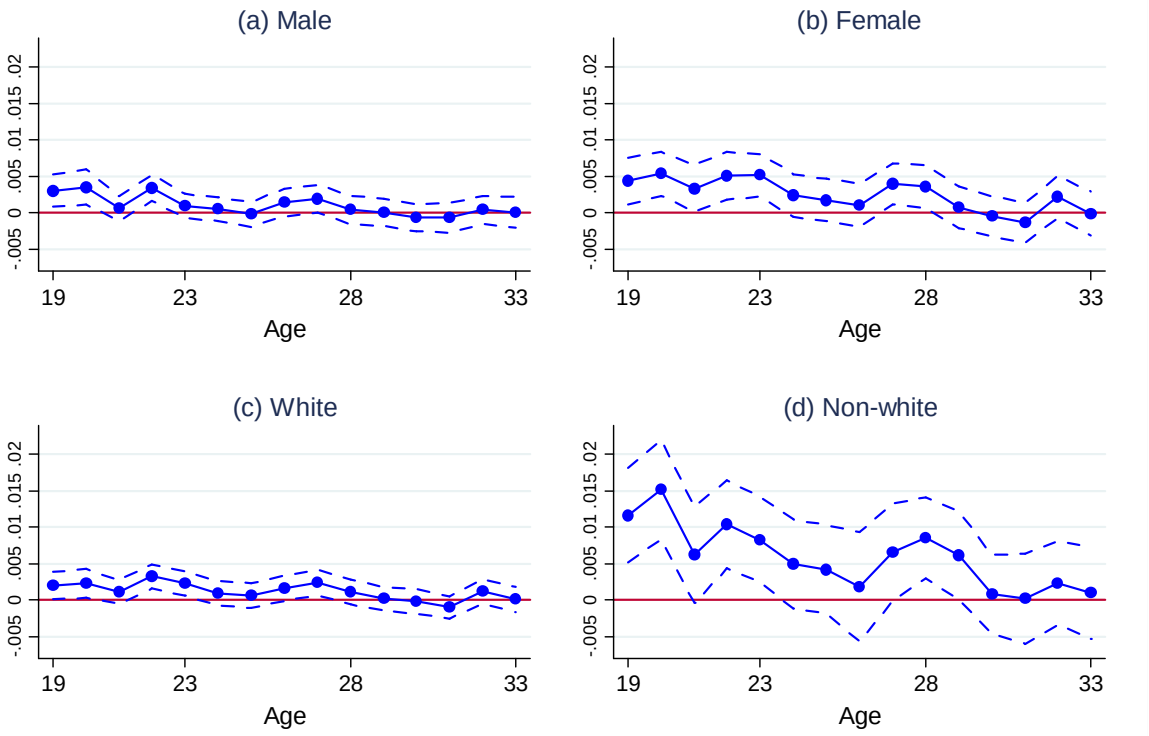
Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent is unemployed. For further explanations see the notes below Figure 6.

Figure 9 - Effect of Age 18 Unemployment Rate on: Married



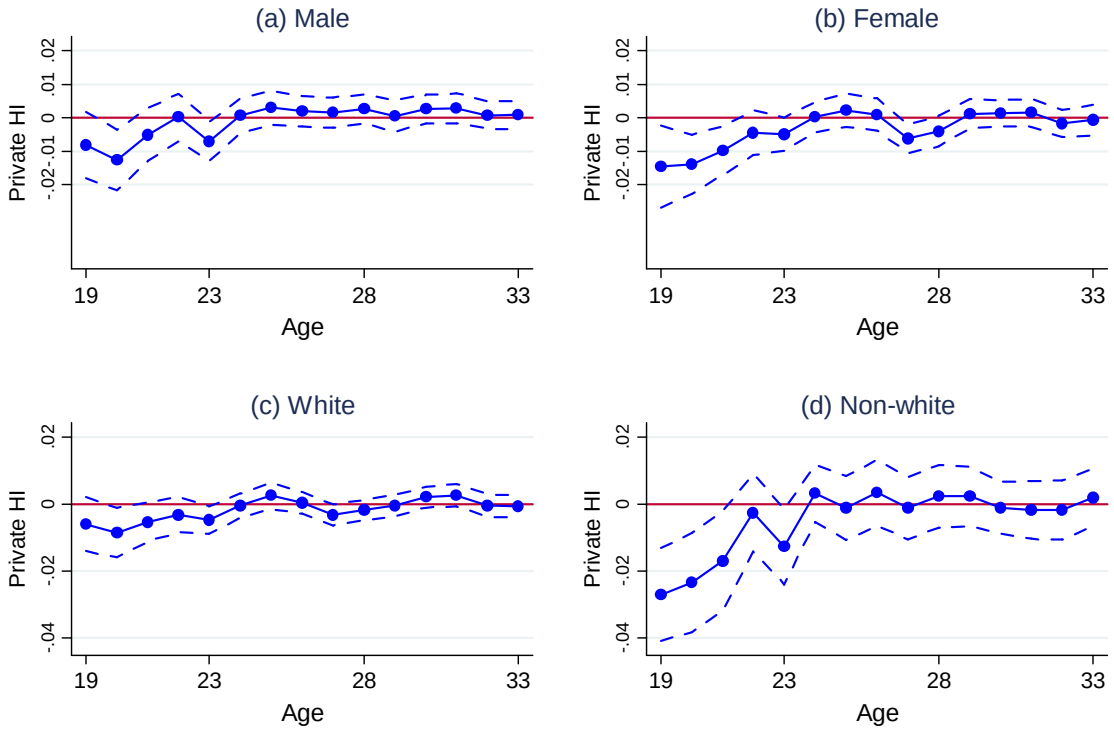
Notes: The dependent variable is a dummy variable indicating whether the respondent is married. For further explanations see the notes below Figure 6.

Figure 10 - Effect of Age 18 Unemployment Rate on: Medicaid



Notes: The Dependent variable in all panels is a dummy variable indicating whether the respondent is covered by Medicaid. For further explanations see the notes below Figure 6. For corresponding regression results see Table 7.

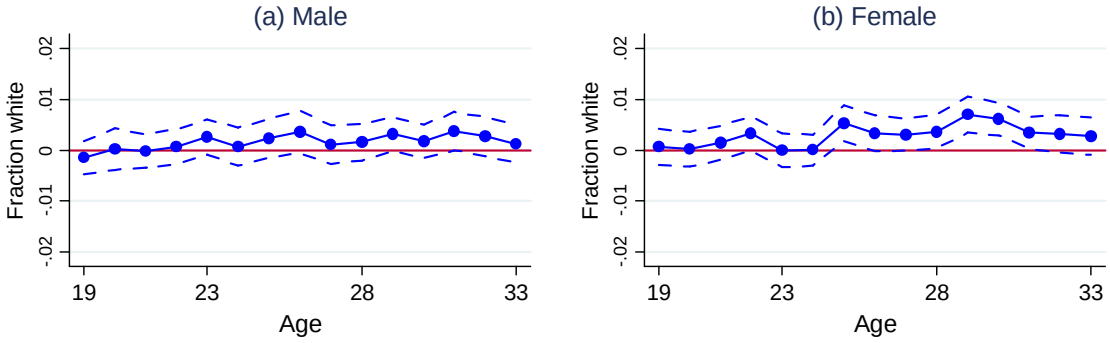
Figure 11 – Effect of Age 18 Unemployment Rate on: Private Health Insurance



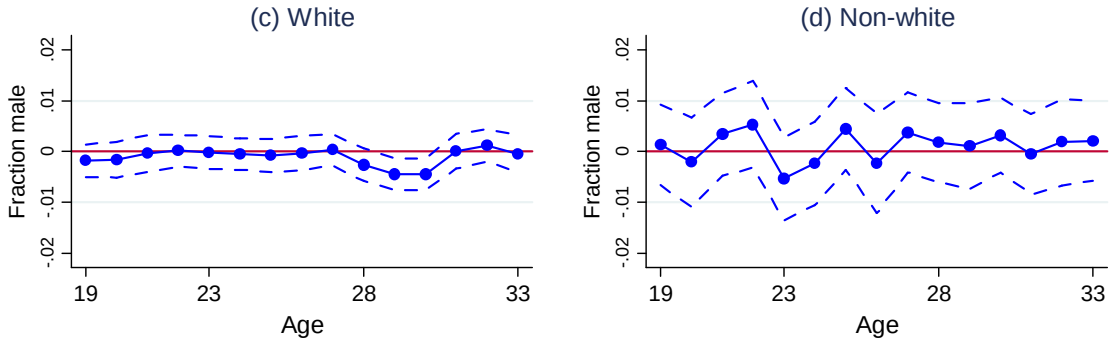
Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent is covered by employer health insurance in the subsample of respondents who worked in the previous year (1980-1987), who had private insurance in their own name during the previous year (1988-1994) or who had employer health insurance in the previous year (1995-2012). For further explanations see the notes below Figure 6.

Figure 12 – Effect of Age 18 Unemployment Rate on: Selection Characteristics

(I) Fraction white

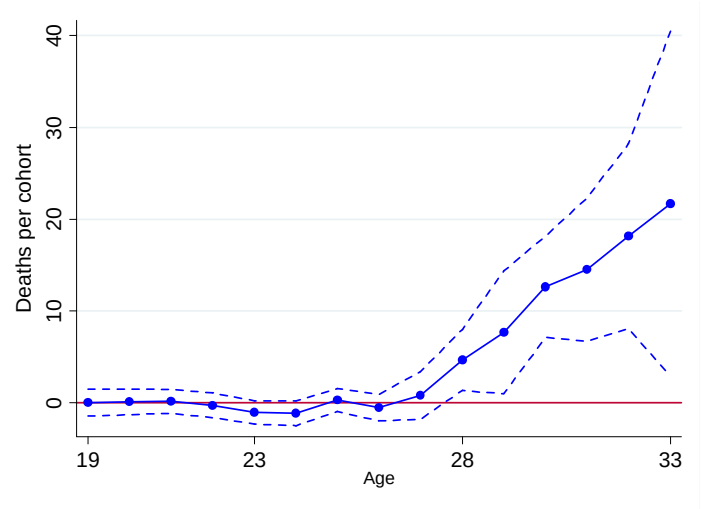


(II) Fraction male



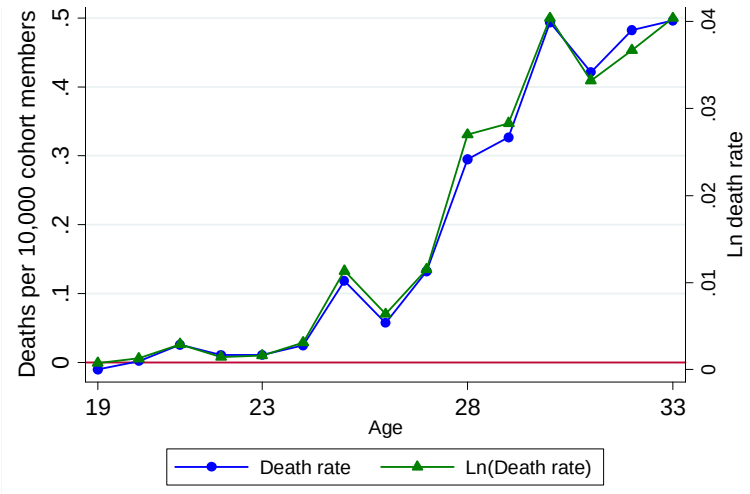
Notes: The dependent variable is a dummy variable indicating whether the respondent is white in panel **(I)** and male in **(II)**. For further explanations see the notes below Figure 6.

Figure 13 – Effect of Age 18 Unemployment Rate on: Number of Deaths per Cohort
US Vital Statistics 1979 - 1991



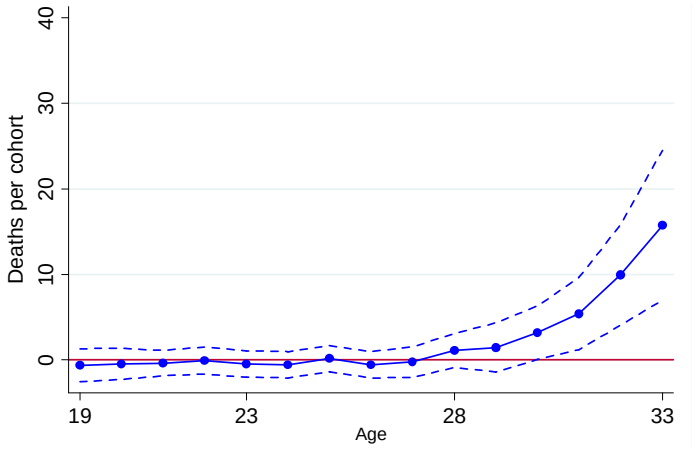
Notes: Coefficients from the regression of the number of deaths per cohort on the unemployment rate at age 18 interacted with dummies for age 19 to 33 are displayed. Regressions include the current unemployment rate as well as fixed effects for year-of-birth, state, calendar year and age. Dashed lines indicate 95 % confidence intervals. For corresponding regression results see Table 8 column (1).

Figure 14 – Effect of Age 18 Unemployment Rate on: Death Rates
US Vital Statistics 1979 - 1991



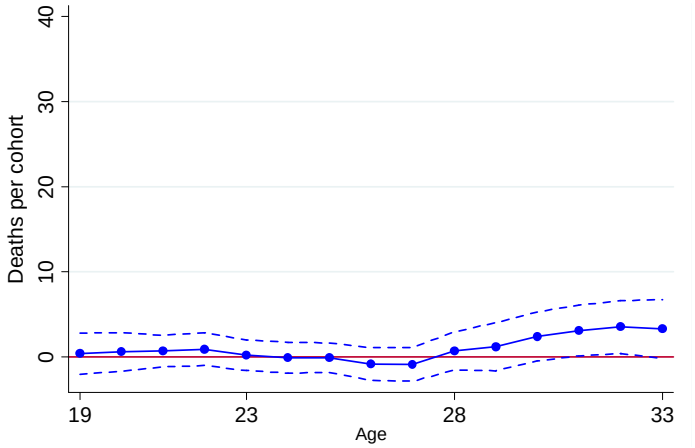
Notes: The dependent variable is the number of deaths per 10,000 cohort members on the left y-axis and the ln of deaths per 10,000 cohort members on the right y-axis. For further comments see Figure 13.

Figure 15 – Effect of Age 18 Unemployment Rate on: Number of Deaths
US Vital Statistics 1979 - 1995



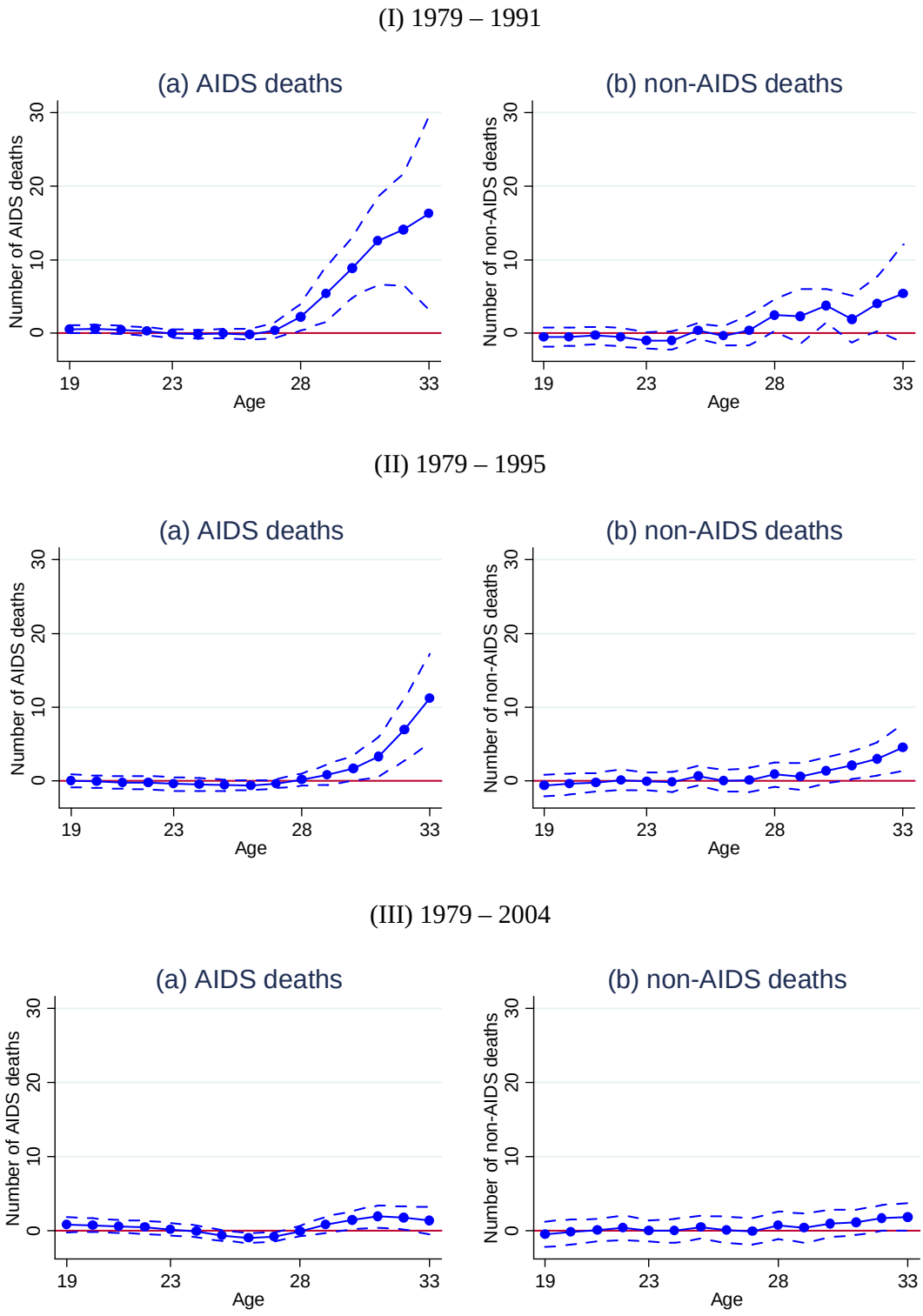
Notes: For comments see Figure 13. For corresponding regression results see Table 8 column (2).

Figure 16 – Effect of Age 18 Unemployment Rate on: Number of Deaths,
US Vital Statistics 1979 - 2004



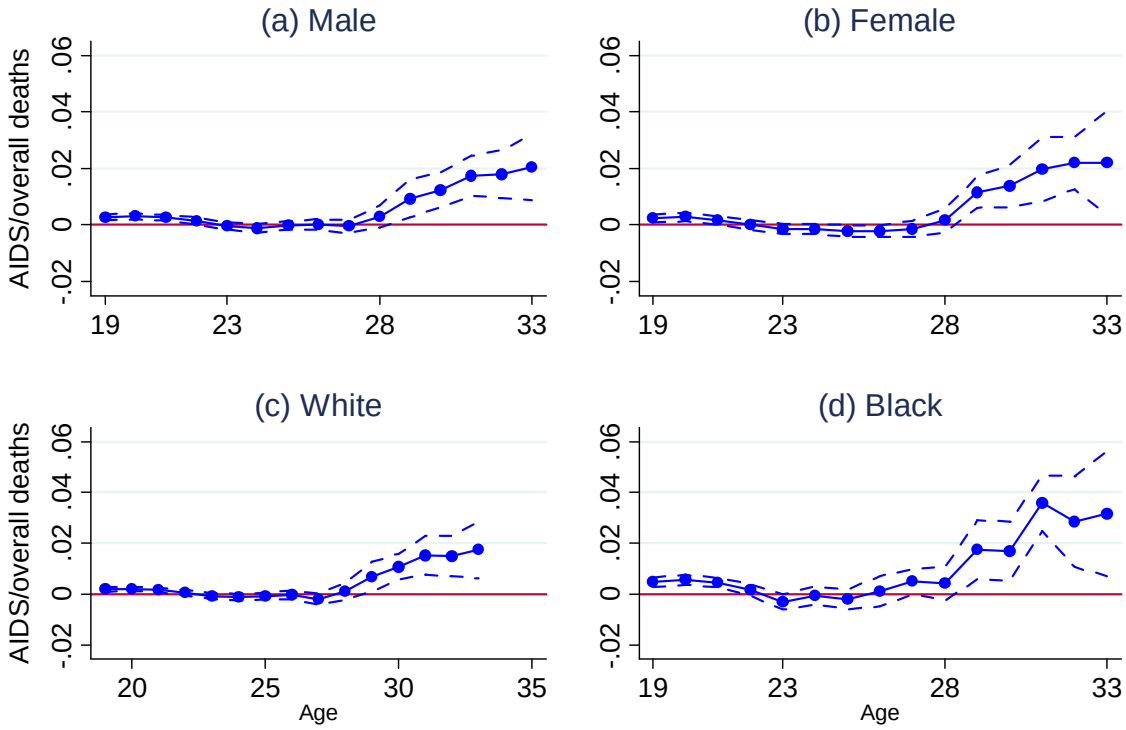
Notes: For further comments see Figure 13. For corresponding regression results see Table 8 column (3).

Figure 17 – Effect of Age 18 Unemployment Rate on: AIDS and non-AIDS Deaths



Notes: Coefficients from separate regressions with the number of AIDS deaths and Non-Aids Deaths as dependent variable are displayed. For further comments see Figure 13. For corresponding regression results see Table 9.

Figure 18 – Effect of Age 18 Unemployment Rate on: AIDS Death Fraction (# AIDS deaths/ # overall deaths), US Vital Statistics 1979 – 1991



Notes: Coefficients from separate regressions for males, females, whites and non-whites with AIDS death ratios (number of AIDS deaths divided by number of non-AIDS deaths) as dependent variable are displayed. For further comments see Figure 13. For corresponding regression results see Table 10.

Table 1: Descriptive Statistics

	Included years		
	1979-1991		1979-2004
	Model 1: Actual grad. year	Model 2: Age 18 grad year	Model 2: Age 18 grad year
Cohort restrictions	Graduation year '76-'91	Age 18 in '76-'91	Age 18 in '76-'91, age<34
Current Population Survey	(1)	(2)	(3)
Age (mean)	23.17	23.40	25.75
% male	50.57%	49.38%	49.52%
% white	83.01%	83.57%	82.44%
Years of education (mean)	12.21	12.38	12.81
Ln income (mean)	9.13	9.16	9.43
% unemployed	7.69%	8.02%	6.79%
% not in labor force	27.85%	24.14%	20.91%
% married	34.18%	34.44%	42.06%
% covered by Medicaid	7.94%	7.53%	7.89%
% ... by employer health insurance	65.03%	64.19%	76.50%
% ... by any priv. health insurance	71.35%	70.86%	69.84%
N	369,794	338,666	601,885
Vital Statistics - Universe of death			
Number of deaths	-	706,181	1,182,642
Deaths per 10,000 (Census pop estimates)		9.75	10.56
Male deaths	-	516,536	854,776
White deaths	-	544,174	876,399
AIDS deaths	-	27,401	83,406
Male AIDS deaths	-	22,824	66,629
White AIDS deaths	-	17,806	47,808
Census population estimates			
Population at age 18	-	58,400,000	

Notes: Model (1) and (2) refer to regression equations (1) and (2), respectively. There are no death statistics for model (1) as the Vital Statistics do not contain the actual graduation year. Samples are restricted to graduation cohorts that graduated 1976-1991 / birth cohorts that were of age 18 1976-1991, because state-level unemployment rates are available only since 1976. For further details see the Data section.

Table 2: Regressions of Ln Income on Graduation / Age 18 Unemployment Rate

	baseline: 1979-1991		extended: 1979-2004
Effect of Unemployment Rate Around Graduation on Ln Income	Grad. Year u-rate (1)	Age 18 u-rate (2)	Age 18 u-rate (3)
Effect at Year 1	-0.035	-0.028	-0.025
	(0.005)	(0.006)	(0.005)
Effect at Year 2	-0.037	-0.036	-0.033
	(0.005)	(0.005)	(0.004)
Effect at Year 3	-0.035	-0.024	-0.023
	(0.005)	(0.005)	(0.004)
Effect at Year 4	-0.038	-0.025	-0.019
	(0.005)	(0.005)	(0.004)
Effect at Year 5	-0.035	-0.028	-0.019
	(0.005)	(0.005)	(0.004)
Effect at Year 6	-0.023	-0.010	-0.009
	(0.006)	(0.005)	(0.004)
Effect at Year 7	-0.026	-0.013	-0.007
	(0.005)	(0.005)	(0.004)
Effect at Year 8	-0.035	-0.015	-0.010
	(0.006)	(0.005)	(0.004)
Effect at Year 9	-0.031	-0.010	-0.005
	(0.006)	(0.005)	(0.004)
Effect at Year 10	-0.024	-0.009	-0.008
	(0.008)	(0.008)	(0.004)
Effect at Year 11	-0.016	0.010	0.004
	(0.008)	(0.008)	(0.003)
Effect at Year 12	-0.016	0.006	0.002
	(0.010)	(0.007)	(0.004)
Effect at Year 13	-0.038	-0.003	0.006
	(0.011)	(0.009)	(0.004)
Effect at Year 14	-0.014	-0.006	-0.002
	(0.012)	(0.012)	(0.003)
Effect at Year 15	-0.015	0.001	-0.009
	(0.014)	(0.014)	(0.005)
State FE, year FE	✓	✓	✓
Graduation cohort FE	✓		
Years since graduation FE	✓		
Birth cohort FE		✓	✓
Age FE		✓	✓
Number of observations (collapsed)	5,967	5,967	11,934
R^2	0.93	0.88	0.88

Notes: Column (1) displays the coefficients from the regression of ln income on the graduation year unemployment rate interacted with dummies for each year since graduation (regression equation [1]). Columns (2) and (3) display the coefficients on the age 18 unemployment rate interacted with dummies for age 19-33 (equation [2]). 'FE' refers to 'fixed effects. For a graphical presentation see figures above.

Table 3: Regressions of the Fraction Unemployed on Graduation / Age 18 Unemployment Rate

	baseline: 1979-1991		extended: 1979-2004
Effect of Unemployment Rate Around Graduation on Fraction Unemployed (*100)	Grad. Year u-rate (1)	Age 18 u-rate (2)	Age 18 u-rate (3)
Effect at Year 1	0.042	0.410	0.386
	(0.068)	(0.116)	(0.110)
Effect at Year 2	0.322	0.315	0.328
	(0.093)	(0.122)	(0.114)
Effect at Year 3	0.265	0.198	0.202
	(0.114)	(0.109)	(0.096)
Effect at Year 4	0.065	0.194	0.090
	(0.102)	(0.105)	(0.096)
Effect at Year 5	0.177	0.100	-0.058
	(0.115)	(0.099)	(0.087)
Effect at Year 6	-0.056	-0.056	-0.112
	(0.109)	(0.099)	(0.084)
Effect at Year 7	-0.027	0.067	-0.101
	(0.102)	(0.100)	(0.079)
Effect at Year 8	0.218	-0.051	-0.235
	(0.098)	(0.105)	(0.086)
Effect at Year 9	-0.011	-0.012	-0.129
	(0.131)	(0.125)	(0.081)
Effect at Year 10	-0.119	-0.151	-0.101
	(0.150)	(0.159)	(0.087)
Effect at Year 11	-0.254	-0.299	-0.175
	(0.176)	(0.131)	(0.080)
Effect at Year 12	-0.045	-0.046	-0.131
	(0.168)	(0.184)	(0.087)
Effect at Year 13	0.137	0.051	-0.323
	(0.156)	(0.168)	(0.069)
Effect at Year 14	0.030	0.306	-0.162
	(0.232)	(0.211)	(0.077)
Effect at Year 15	0.350	0.749	-0.063
	(0.318)	(0.316)	(0.082)
State FE, year FE	✓	✓	✓
Graduation cohort FE	✓		
Years since graduation FE	✓		
Birth cohort FE		✓	✓
Age FE		✓	✓
Number of observations (collapsed)	5,967	5,967	11,934
R^2	0.29	0.32	0.30

Notes: The dependent variable is a dummy variable indicating that the respondent is unemployed. Coefficients and standard errors are multiplied by 100. Further comments as in Table 2. For a graphical presentation see figures above.

Table 4: Regressions of Medicaid Coverage on Graduation / Age 18 Unemployment Rate

	baseline: 1979-1991		extended: 1979-2004
Effect of Unemployment Rate Around Graduation on Fract. with Medicaid (*100)	Grad. Year u-rate (1)	Age 18 u-rate (2)	Age 18 u-rate (3)
Effect at Year 1	0.228	0.319	0.378
	(0.087)	(0.105)	(0.100)
Effect at Year 2	0.405	0.447	0.455
	(0.115)	(0.107)	(0.097)
Effect at Year 3	0.309	0.208	0.200
	(0.112)	(0.098)	(0.095)
Effect at Year 4	0.291	0.367	0.423
	(0.101)	(0.099)	(0.095)
Effect at Year 5	0.362	0.338	0.317
	(0.105)	(0.099)	(0.088)
Effect at Year 6	0.252	0.084	0.153
	(0.117)	(0.106)	(0.090)
Effect at Year 7	0.292	0.171	0.077
	(0.132)	(0.101)	(0.090)
Effect at Year 8	0.240	0.044	0.125
	(0.115)	(0.112)	(0.094)
Effect at Year 9	0.117	0.153	0.291
	(0.150)	(0.121)	(0.093)
Effect at Year 10	-0.070	0.049	0.218
	(0.129)	(0.140)	(0.092)
Effect at Year 11	-0.116	-0.008	0.069
	(0.149)	(0.143)	(0.090)
Effect at Year 12	0.154	-0.062	-0.038
	(0.183)	(0.166)	(0.092)
Effect at Year 13	0.055	-0.137	-0.105
	(0.191)	(0.188)	(0.087)
Effect at Year 14	0.234	0.270	0.123
	(0.236)	(0.221)	(0.089)
Effect at Year 15	0.106	0.492	-0.001
	(0.300)	(0.224)	(0.093)
State FE, year FE	✓	✓	✓
Graduation cohort FE	✓		
Years since graduation FE	✓		
Birth cohort FE		✓	✓
Age FE		✓	✓
Number of observations (collapsed)	5,814	5,814	11,781
R^2	0.31	0.36	0.27

Notes: The dependent variable is a dummy variable indicating Medicaid coverage. Coefficients and standard errors are multiplied by 100. Further comments as in Table 2. For a graphical presentation see figures above.

Table 5: Regressions of Ln Income on Age 18 Unemployment Rate by Gender and Race

	Sample			
Effect of Age 18 Unemployment Rate on Ln Income	Male (1)	Female (2)	White (3)	Non-white (3)
Effect at Age 19	-0.031	-0.018	-0.023	-0.049
	(0.007)	(0.006)	(0.006)	(0.015)
Effect at Age 20	-0.040	-0.024	-0.031	-0.046
	(0.006)	(0.006)	(0.005)	(0.013)
Effect at Age 21	-0.027	-0.018	-0.021	-0.053
	(0.006)	(0.006)	(0.005)	(0.013)
Effect at Age 22	-0.019	-0.019	-0.017	-0.042
	(0.006)	(0.005)	(0.005)	(0.011)
Effect at Age 23	-0.016	-0.020	-0.015	-0.046
	(0.006)	(0.005)	(0.005)	(0.011)
Effect at Age 24	-0.008	-0.007	-0.006	-0.022
	(0.005)	(0.006)	(0.004)	(0.013)
Effect at Age 25	-0.012	0.000	-0.004	-0.024
	(0.005)	(0.006)	(0.004)	(0.010)
Effect at Age 26	-0.010	-0.007	-0.007	-0.028
	(0.005)	(0.006)	(0.004)	(0.012)
Effect at Age 27	-0.002	-0.008	-0.007	0.005
	(0.005)	(0.006)	(0.004)	(0.011)
Effect at Age 28	-0.005	-0.010	-0.008	-0.012
	(0.005)	(0.006)	(0.004)	(0.011)
Effect at Age 29	0.010	0.001	0.002	0.007
	(0.005)	(0.006)	(0.004)	(0.009)
Effect at Age 30	0.005	0.001	0.002	-0.007
	(0.005)	(0.006)	(0.005)	(0.010)
Effect at Age 31	0.007	0.004	0.005	0.009
	(0.004)	(0.006)	(0.004)	(0.010)
Effect at Age 32	0.002	-0.006	-0.001	-0.004
	(0.005)	(0.005)	(0.004)	(0.009)
Effect at Age 33	-0.005	-0.011	-0.010	-0.005
	(0.005)	(0.007)	(0.005)	(0.011)
State FE, year FE	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
Number of observations (collapsed)	11,933	11,932	11,932	10,077
R^2	0.84	0.74	0.87	0.60

Notes: The coefficients from separate regressions for males, females, whites and non-whites of ln income on the age 18 unemployment rate interacted with dummies for age 19-33 are displayed. 'FE' refers to 'fixed effects'. Sample years 1979-2004. For a graphical presentation see figures above.

Table 6: Regressions of the Fraction Unemployed on Age 18 Unemployment Rate by Gender and Race

	Sample			
Effect of Age 18 Unemployment Rate on on Fraction Unemployed (*100)	Male (1)	Female (2)	White (3)	Non-white (3)
Effect at Age 19	0.537	0.258	0.405	0.220
	(0.154)	(0.133)	(0.112)	(0.301)
Effect at Age 20	0.356	0.320	0.327	0.233
	(0.166)	(0.127)	(0.112)	(0.305)
Effect at Age 21	0.152	0.259	0.113	0.630
	(0.138)	(0.106)	(0.095)	(0.287)
Effect at Age 22	0.081	0.100	0.023	0.458
	(0.152)	(0.115)	(0.085)	(0.336)
Effect at Age 23	-0.069	-0.041	-0.065	-0.018
	(0.137)	(0.098)	(0.080)	(0.290)
Effect at Age 24	-0.187	-0.042	-0.126	-0.103
	(0.134)	(0.105)	(0.081)	(0.293)
Effect at Age 25	-0.117	-0.084	-0.108	0.006
	(0.122)	(0.097)	(0.080)	(0.243)
Effect at Age 26	-0.275	-0.198	-0.172	-0.487
	(0.133)	(0.099)	(0.087)	(0.256)
Effect at Age 27	-0.107	-0.155	-0.118	-0.186
	(0.129)	(0.091)	(0.084)	(0.256)
Effect at Age 28	-0.123	-0.069	-0.081	-0.204
	(0.134)	(0.098)	(0.080)	(0.279)
Effect at Age 29	-0.161	-0.176	-0.093	-0.494
	(0.117)	(0.091)	(0.085)	(0.227)
Effect at Age 30	-0.147	-0.109	-0.095	-0.241
	(0.128)	(0.102)	(0.081)	(0.266)
Effect at Age 31	-0.385	-0.263	-0.283	-0.481
	(0.114)	(0.080)	(0.066)	(0.236)
Effect at Age 32	-0.178	-0.152	-0.183	-0.035
	(0.133)	(0.085)	(0.077)	(0.248)
Effect at Age 33	-0.061	-0.069	0.014	-0.405
	(0.120)	(0.088)	(0.082)	(0.230)
State FE, year FE	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
Number of observations (collapsed)	11,933	11,934	11,933	10,552
R^2	0.24	0.15	0.28	0.16

Notes: The dependent variable is a dummy indicating whether the respondent is unemployed. Further comments as in Table 5. For a graphical presentation see figures above.

Table 7: Regressions of Medicaid Coverage on Age 18 Unemployment Rate by Gender and Race

	Sample			
Effect of Age 18 Unemployment Rate on	Male	Female	White	Non-white
Fraction covered by Medicaid (*100)	(1)	(2)	(3)	(3)
Effect at Age 19	0.303	0.437	0.198	1.165
	(0.114)	(0.164)	(0.097)	(0.330)
Effect at Age 20	0.354	0.535	0.228	1.516
	(0.124)	(0.153)	(0.100)	(0.349)
Effect at Age 21	0.060	0.334	0.109	0.622
	(0.086)	(0.164)	(0.083)	(0.337)
Effect at Age 22	0.345	0.511	0.325	1.037
	(0.092)	(0.165)	(0.085)	(0.308)
Effect at Age 23	0.093	0.515	0.227	0.828
	(0.085)	(0.150)	(0.085)	(0.297)
Effect at Age 24	0.051	0.238	0.092	0.496
	(0.082)	(0.151)	(0.087)	(0.311)
Effect at Age 25	-0.019	0.175	0.064	0.421
	(0.087)	(0.148)	(0.086)	(0.310)
Effect at Age 26	0.141	0.102	0.162	0.181
	(0.097)	(0.153)	(0.088)	(0.382)
Effect at Age 27	0.196	0.397	0.240	0.656
	(0.097)	(0.143)	(0.088)	(0.342)
Effect at Age 28	0.040	0.362	0.111	0.855
	(0.098)	(0.153)	(0.087)	(0.284)
Effect at Age 29	0.006	0.077	0.020	0.612
	(0.097)	(0.146)	(0.084)	(0.311)
Effect at Age 30	-0.067	-0.046	-0.020	0.077
	(0.095)	(0.142)	(0.088)	(0.277)
Effect at Age 31	-0.067	-0.137	-0.100	0.018
	(0.104)	(0.137)	(0.078)	(0.316)
Effect at Age 32	0.040	0.221	0.119	0.235
	(0.097)	(0.146)	(0.085)	(0.294)
Effect at Age 33	0.010	-0.012	0.008	0.099
	(0.109)	(0.154)	(0.089)	(0.320)
State FE, year FE	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
Number of observations (collapsed)	11,780	11,781	11,781	10,425
R^2	0.16	0.21	0.28	0.18

Notes: The dependent variable is a dummy indicating Medicaid coverage. Further comments as in Table 5. For a graphical presentation see figures above.

Table 8: Regressions of Deaths on Age 18 Unemployment Rate

	Included Years		
Effect of Age 18 Unemployment Rate on Number of Deaths per Cohort	1979-1991 (1)	1979-1995 (2)	1979-2004 (3)
Effect at Age 19	0.021	-0.601	0.384
	(0.761)	(0.982)	(1.233)
Effect at Age 20	0.096	-0.456	0.599
	(0.713)	(0.928)	(1.158)
Effect at Age 21	0.140	-0.349	0.686
	(0.668)	(0.754)	(0.946)
Effect at Age 22	-0.281	-0.059	0.929
	(0.687)	(0.807)	(0.984)
Effect at Age 23	-1.046	-0.459	0.205
	(0.648)	(0.785)	(0.915)
Effect at Age 24	-1.140	-0.554	-0.080
	(0.692)	(0.786)	(0.917)
Effect at Age 25	0.292	0.166	-0.080
	(0.645)	(0.780)	(0.887)
Effect at Age 26	-0.531	-0.556	-0.827
	(0.740)	(0.786)	(0.979)
Effect at Age 27	0.796	-0.232	-0.868
	(1.326)	(0.917)	(1.016)
Effect at Age 28	4.662	1.107	0.711
	(1.695)	(1.017)	(1.138)
Effect at Age 29	7.675	1.475	1.207
	(3.421)	(1.459)	(1.441)
Effect at Age 30	12.634	3.194	2.423
	(2.815)	(1.591)	(1.466)
Effect at Age 31	14.512	5.433	3.106
	(3.989)	(2.152)	(1.521)
Effect at Age 32	18.148	9.958	3.522
	(5.138)	(2.991)	(1.584)
Effect at Age 33	21.685	15.742	3.274
	(9.582)	(4.472)	(1.762)
State FE, year FE	✓	✓	✓
Birth cohort FE	✓	✓	✓
Age FE	✓	✓	✓
Number of observations (collapsed)	5,967	8,721	11,934
R^2	0.97	0.95	0.94

Notes: Coefficients from regressions of the number of deaths per cohort on the age 18 unemployment rate interacted with dummies for age 19-33 are displayed. Columns differ by the range of included years. Underlying population refers to the size of the included cohorts multiplied by the year in which they are analyzed. 'FE' refers to 'fixed effects', i.e. a set of dummies for the different values of the respective variable. For a graphical presentation see figures above.

Table 9: Regressions of AIDS and Non-AIDS Deaths on Age 18 Unemployment Rate

Effect of Age 18 U-Rate AIDS and non-AIDS Deaths	AIDS (1)	Non-AIDS (2)	AIDS (3)	Non-AIDS (4)	AIDS (5)	Non-AIDS (6)
Effect at Age 19	0.559	0.022	0.846	-0.538	-0.624	-0.462
	(0.275)	(0.452)	(0.529)	(0.671)	(0.747)	(0.881)
Effect at Age 20	0.597	-0.068	0.763	-0.500	-0.388	-0.165
	(0.280)	(0.424)	(0.468)	(0.637)	(0.725)	(0.864)
Effect at Age 21	0.458	-0.180	0.581	-0.318	-0.168	0.105
	(0.289)	(0.436)	(0.447)	(0.608)	(0.650)	(0.779)
Effect at Age 22	0.260	-0.203	0.512	-0.541	0.144	0.416
	(0.293)	(0.458)	(0.450)	(0.648)	(0.708)	(0.835)
Effect at Age 23	-0.066	-0.398	0.218	-0.980	-0.061	-0.012
	(0.296)	(0.472)	(0.434)	(0.550)	(0.607)	(0.733)
Effect at Age 24	-0.144	-0.460	-0.067	-0.997	-0.095	-0.013
	(0.301)	(0.459)	(0.407)	(0.643)	(0.691)	(0.821)
Effect at Age 25	-0.050	-0.554	-0.612	0.341	0.720	0.532
	(0.315)	(0.385)	(0.363)	(0.525)	(0.679)	(0.784)
Effect at Age 26	-0.163	-0.599	-0.962	-0.368	0.043	0.135
	(0.373)	(0.330)	(0.343)	(0.664)	(0.734)	(0.893)
Effect at Age 27	0.402	-0.381	-0.808	0.394	0.149	-0.059
	(0.537)	(0.305)	(0.309)	(1.037)	(0.824)	(0.898)
Effect at Age 28	2.185	0.214	-0.038	2.477	0.893	0.750
	(0.940)	(0.420)	(0.358)	(1.098)	(0.847)	(0.948)
Effect at Age 29	5.367	0.863	0.824	2.308	0.612	0.383
	(1.928)	(0.719)	(0.556)	(1.908)	(0.917)	(0.996)
Effect at Age 30	8.890	1.754	1.440	3.744	1.440	0.983
	(2.071)	(0.874)	(0.638)	(1.165)	(0.881)	(0.950)
Effect at Age 31	12.582	3.294	1.934	1.930	2.139	1.172
	(3.062)	(1.380)	(0.761)	(1.632)	(0.961)	(0.869)
Effect at Age 32	14.122	6.944	1.781	4.026	3.013	1.742
	(3.876)	(2.149)	(0.794)	(1.887)	(1.142)	(0.895)
Effect at Age 33	16.305	11.216	1.372	5.380	4.527	1.902
	(6.691)	(3.093)	(0.943)	(3.416)	(1.611)	(0.939)
State FE, year FE	✓	✓	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓
Observations (collapsed)	5,967	8,721	11,934	5,967	8,721	11,934
R^2	0.66	0.65	0.61	0.98	0.97	0.96

Notes: Coefficients from separate regressions for AIDS deaths and Non-AIDS deaths are displayed. Further comments as in Table 8. For a graphical presentation see figures above.

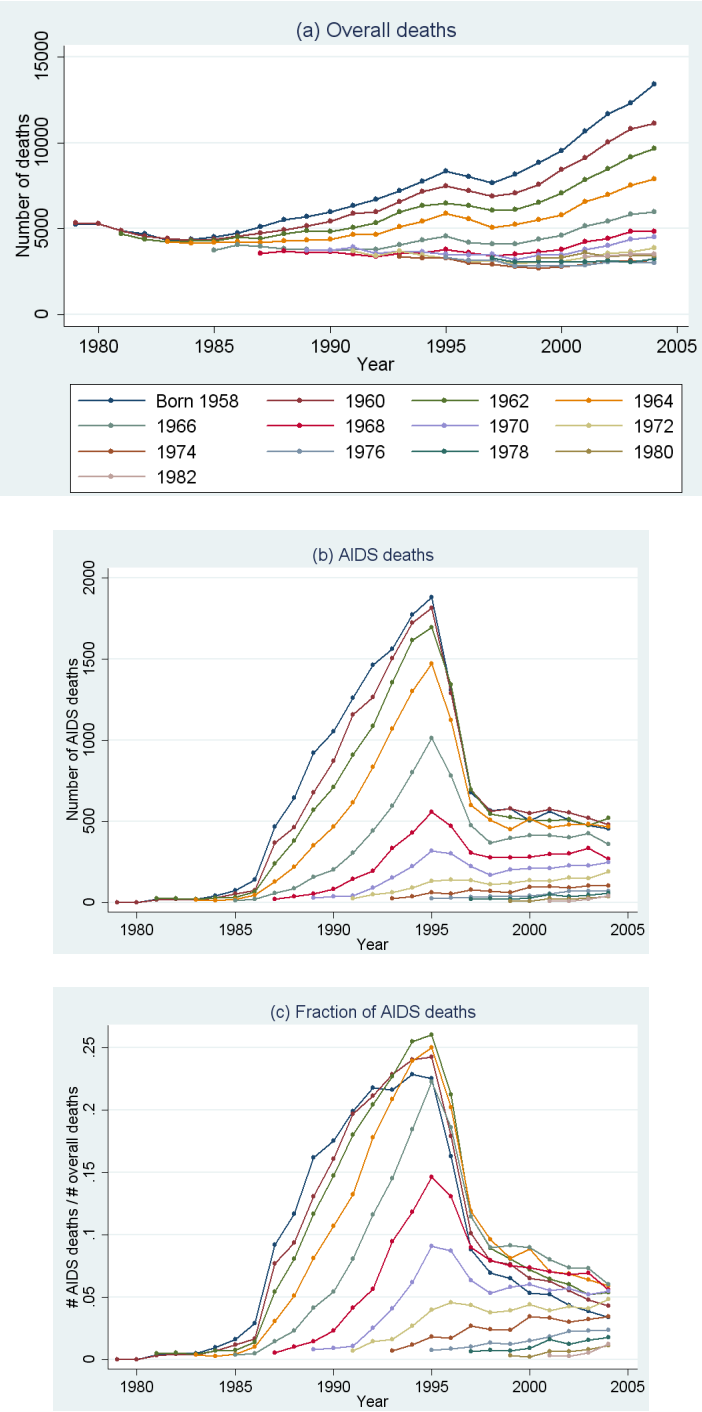
Table 10: Regressions of AIDS/Non-AIDS Death Ratios on Age 18 Unemployment Rate by Gender and Race

	Sample			
Effect of Age 18 Unemployment Rate on AIDS Deaths Ratio (*100)	Male (1)	Female (2)	White (3)	Black (3)
Effect at Age 19	0.275	0.234	0.181	0.467
	(0.054)	(0.069)	(0.049)	(0.095)
Effect at Age 20	0.309	0.281	0.204	0.560
	(0.051)	(0.075)	(0.044)	(0.098)
Effect at Age 21	0.259	0.158	0.159	0.458
	(0.053)	(0.078)	(0.047)	(0.098)
Effect at Age 22	0.146	0.003	0.051	0.159
	(0.063)	(0.086)	(0.056)	(0.111)
Effect at Age 23	-0.039	-0.141	-0.078	-0.310
	(0.066)	(0.095)	(0.059)	(0.153)
Effect at Age 24	-0.117	-0.146	-0.124	-0.056
	(0.073)	(0.089)	(0.066)	(0.179)
Effect at Age 25	-0.016	-0.218	-0.077	-0.195
	(0.077)	(0.108)	(0.068)	(0.200)
Effect at Age 26	0.016	-0.222	-0.041	0.109
	(0.100)	(0.107)	(0.088)	(0.306)
Effect at Age 27	-0.048	-0.140	-0.194	0.491
	(0.119)	(0.148)	(0.106)	(0.253)
Effect at Age 28	0.302	0.158	0.098	0.413
	(0.198)	(0.213)	(0.171)	(0.335)
Effect at Age 29	0.942	1.164	0.679	1.741
	(0.345)	(0.284)	(0.294)	(0.589)
Effect at Age 30	1.243	1.381	1.076	1.692
	(0.315)	(0.382)	(0.260)	(0.592)
Effect at Age 31	1.741	1.974	1.523	3.576
	(0.360)	(0.579)	(0.390)	(0.554)
Effect at Age 32	1.801	2.193	1.491	2.848
	(0.433)	(0.473)	(0.405)	(0.914)
Effect at Age 33	2.039	2.204	1.741	3.172
	(0.594)	(0.943)	(0.572)	(1.253)
State FE, year FE	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
Number of observations (collapsed)	5,966	5,880	5,966	4,511
R^2	0.82	0.43	0.78	0.52

Notes: Coefficients from separate regressions for males, females, whites and non-whites in the baseline period (1979-1991) are displayed. Further comments as in Table 8. For a graphical presentation see figures above.

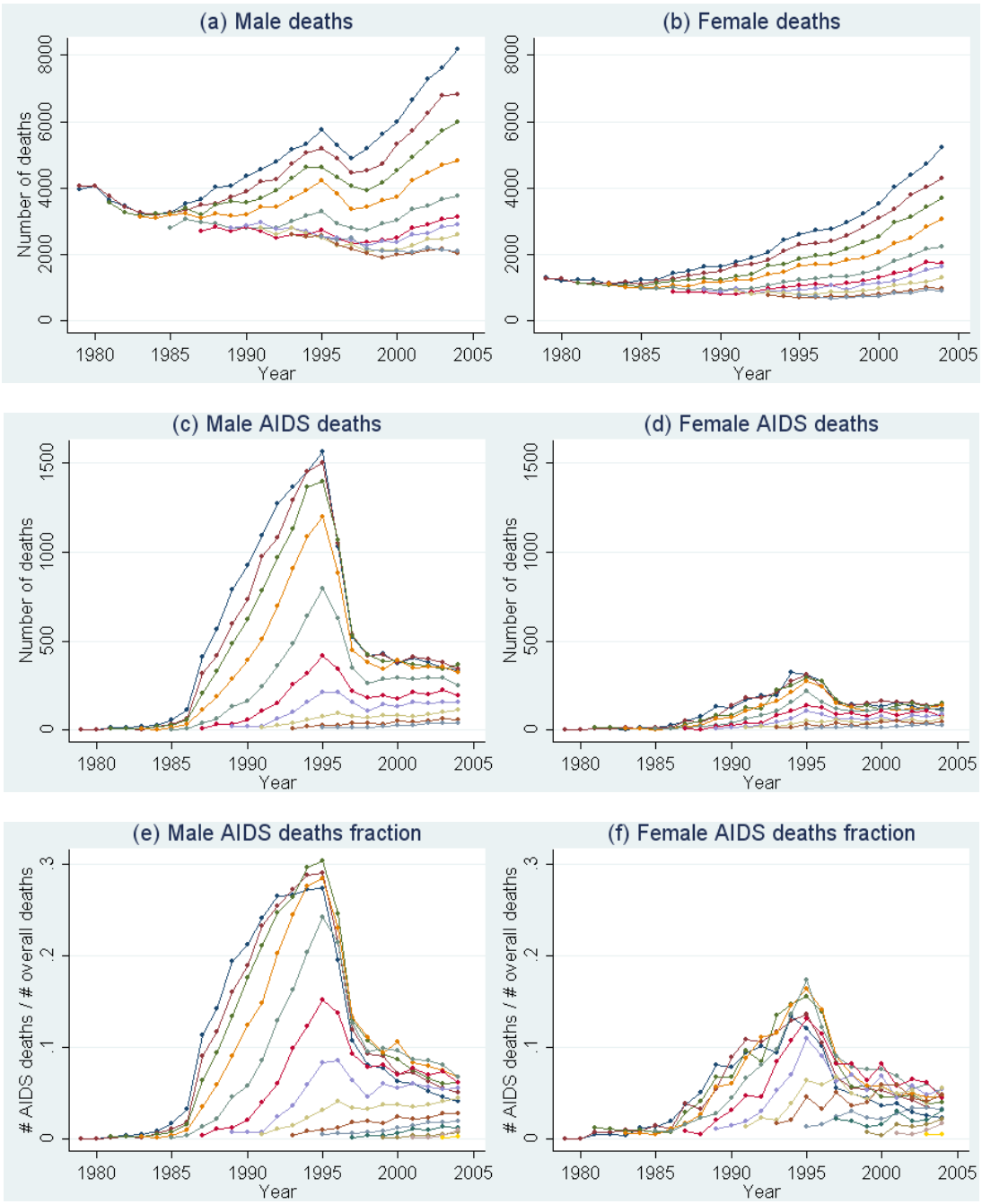
Appendix

Figure A1: Deaths, AIDS Deaths, and AIDS Death Fractions by Cohorts



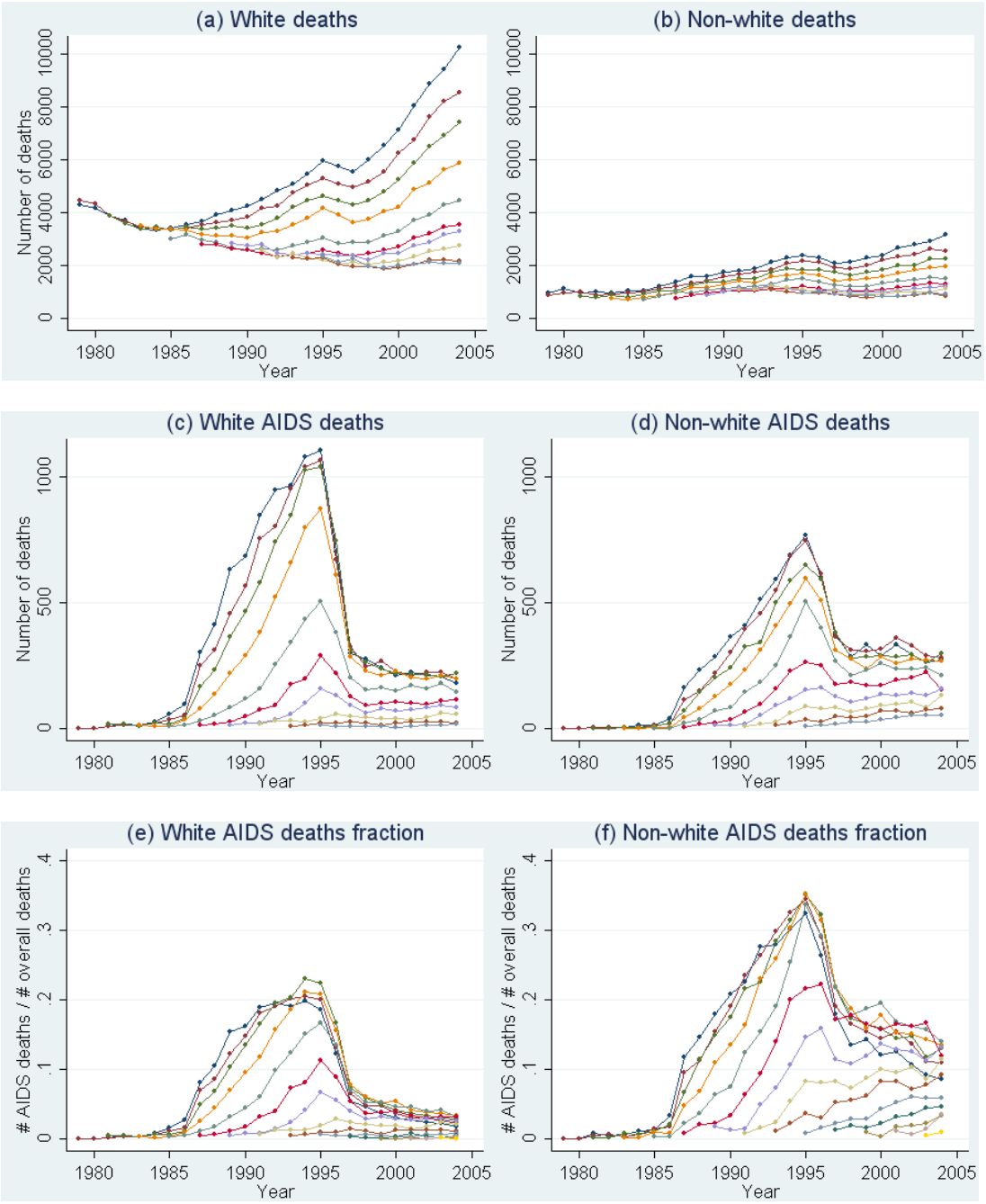
Notes: Figures present the universe of overall deaths, of AIDS deaths, and of AIDS death fractions by birth cohorts, taken from the Vital Statistics. For panels (b) and (c) the same legend applies as in panel (a).

Figure A2: Deaths, AIDS Deaths, and AIDS Death Fractions by Cohorts and Gender



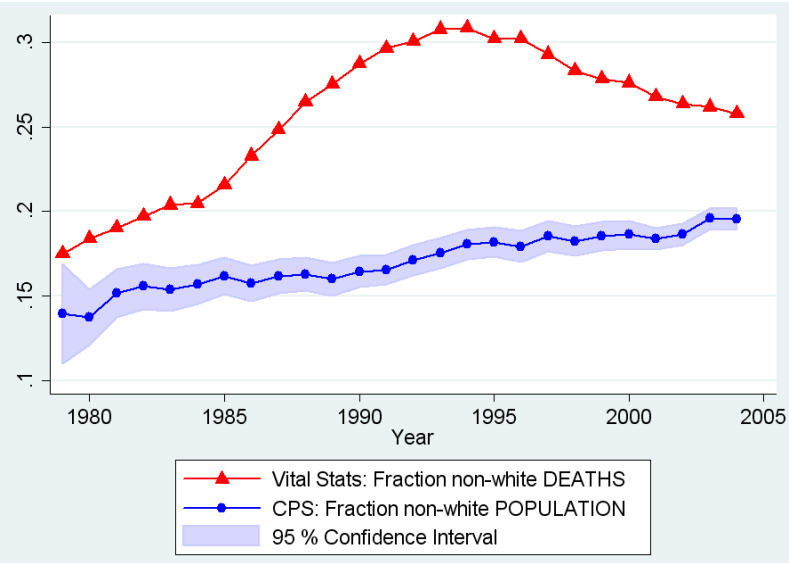
Notes: Figures present - separately for males and females - the universe of overall deaths and of AIDS deaths and AIDS death fractions by birth cohorts, taken from the Vital Statistics. The same legend as in Figure A1 (a) applies.

Figure A3: Deaths, AIDS Deaths, and AIDS Death Fractions by Cohorts and Race



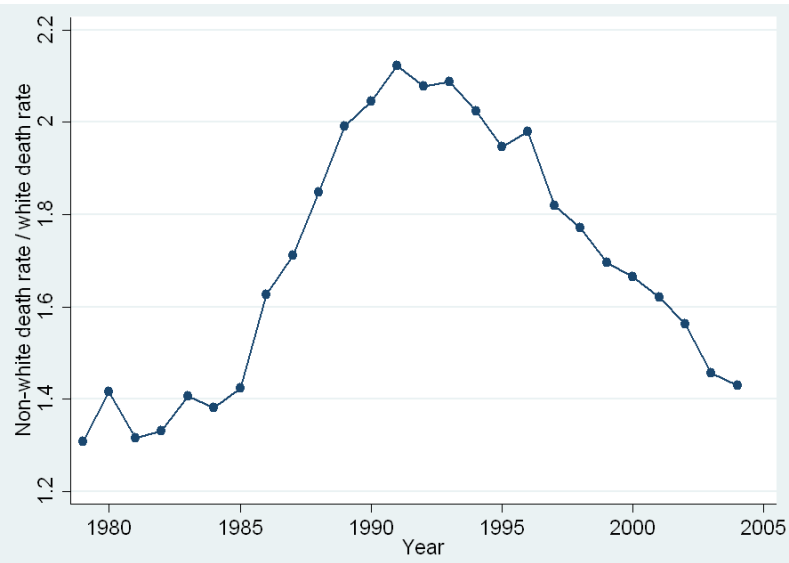
Notes: Figures present - separately for whites and non-whites - the universe of overall deaths and of AIDS deaths and AIDS death fractions by birth cohorts, taken from the Vital Statistics. The same legend as in Figure A1 (a) applies.

Figure A4: Fractions of Non-White Deaths and of the Non-White Population over Time



Notes: The fraction of non-white deaths refers to the ratio of non-white deaths divided by overall deaths, calculated from the Vital Statistics for all decedents born in the US. The fraction of non-white population is estimated from the CPS representing US-born and foreign born citizens. For a discussion see the section on the HIV/AIDS epidemic.

Figure A5: Ratio of Non-White Death Rate / White Death Rate over Time



Notes: The ratio of the non-white death rate divided by the white death rate is plotted. This ratio combines the information from the Vital Stats and the CPS used in Figure A4. The ratio is calculated in each year by: [number of non-white deaths/ fraction non-white population] / [number of white deaths / fraction white population].