# 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 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 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. I argue that the negative effects on income and health insurance coverage are the most plausible mechanisms 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. I document similar effects on alternative income measures. Further, I find strongly negative effects on the probability to be covered by employer-provided 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

[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)

ages 28 to 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 not distinguishable from zero in the overall period from 1979 to 2004. These results indicate 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 are 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 an 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. There is not much of an effect on employment status in the CPS. And the negative effects on income as well as employer-provided health insurance indicates 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

[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).

strong effects on physical and mental health (Finkelstein et al. 2012). Card et al. (2009) 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 also matters for the survival of young adults.

The effects of the business cycle on mortality has been investigated in existing papers. Ruhm (2000) shows in the US Vital Statistics that mortality is procyclical, i.e. negatively related to the contemporaneous unemployment rate. Miller at al. (2010) 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 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 before retirement age on later mortality. They find that higher unemployment in pre-retirment years leads to several years of reduced employment, health insurance coverage and health care utilization as well as a decrease in longevity. Summing up, these papers provide further evidence of the dramatic effects of the business cycle 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 for the analyzed cohorts by cohorts and years. These numbers come from the Vital Statistics. They are not population estimates but represent the universe of deaths in the population that I am analyzing.

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. Table 1 presents summary statistics for these different samples.

I supplement the Vital Stats with intercensus population estimates from the US Census to calculate death rates. Note that in my mortality regressions I exploit variation in deaths at the level of year of birth x state of birth x calendar year. The appropriate denominator to calculate death rates would therefore be population estimates at the same level of aggregation. Unfortunately, to my knowledge population estimates at the level of state of birth do not exist.

The US Census only reports state of current residence. Using the size of a cohort currently residing in a given state as a proxy for the size of the same cohort born in this state is likely to bias my estimates upwards. It seems plausible to expect that graduates facing a recession migrate to other states with more favorable labor market conditions. In this case the graduate cohort remaining in the recession state shrinks while the cohort in the more favorable state increases. Using cohort sizes by state of residence is therefore likely to lead to higher death rates for recession cohorts and lower rates for boom cohorts. In the CPS analysis I am using state of residence and I find that migration does not seem to introduce a selection bias as migration is neither gender nor race specific. However, even non-selective migration from bust to boom states would unbalance population estimates and lead to the described bias in death rates.

I therefore use cohort sizes at age zero as a proxy for the initial population base. In the year of birth only a small fraction of a cohort is subject to migration and this migration is not a response to a recession that is 18 years ahead - as long as there are no cycles of 18 years. Obviously, using the initial cohort size to calculate death rates does not take into account the shrinking due to these deaths.[7] But death rates are still small in the analyzed age range and regression include age fixed effects that control for the natural shrinking rate. And any bias due to faster shrinking of more affected cohorts would attenuate effects in subsequent years.[8]

# 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 randomly assigned.

The economic conditions at different stages in one's life are unpredictable and whether you graduate in a recession or a boom time can be considered to be basically luck. One might be worried though that graduating in one year or the other might be endogenous - we could think of a story those with higher potential earnings manage to continue schooling in a recession until the job market improved. But the existing studies shows 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 is independent of 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 to identify the effect of graduating in a recession on subsequent outcomes are the year of graduation and the state of graduation.

[7] It seems straight-forward to adjust for cohort-specific deaths using the Vital Stats. But as noted above prior to 1979 death certificates do not include the state of birth so that any cohort born before 1979 would be subject to 'blind' years for which we do not know how many deaths occurred.

[8] If affected cohorts shrink faster then the population base that 'generates' deaths is smaller than for these cohorts than for less affected ones and as a consequence the same death rate implies a smaller number of absolute deaths. Dividing by the initial rather than an updated cohort size leads to artificially low death rates for affected cohorts.

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 data either.

In the Vital Statistics 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.[9] 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 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.[10] An advantage of this 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.

[9] 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.

[10] 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 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.

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 age 18 unemployment rate 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 is collapsed by state-of-residence x graduation year x calendar year. Using instead the age 18 unemployment rate I collapse the CPS 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:

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

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. $\gamma$, $\delta$, $\lambda$ and $\theta$ are the coefficients on unrestricted experience, graduation cohort, state and calendar year fixed effects (i.e. dummy variables for each value of the respective variable). The coefficient vector of interest, $\beta_e$, 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 $\beta_e$ captures deviations from the typical experience profiles in the different outcomes that are uncorrelated contemporaneous nation-wide

shocks and related to cohort-state specific variations in graduation unemployment rates. I include the current state unemployment rate, $u_{g,s,t}$, in all regressions to isolate the effects of the graduation unemployment rate from the subsequent evolution of the local labor market conditions (year fixed effects control for the current business cycle at the national level).

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

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

With $c$ indicating the year of birth (instead of the year of graduation) and $a$ years of age. $u_{c,s}^{A18}$ 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.

# 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 - 9 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 years of education are not reported. 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 1 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 1 (a) 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 about 3 %. This is a huge effect. A typical recession with an increase in unemployment by 5 percentage points would be associated with an income loss of about 15 %. This effect if strongly persistent over time. Even 15 years after leaving high school or college a recession graduate faces an annual income loss of about 10 %.

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.

Figure 1 (b) 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 1 (a) 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 4 % 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 Figure 1 (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 1 (c) 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 due to larger sample sizes. Further the effect at age 20 is particularly strong while it is only slightly negative thereafter. It is difficult to explain why results are different from

Figure 1 (b), in particular as no comparison with a specification using actual graduation year is possible. The main take away for the (c) specification is that results do not change much when including more waves. And since the mortality regressions are mostly focused on the period 1979 to 1991, differences of individual estimates in the extended sample do not need to be overinterpreted at this point.

Figures 2 (a)-(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 3 illustrates that there does not seem to be much of an effect of the graduation unemployment rate on subsequent employment status. This might seem surprising, but it should be kept in mind that I control for the current unemployment rate in order to isolate the effect of the unemployment rate at entry from the subsequent evolution of labor market conditions. Without this control there are - as one would expect - strongly negative effects on employment status.

Figure 4 presents effects on the probability to live in a household with household income below the official poverty threshold. Not surprisingly, these figures largely mirror the effects for household income illustrated in Figures 2. Notably, the effects in specifications (a) and (b) are even more similar than for household income.

Figure 5 (a) 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. When using the age 18 unemployment rate (Figure 5 [b]) the effect of marriage becomes slightly weaker and less precise, but the pattern is similar until age 25 where after the point-estimates are essentially zero.

Figures 6 and 7 illustrate the effects on health insurance coverage (for corresponding regression results see Tables 3 and 4). Graduating in a recession with a 5 % higher unemployment rate decreases the probability to be covered by employer health insurance by 5 percentage point while the chance to have Medicaid increases by 2 percentage point. Given average coverage rates in the sample of 38 % for employer health insurance and 7 % for Medicaid, these effects on health insurance are large (13 % and 28 %, respectively). For employer health insurance the estimated effect shifts towards zero after 5 years or

after age 23 but point estimates stay below zero for 14 years or until age 32. For Medicaid coverage point estimates go to zero after 8 years or age 27. In all cases, however, these estimates indicate that recession graduates suffer an extended period of worse health insurance coverage.

Finally the results in Figure 8 demonstrate that the race composition is similar in recession and non-recession cohorts. This suggests that selection due to migration or - in specification (a) - due to endogenous graduation timing is not an important issue. If those with better socio-economic background and therefore higher potential earnings would delay graduation or would migrate to states with lower unemployment we should see a negative relationship between the graduation unemployment rate and the probability of being white - since race strongly reflect socio-economic status. But the race composition is unrelated to the economic conditions both in the actual year of graduation as well as at age 18. When including CPS waves up to year 2004 there seems to be a positive effect after age 25. While this pattern seems difficult to explain it poses some doubt on whether one should rely on this identification strategy in the extended sample without being able to check results with the actual year of graduation.

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, however, it is useful to look whether different socio-economic groups are affected differently by graduating in a recession. Figures 9 to 16 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 1991 (specification [b]), as this is the main specification that I am using the mortality regressions. In some cases I omit confidence intervals for the non-white regression estimates to keep reasonable ranges for the commonly scaled figures. For regression tables corresponding to Figures 9, 14 and 15 see Tables 5 to 7.

Across gender estimated effects are quite homogeneous for most outcomes, with two exceptions. The negative effect of graduating in a recession on the probability of being married seems to be driven by males (Figures 12 [a] and [b]). This is plausible. For men the decision to get married is mostly about the cost of paying for a family and being less flexible to take jobs in another region. Both of these factors imply that having a worse job due to graduating in a recession results in a lower probability to get married. For women, who are typically not the main earner in a family, marriages are likely to relax the constraints implied by having a worse job. The other exception is Medicaid coverage. As illustrated in Figures 15 (a) and (b), the positive effect on the probability to be covered by Medicaid is more prolonged for women. This also makes sense because Medicaid is particularly directed to women. This is surprising given that we might expect stronger effects on job quality (i.e. less income and worse health insurance coverage) to lead also to lower marriage rates.

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, poverty as well as insurance coverage are about twice to three times as large as for whites. Only for the probability to be married effects do not seem to differ between these two groups.

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 16 (a) - (d) test for an effect of the age 18 unemployment rate on cohorts' race composition in the gender regressions and on the gender composition in the race regressions. In neither of the four regressions significant effects are detectable providing further evidence that selection into graduation years and/or states of residence is not an important issue.

To sum up, the CPS data revealed strongly negative effects of graduating in a recession on income and health insurance which are similar across gender but stronger for non-whites. Marital status is negatively affected for men but not for women and this effect does not seem to differ between whites and non-whites.

# 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 as well as for the current state unemployment rate. I restrict the main analysis to the years 1979 to 1991, the time period for which I could compare my identification strategy in the CPS to the specification using the actual year of graduation.

Figure 17 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). 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 22 deaths. This implies that a 1 percentage point increase in the unemployment rate at age 18 leads to 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? Notice that I am controlling for state and for year of birth fixed effects. This 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 18 shows on the left y-axis the effect on the death rate in terms of 10,000 cohort members.[11] This means that every observation for a given state-of-birth x year-of-birth cohort is divided by the same constant.[12] In this specification a small hump shows up already at age 25, but the overall pattern is very similar to that in Figure 17. While effects are close to zero for the early 20s, estimates linearly increase after age 26. At age 33 the effect reaches 0.5, meaning that the effect of 22 deaths displayed in Figure 17 refers to 5 deaths per 100,000 cohort members.

On the right y-axis of Figure 18 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 1992 to 1995 the effects decrease strongly (Figure 19). The estimate at age 33 decreases from 22 to 15 additional deaths.

When including the years 1992 to 2004 in Figure 20, the effect diminishes to 2.3 and is no longer distinguishable from zero. Furthermore, effects seem to be slightly negative until age 27. It is difficult to interpret these slightly negative effects for the extended time period for which I cannot analyze my identification strategy in the CPS. However, Figures 19 and 20 suggest that the strong positive age effect displayed in Figures 17 and

[11] I take cohort sizes from the annual intercensus estimates. As these estimates are only available per state of residence I use cohort sizes by state of residence at age zero as a proxy for initial cohort sizes by state of birth. See the data section for a more detailed discussion.

[12] Alternatively I could take the logarithm of the number of births and include state-of-birth x year-of-birth fixed effects. This does not allow to estimate the level of the effects but the effect changes over age resulting from such a specification are very similar to those presented in Figure 18.

18 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.

Figures 21 (a) and (b) illustrate estimates from such separate regressions. As it can be seen the positive and increasing effect on mortality after age 27 is mostly driven by AIDS deaths. Estimates for non-AIDS deaths in Figure 21 (b) do also increase after age 28 but this increase is small compared to the effects in Figure 21 (a) and for most years not significant. A potential explanation for the similar, though much weaker effect pattern in Figure 21 (b) could be that not all AIDS deaths are diagnosed as such in the death certificate. Overall, the results in Figure 21 clearly suggest that the effects on mortality are driven by AIDS deaths. But why do estimated effects decrease when including the years 1992 to 1995 (Figure 19), 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 1991 and was directed in particular to 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 seems to have softened socio-economic differences in mortality rates already since 1992 (Figure A5).

In Figures 22 (a) to (d) I analyze the effects separately for men and women as well as for whites and non-whites. For such subgroup analysis effects in terms of absolute numbers of deaths would not be informative because these groups vary considerably in size. One reasonable way to rescale effects would be to construct group specific death rates. However, as mentioned above population estimates by state of birth x year of birth are difficult to obtain. Constructing even smaller cells by gender and race would make this task yet more difficult. A useful shortcut is to take non-AIDS deaths as a proxy for cohort size and use the ratio of AIDS deaths to non-AIDS deaths as dependent variable. To the extend that the age 18 unemployment rate also positively affects non-AIDS deaths (e.g. if not all AIDS deaths are categorized as such in the death certificates) estimated effects on these ratios are attenuated towards zero.

The comparison of the mortality effects across gender and race resembles astonishingly

the heterogeneity found for income and health insurance effects in the CPS. The effects of the age 18 unemployment rate on AIDS death ratios is very similar for males and females (Figures 22 [a] and [b]). The race regressions, however, reveal much stronger effects for non-whites than for whites (Figures 22 [c] and [d]). Effects for non-whites are about twice as large as those for whites.

# 7 Interpretation

I find a 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 non-whites.

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 merely picks up a spurious correlation of the business cycle with the dramatic increases in AIDS deaths. However, notice that 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, could there be a spurious or confounded 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 strong by the AIDS epidemic a few years later? In this case we should expect my 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 even though the gender gap in overall AIDS deaths is even more pronounced than the race gap (Tables A2 and A3). Moreover, I find the same effect pattern across gender and race for income and health insurance in the CPS. In other words, the pattern of mortality effects is precisely what we should expect if these effects are driven by the same causes as the effects in the CPS but not if they are due to a spurious relationship of local recessions with the outbreak of the AIDS epidemic.

Hence, it seems plausible that the differences in AIDS deaths are actually caused by differences in the economic conditions when these cohorts entered the labor market. These effects could be driven by two 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 and 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. However, the CPS results for marital status show that - if anything - we should expect to see a difference in mortality effects across gender, as males seem to be less likely to get married when graduating in worse economic conditions (Figure 18). Across race, on the other hand, there is no differential effect on marital status but the mortality effects are stronger. To the extent that married individuals have less sex partners this evidence suggests that higher infection rates through increased sexual contacts is not the main driver behind the mortality effects.

The opportunity cost story, on the other hand, seems not appealing given that there is not much of an effect on the probability of being unemployed. 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 I find that recession cohorts have worse health insurance. This also implies that the direct costs of unhealthy behavior are larger for these cohorts.

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.

However, these arguments are rather suggestive and not strong evidence against infection rates driving my results. Testing for this mechanism directly with data on HIV/AIDS

prevalence rates will be the next step to gain further insights.

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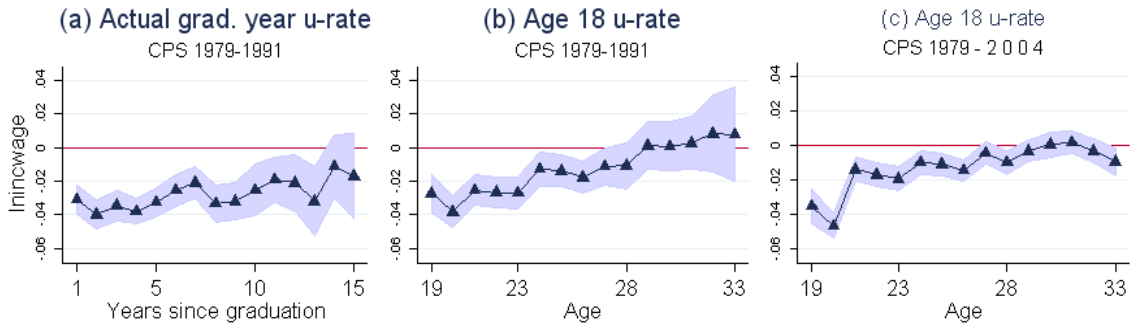
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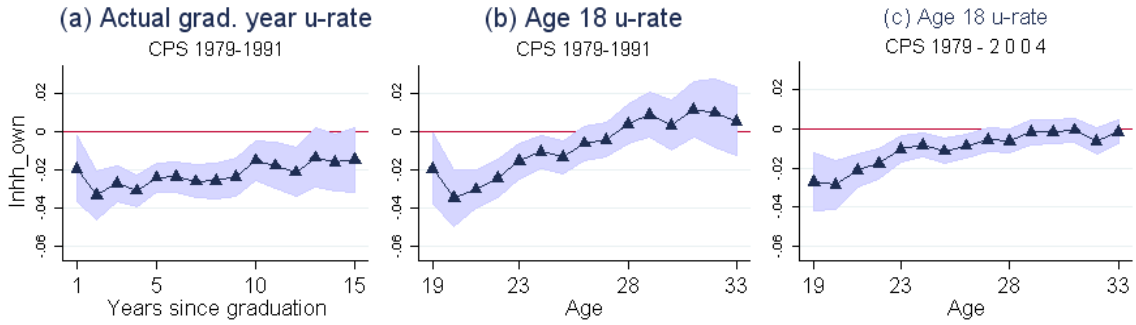
# 9 Figures and Tables

**Figure 1 – Effect of Unemployment Rate around Graduation on: Ln Wage**



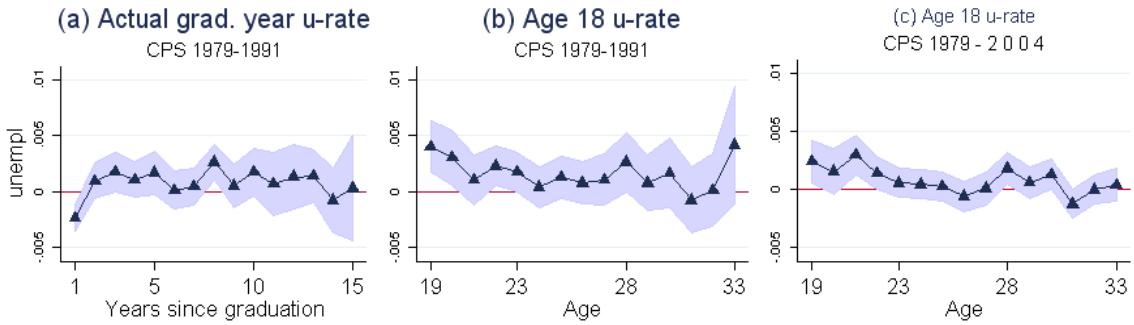

Notes: The dependent variable in all panels is the ln of individual wage. Panel (a) displays the coefficients of the unemployment rate at the year of graduation interacted with dummies for the years since graduation. Panels (b) and (c) displays the coefficients of the unemployment rate at age 18 interacted with dummies for age 19 to 33. Shaded areas 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 2.

**Figure 2 – Effect of Unemployment Rate around Graduation on: Ln Household Income**



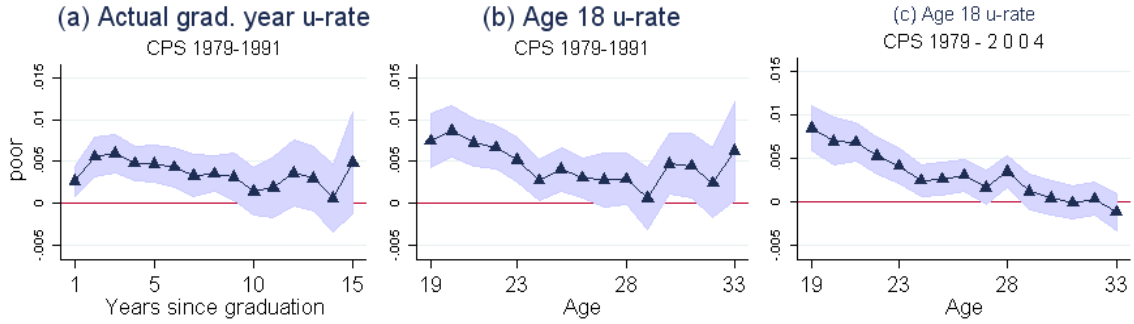

Notes: The dependent variable in all panels is the ln of household income. For further explanations see the notes below Figure 1.

**Figure 3 – Effect of Unemployment Rate around Graduation on: Unemployed**



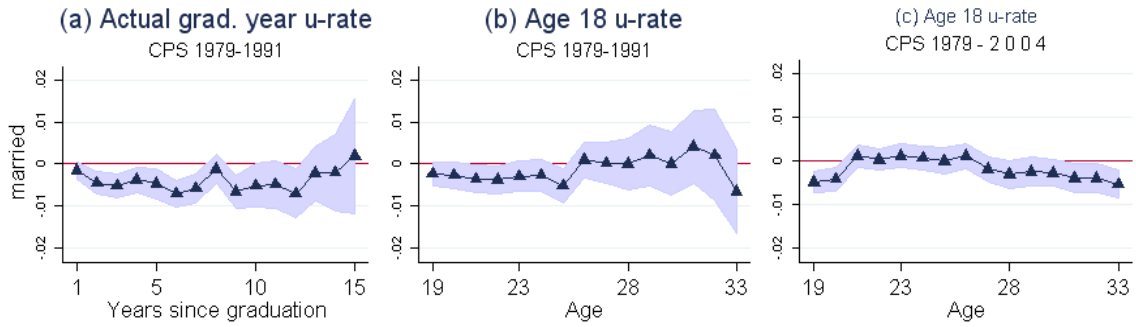

Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent is unemployed. For further explanations see the notes below Figure 1.

**Figure 4 – Effect of Unemployment Rate around Graduation on: Below Poverty Threshold**



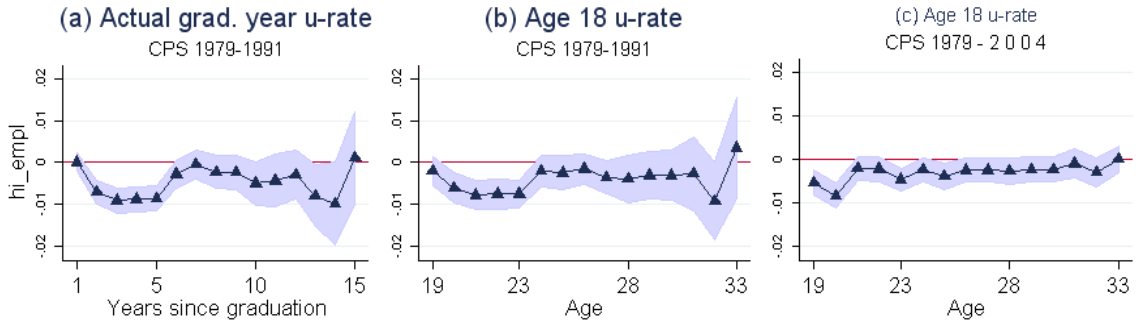

Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent's family income is below the official poverty threshold. For further explanations see the notes below Figure 1.

**Figure 5 - Effect of Unemployment Rate around Graduation on: Married**



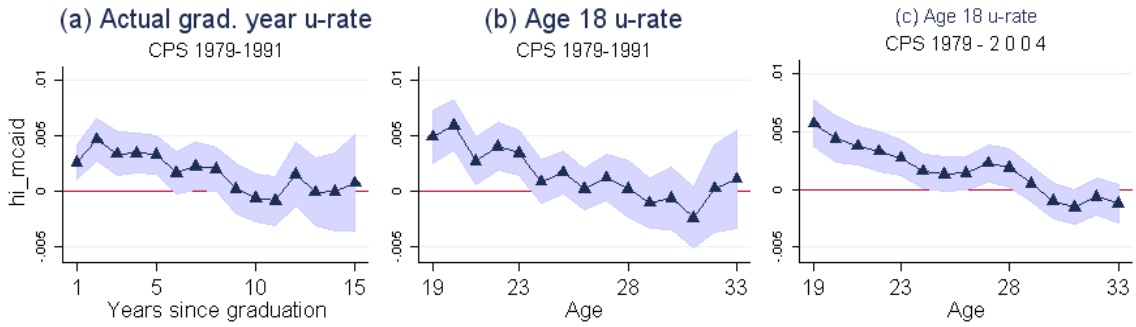

Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent is married. For further explanations see the notes below Figure 1.

**Figure 6 – Effect of Unemployment Rate around Graduation on: Employer Health Insurance**



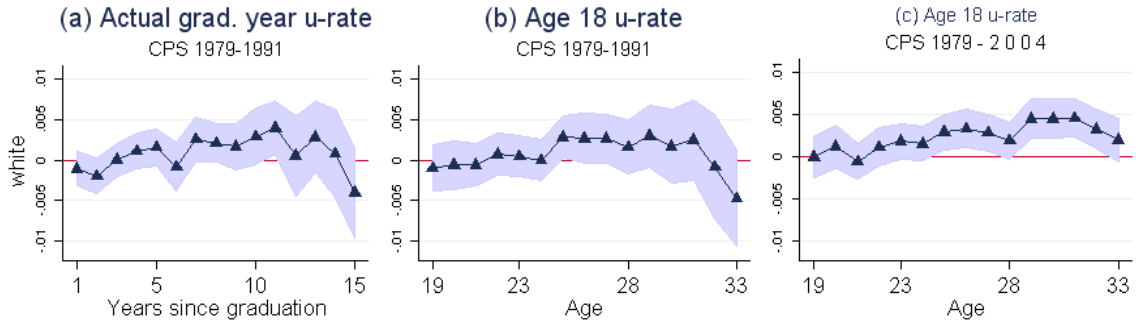

Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent is covered by employer health insurance. For further explanations see the notes below Figure 1.For corresponding regression results see Table 3.

**Figure 7 - Effect of Unemployment Rate around Graduation on: Medicaid**



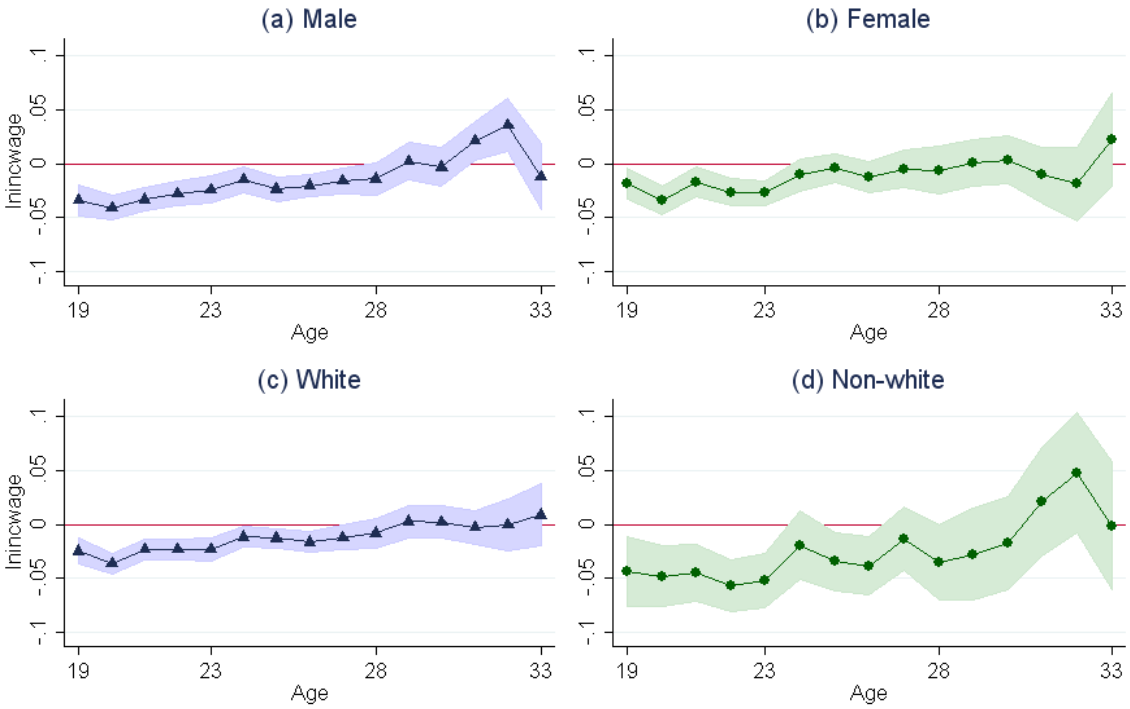

Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent is covered by Medicaid. For further explanations see the notes below Figure 1. For corresponding regression results see Table 4.

**Figure 8 - Effect of Unemployment Rate around Graduation on: White**



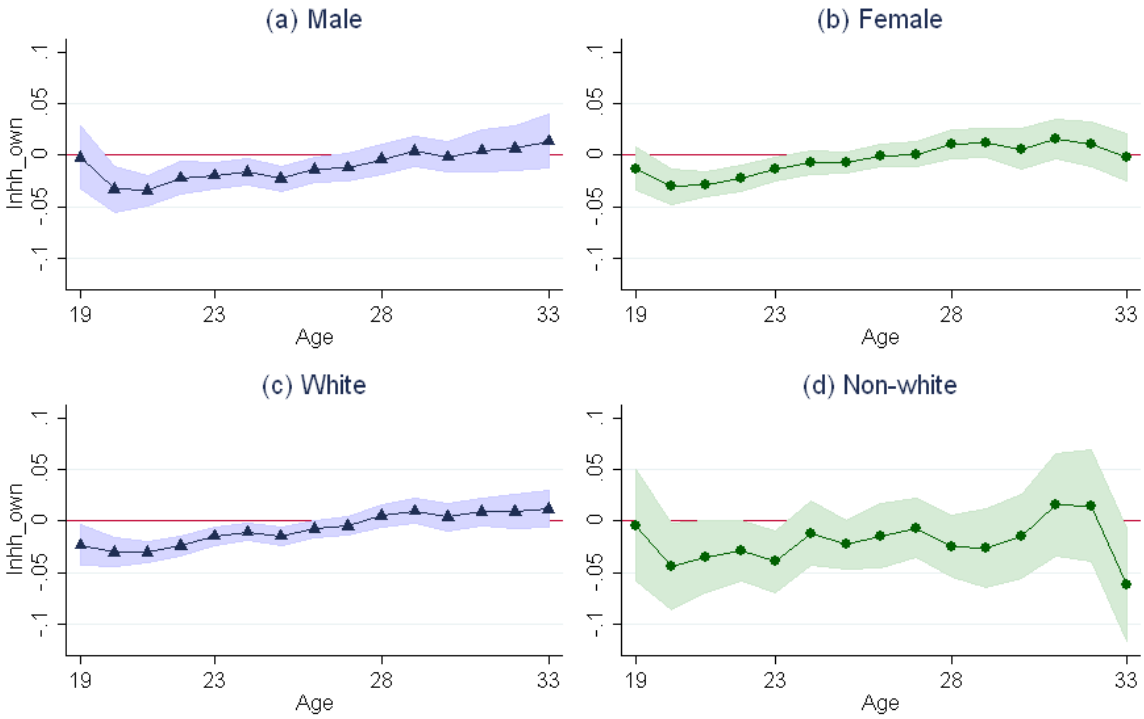

Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent is white. For further explanations see the notes below Figure 1.

**Figure 9 – Effect of Age 18 Unemployment Rate on: Ln Wage**



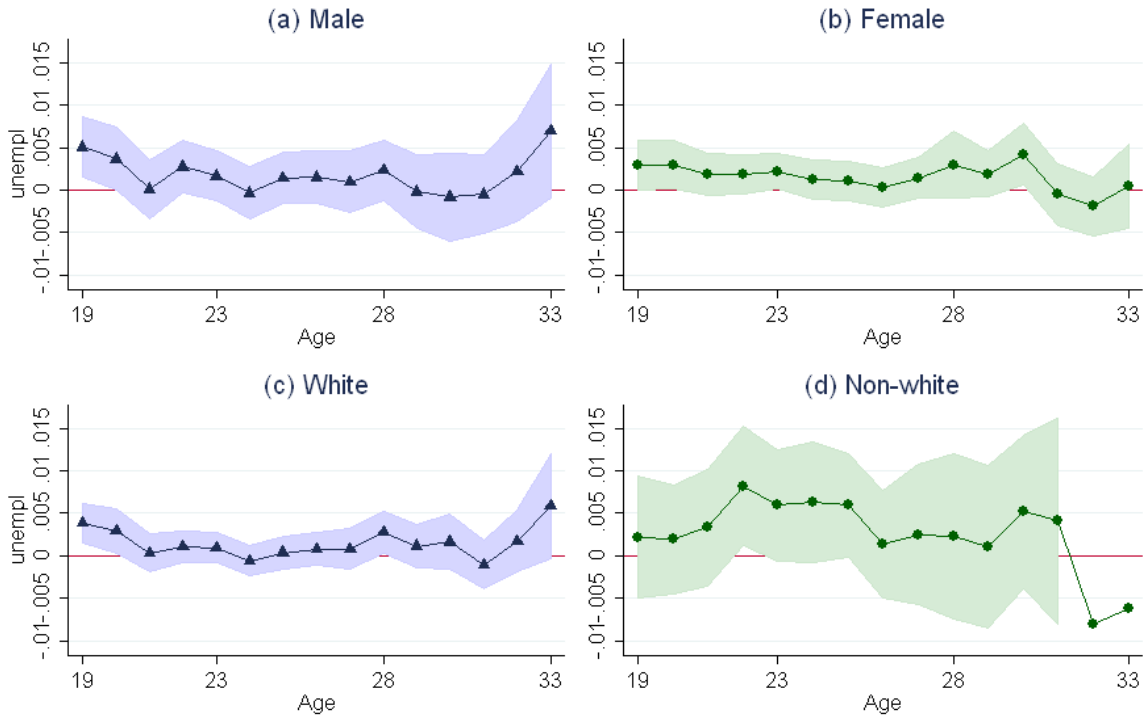

Notes: The dependent variable is the ln of individual wages. The coefficients of the unemployment rate at age 18 interacted with dummies for age 19 to 33 are displayed from separate regressions for male, female, white and non-white. Shaded areas indicate 95 % confidence intervals. Regressions control 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 10 – Effect of Age 18 Unemployment Rate on: Ln Household Income**



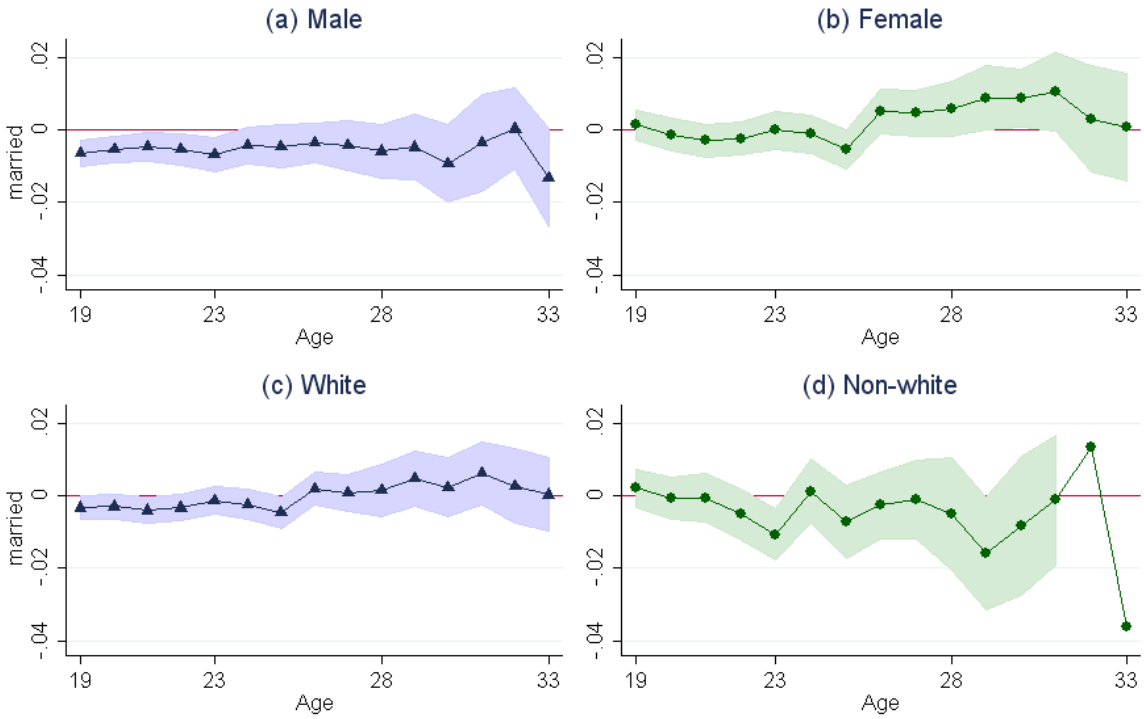

Notes: The dependent variable in all panels is the ln of household income. For further explanations see the notes below Figure 9.

**Figure 11 – Effect of Age 18 Unemployment Rate on: Unemployed**



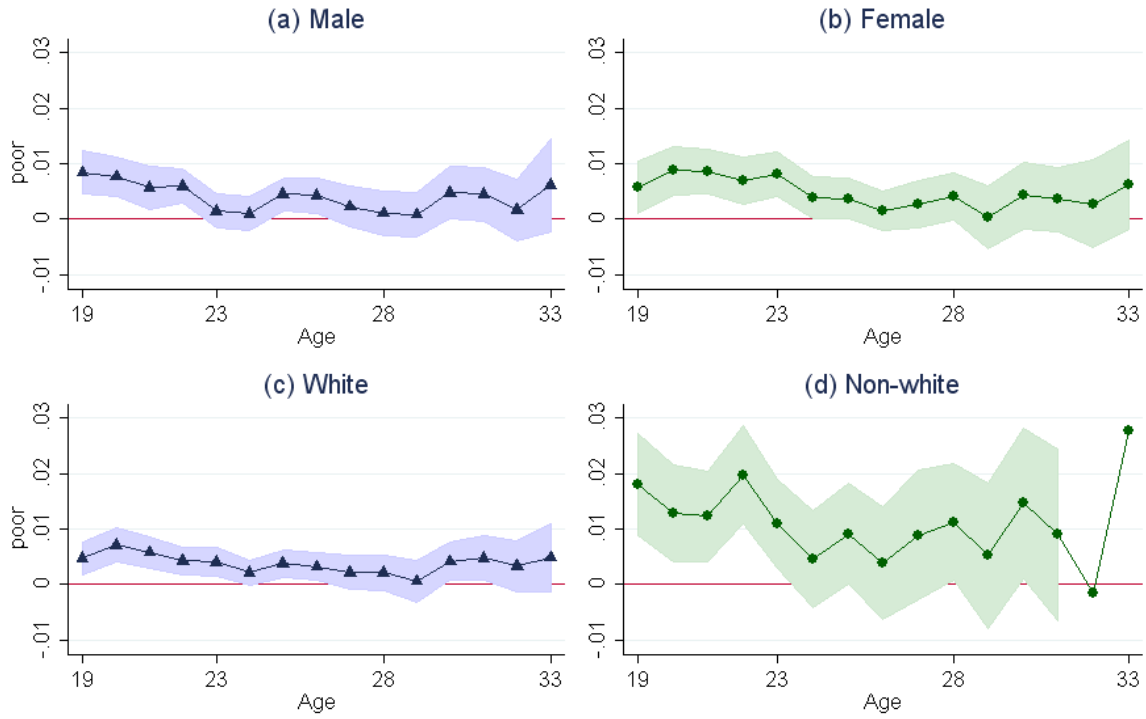

Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent is unemployed. Confidence intervals for the last two coefficients in panel (d) are suppressed to keep scales in reasonable ranges. For further explanations see the notes below Figure 9.

**Figure 12 - Effect of Age 18 Unemployment Rate on: Married**



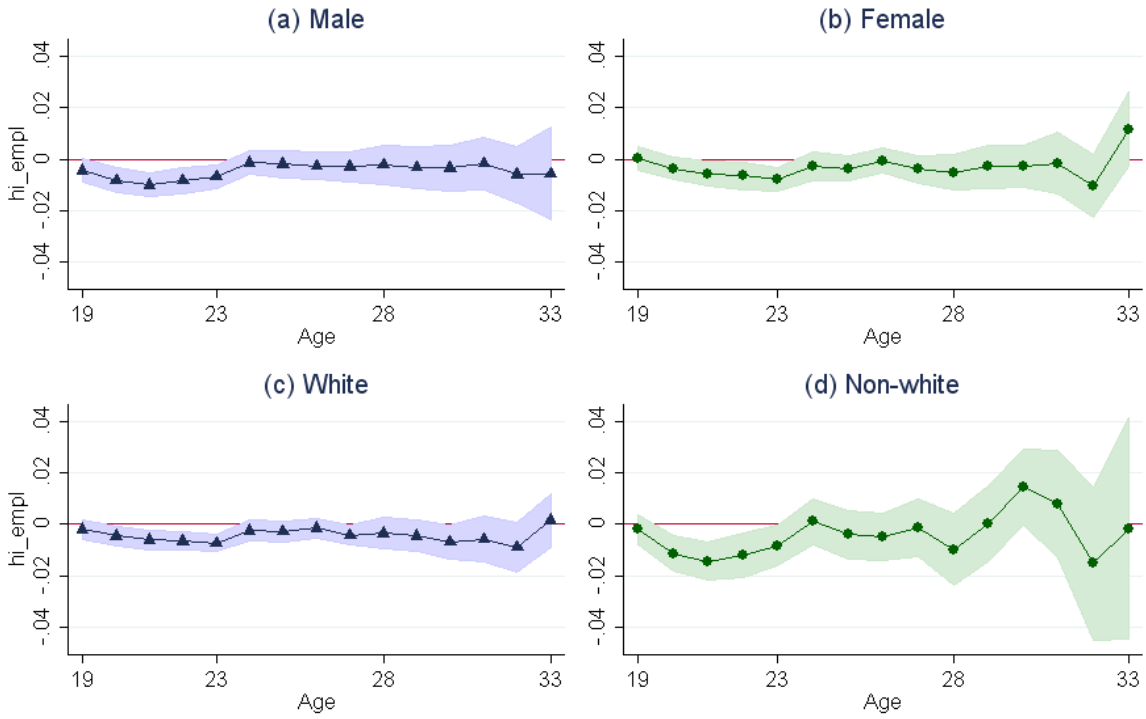

Notes: The dependent variable is a dummy variable indicating whether the respondent is married. Confidence intervals for the last two coefficients in panel (d) are suppressed to keep scales in reasonable ranges. For further explanations see the notes below Figure 9.

**Figure 13 – Effect of Age 18 Unemployment Rate on: Below Poverty Threshold**



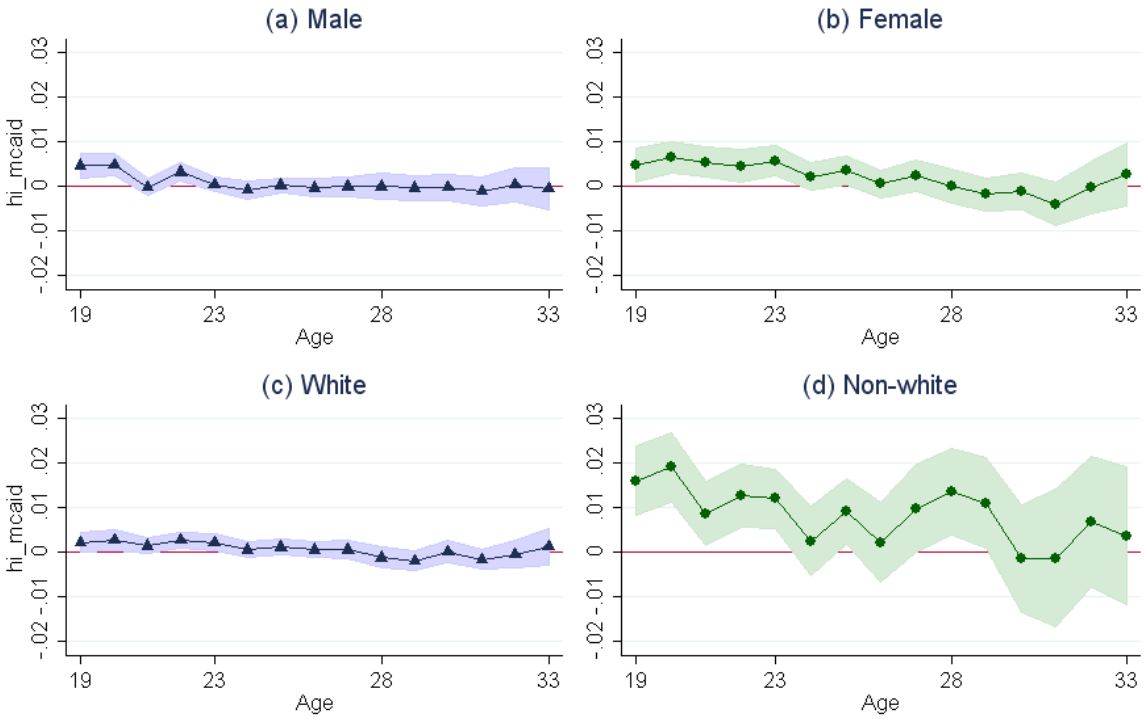

Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent's family income is below the official poverty threshold. Confidence intervals for the last two coefficients in panel (d) are suppressed to keep scales in reasonable ranges. For further explanations see the notes below Figure 9.

**Figure 14 – Effect of Age 18 Unemployment Rate on: Employer Health Insurance**



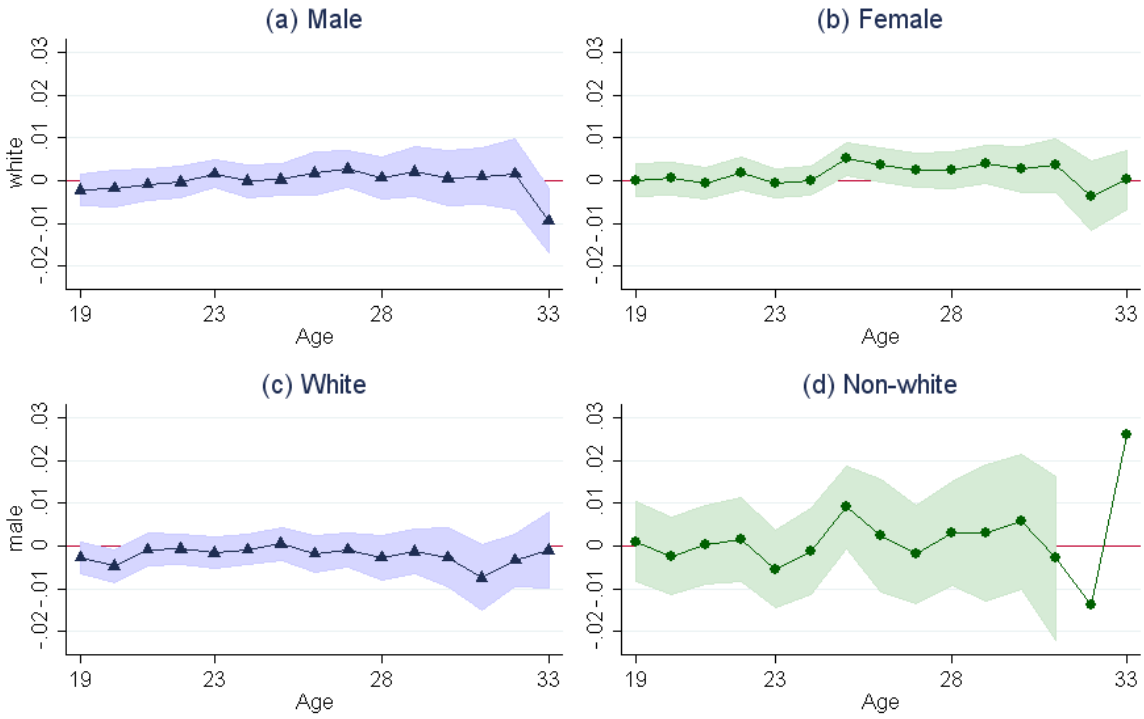

Notes: The dependent variable in all panels is a dummy variable indicating whether the respondent is covered by employer health insurance. For further explanations see the notes below Figure 9.For corresponding regression results see Table 6.

**Figure 15 - Effect of Age 18 Unemployment Rate on: Medicaid**



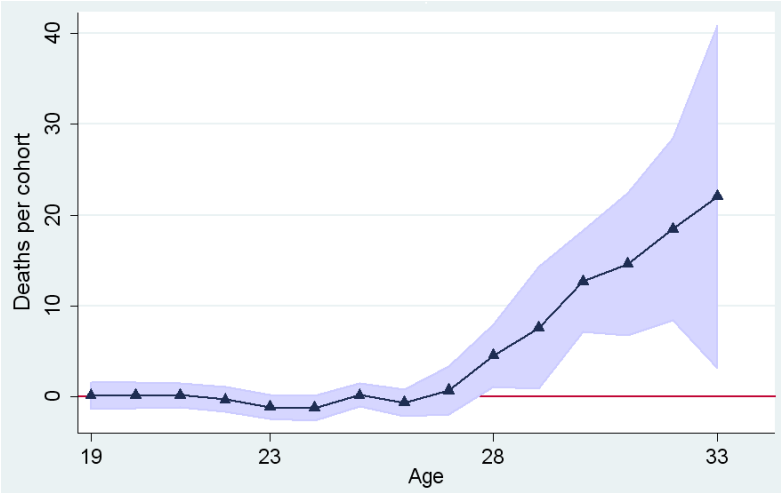

Notes: The Dependent variable in all panels is a dummy variable indicating whether the respondent is covered by Medicaid. For further explanations see the notes below Figure 9. For corresponding regression results see Table 7.

**Figure 16 – Effect of Age 18 Unemployment Rate on: Male / White**



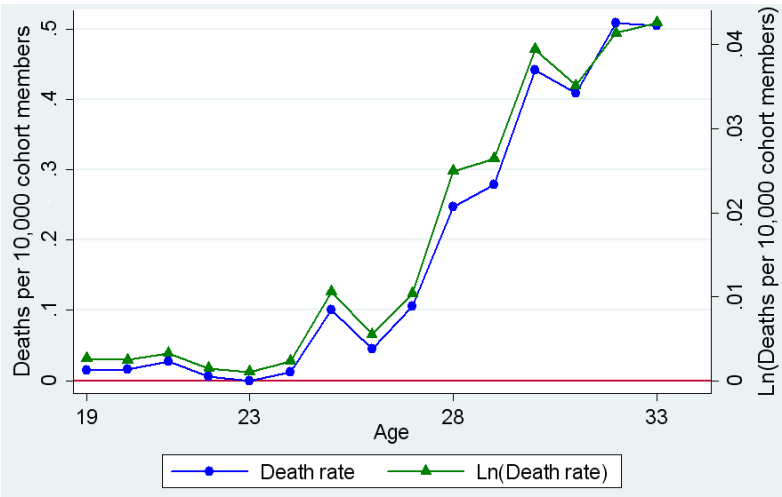

Notes: The dependent variable is a dummy variable indicating whether the respondent is male in panels (a) and (b) and white in (c) and (d). Confidence intervals for the last two coefficients in panel (d) are suppressed to keep scales in reasonable ranges. For further explanations see the notes below Figure 9.

**Figure 17 – Effect of Age 18 Unemployment Rate on: Number of Deaths per Cohort**
**US Vital Statstics 1979 - 1991**



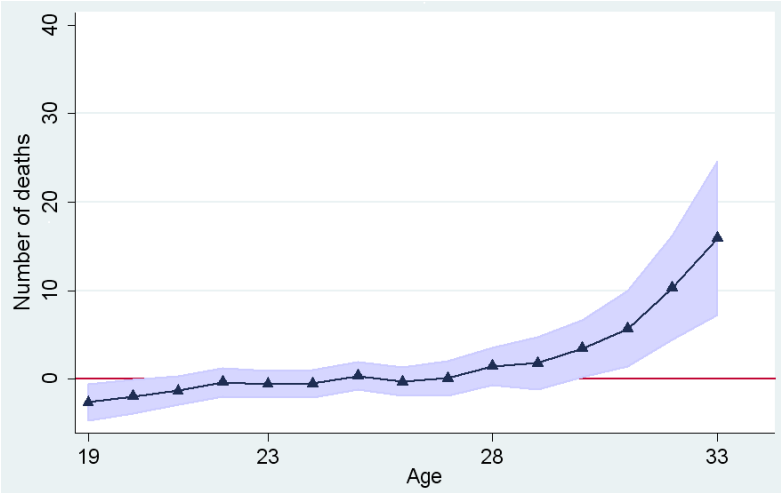

Notes: Coefficients from the regression of the number of deaths per cohort on the unemployment rate at age 18 interacted with dummies for age 19 to 33 are displayed. Regressions include the current unemployment rate as well as fixed effects for year-of-birth, state, calendar year and age. Shaded areas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

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



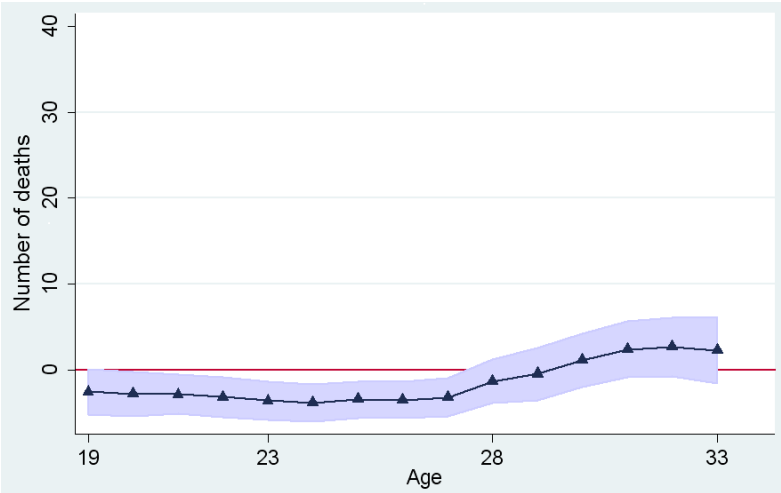

Notes: The dependent variable is the number of deaths per 10,000 cohort members on the left and the ln of deaths per 10,000 cohort members on the right-hand side. For further comments see Figure 17.

**Figure 19 – Effect of Age 18 Unemployment Rate on: Number of Deaths**
**US Vital Statstics 1979 - 1995**



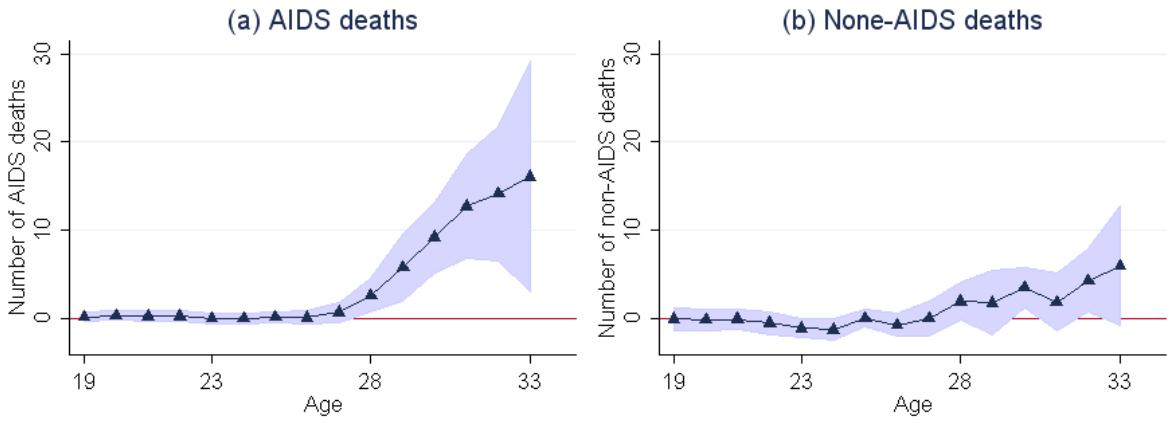

Notes: Coefficients from the regression of the number of deaths per cohort on the unemployment rate at age 18 interacted with dummies for age 19 to 33 are displayed. Regressions include the current unemployment rate as well as fixed effects for year-of-birth, state, calendar year and age. Shaded areas indicate 95 % confidence intervals. For corresponding regression results see Table 8 column (2).

**Figure 20 – Effect of Age 18 Unemployment Rate on: Number of Deaths,**
**US Vital Statstics 1979 - 2004**



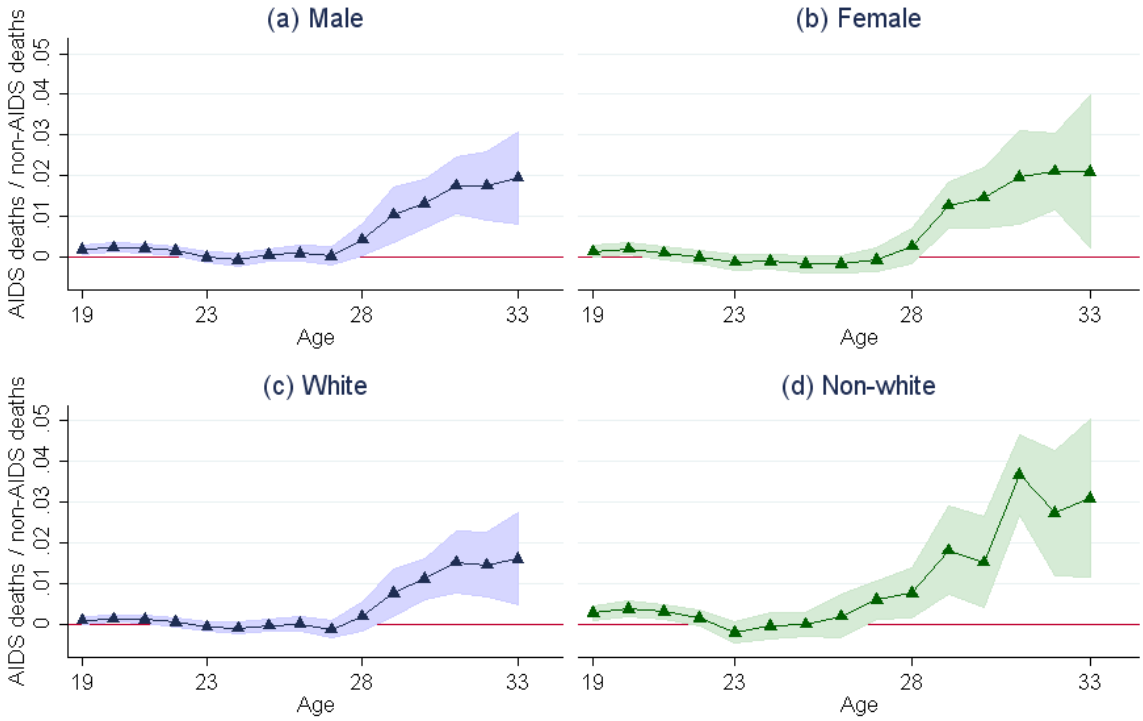

Notes: For further comments see Figure 19. For corresponding regression results see Table 8 column (3).

**Figure 21 – Effect of Age 18 Unemployment Rate on: AIDS and non-AIDS Deaths, US Vital Statstics 1979 – 1991**




Notes: Coefficients from separate regressions with AIDS deaths and Non-Aids Deaths as dependent variable are displayed. For further comments see Figure 19. For corresponding regression results see Table 9.

**Figure 22 – Effect of Age 18 Unemployment Rate on: AIDS Death Ratios (AIDS / non-AIDS), US Vital Statstics 1979 – 1991**




Notes: Coefficients from separate regressions for males, females, whites and non-whites with AIDS death ratios as dependent variable are displayed. For further comments see Figure 19. 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>1975 | Year when age 18>1975 | Year when age 18>1975; age<34 |
| *Current Population Survey* | (1) | (2) | (3) |
| Age (mean) | 23.55 | 24.09 | 25.56 |
| % male | 50.07% | 49.65% | 50.04% |
| % white | 83.98% | 84.06% | 82.16% |
| Years of education (mean) | 12.56 | 12.98 | 13.08 |
| Ln income (mean) | 9.16 | 9.35 | 9.48 |
| % unemployed | 7.32% | 7.85% | 6.92% |
| % married | 35.68% | 39.03% | 40.26% |
| % below poverty threshold | 13.96% | 13.85% | 13.92% |
| % covered by employer health insurance | 38.30% | 42.36% | 41.02% |
| % covered by Medicaid | 7.31% | 6.91% | 8.14% |
| N | 385,870 | 265,264 | 615,650 |
| *Vital Statistics - Universe of death* | | | |
| Number of deaths | - | 523,471 | 1,247,521 |
| Male deaths | - | 389,161 | 910,956 |
| White deaths | - | 395,413 | 903,284 |
| AIDS deaths | - | 26,614 | 85,873 |
| Male AIDS deaths | - | 22,381 | 67,886 |
| White AIDS deaths | - | 17,189 | 48,112 |
| *Annual intercensus estimates* | | | |
| Initial population base (size of analyzed cohorts at year of birth) | - | 57,600,000 | 102,000,000 |

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 after 1975 / birth cohorts that were of age 18 after 1975, 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 | Grad. Year u-rate | Age 18 u-rate | Age 18 u-rate |
| Graduation on Ln Income | (1) | (2) | (3) |
| Effect at Year 1 | -0.031 | -0.027 | -0.035 |
| | (0.004) | (0.006) | (0.005) |
| Effect at Year 2 | -0.04 | -0.038 | -0.047 |
| | (0.005) | (0.005) | (0.004) |
| Effect at Year 3 | -0.035 | -0.025 | -0.014 |
| | (0.005) | (0.005) | (0.004) |
| Effect at Year 4 | -0.038 | -0.027 | -0.017 |
| | (0.004) | (0.005) | (0.004) |
| Effect at Year 5 | -0.032 | -0.027 | -0.019 |
| | (0.004) | (0.005) | (0.004) |
| Effect at Year 6 | -0.025 | -0.013 | -0.01 |
| | (0.005) | (0.005) | (0.004) |
| Effect at Year 7 | -0.021 | -0.014 | -0.011 |
| | (0.005) | (0.005) | (0.003) |
| Effect at Year 8 | -0.033 | -0.018 | -0.014 |
| | (0.006) | (0.005) | (0.003) |
| Effect at Year 9 | -0.032 | -0.011 | -0.004 |
| | (0.006) | (0.006) | (0.003) |
| Effect at Year 10 | -0.025 | -0.011 | -0.01 |
| | (0.008) | (0.007) | (0.004) |
| Effect at Year 11 | -0.019 | 0.001 | -0.003 |
| | (0.007) | (0.007) | (0.003) |
| Effect at Year 12 | -0.021 | 0.001 | 0.001 |
| | (0.009) | (0.007) | (0.004) |
| Effect at Year 13 | -0.032 | 0.003 | 0.002 |
| | (0.010) | (0.008) | (0.003) |
| Effect at Year 14 | -0.011 | 0.008 | -0.003 |
| | (0.010) | (0.012) | (0.003) |
| Effect at Year 15 | -0.017 | 0.008 | -0.009 |
| | (0.013) | (0.014) | (0.004) |
| State FE, year FE | ✓ | ✓ | ✓ |
| Graduation cohort FE | ✓ | | |
| Years since graduation FE | ✓ | | |
| Birth cohort FE | | ✓ | ✓ |
| Age FE | | ✓ | ✓ |
| Number of observations (collapsed) | 5,967 | 5,966 | 7,955 |
| Underlying observations (individual level) | 385,870 | 265,263 | 615,645 |
| $R^2$ | 0.81 | 0.85 | 0.84 |

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 19-23 (equation [2]). 'FE' refers to 'fixed effects', i.e. a set of dummies for the different values of the respective variable. For a graphical presentation see Figure 1.

## Table 3: Regressions of Employer Health Insurance Coverage on Graduation / Age 18 Unemployment Rate

| | baseline: 1979-1991 | | extended: 1979-2004 |
|---|---|---|---|
| Effect of Unemployment Rate Around | Grad. Year u-rate | Age 18 u-rate | Age 18 u-rate |
| Graduation on Employer Health Insurance | (1) | (2) | (3) |
| Effect at Year 1 | 0.000 | -0.002 | -0.005 |
| | (0.001) | (0.002) | (0.001) |
| Effect at Year 2 | -0.007 | -0.006 | -0.008 |
| | (0.001) | (0.002) | (0.001) |
| Effect at Year 3 | -0.009 | -0.008 | -0.002 |
| | (0.002) | (0.002) | (0.001) |
| Effect at Year 4 | -0.009 | -0.008 | -0.002 |
| | (0.001) | (0.002) | (0.001) |
| Effect at Year 5 | -0.009 | -0.008 | -0.005 |
| | (0.002) | (0.002) | (0.001) |
| Effect at Year 6 | -0.003 | -0.002 | -0.002 |
| | (0.002) | (0.002) | (0.001) |
| Effect at Year 7 | -0.001 | -0.003 | -0.004 |
| | (0.002) | (0.002) | (0.001) |
| Effect at Year 8 | -0.002 | -0.002 | -0.002 |
| | (0.002) | (0.002) | (0.001) |
| Effect at Year 9 | -0.002 | -0.004 | -0.002 |
| | (0.002) | (0.002) | (0.001) |
| Effect at Year 10 | -0.005 | -0.004 | -0.003 |
| | (0.003) | (0.003) | (0.002) |
| Effect at Year 11 | -0.004 | -0.003 | -0.002 |
| | (0.003) | (0.003) | (0.002) |
| Effect at Year 12 | -0.003 | -0.003 | -0.002 |
| | (0.003) | (0.003) | (0.001) |
| Effect at Year 13 | -0.008 | -0.003 | -0.001 |
| | (0.004) | (0.005) | (0.002) |
| Effect at Year 14 | -0.010 | -0.009 | -0.003 |
| | (0.005) | (0.005) | (0.002) |
| Effect at Year 15 | 0.001 | 0.003 | 0.000 |
| | (0.006) | (0.006) | (0.002) |
| State FE, year FE | ✓ | ✓ | ✓ |
| Graduation cohort FE | ✓ | | |
| Years since graduation FE | ✓ | | |
| Birth cohort FE | | ✓ | ✓ |
| Age FE | | ✓ | ✓ |
| Number of observations (collapsed) | 5,814 | 5,814 | 7,880 |
| Underlying observations (individual level) | 378,611 | 262,328 | 612,714 |
| $R^2$ | 0.70 | 0.75 | 0.71 |

Notes: The dependent variable is the probability to be covered by employer-provided health care. Further comments as in Table 2. For a graphical presentation see Figure 6.

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

| | baseline: 1979-1991 | | extended: 1979-2004 |
|---|---|---|---|
| Effect of Unemployment Rate Around | Grad. Year u-rate | Age 18 u-rate | Age 18 u-rate |
| Graduation on Medicaid Coverage | (1) | (2) | (3) |
| Effect at Year 1 | 0.003 | 0.005 | 0.006 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 2 | 0.005 | 0.006 | 0.004 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 3 | 0.003 | 0.003 | 0.004 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 4 | 0.003 | 0.004 | 0.003 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 5 | 0.003 | 0.003 | 0.003 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 6 | 0.002 | 0.001 | 0.002 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 7 | 0.002 | 0.002 | 0.001 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 8 | 0.002 | 0.000 | 0.001 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 9 | 0.000 | 0.001 | 0.002 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 10 | -0.001 | 0.000 | 0.002 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 11 | -0.001 | -0.001 | 0.000 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 12 | 0.002 | -0.001 | -0.001 |
| | (0.001) | (0.001) | (0.001) |
| Effect at Year 13 | 0.000 | -0.002 | -0.002 |
| | (0.002) | (0.001) | (0.001) |
| Effect at Year 14 | 0.000 | 0.000 | -0.001 |
| | (0.002) | (0.002) | (0.001) |
| Effect at Year 15 | 0.001 | 0.001 | -0.001 |
| | (0.002) | (0.002) | (0.001) |
| State FE, year FE | ✓ | ✓ | ✓ |
| Graduation cohort FE | ✓ | | |
| Years since graduation FE | ✓ | | |
| Birth cohort FE | | ✓ | ✓ |
| Age FE | | ✓ | ✓ |
| Number of observations (collapsed) | 5,814 | 5,814 | 7,880 |
| Underlying observations (individual level) | 378,611 | 262,328 | 612,714 |
| $R^2$ | 0.19 | 0.34 | 0.32 |

Notes: The dependent variable is the probability to be covered by Medicaid. Further comments as in Table 2. For a graphical presentation see Figure 7.

## 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, 1979-1991 | Male (1) | Female (2) | White (3) | Non-white (3) |
| Effect at Age 19 | -0.034 | -0.018 | -0.025 | -0.044 |
| | (0.007) | (0.007) | (0.006) | (0.016) |
| Effect at Age 20 | -0.041 | -0.034 | -0.036 | -0.048 |
| | (0.006) | (0.007) | (0.005) | (0.015) |
| Effect at Age 21 | -0.033 | -0.017 | -0.023 | -0.045 |
| | (0.006) | (0.007) | (0.005) | (0.014) |
| Effect at Age 22 | -0.028 | -0.026 | -0.023 | -0.057 |
| | (0.006) | (0.006) | (0.005) | (0.012) |
| Effect at Age 23 | -0.024 | -0.027 | -0.023 | -0.052 |
| | (0.006) | (0.006) | (0.006) | (0.013) |
| Effect at Age 24 | -0.015 | -0.010 | -0.011 | -0.019 |
| | (0.006) | (0.008) | (0.005) | (0.016) |
| Effect at Age 25 | -0.024 | -0.004 | -0.013 | -0.035 |
| | (0.006) | (0.007) | (0.005) | (0.014) |
| Effect at Age 26 | -0.020 | -0.012 | -0.016 | -0.038 |
| | (0.005) | (0.007) | (0.005) | (0.014) |
| Effect at Age 27 | -0.016 | -0.005 | -0.012 | -0.013 |
| | (0.006) | (0.009) | (0.006) | (0.015) |
| Effect at Age 28 | -0.014 | -0.006 | -0.008 | -0.036 |
| | (0.008) | (0.011) | (0.007) | (0.018) |
| Effect at Age 29 | 0.002 | 0.001 | 0.003 | -0.028 |
| | (0.009) | (0.011) | (0.008) | (0.022) |
| Effect at Age 30 | -0.003 | 0.003 | 0.002 | -0.017 |
| | (0.009) | (0.011) | (0.007) | (0.022) |
| Effect at Age 31 | 0.022 | -0.011 | -0.003 | 0.021 |
| | (0.009) | (0.013) | (0.008) | (0.026) |
| Effect at Age 32 | 0.036 | -0.019 | 0.000 | 0.048 |
| | (0.012) | (0.018) | (0.012) | (0.029) |
| Effect at Age 33 | -0.012 | 0.022 | 0.009 | -0.001 |
| | (0.016) | (0.022) | (0.015) | (0.030) |
| State FE, year FE | ✓ | ✓ | ✓ | ✓ |
| Birth cohort FE | ✓ | ✓ | ✓ | ✓ |
| Age FE | ✓ | ✓ | ✓ | ✓ |
| Number of observations (collapsed) | 5,962 | 5,962 | 5,961 | 4,814 |
| Underlying observations (individual level) | 128,733 | 136,521 | 227,089 | 37,767 |
| $R^2$ | 0.80 | 0.69 | 0.84 | 0.55 |

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 19-23 are displayed. 'FE' refers to 'fixed effects', i.e. a set of dummies for the different values of the respective variable. For a graphical presentation see Figure 9.

## Table 6: Regressions of Employer Health Insurance Coverage on Age 18 Unemployment Rate by Gender and Race

| | Sample | | | |
|---|---|---|---|---|
| Effect of Age 18 Unemployment Rate on | Male | Female | White | Non-white |
| Employer Health Insurance, 1979-1991 | (1) | (2) | (3) | (3) |
| Effect at Age 19 | -0.004 | 0.000 | -0.002 | -0.002 |
| | (0.002) | (0.002) | (0.002) | (0.003) |
| Effect at Age 20 | -0.008 | -0.004 | -0.005 | -0.011 |
| | (0.002) | (0.002) | (0.002) | (0.003) |
| Effect at Age 21 | -0.010 | -0.006 | -0.006 | -0.014 |
| | (0.002) | (0.002) | (0.002) | (0.004) |
| Effect at Age 22 | -0.008 | -0.007 | -0.006 | -0.012 |
| | (0.003) | (0.003) | (0.002) | (0.004) |
| Effect at Age 23 | -0.007 | -0.008 | -0.007 | -0.008 |
| | (0.002) | (0.002) | (0.002) | (0.004) |
| Effect at Age 24 | -0.001 | -0.003 | -0.002 | 0.001 |
| | (0.002) | (0.003) | (0.002) | (0.005) |
| Effect at Age 25 | -0.002 | -0.004 | -0.003 | -0.004 |
| | (0.003) | (0.003) | (0.002) | (0.005) |
| Effect at Age 26 | -0.003 | 0.000 | -0.001 | -0.005 |
| | (0.003) | (0.002) | (0.002) | (0.005) |
| Effect at Age 27 | -0.003 | -0.004 | -0.004 | -0.001 |
| | (0.003) | (0.003) | (0.002) | (0.006) |
| Effect at Age 28 | -0.002 | -0.005 | -0.004 | -0.010 |
| | (0.004) | (0.004) | (0.003) | (0.007) |
| Effect at Age 29 | -0.003 | -0.003 | -0.004 | 0.000 |
| | (0.004) | (0.004) | (0.003) | (0.008) |
| Effect at Age 30 | -0.003 | -0.003 | -0.007 | 0.015 |
| | (0.005) | (0.004) | (0.003) | (0.008) |
| Effect at Age 31 | -0.002 | -0.002 | -0.006 | 0.008 |
| | (0.005) | (0.006) | (0.005) | (0.011) |
| Effect at Age 32 | -0.006 | -0.011 | -0.009 | -0.015 |
| | (0.006) | (0.006) | (0.005) | (0.015) |
| Effect at Age 33 | -0.006 | 0.012 | 0.002 | -0.002 |
| | (0.009) | (0.008) | (0.005) | (0.022) |
| State FE, year FE | ✓ | ✓ | ✓ | ✓ |
| Birth cohort FE | ✓ | ✓ | ✓ | ✓ |
| Age FE | ✓ | ✓ | ✓ | ✓ |
| Number of observations (collapsed) | 5,813 | 5,813 | 5,812 | 5,006 |
| Underlying observations (individual level) | 127,367 | 134,961 | 224,561 | 37,767 |
| $R^2$ | 0.68 | 0.54 | 0.72 | 0.45 |

Notes: The dependent variable is the probability to be covered by employer-provided health care. Further comments as in Table 5. For a graphical presentation see Figure 14.

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

| | Sample | | | |
|---|---|---|---|---|
| Effect of Age 18 Unemployment Rate on Medicaid Coverage, 1979-1991 | Male (1) | Female (2) | White (3) | Non-white (3) |
| Effect at Age 19 | 0.005 | 0.005 | 0.002 | 0.016 |
| | (0.001) | (0.002) | (0.001) | (0.004) |
| Effect at Age 20 | 0.005 | 0.006 | 0.003 | 0.019 |
| | (0.001) | (0.002) | (0.001) | (0.004) |
| Effect at Age 21 | 0.000 | 0.005 | 0.001 | 0.009 |
| | (0.001) | (0.002) | (0.001) | (0.004) |
| Effect at Age 22 | 0.003 | 0.005 | 0.003 | 0.013 |
| | (0.001) | (0.002) | (0.001) | (0.004) |
| Effect at Age 23 | 0.000 | 0.006 | 0.002 | 0.012 |
| | (0.001) | (0.002) | (0.001) | (0.003) |
| Effect at Age 24 | -0.001 | 0.002 | 0.001 | 0.002 |
| | (0.001) | (0.002) | (0.001) | (0.004) |
| Effect at Age 25 | 0.000 | 0.004 | 0.001 | 0.009 |
| | (0.001) | (0.002) | (0.001) | (0.004) |
| Effect at Age 26 | 0.000 | 0.000 | 0.001 | 0.002 |
| | (0.001) | (0.002) | (0.001) | (0.005) |
| Effect at Age 27 | 0.000 | 0.002 | 0.001 | 0.010 |
| | (0.001) | (0.002) | (0.001) | (0.005) |
| Effect at Age 28 | 0.000 | 0.000 | -0.001 | 0.014 |
| | (0.001) | (0.002) | (0.001) | (0.005) |
| Effect at Age 29 | 0.000 | -0.002 | -0.002 | 0.011 |
| | (0.001) | (0.002) | (0.001) | (0.005) |
| Effect at Age 30 | 0.000 | -0.001 | 0.000 | -0.001 |
| | (0.001) | (0.002) | (0.001) | (0.006) |
| Effect at Age 31 | -0.001 | -0.004 | -0.002 | -0.001 |
| | (0.002) | (0.002) | (0.001) | (0.008) |
| Effect at Age 32 | 0.000 | 0.000 | 0.000 | 0.007 |
| | (0.002) | (0.003) | (0.002) | (0.008) |
| Effect at Age 33 | -0.001 | 0.003 | 0.001 | 0.004 |
| | (0.002) | (0.004) | (0.002) | (0.008) |
| State FE, year FE | ✓ | ✓ | ✓ | ✓ |
| Birth cohort FE | ✓ | ✓ | ✓ | ✓ |
| Age FE | ✓ | ✓ | ✓ | ✓ |
| Number of observations (collapsed) | 5,813 | 5,813 | 5,812 | 5,006 |
| Underlying observations (individual level) | 127,367 | 134,961 | 224,561 | 37,767 |
| $R^2$ | 0.26 | 0.25 | 0.31 | 0.22 |

Notes: The dependent variable is the probability to be covered by Medicaid. Further comments as in Table 5. For a graphical presentation see Figure 15.

## Table 8: Regressions of Deaths on Age 18 Unemployment Rate

| | Included Years | | |
|---|---|---|---|
| Effect of Age 18 Unemployment Rate on | 1979-1991 | 1979-1995 | 1979-2004 |
| Number of Deaths per Cohort | (1) | (2) | (3) |
| Effect at Age 19 | 0.137 | -2.596 | -2.507 |
| | (0.757) | (1.059) | (1.322) |
| Effect at Age 20 | 0.176 | -1.966 | -2.744 |
| | (0.710) | (0.993) | (1.322) |
| Effect at Age 21 | 0.193 | -1.304 | -2.810 |
| | (0.663) | (0.816) | (1.165) |
| Effect at Age 22 | -0.275 | -0.337 | -3.115 |
| | (0.689) | (0.817) | (1.201) |
| Effect at Age 23 | -1.084 | -0.558 | -3.536 |
| | (0.657) | (0.777) | (1.146) |
| Effect at Age 24 | -1.222 | -0.493 | -3.784 |
| | (0.698) | (0.790) | (1.111) |
| Effect at Age 25 | 0.211 | 0.367 | -3.397 |
| | (0.653) | (0.793) | (1.083) |
| Effect at Age 26 | -0.632 | -0.289 | -3.458 |
| | (0.757) | (0.823) | (1.077) |
| Effect at Age 27 | 0.707 | 0.111 | -3.131 |
| | (1.340) | (0.987) | (1.141) |
| Effect at Age 28 | 4.567 | 1.478 | -1.320 |
| | (1.737) | (1.080) | (1.291) |
| Effect at Age 29 | 7.606 | 1.817 | -0.409 |
| | (3.427) | (1.520) | (1.554) |
| Effect at Age 30 | 12.714 | 3.465 | 1.179 |
| | (2.840) | (1.626) | (1.582) |
| Effect at Age 31 | 14.603 | 5.687 | 2.413 |
| | (3.980) | (2.169) | (1.648) |
| Effect at Age 32 | 18.458 | 10.316 | 2.703 |
| | (5.094) | (2.991) | (1.723) |
| Effect at Age 33 | 22.040 | 15.923 | 2.303 |
| | (9.624) | (4.429) | (1.939) |
| State FE, year FE | ✓ | ✓ | ✓ |
| Birth cohort FE | ✓ | ✓ | ✓ |
| Age FE | ✓ | ✓ | ✓ |
| Number of observations (collapsed) | 5,928 | 8,987 | 15,872 |
| Underlying population | 468,440,679 | 687,489,342 | 1,146,700,000 |
| $R^2$ | 0.97 | 0.95 | 0.92 |

Notes: Coefficients from regressions of the number of deaths per cohorton on the age 18 unemployment rate interacted with dummies for age 19-23 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 17, 19 and 20.

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

| Effect of Age 18 Unemployment Rate on AIDS and non-AIDS Deaths, 1979-1991 | AIDS Deaths (1) | none-AIDS Deaths (2) |
|---|---|---|
| Effect at Age 19 | 0.208 | -0.071 |
| | (0.276) | (0.659) |
| Effect at Age 20 | 0.323 | -0.147 |
| | (0.281) | (0.638) |
| Effect at Age 21 | 0.283 | -0.090 |
| | (0.295) | (0.606) |
| Effect at Age 22 | 0.238 | -0.513 |
| | (0.301) | (0.652) |
| Effect at Age 23 | 0.014 | -1.098 |
| | (0.309) | (0.550) |
| Effect at Age 24 | 0.016 | -1.238 |
| | (0.317) | (0.642) |
| Effect at Age 25 | 0.159 | 0.052 |
| | (0.335) | (0.519) |
| Effect at Age 26 | 0.074 | -0.706 |
| | (0.388) | (0.668) |
| Effect at Age 27 | 0.687 | 0.020 |
| | (0.557) | (1.000) |
| Effect at Age 28 | 2.617 | 1.951 |
| | (0.989) | (1.081) |
| Effect at Age 29 | 5.800 | 1.806 |
| | (1.932) | (1.893) |
| Effect at Age 30 | 9.217 | 3.497 |
| | (2.046) | (1.194) |
| Effect at Age 31 | 12.745 | 1.858 |
| | (3.040) | (1.650) |
| Effect at Age 32 | 14.164 | 4.293 |
| | (3.868) | (1.841) |
| Effect at Age 33 | 16.094 | 5.946 |
| | (6.677) | (3.455) |
| State FE, year FE | ✓ | ✓ |
| Birth cohort FE | ✓ | ✓ |
| Age FE | ✓ | ✓ |
| Number of observations (collapsed) | 5,928 | 5,928 |
| Underlying population | 468,440,679 | 468,440,679 |
| $R^2$ | 0.66 | 0.98 |

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 21.

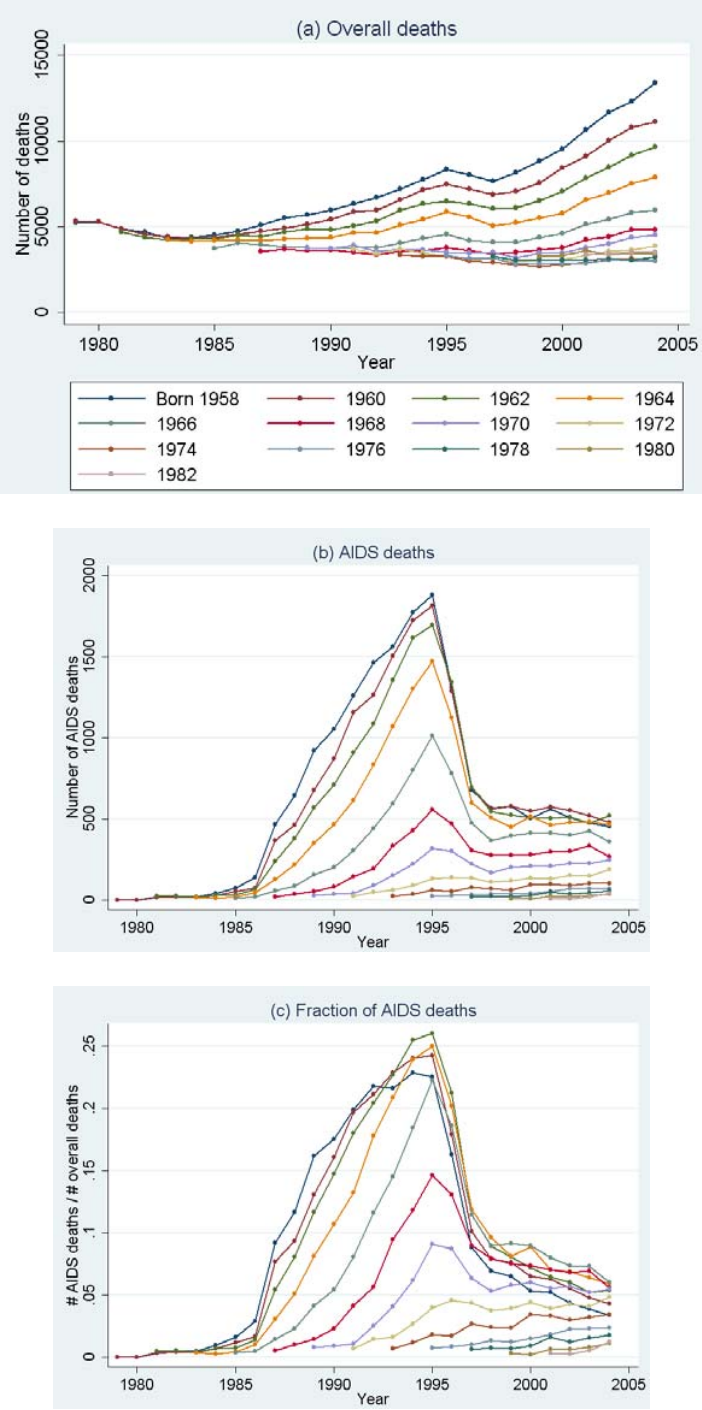
**Table 10: Regressions of AIDS/Non-AIDS Death Ratios on Age 18 Unemployment Rate by Gender and Race**

| | Sample | | | |
|---|---|---|---|---|
| Effect of Age 18 Unemployment Rate on | Male | Female | White | Non-white |
| AIDS Deaths Ratio, 1979-1991 | (1) | (2) | (3) | (3) |
| Effect at Age 19 | 0.002 | 0.001 | 0.001 | 0.003 |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Effect at Age 20 | 0.002 | 0.002 | 0.001 | 0.004 |
| | (0.001) | (0.001) | (0.000) | (0.001) |
| Effect at Age 21 | 0.002 | 0.001 | 0.001 | 0.003 |
| | (0.001) | (0.001) | (0.000) | (0.001) |
| Effect at Age 22 | 0.001 | 0.000 | 0.000 | 0.002 |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Effect at Age 23 | 0.000 | -0.001 | -0.001 | -0.002 |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Effect at Age 24 | -0.001 | -0.001 | -0.001 | 0.000 |
| | (0.001) | (0.001) | (0.001) | (0.002) |
| Effect at Age 25 | 0.001 | -0.002 | 0.000 | 0.000 |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Effect at Age 26 | 0.001 | -0.002 | 0.000 | 0.002 |
| | (0.001) | (0.001) | (0.001) | (0.003) |
| Effect at Age 27 | 0.000 | -0.001 | -0.001 | 0.006 |
| | (0.001) | (0.002) | (0.001) | (0.002) |
| Effect at Age 28 | 0.004 | 0.003 | 0.002 | 0.008 |
| | (0.002) | (0.002) | (0.002) | (0.003) |
| Effect at Age 29 | 0.010 | 0.013 | 0.008 | 0.018 |
| | (0.003) | (0.003) | (0.003) | (0.006) |
| Effect at Age 30 | 0.013 | 0.015 | 0.011 | 0.015 |
| | (0.003) | (0.004) | (0.003) | (0.006) |
| Effect at Age 31 | 0.018 | 0.020 | 0.015 | 0.037 |
| | (0.004) | (0.006) | (0.004) | (0.005) |
| Effect at Age 32 | 0.018 | 0.021 | 0.015 | 0.027 |
| | (0.004) | (0.005) | (0.004) | (0.008) |
| Effect at Age 33 | 0.019 | 0.021 | 0.016 | 0.031 |
| | (0.006) | (0.010) | (0.006) | (0.010) |
| State FE, year FE | ✓ | ✓ | ✓ | ✓ |
| Birth cohort FE | ✓ | ✓ | ✓ | ✓ |
| Age FE | ✓ | ✓ | ✓ | ✓ |
| Number of observations (collapsed) | 5,927 | 5,841 | 5,927 | 5,300 |
| $R^2$ | 0.83 | 0.44 | 0.78 | 0.59 |

Notes: Coefficients from separate regressions for males, females, whites and non-whites are displayed. Further comments as in Table 8. For a graphical presentation see Figures 22.

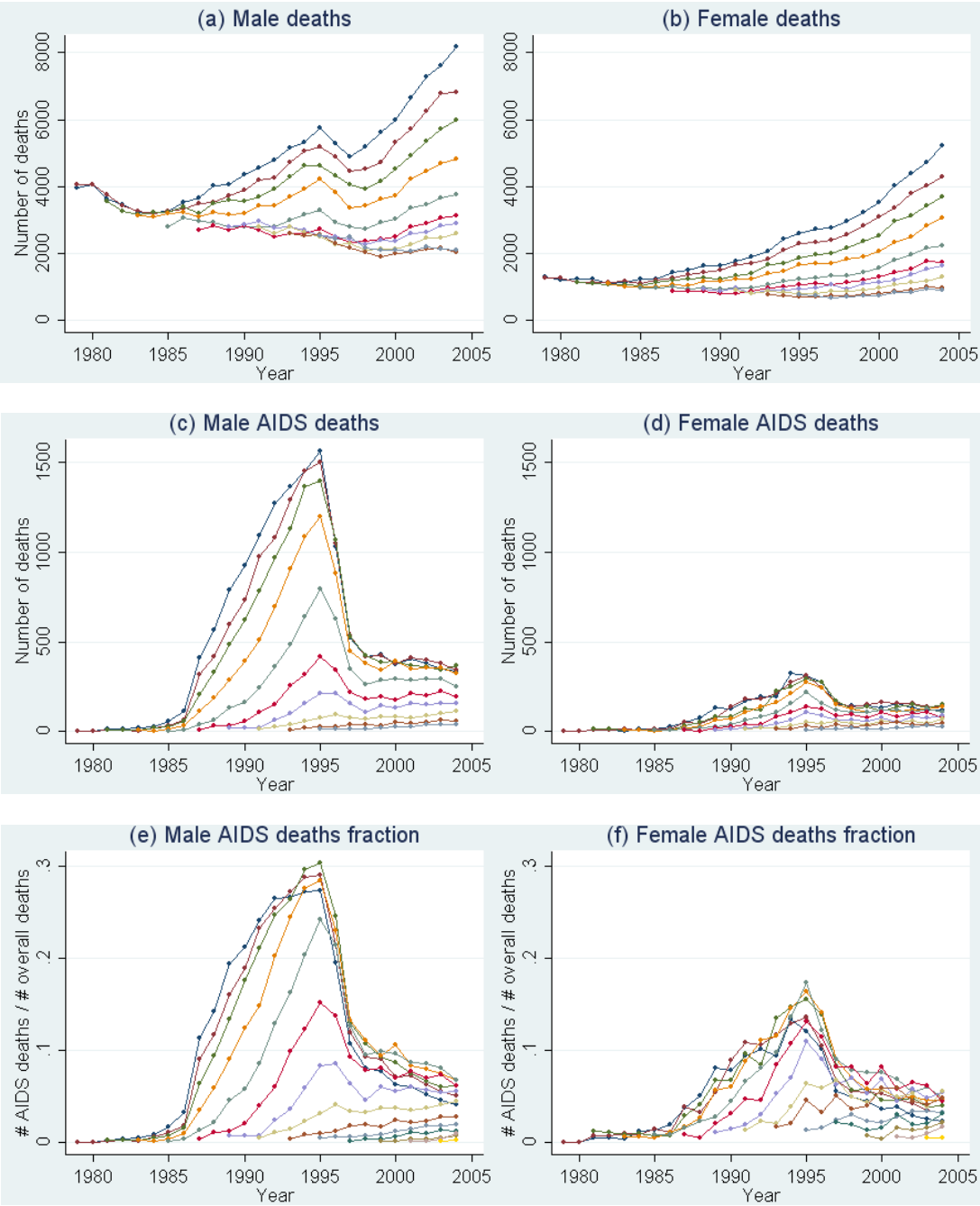
# Appendix

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



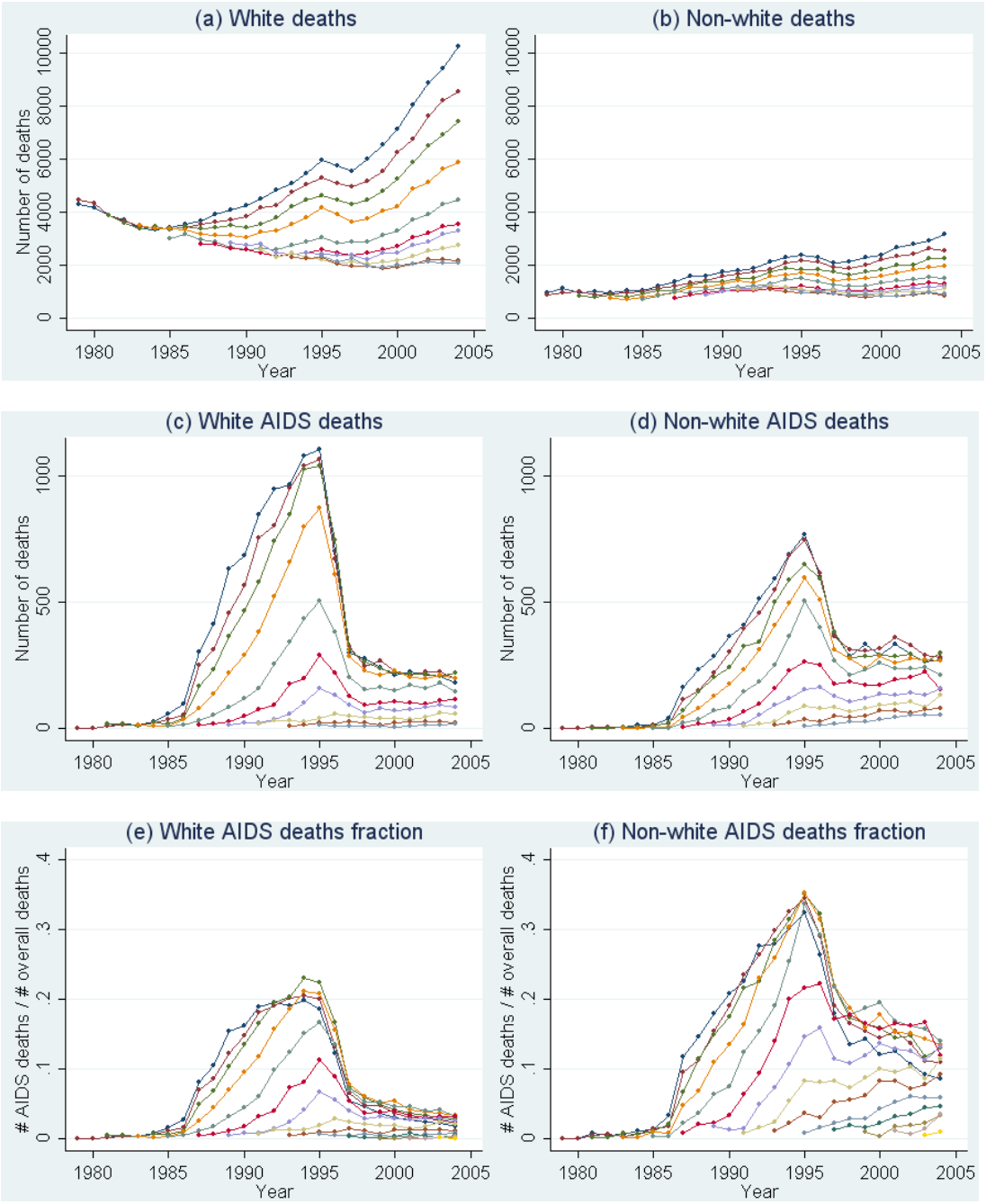

Notes: Figures present the universe of overall deaths, of AIDS deaths, and of AIDS death fractions for the sample cohorts, taken from the Vital Statistics. For panels (b) and (c) the same legend applies as in panel (a).

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



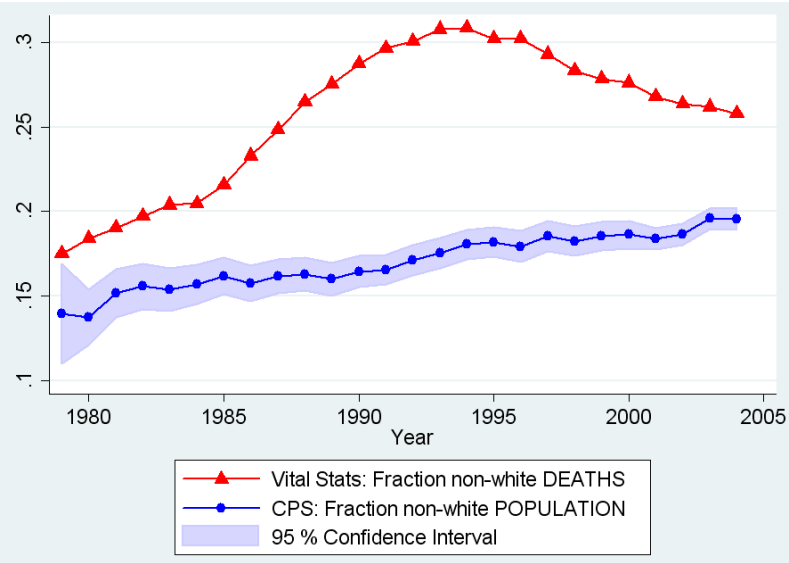

Notes: Figures present - separately for males and females - the universe of overall deaths and of AIDS deaths and AIDS death fractions for the sample cohorts, taken from the Vital Statistics. The same legend as in Figure A1 (a) applies.

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



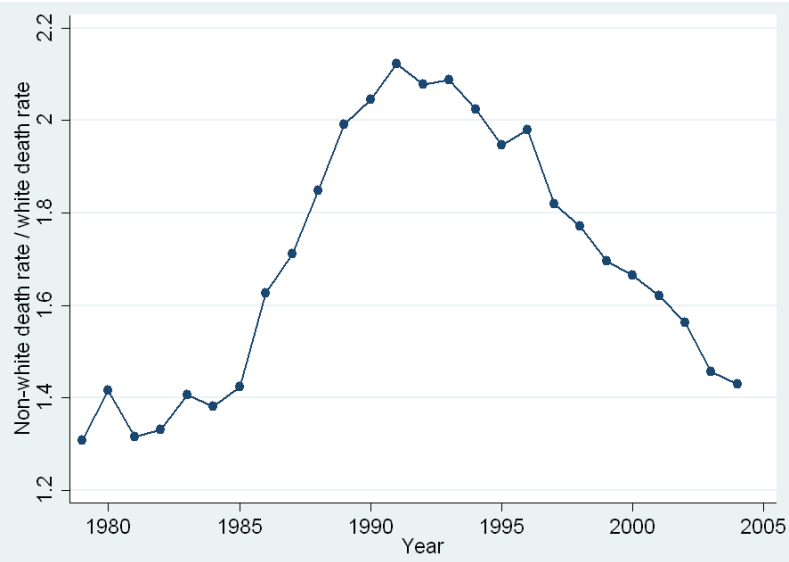

Notes: Figures present - separately for whites and non-whites - the universe of overall deaths and of AIDS deaths and AIDS death fractions for the sample cohorts, taken from the Vital Statistics. The same legend as in Figure A1 (a) applies.

**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. The fraction of non-white population is estimated from the CPS. 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]