Unlucky Cohorts: Estimating the Long-Term Effects of Entering the Labor Market in a Recession in Large Cross-Sectional Data Sets

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This paper studies the differential persistent effects of initial economic conditions for labor market entrants in the United States from 1976 to 2015 by education, gender, and race using labor force survey data. We find persistent earnings and wage reductions, especially for less advantaged entrants, that increases in government support only partly offset. We confirm that the results are unaffected by selective migration and labor market entry by also using a double-weighted average unemployment rate at labor market entry for each birth cohort and state-of-birth cell based on average state migration rates and average cohort education rates from census data.

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I. Introduction

The first years after entering the labor market are typically a very productive period for young workers. During this period, young workers' wages grow rapidly, and they frequently switch toward better paying jobs.[1] At the same time, young workers are particularly vulnerable to adverse conditions in the labor market. For example, it is well known that young workers bear the brunt of recessions in terms of higher unemployment rates, partly because wages tend to fall most for those workers entering new jobs (e.g., Elsby, Shin, and Solon 2016). Economists and policy makers alike have long been concerned that interruptions of the initial process of career progression caused by recessions can have lasting consequences on earnings and other relevant outcomes, including health insurance coverage, health effects, and family formation (e.g., von Wachter 2012).

There is indeed increasing evidence from careful studies of college graduates that even temporary exposure to increased unemployment rates can lead to persistent earnings reductions. For example, using data from the National Longitudinal Study of Youth (NLSY), Kahn (2010) shows that college graduates entering the labor market during the deep recession in the early 1980s experienced reductions in earnings lasting up to 15 years. Oyer (2006, 2008) presents evidence on the persistent effect on career choice for MBAs and PhD economists. Oreopoulos, von Wachter, and Heisz (2012) show that college graduates in Canada suffer persistent earnings losses and that these losses are substantially larger for those graduates predicted to have low earnings to begin with.[2] Outcomes other than earnings appear to respond to initial labor market entry as well.[3] For example, based on the NLSY, Maclean (2013) finds that male entrants during the early 1980s recession have experienced long-lasting effects on self-reported health, which is consistent with findings based on mature workers that show that labor market shocks can have long-term effects on health, including mortality.[4]

These studies provide powerful evidence that the fear that recessions can have lasting repercussions for young workers is well founded. These findings are worrisome, in particular because new college graduates are typically not eligible for programs meant to buffer temporary earnings losses.

[1] For example, Topel and Ward (1992) and Murphy and Welch (1990).

[2] See also Altonji, Kahn, and Speer (2016) for analysis of differential effects of graduating in a recession by college majors.

[3] For example, Altonji, Kahn, and Speer (2016) analyze earnings and occupational choice of college graduates using multiple data sources, spanning a similar time period as we do; Giuliano and Spilimbergo (2014) analyze the effects on attitudes; Oreopoulos, von Wachter, and Heisz (2012) study employer characteristics.

[4] For example, Sullivan and von Wachter (2009) show that mature job losers suffer long-term increases in mortality rates. We explore the effect of entering the labor market in a recession on long-term mortality in a companion paper (Schwandt and von Wachter 2018).

By focusing on college graduates, the aforementioned papers have been able to provide empirical evidence that is highly compelling and that provides important proof-of-concept results establishing the significance of persistent effects of early labor market conditions. This is partly because they can exploit high-quality longitudinal data that allow measuring the state in which workers first entered the labor market, among others things, and partly because the date of entry and typical career progression is well defined for college graduates.

However, it is well known that less advantaged groups in the labor market, such as low-educated workers or minorities, experience much larger increases in unemployment during recessions (e.g., Hoynes, Miller, and Schaller 2012). These groups are thus at risk of suffering even larger longer-term effects than the highly educated workers studied in depth in the existing literature. At the same time, in contrast to college graduates, these workers are more likely to have access to the social safety net. Indeed, recent work from European countries suggests that entry conditions have a stronger effect on a range of outcomes, including self-reported health, for lower-educated individuals (e.g., Cutler, Huang, and Lleras-Muney 2015), despite the widespread prevalence of generous social support systems in these counties. Despite the concern that less advantaged individuals could fare worse, the effect of adverse labor market entry for these groups of workers has not been studied extensively in the literature, especially for the United States, where safety nets are less extensive. This is partly because typical longitudinal data do not have sufficient samples to study these groups and partly because career progression, and hence initial conditions, are harder to measure.[5]

To advance the literature, in this paper we examine the persistent effects of entering the labor market in a recession on a broad range of socioeconomic outcomes for all young workers who entered the labor market in the United States from 1976 to 2015. Our study includes college graduates but also focuses on groups not typically analyzed separately, such as women, nonwhites, and individuals with less than a college degree. To identify the effect of initial labor market conditions, we exploit year-to-year variation in unemployment rates in the state of labor market entry. To estimate these effects, we use several data sets with extensive coverage over time and large

[5] In an exception, Speer (2016) uses the NLSY to study the effects of initial labor market conditions for lower-educated men graduating chiefly in the early 1980s recession and finds results similar to ours. There is a separate body of literature on the scarring effects of individual labor market shocks for young workers, such as unemployment spells, occurring independently of macroeconomic conditions (e.g., Gardecki and Neumark 1998). While identification is difficult, von Wachter and Bender (2006) pursue an instrumental variable (IV) approach based on temporary condition at workers' first employer.

sample sizes: repeated cross sections from the Annual Social and Economic Supplement (ASEC) to the Current Population Survey (CPS), decennial census data, and American Community Survey (ACS) data.

Given that these data sets contain information on a large number of entry cohorts in the labor market, they allow us to generate the first estimates of entering the labor market in a recession for a typical young labor market entrant in the United States. Another key advantage is that the large sample sizes allow us to study with sufficient precision the effect for smaller groups, such as high school dropouts. Finally, the data contain information on a range of additional outcomes that have not been studied for young workers in the US labor market. This includes information about the role of the social insurance system for young workers, such as receipt of Medicaid and Supplemental Nutrition Assistance Program (SNAP) benefits (formerly known as food stamps), as well as measures of poverty.

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

Based on this approach, we obtain three key findings. First, for the full sample of labor market entrants in the United States from 1976 to 2015, we find that entering the labor market in times of high unemployment leads to a substantial initial effect on earnings. Consistent with findings in the previous literature, this effect fades gradually but persists for 10 years into workers' careers. Our findings imply that for a moderate recession that raises unemployment rates by 3 points, the loss on cumulated earnings is predicted to be on the order of 60% of a year of earnings. These effects are substantial and very robust to controls for selective migration or endogenous entry into the labor market. Further analysis suggests that the initial effect is due to both employment and wage reductions, whereas the longer-term effect is mainly due to persistent declines in wages.

[6] However, our analysis of lower-educated groups has to deal with the possibility that our approach inappropriately includes individuals whose education is in progress. We discuss this explicitly in the paper.

Second, we find that the effect on earnings varies considerably in the population. While all groups we studied experience persistent effects from adverse initial labor market conditions—including women, high school graduates, and those with some college—the effects are particularly large for two groups: nonwhites and high school dropouts. Although smaller samples lower the precision for these groups, the earnings losses are substantial. In examining the sources, we find these differences are partly driven by greater losses in employment, measured in terms of the number of weeks worked in the past year, for nonwhites and high school dropouts.

Third, we find that the US social insurance system provides a buffer for unlucky labor market entrants and that these effects are largest for those who suffer the greatest earnings losses. We find precisely estimated temporary but long-lasting increases in the probability of receiving SNAP benefits (formerly known as food stamps) for the full sample. The effects are present for both men and women and whites and nonwhites and are driven by a rise in the probability of receiving benefits among those with a high school degree and high school dropouts. As a result, the effect on household income—our measure of income available from all sources—is lower than the effect on annual earnings. However, the insurance provided is imperfect, and we find effects on poverty lasting up to 6 years among all groups except those with at least some college or more.

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

These findings extend the literature on persistent effects of temporary labor market conditions along several dimensions. The foregoing literature, especially on US youth, concentrated mainly on college graduates. Although there are some findings based on broader samples, this is the first study to comprehensively address and compare differences in the persistent effect of initial labor market conditions for labor market entrants in the United States.

Our results also provide useful information regarding the importance and effect of the social insurance system in buffering cyclical employment effects. Most of the focus in this area is on mature workers and in particular what sources of income are available for the long-term unemployed (e.g., Rothstein and Valletta 2017). But little is known about how the social insurance system helps labor market entrants weather weak economic condi-

tions. Young workers are typically not covered by unemployment insurance and are usually single, which excludes them from typical welfare programs, such as Temporary Assistance for Needy Families.

Beyond our substantive contributions to this literature, our paper provides a novel methodological approach with two key advantages that will be helpful to researchers studying this phenomenon who seek to go beyond smaller longitudinal data sets that may not have sufficient precision for this type of analysis. First, our approach allows bias from selection and measurement error due to endogenous migration and education decisions. Second, it allows for the harnessing of much larger cross-sectional data sets as long as information on state of birth is used. The approach constructs the relevant unemployment rate for each birth cohort and state-of-birth cell on which our main analysis is based by properly weighting and aggregating current state unemployment rates using average state migration rates and average cohort education rates.

Finally, our results provide a useful practical contribution to the literature by analyzing the persistent effects of the local environment during youth and early adulthood. These effects, among others, have received recent attention in studies of intergenerational transmission of income and the role of neighborhood effects based on rich longitudinal data (e.g., Chetty, Hendren, and Katz 2016; Chetty and Hendren 2018).[7] Our findings suggest that labor market mobility may be sufficiently low and idiosyncratic that the state of residence after entry into the labor market (or state of birth) approximates the characteristics of the initial location at the time of entry into the labor market sufficiently well on average. These findings confirm earlier results of Card and Krueger (1992), who find that mobility adjustments do not affect the results of the earnings effects of school characteristics, as well as the evidence from Autor et al. (2014), who shows that local trade shocks do not lead to significant migration to less affected areas.

The remainder of the paper is structured as follows. Section II describes our empirical approach, our data, and how we assess whether the cross-sectional data can be successfully used to estimate the long-term effect of initial conditions. Section III summarizes the effect of initial unemployment rates on the socioeconomic outcomes we study, including annual earnings, hourly wages, employment, program receipt, and health insurance coverage. Section IV concludes.

II. Empirical Approach and Data

We seek to extend the existing literature that focuses on college graduates by studying the effect of entering the labor market in a recession for more

[7] Another related literature analyzes the effects of local economic conditions on fertility (Currie and Schwandt 2014).

disadvantaged groups in the labor market. To do so, we use data from repeated cross sections in the March CPS, the decennial census, and the ACS. This approach has several advantages in our context. It allows us to work with much larger samples and hence enables us to study the responses of smaller subgroups, such as nonwhites or low-educated workers. The data cover a longer time period, thereby allowing us to analyze the effects of entering the labor market for all graduating cohorts from 1976 to 2015. This is the first paper to do so in the United States, and it is possible only because of the use of cross-sectional data. In addition, information in the March CPS data allow us to analyze additional outcomes that are particularly relevant for lower-income workers.

Working with cross-sectional data has drawbacks as well, all of which we address directly. An important limitation is that we know neither the actual state nor the exact timing of entry into the labor market. Moreover, both state and time of labor market entry might be endogenous if people respond to local recessions by moving into other states or by postponing graduation. In what follows, we will start with a hypothetical ideal regression that does not suffer from endogeneity or measurement issues. In Section II.B, we discuss how one can proxy for the state and year of graduation in the data given this ideal empirical model and which biases may arise from this specification. In Section II.C, we develop an approach that accounts for these biases in the census and ACS data.

A. The Ideal Regression

The ideal regression would relate different outcomes, such as labor income at different years of experience, to the economic conditions (ec_{i0}) an individual i faced at her or his labor market entry.

$$y_{i,t} = \alpha + \beta_e ec_{i0} + \gamma_e + \epsilon_{i,t}, \tag{1}$$

where γ_e are experience fixed effects. The coefficients β_e represent deviations from the typical experience profile resulting from differences in local labor market conditions. A causal interpretation of the coefficient estimates for β_e requires that the economic conditions at labor market entry are uncorrelated with other determinants of the respective outcome. This equation can be derived from economic models relating wages and employment to local labor market conditions (e.g., Blanchflower and Oswald 1994). To implement the equation, we have to choose a level aggregation for the appropriate labor market. For our analysis, we follow the majority of the literature and use annual state-level unemployment rates.

B. Mincerian Specification

Our main analysis is based on CPS data that provide detailed individual-level information on labor market outcomes. However, the CPS does not

report the year when an individual completed her education or the state where she entered the labor market. We use as a proxy the "Mincerian" graduation year, that is, the sum of the year of birth, plus 6, plus the years of reported education. The state of labor market entry is proxied by the state of current residence, which is the only state identifier reported in the CPS.

For our baseline specification, we follow the literature (e.g., Oreopoulos, von Wachter, and Heisz 2012) and work with a cell-based model that aggregates the outcome at the level of current state of residence (s), year of graduation (g), calendar year (t), and education groups (d). Working with the cell-level data is sufficient because we do not use control variables varying at the individual level, and it allows us to work close to our main source of variation coming from local unemployment rates. We regress the average cell-level outcome on the relevant initial unemployment rate and control for year of graduation, state, and year fixed effects:

$$\bar{y}_{s,g,t,d} = \alpha + \beta_e u_{s,g} + \gamma_e + \lambda_s + \delta_g + \theta_t + \pi_d + \epsilon_{s,g,t}, \tag{2}$$

where $u_{s,g}$ is the unemployment rate in the state of current residence s at the Mincerian year of graduation g.[8] The term e refers to years of potential experience (years since graduation), and t refers to the calendar year. We additionally include education group fixed effects π_d. In contrast to previous work, our analysis includes all educational groups in one specification given that state cohort–level variation in educational attainment could be a confounding factor. Notice that we do not include the current state unemployment rate in our main results; therefore, β_e captures the effect of graduating in a recession, given the regular subsequent evolution of the local labor market conditions.[9] To properly represent population-level relationships, the cell-level observations are weighted by the corresponding cell sizes. Standard errors are clustered at the level of graduation year by state to account for cohort-specific serial correlation in labor market outcomes.

Given the included fixed effects, the coefficient vector β_e captures deviations from the typical experience profiles related to cohort state–specific variation in the unemployment rate at labor market entry that are uncorre-

[8] For ease of reference, in some cases we will refer to the implied year of entry as a graduation cohort (even though in some cases individuals do not literally graduate) and to the unemployment rate in the implied year of entry as the graduation unemployment rate. Similarly, if we refer to "state" without further clarification, the state of current residence is intended.

[9] The effect of the initial unemployment rate consists of its own direct effect plus the weighted effect of subsequent unemployment rates correlated with it (for a more detailed discussion, see Oreopoulos, von Wachter, and Heisz 2012). In comparing two cohorts with different initial conditions, this captures the full difference in lifetime earnings due to adverse labor market entry. Results controlling for the current unemployment rate are shown in the appendix (Sec. XI).

lated with contemporaneous nationwide shocks. However, the specification does not account for cohort state–specific variation driven by endogenous graduation timing and migration that might bias our estimates.

1. Endogenous Graduation Timing

Our baseline specification treats the time of labor market entry (proxied by years of education plus 6) as exogenous. But people might prolong their educational attainment in order to avoid unfavorable conditions at entry or end their education prematurely in order to benefit from good labor market conditions. Such endogenous timing of labor market entry attenuates our estimates toward zero if it is uniformly distributed among new labor market entrants. If there is selection into timing, the bias can go either way. For example, if those with higher potential earnings are better in timing their labor market entry, then we would tend to overstate the effects of initial labor market conditions.[10]

2. Endogenous Migration Before and After Graduation

In the CPS data, we proxy for the state of graduation using the current state of residence. However, in response to a local recession around the time of labor market entry, people might migrate into other states that are less affected. Such directed migration would lead to an attenuation bias in our data, as the migrants from poorly performing states would be erroneously assigned the better economic conditions in their new state of residence. If there is selection in who tends to leave in response to adverse economic conditions, the bias could go either way. We tested for such selection effects with balancing regressions that use the racial or gender composition as a dependent variable in Section IV of the appendix (available online) and found numerically small effects (Pei, Pischke, and Schwandt 2018).

3. Undirected Migration after Graduation

People might migrate independently of local labor market circumstances ("undirected" migration). The implied mismeasurement of initial labor market conditions would lead to attenuation bias too, although it would be less strong than in the case of endogenous migration. However, in both cases the bias worsens over time as the share of migrants accumulates within graduation cohorts and the current state of residence becomes an increasingly poor proxy for the state of graduation.

[10] We can test for the presence of endogenous timing by regressing a cohort's share of high school and college graduates on the unemployment rate at age 18. Selection into timing can additionally be explored by looking at the racial or gender composition of these graduation cohorts. These results are shown in the appendix (Secs. III and IV, respectively).

C. Double-Weighted Specification

We explore the role of these potential biases in the decennial census and ACS data by comparing the Mincerian specification we estimate based on CPS data to a double-weighted specification that is not affected by endogenous timing or migration. The census and ACS report not only the state of residence but also individuals' state of birth. We use this information to construct a proxy for the graduation year unemployment rate that relies on individuals' state of birth and year of birth. Because these characteristics are fixed, they cannot be affected by labor market conditions around graduation.[11]

In particular, we separately estimate average migration rates at different ages and education shares, which are then used to construct a double-weighted average unemployment rate for each graduation year.[12] This double-weighted measure provides a proxy for the typical exposure of a cohort to economic conditions across the United States and across different potential graduation years that is independent of a cohort's actual migration or graduation timing.[13]

To implement this approach, we first estimate migration shares $m^A_{b,s}$ as the average share of cohorts in our sample born in state b that live in state s at ages $A = 16$, 18, 20, and 22. Note that we use only state-specific *average* migration rates (i.e., state fixed effects in the underlying regression model of individual-level migration indicators) rather than migration rates of a specific birth cohort (state cohort fixed effects), which could be driven by an endogenous response to contemporaneous labor market conditions. Next, we estimate average graduation shares $e^A_{b,c}$, indicating the share of sample cohorts born in state b in year c who graduate at age $A = 16$, 18, 20, and ≥22. To predict graduation shares, we regress cohort state–specific shares on state fixed effects and countrywide cohort fixed effects. We then obtain $e^A_{b,c}$ by adding state and cohort fixed effects, such that, again, we take out the potentially endogenous cohort-by-state variation. The double-

[11] This approach is based on synthetic cohorts (Deaton 1985) and a further development of Currie and Schwandt (2014), who link maternal life cycles to unemployment rates using mothers' own state and year of birth.

[12] The underlying assumption is that migration rates are similar across education groups. While this is not borne out in typical studies of migration, given low average migration rates it makes little difference in our analysis.

[13] To account for endogenous migration, we could simply match people to the graduation year unemployment rate in their state of birth. However, this would imply a mismatch for those who migrated before graduation, leading to attenuation bias. For example, in the 2000 census about 20% of 18-year-olds live outside their state of birth. This attenuation is likely to be more dramatic than the one caused by random migration after graduation because migration rates are much higher during the first 2 decades of people's lives than during the following 2 decades.

weighted (DW) average graduation year unemployment rate is then given by[14]

$$u_{b,c}^{\mathrm{DW}} = \sum_{A} e_{b,c}^{A} \sum_{s=1}^{50} m_{b,s}^{A} u_{s,c+A}. \tag{3}$$

Because we are averaging across both locations and graduation years, this adjustment procedure reduces the amount of variation available for the estimation of our effects of interest. As a consequence, coefficients based on the double-weighted unemployment rate will be estimated less precisely. More important, however, such estimates will be free of potential bias.

Endogenous migration or timing in response to a local recession are not contained in the double-weighted unemployment rate, as it is constructed using only state averages over time and national averages across periods but not state-by-period variation. Moreover, because the cohorts are assigned their graduation state on the basis of their state of birth—a fixed characteristic— the double-weighted unemployment rate is not affected by state cohort–specific migration after graduation.[15]

We regress earnings and income in the census and ACS data on the double-weighted unemployment rate using the following specification:

$$\bar{y}_{b,c,a} = \alpha + \beta_a u_{b,c}^{\mathrm{DW}} + \gamma_a + \lambda_b + \delta_c + \theta_t + \epsilon_{c,b,a}. \tag{4}$$

Because the double-weighted unemployment rate is predicted at the cohort level, this approach requires collapsing the data by state and year of birth instead of state and year of graduation. This means we restrict our sample to US natives and track effects over cohorts' age rather than experience profiles. The indices b, c, a, and t refer to the birth state, birth year, age, and calendar year; hence, γ, λ, δ, and θ are the coefficients on a full set of age, birth state, birth cohort, and calendar year fixed effects, respectively.

Our main approach is to compare the results of our main specification in equation (2) based on the CPS and the results in model (4) based on the decennial census and the ACS. If the results are similar, this indicates that migration and timing of graduation are not problems in our sample, and we

[14] For example, assume that cohorts born in California have a typical out-migration to Nevada of 20% by age 18 and of 25% by age 22. Also assume that 60% get 12 years of education, while the remaining 40% getting 16 years of education. High school and college graduates for, say, the 1980 California-born cohort would enter the labor market in 1998 and 2002, respectively. The double-weighted average graduation year unemployment rate for the 1980 California-born cohort would then be $0.8 \times 0.6 \times u_{1998}^{\mathrm{CA}} + 0.2 \times 0.6 \times u_{1998}^{\mathrm{NV}} + 0.75 \times 0.4 \times u_{2002}^{\mathrm{CA}} + 0.25 \times 0.4 \times u_{2002}^{\mathrm{NV}}$.

[15] In contrast, the actual mean unemployment rate at entry into the labor market for a given birth cohort (c) and state of birth (b) would depend on cohort-specific mobility and graduation rates (indicated by an asterisk), potentially leading to biases if migration responds to initial economic conditions: $E(u_{s,g}|b,c) = \Sigma_A e_{b,c}^{*A} \Sigma_{s=1}^{50} m_{b,s,c}^{*A} u_{s,c+A}$.

proceed with equation (2), a specification that can be used in the rich CPS data.

An alternative approach is to use the double-weighted unemployment rate as an instrument for the actual endogenous unemployment rate a cohort faces at graduation proxied by the Mincerian rate in equation (2). We provide the two-stage least-squares (2SLS) results from such IV regressions, which include the full set of fixed effects contained in both equations (2) and (4). Since the estimates of equation (2) are simply the reduced-form estimates, the 2SLS effectively rescales our main results by the regression coefficients of the first-stage regression of the Mincerian on the double-weighted unemployment rate.

Figure 1 plots the Mincerian and double-weighted graduation year unemployment rates across our sample cohorts for four large states. Since

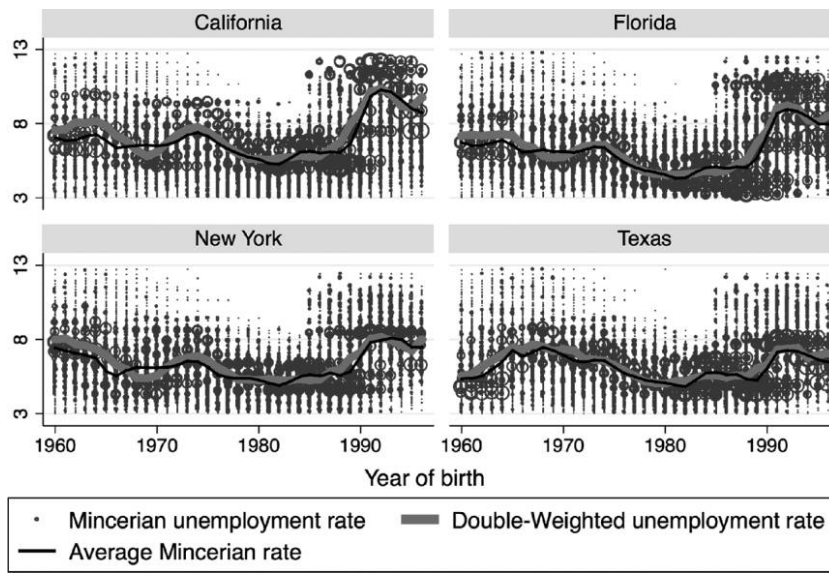

FIG. 1.—Mincerian and double-weighted graduation year unemployment rates across four states. The Mincerian graduation year unemployment rate (circles) refers to the unemployment rate in the current state of residence at the Mincerian year of graduation (the sum of the year of birth, plus 6, plus the years of reported education). The double-weighted graduation year unemployment rate (gray line) refers to the average unemployment rate across cohorts' typical graduation ages and across the states to which cohorts typically migrate before graduation. See Sections II.B and II.C for further details. There is only one double-weighted unemployment rate for each birth cohort but several Mincerian rates given different graduation times and migration to different states. Rates above 13 and below 3 are omitted to preserve a transparent scale. A color version of this figure is available online.

the double-weighted rate is constructed at the level of birth year and birthplace, there is only one double-weighted rate for each birth cohort in a given state (thick solid gray line). However, there is a large number of different Mincerian rates for each birth cohort (circles), due to labor market entry at different ages and migration across states. The thin solid black line shows the average Mincerian rate for each cohort, weighted by the size of each graduation age × state cell. The key difference between the average Mincerian and the average double-weighted rate is that the latter accounts for endogenous migration and graduation timing. That the two solid lines follow each other closely suggests that the bias arising from such endogenous responses in the simple Mincerian specification are unlikely to be very large.[16]

D. Sample Restrictions and Summary Statistics

State-level unemployment rates are available from the Bureau of Labor Statistics only since 1976. Therefore, we exclude individuals who graduated before 1976 when using the actual graduation year and individuals who were born before 1960—that is, age 16 before 1976–when using the double-weighted unemployment rate (which is based on the unemployment rates cohorts face at age 16 and above). The CPS data we use are from the ASEC fielded in March. Our last year of data is 2016. We confine the CPS analysis to individuals between ages 16 and 40. In addition, we limit the analysis to individuals with no more than 15 years of potential experience (when using the actual graduation unemployment rate) or are no older than age 33 (when using the double-weighted unemployment rate). The sample for the double-weighted specification is further restricted to individuals born in the United States.[17,18]

[16] Corresponding numbers are shown in the appendix (Sec. II).

[17] For a more detailed description of sample construction, see the appendix (Sec. I). The main CPS-based results include foreign-born individuals, as these are part of the US labor market. Excluding the foreign born has no bearing on our findings (see the appendix, Sec. V).

[18] Given that the CPS does not contain information on timing of entry into the labor market, we infer the year of entry into the labor market with the year in which the potential experience of an individual is equal to zero. This leaves some ambiguity in particular for young individuals without a high school degree, who might be still attending school and have not actually entered into the labor market full time. This should not be a problem for our main estimates, since they measure the differential effect on earnings and employment from the baseline development with potential experience. Since individuals that are still in school should not be affected, our main findings for the group of high school dropouts are likely to be based on individuals that have already entered the labor market. To assess directly whether there may be a contribution from students holding summer jobs, whose earnings presumably could be affected by the concurrent unemployment rate, we confirmed that our findings are similar when we exclude from the analysis children without a high school degree that had less than 13 weeks of employment (appendix, Sec. IX).

Table 1 presents summary statistics for our main sample and lists the variables we analyze. In addition to studying the effect of entering the labor market in a recession on log annual earnings, we also examine the effect of an unlucky start on a range of other factors related to employment, income, and the social insurance system. In particular, we study the effects on wages and weeks worked as well as the receipt of support from programs such as SNAP, unemployment insurance, and welfare and the use of Medicaid and receipt of health insurance more generally. To assess the incidence of initial labor market shocks on resources available to individuals, we also study the effects on household income, a more comprehensive measure that should reflect both labor earnings and income received from social insurance programs and spousal earnings. These outcomes are of particular relevance for less advantaged workers and might provide mediating factors for the effects on other outcomes analyzed in the recent literature, such as mortality (e.g., Schwandt and von Wachter 2018).

The upper part of table 1 shows expected differences in earnings, hourly wages, household income, and labor supply across groups. These differences help to underscore that nonwhites and lower-educated workers in particular have a substantially different average career outlook than college graduates, on whom the existing literature has focused.[19] The bottom part of table 1 confirms that these less advantaged groups also benefit in particular from social insurance programs such as SNAP or Medicaid. As has been documented elsewhere, there have been ongoing differential trends in labor market outcomes, especially by education groups. We control for those trends directly when we present separate estimates by education group. Although we do not control for group-specific trends in our main specification, there is no evidence that these secular nationwide trends strongly correlate with cyclical fluctuations in local labor market conditions at graduation.

III. The Effect of Entering the Labor Market in a Recession on Socioeconomic Outcomes

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

1. Baseline Estimates

Figure 2 shows the effects of the initial unemployment rate on log annual earnings in the first 15 years in the labor market. The figure displays the

[19] It is worth noting that compared with earnings, the level of household income for high school dropouts is inflated because some of these individuals are still living at home and household income likely refers to parental income. Again, this should not be a problem for our main estimates, since they measure the differential effect on earnings and employment from the baseline development with potential experience.

Table 1
Sample Statistics for Cohorts of Workers Entering the Labor Market from 1976 to 2015 with 1–15 Years of Potential Labor Market Experience

						Years of Schooling			
	Full Sample	Men	Women	White	Nonwhite	<12	12	13–15	≥16
Average annual earnings last year ($)	18,343	22,125	14,492	19,115	15,349	4,143	13,999	17,065	34,750
Average hourly earnings last year ($)	13	14	12	13	13	8	10	12	20
Average household income last year ($)	57,415	59,481	55,311	59,764	48,313	46,995	46,951	56,744	77,701
Average weeks worked last year	34.54	37.21	31.81	35.73	29.90	16.93	35.32	37.50	42.91
Average usual weekly hours in survey week	37.91	40.01	35.51	38.01	37.48	30.45	38.27	37.27	41.17
Employed in survey week	.69	.73	.65	.71	.60	.38	.70	.73	.85
Fraction receiving SNAP benefits (food stamps)	.10	.08	.12	.08	.18	.20	.13	.07	.02
Average value of SNAP benefits ($)	2,306	2,209	2,369	2,135	2,581	2,591	2,200	2,098	1,783
Fraction with any health insurance	.75	.72	.79	.77	.71	.68	.67	.76	.88
Fraction with private insurance	.67	.67	.67	.70	.56	.49	.56	.70	.87
Fraction receiving Medicaid	.10	.07	.13	.08	.17	.21	.12	.08	.02

SOURCE.—Annual Social and Economic Supplement to the Current Population Survey.
NOTE.—Potential labor market experience is defined as age minus years of completed schooling minus 6. Year of labor market entry is the implied calendar year after the year of completion of the highest level of schooling starting from age 6. All dollar values are deflated by the consumer price index with base year 2000. See the text for further details. SNAP = Supplemental Nutrition Assistance Program.

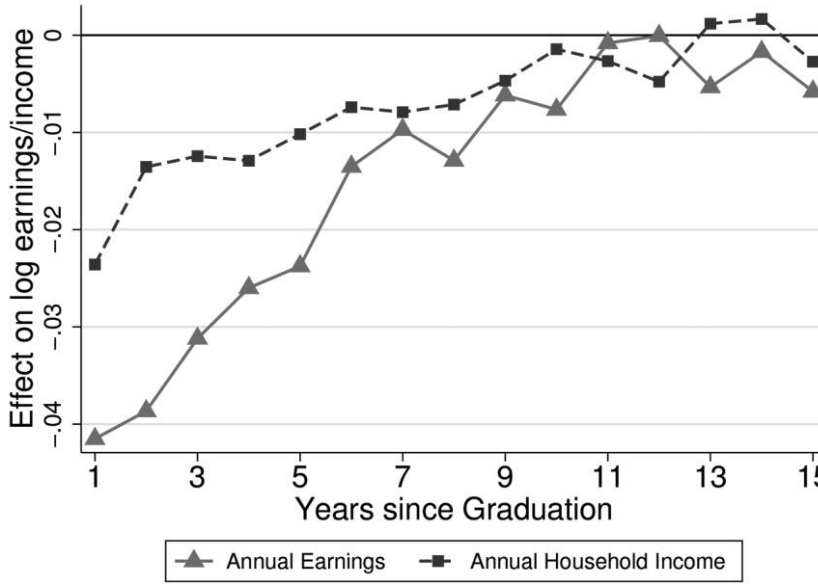

FIG. 2.—Effect of state unemployment rate at labor market entry on log annual earnings and household income for the full sample. Results are based on the Mincerian specification (eq. [2]), using data from the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

coefficients β_e on the interaction of dummies for potential experience with the unemployment obtained from estimating equation (2) for the entire sample of labor market entrants. The point estimates with standard errors for five experience groups are shown in table 2. The results clearly show, as expected, that earnings at labor market entry fall when the local unemployment rate rises. The effects are substantial: for a 3-point rise in the unemployment rate—roughly the typical increase from peak to trough of the business cycle—the point estimates in table 2 suggest an initial reduction of earnings by approximately 11%. This effect only slowly declines with time spent in the labor market. The reduction is still significantly different from zero 10 years after graduation (a reduction of 2.6% for a 3-point rise in initial unemployment rates) but then fades to zero, as shown in figure 2.

These estimates confirm previous findings mostly based on college graduates and for more narrow time periods. Our results tend to be somewhat larger than previous studies. While the estimates are difficult to compare because of differences in cohorts and time periods included, the study by Oreopoulos, von Wachter, and Heisz (2012) is most comparable because it also includes a broad number of cohorts covering multiple recessions; focusing on college graduates, they find an initial earnings loss of approximately 6% for a 3-point rise in the local unemployment rate that fades over

Table 2
Effects of State Unemployment Rates at Labor Market Entry on Earnings, Income, Wages, and Employment for Cohorts Entering from 1976 to 2015 for the Full Sample, by Gender and by Race

Variable, Experience Group	Full Sample	Men	Women	White	Nonwhite
Log annual earnings:					
1–3	−.0380	−.0428	−.0324	−.0375	−.0436
	(.0023)	(.0031)	(.0029)	(.0024)	(.0060)
4–5	−.0250	−.0263	−.0223	−.0230	−.0401
	(.0024)	(.0028)	(.0033)	(.0025)	(.0057)
6–7	−.0117	−.0118	−.0101	−.0126	−.0105
	(.0023)	(.0030)	(.0031)	(.0025)	(.0056)
8–10	−.0090	−.0094	−.0084	−.0105	−.0087
	(.0022)	(.0026)	(.0032)	(.0022)	(.0057)
Log household income:					
1–3	−.0178	−.0211	−.0144	−.0176	−.0171
	(.0017)	(.0020)	(.0021)	(.0018)	(.0034)
4–5	−.0118	−.0116	−.0115	−.0124	−.0128
	(.0017)	(.0022)	(.0022)	(.0018)	(.0044)
6–7	−.0076	−.0078	−.0070	−.0078	−.0110
	(.0017)	(.0022)	(.0021)	(.0017)	(.0044)
8–10	−.0044	−.0037	−.0045	−.0049	−.0084
Log hourly wages:	(.0017)	(.0020)	(.0023)	(.0016)	(.0045)
1–3	−.0139	−.0154	−.0125	−.0149	−.0138
	(.0013)	(.0018)	(.0017)	(.0014)	(.0033)
4–5	−.0127	−.0143	−.0101	−.0123	−.0177
	(.0015)	(.0020)	(.0020)	(.0016)	(.0038)
6–7	−.0074	−.0075	−.0067	−.0083	−.0058
	(.0014)	(.0019)	(.0017)	(.0015)	(.0036)
8–10	−.0060	−.0063	−.0065	−.0070	−.0050
	(.0013)	(.0017)	(.0019)	(.0014)	(.0036)
Log weeks worked:					
1–3	−.0150	−.0174	−.0113	−.0129	−.0230
	(.0014)	(.0018)	(.0020)	(.0014)	(.0038)
4–5	−.0063	−.0067	−.0055	−.0050	−.0147
	(.0013)	(.0016)	(.0020)	(.0013)	(.0034)
6–7	−.0017	−.0026	−.0003	−.0012	−.0035
	(.0013)	(.0016)	(.0019)	(.0013)	(.0032)
8–10	−.0008	−.0008	−.0004	−.0008	−.0029
	(.0012)	(.0014)	(.0018)	(.0012)	(.0030)

SOURCE.—Annual Social and Economic Supplement to the Current Population Survey.
NOTE.—Regressions control for fixed effects for potential labor market experience, calendar year, year of graduation, state of current residence, and three education groups.

time. For US college graduates during the severe 1980s recession, Kahn (2010) finds somewhat larger estimates. According to our estimates, a 1-point rise in the initial unemployment rate reduces cumulated earnings by approximately 20% of average annual earnings in the sample (summing the coefficients in table 2 and scaling for the number of experience years they represent). Hence, a recession—roughly corresponding to a 3-point rise in unemployment rates—would lead to a reduction of cumulated earn-

ings by approximately 60% of average annual earnings, a substantial effect. Relative to total earnings in the first 10 years of the labor market, the effect is approximately 6%.[20] That our findings tend to be somewhat larger is likely due to the fact that we include in our analysis more vulnerable groups than are typically studied, something we return to in Section III.C. Before turning to that, we first use our double-weighted unemployment rate measure to show that these effects are not due to the fact that we are using cross-sectional data.

It is worth noting that our estimates on the effect of initial unemployment rates on annual earnings are likely to be underestimates of the actual total effect. This is because, as further discussed below, we find that the overall rate of employment declines temporarily because of a high initial unemployment rate. This leads to two sources of underestimation of the true loss in earnings due to initial labor market conditions. Our estimates exclude workers that have zero earnings and hence do not count the losses deriving from longer spells of nonemployment. In addition, given that we find that negative employment effects tend to be strongest for lower-educated individuals, a model of monotone selectivity into employment based on, say, underlying skill would suggest that the resulting sample selection bias is positive. In other words, individuals with the lowest earnings potential are less likely to work as a result due to the initial shocks, biasing the result toward zero. We have assessed the potential effect for our estimates based on log annual earnings by imputing a low earnings amount, confirming that our estimates likely understate the true loss by a moderate but nonnegligible amount (see the appendix, Sec. VII).

2. *Correcting for Interstate Mobility and Endogenous Labor Market Entry*

In Section II.C, we describe the biases that can arise from interstate migration and graduation timing and how they can be corrected using the decennial census and ACS data. Figure 3 shows how these corrections affect our baseline estimates. The triangles show the CPS estimates for annual earnings, as in figure 2. The squares show that the baseline specification results in very similar estimates in the census/ACS data.

For the third set of estimates in figure 3, the solid black line without markers, we use respondents' state of birth instead of state of residence as a proxy for the state of graduation. As explained above, this specification is not affected by any endogenous or random migration after graduation, but there is likely attenuation due to migration before graduation. The resulting estimates are attenuated by about 20% in the first years after graduation in comparison with

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

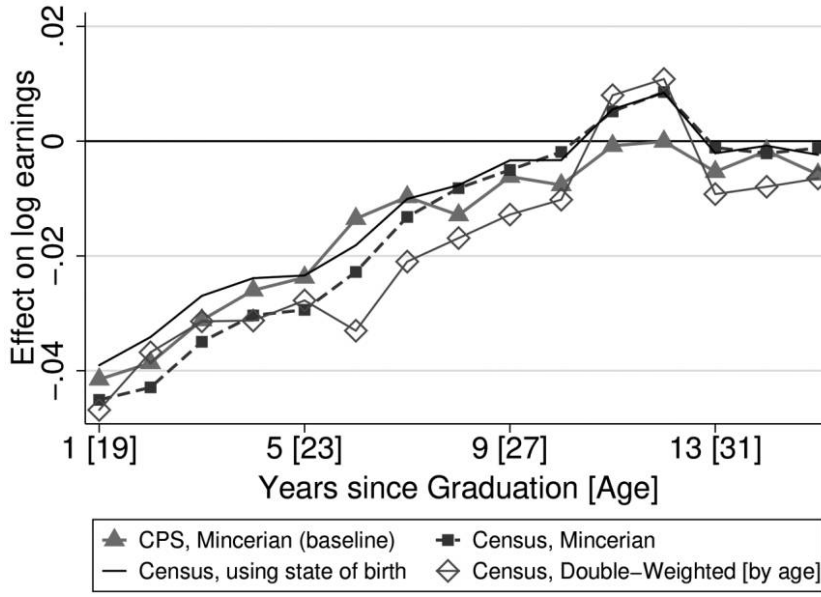

FIG. 3.—Adjusting for migration and graduation timing in decennial census and American Community Survey (ACS) data. The Current Population Survey (CPS) baseline results are based on the Annual Social and Economic Supplement to the CPS from 1976 to 2016. The census results are based on the 1980/1990/2000 decennial censuses and the ACS from 2001 to 2015. The Mincerian graduation year unemployment rate refers to the unemployment rate in the current state of residence at the Mincerian year of graduation (the sum of the year of birth, plus 6, plus the years of reported education). The double-weighted graduation year unemployment rate refers to the average unemployment rate across cohorts' typical graduation ages and across the states to which cohorts typically migrate before graduation. See Sections II.B and II.C for further details. A color version of this figure is available online.

the baseline specification, as one would expect given a pregraduation migration rate of about 20%. The difference between estimates fades at higher experience years, as accumulative migration after graduation attenuates the baseline estimates but not those based on the state of birth.

Finally, the hollow markers show estimates based on the double-weighted unemployment rate, which corrects for migration (both before and after graduation) as well as for endogenous labor market entry. The effect profile is shown over age instead of experience, as the double-weighted unemployment rate is constructed at the level of year and state of birth. The adjusted effects seem a bit more noisy, likely because of the adjustment-induced reduction in the amount of identifying variation. But despite the loss in precision, effects are very similar to the baseline specification in the first years of age/experience plotted in the figure. And in line with a slight attenuation of

the baseline specification due to accumulative random migration after graduation, the adjusted effects are somewhat stronger in later years. Overall, these results suggest that in our baseline specification, any bias due to endogenous timing of labor market entry and interstate migration is very limited. This conclusion is in line with the close correspondence of the Mincerian and the double-weighted unemployment rate shown in figure 1.

Figure 4 shows results from an IV specification that uses the double-weighted unemployment rate as an instrument for the endogenous Mincerian rate. The triangles, squares, and hollow markers repeat, respectively, the two baseline specifications and the double-weighted specification from figure 3. Note that the double-weighted specification represents the reduced form in the IV setting. The first-stage regression of the Mincerian

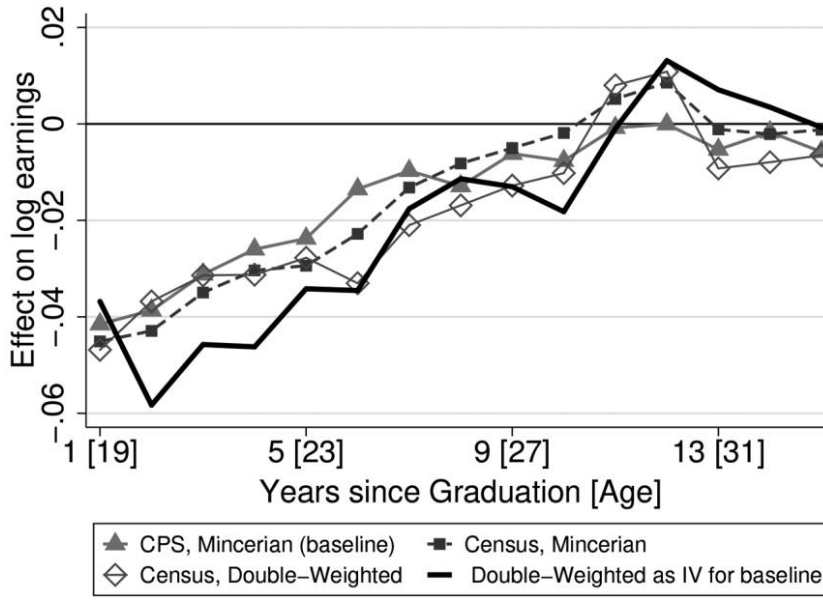

FIG. 4.—Using the double-weighted average graduation year unemployment rate as an instrumental variable for state unemployment rate at labor market entry. The Current Population Survey (CPS) baseline results are based on the Annual Social and Economic Supplement to the CPS from 1976 to 2016. The census results are based on the 1980/1990/2000 censuses and the ACS from 2001 to 2015. The Mincerian graduation year unemployment rate refers to the unemployment rate in the current state of residence at the Mincerian year of graduation (the sum of the year of birth, plus 6, plus the years of reported education). The double-weighted graduation year unemployment rate refers to the average unemployment rate across cohorts' typical graduation ages and across the states to which cohorts typically migrate before graduation. See Sections II.B and II.C for further details. A color version of this figure is available online.

on the double-weighted unemployment rate is 0.7, with a t-statistic of 24. The thick solid black line in figure 4 shows the IV estimate, and it is very similar to the other specifications. Our main takeaway is that these different specifications can be used interchangeably given the limited role of endogenous graduation timing and interstate migration. With this in mind, we return to the CPS analysis that is based on the Mincerian specification.

B. The Effect on Other Outcomes for the Full Sample

1. Effects on Weeks Worked and Wages

The CPS data allow us to decompose the earnings effect into an effect stemming from a reduction in the number of annual weeks worked, a reduction in usual hours worked per week, and a reduction in hourly wages (calculated by dividing total annual earnings last year by approximate total annual hours in the survey week, a common procedure). The results, shown in figure 5, indicate some interesting patterns. First, exposure to high unemployment rates at labor market entry leads to a precisely estimated persistent reduction in hourly wages lasting all 15 experience years included in the analysis. While the effect after 10 years in the labor market is small, an unlucky initial start clearly depresses earnings even for those individuals

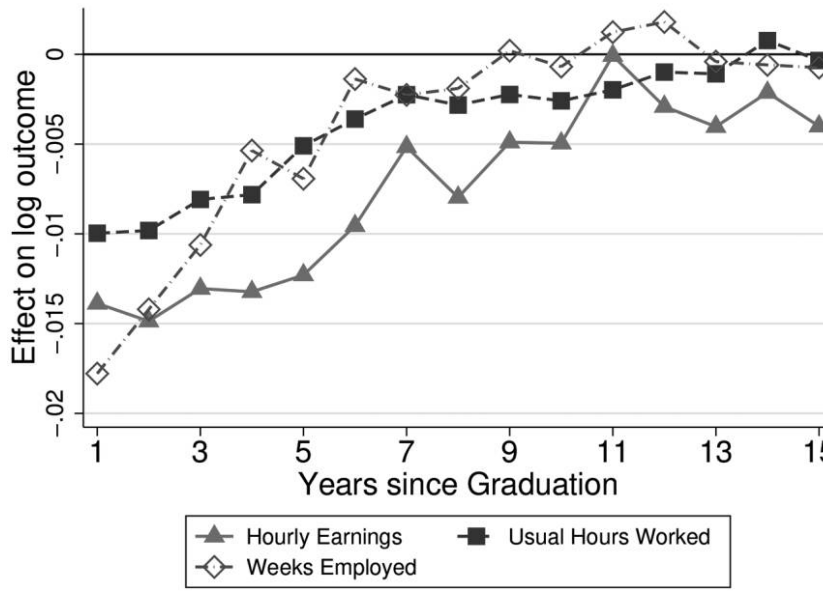

FIG. 5.—Effect of state unemployment rate at labor market entry on employment and wages. Results are based on the Mincerian specification (eq. [2]), using data from the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

obtaining a job. Given that these estimates are based on a potentially positively selected group of individuals who found jobs, they may understate the true reduction in earnings capacity for unlucky entrants. Second, we find nonnegligible effects on weeks worked that are concentrated in the first 5 years after labor market entry. Finally, we find a smaller but surprisingly persistent reduction in usual hours worked. An examination of the point estimates shown in table 2 show that about two-thirds of the effect on annual earnings we find in the first 3 years is driven by a reduction in total hours worked (weeks worked times usual hours). This drops to 50% in experience years 4 to 5. In contrast, two-thirds of the longer-term effect of adverse initial labor market entry on annual earnings is driven by a reduction in hourly wages.

It is worth noting that these findings are based on using the log of weeks worked last year as an outcome and hence do not count the loss in labor supply arising because of nonemployment spells lasting the entire survey year. We have separately analyzed the incidence of zero employment in the survey year (i.e., zero weeks worked). For added context, we also compared it to the incidence of employment, unemployment, and rate of being out of the labor force during the survey week. The results, shown in the appendix (Sec. VI), confirm that entering the labor market when local unemployment rates are high depresses employment for about 5–6 years, including a rise in the incidence of longer-term nonemployment spells.[21]

2. *Effects on Social Welfare and Total Income*

Figure 2 shows the effect of high initial unemployment rates on the log of family income. There is a clearly visible negative and persistent effect of initial labor market conditions that is precisely estimated even 10 years into the labor market. However, the impact is smaller than for annual earnings, especially in the first years after labor market entry. A cumulation of the coefficients in table 2 implies that the lifetime effect of a 3-point rise in the initial unemployment rate is roughly 30%, about half the effect of earnings discussed in Section III.A.1.

Transfers from the social insurance system, such as SNAP or welfare payments, account for one source of difference. We exploited the available information in the ASEC to the March CPS to directly assess the effect of initial unemployment rates on receipt of transfer income. The results are shown in figure 6 and table 3. We find a persistent increase in the probability

[21] Interestingly, most of these longer spells appear to be related to exits from the labor force rather than unemployment. This is not surprising given that self-reported unemployment is often associated with receipt of unemployment insurance benefits, for which many unlucky young labor market participants do not yet qualify because of a lack of earnings history.

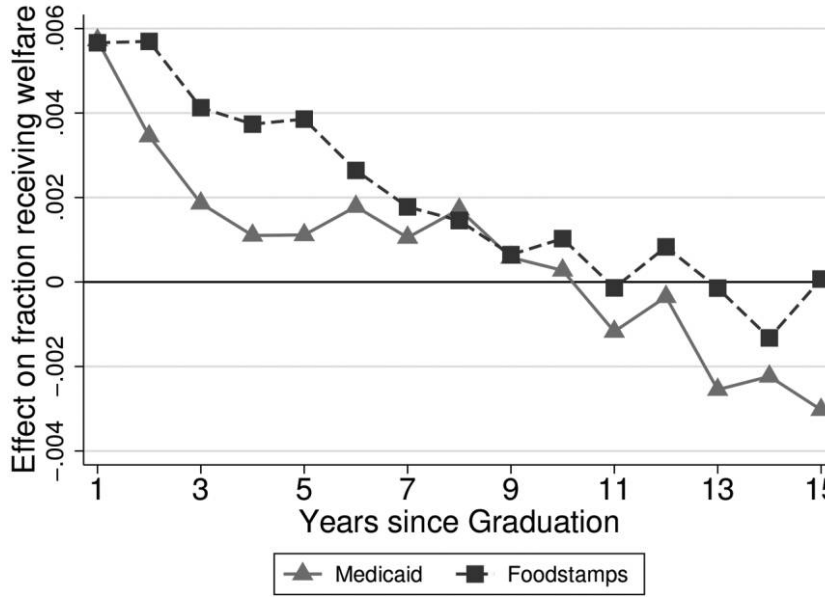

FIG. 6.—Effect of state unemployment rate at labor market entry on Supplemental Nutrition Assistance Program benefits (food stamps) and Medicaid for the full sample. Results are based on the Mincerian specification (eq. [2]), using data from the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

of receiving SNAP benefits but no effect for other outcomes, such as unemployment insurance, receipt of earned income tax credits, or welfare receipt (not shown). This confirms that unemployment insurance in particular does little to smooth adverse initial conditions in the labor market for young workers, partly because of lack of eligibility and limited duration and partly because a substantial portion of the effect goes through persistent reductions in wages.

While the point estimates of the effect on SNAP benefits appear small, they are precisely estimated even up to 10 years after labor market entry. Relative to the average fraction of the sample receiving SNAP benefits, the effect is nonnegligible. For a 3-point rise in unemployment rates, the initial effect is about a 1.5 percentage point rise (a 15% rise with respect to the mean in table 1), and the cumulated effect over 10 years is approximately 3.7 points. Given that about 10% of individuals receive SNAP benefits and 23% of Americans report to have received benefits at some point, these are substantial effects. We also analyzed the effect of initial unemployment rates on the amount of benefits received among SNAP recipients (not shown). We find that in the first 4–5 years after entry, income from SNAP benefits rose by a precisely estimated 2%–3%. This has the potential to

Table 3
Effects of State Unemployment Rates at Labor Market Entry on Receipt of Food Stamps, Health Insurance, and Poverty for Cohorts Entering from 1976 to 2015 for the Full Sample, by Gender and by Race

Variable, Experience Group	Full Sample	Men	Women	White	Nonwhite
Incidence of receipt of SNAP benefits (food stamps):					
1–3	.0053	.0050	.0055	.0038	.0111
	(.0005)	(.0006)	(.0007)	(.0005)	(.0016)
4–5	.0038	.0026	.0049	.0027	.0093
	(.0006)	(.0007)	(.0008)	(.0006)	(.0017)
6–7	.0022	.0022	.0020	.0017	.0062
	(.0006)	(.0008)	(.0008)	(.0006)	(.0016)
8–10	.0011	.0003	.0015	.0006	.0056
	(.0006)	(.0007)	(.0008)	(.0005)	(.0017)
Incidence of poverty:					
1–3	.0056	.0065	.0048	.0046	.0096
	(.0006)	(.0007)	(.0008)	(.0006)	(.0014)
4–5	.0033	.0031	.0032	.0029	.0062
	(.0007)	(.0009)	(.0009)	(.0007)	(.0018)
6–7	.0013	.0012	.0011	.0015	.0023
	(.0006)	(.0008)	(.0009)	(.0006)	(.0018)
8–10	.0014	.0011	.0013	.0012	.0047
	(.0006)	(.0007)	(.0008)	(.0006)	(.0016)
Incidence of Medicaid receipt:					
1–3	.0041	.0037	.0044	.0036	.0054
	(.0006)	(.0006)	(.0007)	(.0006)	(.0015)
4–5	.0012	.0016	.0005	.0009	.0030
	(.0007)	(.0008)	(.0009)	(.0007)	(.0018)
6–7	.0014	.0006	.0018	.0014	.0027
	(.0007)	(.0007)	(.0009)	(.0007)	(.0017)
8–10	.0009	−.0002	.0015	.0008	.0035
	(.0006)	(.0006)	(.0009)	(.0006)	(.0017)
Incidence of receipt of private health insurance:					
1–3	−.0085	−.0096	−.0074	−.0079	−.0102
	(.0009)	(.0012)	(.0012)	(.0010)	(.0019)
4–5	−.0019	−.0016	−.0023	−.0019	−.0046
	(.0012)	(.0016)	(.0014)	(.0012)	(.0025)
6–7	−.0008	−.0016	.0000	−.0011	−.0021
	(.0010)	(.0014)	(.0012)	(.0011)	(.0020)
8–10	.0006	.0005	.0007	.0002	−.0011
	(.0009)	(.0012)	(.0012)	(.0009)	(.0019)

SOURCE.—Annual Social and Economic Supplement to the Current Population Survey.
NOTE.—Regressions control for fixed effects for potential labor market experience, calendar year, year of graduation, state of current residence, and three education groups. SNAP = Supplemental Nutrition Assistance Program.

explain a substantial part of the difference between labor earnings and household income shown in figure 2.[22]

3. *Effects on Health Insurance*

As health insurance receipt was chiefly tied to employment throughout most of the period under study, the employment effects we find can imply a loss in private health insurance coverage. Based on the ASEC, we can analyze receipt of private health insurance, availability of any health insurance, and receipt of Medicaid as separate outcomes. As expected, we see in table 3 that exposure to a high initial unemployment rate leads to a reduction in the incidence of private health insurance. Consistent with our findings on the reduction in employment, the effect fades after about 4–5 years in the labor market. In contrast, the losses in overall access to health insurance are concentrated in the first few years after labor market entry. The difference is explained by a persistent rise in the incidence of receiving Medicaid, shown in figure 6. As shown in table 3, this effect is significantly different from zero for about 7 years after labor market entry. The point estimates are small but have to be compared against the average of 10% in Medicaid receipt in our sample (see table 1). Scaling the coefficient again by 3 suggests that labor market entry in recessions increases the probability of receiving Medicaid by about 12% relative to baseline.

We have also analyzed the effect on family outcomes, such as marital status, childbearing, single parenthood, or living with parents. In contrast to other studies suggesting that increasing unemployment rates delay household formation, we find no effect of initial unemployment rate on marriage or childbearing. However, results shown in the appendix (Sec. VIII) suggest that there is an increase in the incidence of individuals living with their parents. This rise lasts for only 4–5 years after entry, suggesting that individuals move into a place of their own once they have found stable employment.[23]

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

Figure 7 and table 2 show the effects of initial unemployment rates on annual earnings and family income by gender and by whites versus nonwhites. The results for men and women are qualitatively quite similar, with the exception of the effects in the first years after labor market entry. Initially, men experience larger losses in both earnings and family income. Turning to race, the losses in annual earnings for nonwhites are substantially larger than for whites, especially in the first 5 years in the labor market. As shown in figure 9, this difference is mainly driven by greater employment losses for

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

[23] While these results are precisely estimated for the full sample, there was not sufficient precision to replicate it by demographic or education groups.

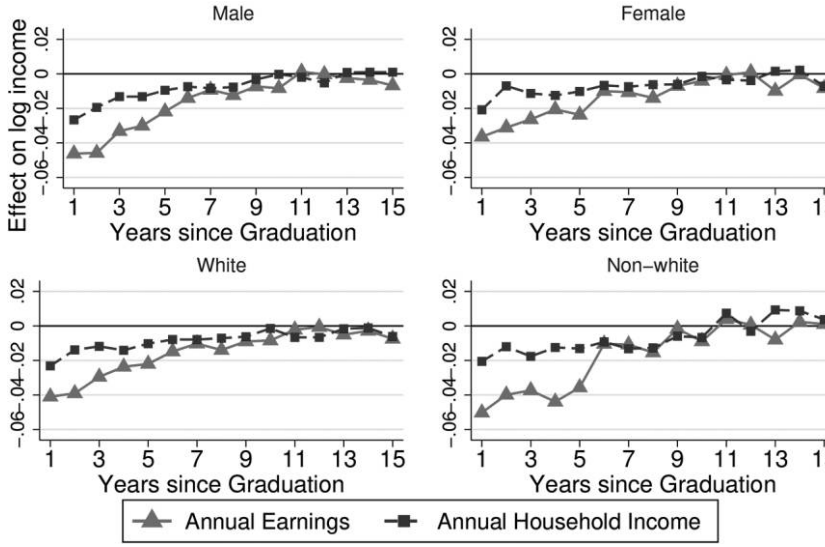

FIG. 7.—Effect of state unemployment rate at labor market entry on earnings and income, by demographic group. Results are based on the Mincerian specification (eq. [2]), using data from the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

nonwhites. In contrast, the effect on household income is quite comparable, suggesting that social insurance mechanisms help to buffer the bigger effects for nonwhites, something which we turn to below.

Considering figure 9, it is remarkable how, despite some differences in the initial effect, all considered groups experienced persistent losses in hourly wages. In contrast, the temporary losses in employment (as measured by weeks worked last year) are somewhat more disparate across groups. In particular, men appear to experience a more persistent reduction in employment, and nonwhites experience the largest losses. Yet after 5 years in the labor market, these effects have completely faded.

Figure 8 and table 4 show that there are substantial differences in the effect of adverse initial conditions by education groups. Middle-educated workers—those with high school or some college—experience patterns comparable to those for the full sample shown in figure 2. In contrast, college graduates show markedly smaller and shorter-lived effects on annual earnings. The size of the effect is about half of that of the full sample and more similar in magnitude to effects found for college graduates in Canada by Oreopoulos, von Wachter, and Heisz (2012): they are somewhat smaller than findings by Kahn (2010) for college graduates entering during the severe 1982 recession. The largest effects we find are the initial losses in annual earnings experienced by high school dropouts. These average a 5% reduction over the first 3 years and then converge in a similar fashion as the effect

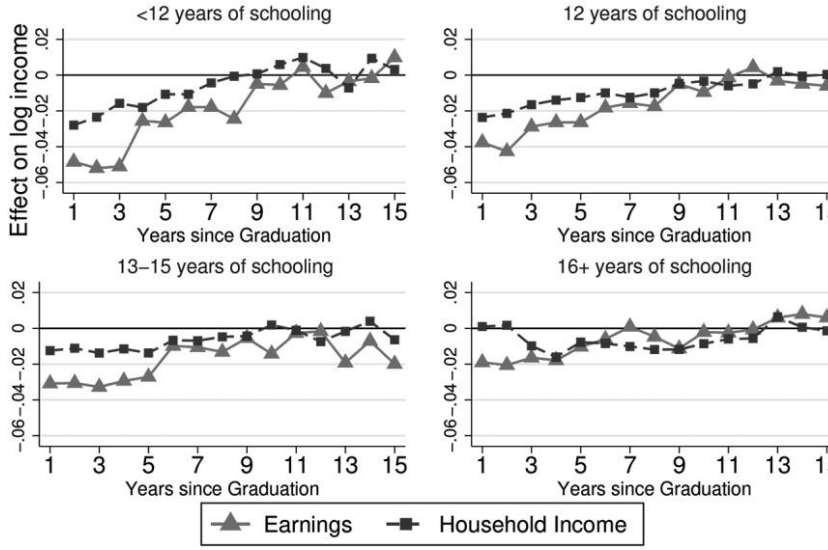

FIG. 8.—Effect of state unemployment rate at labor market entry on earnings and income, by education group. Results are based on the Mincerian specification (eq. [2]), using data from the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

for those with a high school degree. The results are substantial losses in cumulated earnings for this group. Not surprisingly, government transfers play a particularly important role in delivering a more muted impact on income for high school dropouts, something we return to below.

As shown in figure 10 to an important degree, the differences in earnings losses result from differences in employment losses. High school dropouts experience substantial employment reductions, whereas college graduates essentially experience no significant employment reduction. In contrast, again all groups experience reductions in hourly wages lasting at least 10 years into the career. As shown in table 5, even for workers with at least a college degree or with some college, the reduction is statistically significantly different from zero 10 years after labor market entry. Interestingly, while reduced weekly hours is a phenomenon relevant for all demographic and education groups, there is little noticeable variance in the effect of adverse initial labor market conditions.

D. The Effect on Other Outcomes by Education, Race, and Gender

1. Welfare Effects by Demographic Groups

Figure 11 shows the impact of initial unemployment rates on our two key variables intended to capture the role of the social insurance system—receipt of SNAP and Medicaid—by demographic group. Women have a

Table 4
Effects of State Unemployment Rates at Labor Market Entry on Earnings, Income, Wages, and Employment for Cohorts Entering from 1976 to 2015, by Education Group

		Years of Schooling			
Variable, Experience Group	Full Sample	<12	12	13–15	≥16
Log annual earnings:					
1–3	−.0380	−.0494	−.0362	−.0314	−.0182
	(.0023)	(.0056)	(.0035)	(.0037)	(.0038)
4–5	−.0250	−.0259	−.0265	−.0284	−.0140
	(.0024)	(.0094)	(.0036)	(.0044)	(.0037)
6–7	−.0117	−.0178	−.0169	−.0102	−.0028
	(.0023)	(.0092)	(.0039)	(.0038)	(.0033)
8–10	−.0090	−.0121	−.0107	−.0110	−.0061
	(.0022)	(.0074)	(.0035)	(.0037)	(.0032)
Log household income:					
1–3	−.0178	−.0256	−.0205	−.0124	−.0028
	(.0017)	(.0030)	(.0030)	(.0029)	(.0031)
4–5	−.0118	−.0148	−.0133	−.0125	−.0117
	(.0017)	(.0052)	(.0029)	(.0031)	(.0025)
6–7	−.0076	−.0075	−.0111	−.0067	−.0094
	(.0017)	(.0060)	(.0029)	(.0030)	(.0027)
8–10	−.0044	.0018	−.0060	−.0024	−.0108
	(.0017)	(.0051)	(.0027)	(.0028)	(.0029)
Log hourly wages:					
1–3	−.0139	−.0122	−.0165	−.0156	−.0094
	(.0013)	(.0037)	(.0020)	(.0021)	(.0025)
4–5	−.0127	−.0069	−.0150	−.0138	−.0126
	(.0015)	(.0051)	(.0023)	(.0023)	(.0025)
6–7	−.0074	−.0053	−.0113	−.0076	−.0046
	(.0014)	(.0050)	(.0026)	(.0026)	(.0023)
8–10	−.0060	−.0023	−.0090	−.0047	−.0056
	(.0013)	(.0043)	(.0022)	(.0024)	(.0021)
Log weeks worked:					
1–3	−.0150	−.0282	−.0112	−.0063	−.0024
	(.0014)	(.0040)	(.0020)	(.0020)	(.0017)
4–5	−.0063	−.0135	−.0060	−.0072	.0009
	(.0013)	(.0062)	(.0020)	(.0027)	(.0014)
6–7	−.0017	−.0100	−.0031	−.0004	.0017
	(.0013)	(.0062)	(.0018)	(.0019)	(.0014)
8–10	−.0008	−.0075	−.0006	−.0017	−.0001
	(.0012)	(.0048)	(.0018)	(.0018)	(.0013)

SOURCE.—Annual Social and Economic Supplement to the Current Population Survey.
NOTE.—Regressions control for fixed effects for potential labor market experience, calendar year, year of graduation, state of current residence, and three education groups.

slightly higher rate of initial receipt of SNAP, and this may contribute to their lower income losses, but the difference is relatively small. However, we see a large difference in the propensity to receive SNAP benefits by race groups. In the first 5 years, nonwhites have an increased probability of receiving SNAP benefits of 1 percentage point (table 3). At a mean receipt of

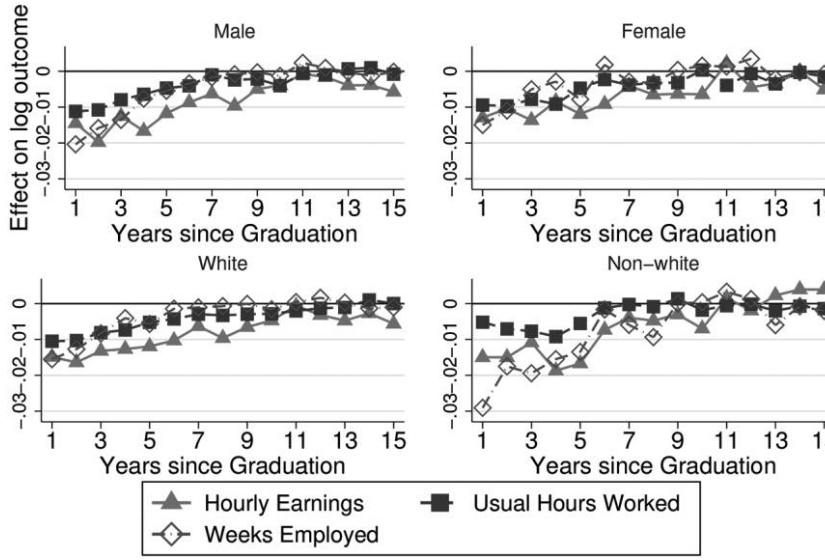

FIG. 9.—Effect of state unemployment rate at labor market entry on employment and wages, by demographic group. Results are based on the Mincerian specification (eq. [2]), using data from the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

18% in our population, that 3-point rise in unemployment rates leads to a 15% increase relative to the mean. For whites, the effect is 0.4 and 0.3 points in years 1–3 and 2–5, respectively. For a 3-point recession, this implies approximately an 11% effect relative to the mean (8%). Interestingly, for all demographic groups, conditional on receiving SNAP benefits, the increase in the amount is relatively similar (not shown).

We also assessed the effect on other potential sources of transfer income. Although we did not find precisely estimated effects for either men or both race groups, we found a nonnegligible rise in the amount of unemployment insurance income received for women.

Overall, despite the increased probability of receiving SNAP benefits and its effect on income, entering the labor market during slack labor markets has a significant effect on poverty rates. Figure 13 shows that poverty rates rose persistently for the first 5 years after labor market entry—and in some cases even up to 9 years after—for all demographic groups, an effect that was largest for blacks. The effects are nonnegligible: relative to the mean poverty rates for the different demographic groups in the age ranges shown in table 1, the effects are in the range of 10%–15%.

Figure 11 also shows the rise in the incidence of receipt of Medicaid by demographic groups. There are little discernible differences by gender, with

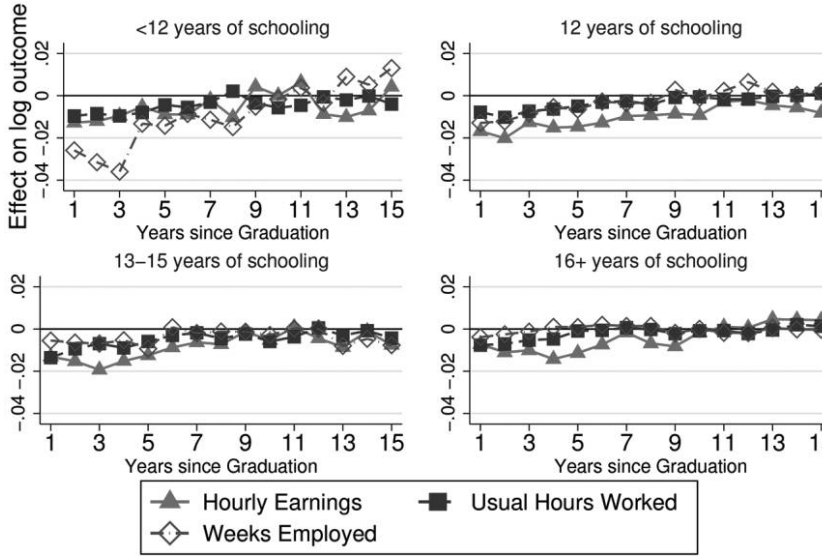

FIG. 10.—Effect of state unemployment rate at labor market entry on employment and wages, by education group. Results are based on the Mincerian specification (eq. [2]), using data from the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

relatively short-lived effects. The same is true for whites. In contrast, for nonwhites the figure and table show precisely estimated larger increases lasting 10 years into the labor market. A 3-point rise in the initial unemployment rate is predicted to trigger a rise of approximately 1.5 points initially (an effect of about 10% relative to the mean; see table 1) and a rise of approximately 1 point thereafter.

The Medicaid results relate to the coverage by health insurance more generally. Table 3 shows that there is a steep loss in private health insurance concentrated in the first 2 years in the labor market for whites and both genders (with men suffering slightly larger losses) and in the first 4 years for nonwhites. Because of the increased probability of receiving Medicaid, the effect on having any health insurance shown in the appendix (Sec. X) is substantially muted for women and nonwhites and reduced for men and whites as a whole. Hence, it appears that Medicaid is successful at providing a partial buffer against the temporary loss in employer-provided health insurance.

2. *Welfare Effects by Four Education Groups*

Figure 12 and table 1 show the effect of initial unemployment rates on receipt of SNAP benefits and Medicaid by education groups. The results are

Table 5
Effects of State Unemployment Rates at Labor Market Entry on Receipt of Food Stamps, Health Insurance, and Poverty for Cohorts Entering from 1976 to 2015, by Education Group

Variable, Experience Group	Full Sample	Years of Schooling <12	12	13–15	≥16
Incidence of receipt of SNAP benefits (food stamps):					
1–3	.0053	.0109	.0052	.0009	.0003
	(.0005)	(.0012)	(.0009)	(.0008)	(.0004)
4–5	.0038	.0106	.0039	.0038	.0007
	(.0006)	(.0022)	(.0011)	(.0013)	(.0005)
6–7	.0022	.0125	.0024	.0008	−.0003
	(.0006)	(.0026)	(.0011)	(.0010)	(.0004)
8–10	.0011	.0065	.0010	.0002	−.0001
	(.0006)	(.0024)	(.0011)	(.0009)	(.0004)
Incidence of poverty:					
1–3	.0056	.0088	.0078	.0014	−.0001
	(.0006)	(.0012)	(.0011)	(.0011)	(.0009)
4–5	.0033	.0081	.0047	.0026	.0007
	(.0007)	(.0024)	(.0012)	(.0012)	(.0008)
6–7	.0013	.0048	.0033	.0001	.0003
	(.0006)	(.0027)	(.0012)	(.0011)	(.0007)
8–10	.0014	.0036	.0028	.0006	.0010
	(.0006)	(.0023)	(.0010)	(.0010)	(.0007)
Incidence of Medicaid receipt:					
1–3	.0041	.0062	.0045	.0008	.0003
	(.0006)	(.0013)	(.0011)	(.0009)	(.0005)
4–5	.0012	.0046	.0027	−.0010	.0003
	(.0007)	(.0021)	(.0013)	(.0011)	(.0006)
6–7	.0014	.0099	.0012	−.0001	.0002
	(.0007)	(.0024)	(.0012)	(.0012)	(.0006)
8–10	.0009	.0072	.0018	−.0005	.0005
	(.0006)	(.0022)	(.0011)	(.0009)	(.0005)
Incidence of private health insurance receipt:					
1–3	−.0085	−.0098	−.0135	−.0067	.0001
	(.0009)	(.0018)	(.0018)	(.0017)	(.0016)
4–5	−.0019	.0015	−.0060	−.0022	−.0022
	(.0012)	(.0030)	(.0020)	(.0022)	(.0015)
6–7	−.0008	−.0038	−.0032	.0009	−.0007
	(.0010)	(.0029)	(.0018)	(.0019)	(.0014)
8–10	.0006	.0012	−.0016	.0020	−.0011
	(.0009)	(.0024)	(.0015)	(.0016)	(.0013)

SOURCE.—Annual Social and Economic Supplement to the Current Population Survey.
NOTE.—Regressions control for fixed effects for potential labor market experience, calendar year, year of graduation, state of current residence, and three education groups. SNAP = Supplemental Nutrition Assistance Program.

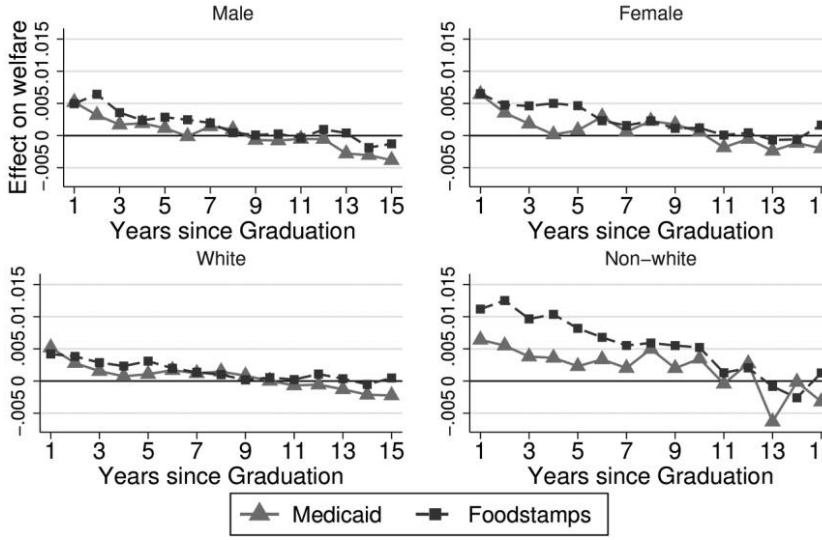

FIG. 11.—Effect of state unemployment rate at labor market entry on Supplemental Nutrition Assistance Program benefits (food stamps) and Medicaid, by demographic group. Results are based on the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

quite clear. Labor market entrants with some college or more do not appear to experience an increase in the receipt of these social insurance programs due to adverse initial labor market conditions. Labor market entrants with 12 years of education experience a precisely estimated but moderate increase in the probability of obtaining these programs lasting up to 7 years after labor market entry. In contrast, high school dropouts experience substantial increases in the probability of receiving SNAP that, albeit declining somewhat over time, lasts up to 15 years into the labor market. This suggests that the effects for broader groups discussed so far are mainly driven by responses for lower-skilled workers. The amounts received are higher initially and then decline somewhat for college dropouts, but the changes with experience are not precisely estimated (see the appendix, Sec. X).

When considering unemployment insurance benefit receipt, we found relatively imprecisely estimated effects for high school dropouts in the first few years after labor market entry, but no effects for any of the other groups.

Taken together, the results for SNAP benefits and household income suggest that social insurance mechanisms do buffer the effect of adverse conditions at initial labor market entry. However, the insurance is imperfect. As shown in figure 14, poverty rates rise persistently for both high school graduates and high school dropouts. Given typical poverty rates

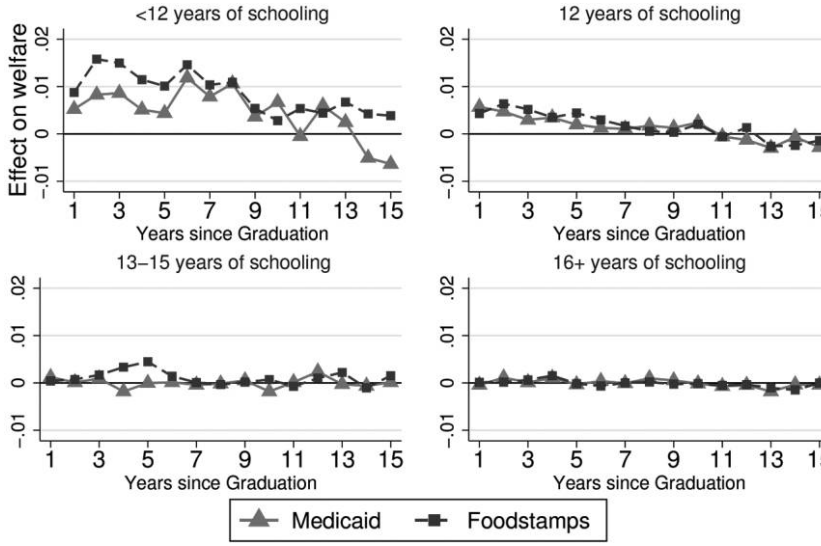

FIG. 12.—Effect of state unemployment rate at labor market entry on Supplemental Nutrition Assistance Program benefits (food stamps) and Medicaid, by education group. Results are based on the Mincerian specification (eq. [2]), using data from the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

in our sample of a bit over 20% (table 1), the estimates imply a rise in poverty rates of approximately 15% relative to the average poverty rate during a moderate recession. It would be 25% in a larger downturn with a 5-point rise in unemployment rates, such as the Great Recession.

Figure 12 and table 1 also show the effect on Medicaid. Again, as expected there is no increase in the probability of receiving Medicaid by labor market entrants with some college or more. In contrast, less educated labor market entrants see a persistent increase in Medicaid receipt following an adverse labor market entry. Relative to mean Medicaid receipt for the lowest education group (20.5%; table 1), the effect is approximately 5%–10%.

Again, these pattern are connected to health insurance availability more generally (results are shown in the appendix, Sec. X). Interestingly, temporary losses in employer-provided health insurance are concentrated among high school graduates and those entrants with some college; in contrast, neither high school dropouts nor college graduates experience a reduction. As a result, high school graduates can partly rely on Medicaid to help buffer the temporary loss of employer-provided health insurance, whereas those with some college cannot.

Overall, it appears that persistent increases in the receipt of SNAP benefits and Medicaid help those young labor market entrants that are most affected by adverse initial labor market conditions—nonwhites and high

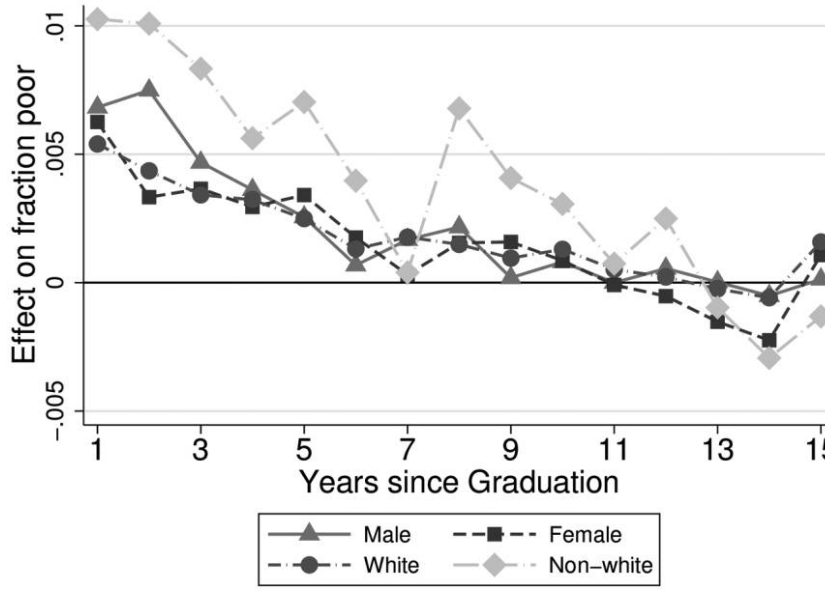

FIG. 13.—Effect of state unemployment rate at labor market entry on the incidence of poverty, by demographic group. Results are based on the Mincerian specification (eq. [2]), using data from the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

school dropouts, and to some degree high school graduates. The buffer appears most successful for access to any health insurance, which appears to decline only in the immediate years after graduation when employment losses are most severe. In contrast, the rise in transfer payments cannot prevent a temporary but long-lasting rise in poverty, which reflects the large and persistent earnings losses that these less advantaged groups experience on graduation during recessions.

IV. Conclusion

Economists and policy makers alike have long been concerned about whether young workers entering the labor market during a recession suffer permanent consequences from their initial bad luck. While this question has been studied extensively for male college graduates, less advantaged workers, such as lower-educated workers, nonwhites, and women, have received less attention. To be able to study the long-term effects for these smaller groups, we have introduced a new approach to deal with selective migration and labor market entry that exploits information on place of birth available in the decennial census and the ACS. This approach allowed us to exploit cross-sectional data from the CPS, which spans more than 4 decades, to

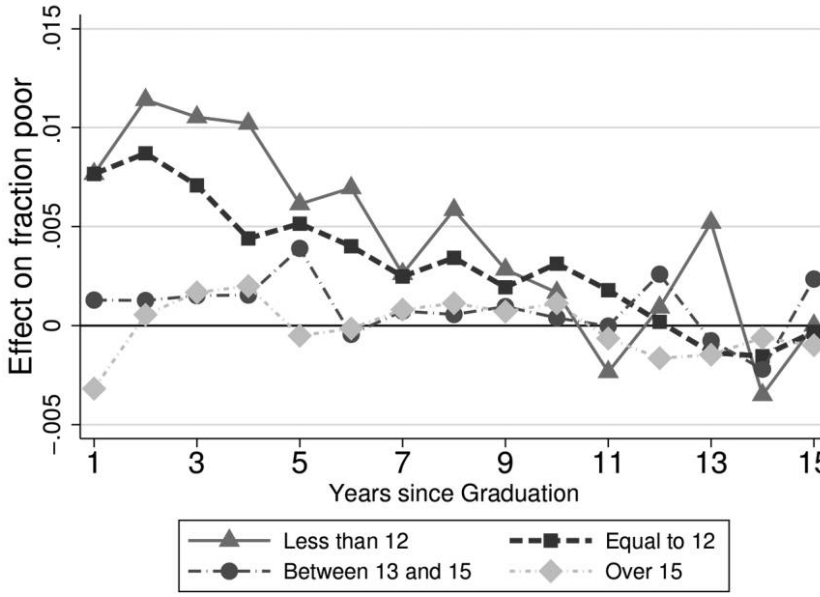

FIG. 14.—Effect of state unemployment rate at labor market entry on the incidence of poverty, by demographic group. Results are based on the Mincerian specification (eq. [2]), using data from the Annual Social and Economic Supplement to the Current Population Survey from 1976 to 2016. A color version of this figure is available online.

study the effect of adverse initial conditions for multiple groups of workers. Not only did our data allow us to study workers that are at higher risk of lasting adverse consequences, but they also allowed us to analyze whether the adverse effects on earnings are buffered by the social insurance system for these less advantaged workers.

We confirm that all labor market entrants experience persistent reductions in earnings, employment, and wages from entering the labor market in a recession that last at least 10 years. We show that these effects are substantially larger for less advantaged workers, in particular high school dropouts and nonwhites, but that they are also present for high school graduates. The losses in earnings we find are partly offset by increases in the receipt of SNAP benefits for the least advantaged groups, reducing the impact on the reduction in household income. Nevertheless, our results imply that entering the labor market leads to persistent increases in poverty.

Overall, these findings help to complete the picture of persistent consequences of cyclical conditions for young workers. It becomes increasingly apparent that adverse early labor market conditions affect all groups in the population and influence many aspects of individual workers' socioeconomic outcomes. These findings highlight several important and as yet open

questions. We know relatively little so far as to the sources of the persistent reduction in employment and wages that we and others document. An important source of wage losses for college graduates appears to be a reduction in employer quality (Oreopoulos, von Wachter, and Heisz 2012). This is consistent with the fact that employment fluctuations are more pronounced at higher-paying employers (e.g, Kahn and McEntarfer 2014), leading to cyclical downgrading of labor (e.g., McLaughlin and Bils 2001). Similar forces are likely to be present for lower-skilled labor, who are at the bottom of the job ladder. Another important question concerns the longer-term consequences of adverse initial labor market conditions. Gibbons and Waldman (2006), for example, hypothesize that worse occupational outcomes and human capital accumulation makes these workers more vulnerable to future economic shocks. In contrast, findings by Schmieder and von Wachter (2010) suggest that below-average wages as a result of adverse initial conditions may reduce the chance of future layoff. Vulnerability or resilience may also be present in long-term health outcomes. The study of these and other questions must be postponed until data with additional information on career outcomes and longer time ranges are available.

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