How Increased Labor Demand at the Start of Your Career Can Improve Long Run Outcomes

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How Increased Labor Demand at the Start of Your Career Can Improve Long Run Outcomes*

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Abstract

The literature has traditionally focused on the local unemployment rate to estimate how initial economic conditions affect long-run outcomes. Using Job Openings and Labor Turnover Survey, or JOLTS, State Estimates for job openings, hires, and separations along with Local Area Unemployment Statistics, I test how changes in other aggregate measures of labor market activity affect long run outcomes. I find that for every one point increase in the local unemployed-to-job-opening ratio, annual earnings are reduced by 4.45% and remain depressed for over 13 years. Conversely, I find that a one percentage point increase in the local job openings rate or a one point increase in the local vacancy/unemployment ratio, *increases* initial annual earnings by 8.18% and 17.16%, respectively, which persists for nearly 11 years. I similarly find a positive and persistent effect on annual earnings from other measures of labor market tightness like the job-to-job transition rate, quits rate, job finding rate, and the labor-leverage ratio.

Keywords: wage scarring, labor discrimination, Job Openings and Labor Turnover Survey

JEL Codes: J11, J15, J16, J24, J31

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1 Introduction

The state of the labor market at the start of one's career can significantly influence long-run career outcomes. An extensive literature has shown[1] that increased unemployment during this period can lead to long-lasting negative outcomes like depressed employment, health, and earnings. This paper takes a novel approach to this issue by leveraging recently disaggregated state-level data from the Job Opening and Labor Turnover Survey (JOLTS) to test how other measures of initial labor market conditions, notably measures of labor market tightness, can affect outcomes over a career. The potential of this additional nuance between initial labor market conditions and long-run outcomes carries significant benefits. It not only enriches our academic understanding of wage formation but also arms policymakers with empirical evidence to make more informed decisions. In a Washington Post article[2] dated October 23, 2022, Federal Reserve Chair Jerome Powell noted that, "Vacancies are still almost at a 2-to-1 ratio to unemployed people [...] that, and quits, are really very good ways to look at how tight the labor market is and how different it is from other cycles [...] We think those things have, for quite a time now, really added value in terms of understanding where the labor market is." The comments underscore the importance of investigating long-term wage dynamics from changes in these measures as policymakers already rely on these variables to make decisions.

This paper delves deeper into this issue by building on Forsythe (2022b) and a method developed by Schwandt and Von Wachter (2019). I first investigate how annual earnings over a 15 year career respond to changes in the state-level unemployment rate, the unemployment-to-job openings ratio, the job openings rate, and vacancy-to-unemployment ratio at the point of labor market entry. In Figure 1, I show how national measures of the unemployment rate and the unemployed-to-job openings evolve over time. Similarly, in Figure 2, I show how na-

[1] See Kahn (2010), Oreopoulus et al (2012), Maclean (2013), Schwandt and Von Wachter (2019), Schwandt and Von Wachter (2020), Rinz (2021), Rothstein (2021), Forsythe (2022a).

[2] https://www.washingtonpost.com/business/2022/10/23/federal-reserve-job-vacancies-labor/

tional measures of the job openings rate and vacancy-to-unemployment (V/U) ratio change over time. During a recession, the unemployment rate and unemployed-to-job-openings ratio rise while the job openings rate and the V/U ratio typically fall. While the unemployment rate and unemployed-to-job-openings ratio measurements in Figure 1 are correlated, as are the job openings rate and V/U ratio measurements in Figure 2, there are also notable differences in the trajectory of these respective measurements, particularly around recessions, or the shaded regions of the figures. In Figure 3, I show how exploiting variation in these variables yields different long-term outcomes for workers. In line with the literature on initial economic conditions, I show that as the job market entry state-level unemployment rate rises, annual earnings in the first year of a career are depressed by 3%. As people gain experience, this disparity dissipates. With recently disaggregated JOLTS data, I am able to similarly measure the effect of a rise in the unemployed/job openings ratio, which yields a larger magnitude effect with similar persistence. Conversely, focusing on pure labor demand changes, I show that increases in the job market entry state-level job openings rate and V/U ratio lead to substantial *increases* in annual earnings in the first year. I also find evidence that these increases are persistent over a 15-year career. In Figure 4, I extend this analysis to other measures of labor market tightness, such as the job-to-job transition rate, quits rate, job finding rate, and labor-leverage ratio[3], and likewise find large positive effects that persist for years.

The significance of these variables in understanding wage dynamics has also been highlighted in recent literature. For instance, Autor, Dube, and McGrew (2023) emphasize the role of labor market tightness in wage compression and the increase in quit elasticity, while Domash and Summers (2022) consider different labor market indicators, including quit rates and vacancy rates, as predictors of wage inflation. Karahan et al. (2017) emphasize the role of job-to-job transitions in wage dynamics over the business cycle. While the unemployment

[3] Sojourner and DiVito (2022)

rate provides a useful starting point for understanding how initial economic conditions affect career earnings, these studies suggest a more nuanced approach, incorporating variables such as job openings, quits rates, and job-to-job transitions, would provide a much more comprehensive understanding of this phenomena. This paper's findings also provide crucial implications for policymakers, employers, and workers by offering a much more nuanced understanding of wage formation and persistence.

2 Conceptual Framework

2.1 Scarring

Scarring begins when inexperienced workers start their careers in a recession. In a healthy labor market, these workers may have multiple job offers and sort to jobs that best fit their abilities. However, in a recession, these workers face increased competition from an elevated labor supply as previously employed workers are laid off. These increases in available workers are also coupled with decreases in job openings. These changes in labor market conditions put downward pressure on wages. Inexperienced workers are the most sensitive to these market pressures, as they have no prior experience to draw on for negotiating leverage. The persistence of this effect is theoretically ambiguous though. Beaudry and Dinardo (1991) argue that in a spot labor market, any wage disparity from entering the labor market during a recession should disappear once the labor market recovers.

However, Kahn (2010) and Oreopoulus et al (2012) both show that this wage disparity can linger for 10 or 20 years. This could be the result of either job mismatch (Kahn 2010) or job search friction (Oreopoulus et al 2012). Job mismatch posits that workers who start their careers during a recession take jobs that are poorly matched to their skillset and potential. The subsequent on-the-job human capital they acquire is therefore less valuable. This results in a permanent loss in productivity, controlling for experience. Arellano-Bover (2022) emphasizes the critical role of initial labor market conditions on long-term skill development. Job search friction (Oreopoulus et al 2012) argues that the severity and length of a recession matters because workers face higher costs with switching jobs as they age. If a recession is severe and long enough, these workers are permanently scarred as they are less able to switch jobs after labor market conditions improve. An alternative perspective is that the job market may take initial job placement as a signal of ability but fail to account for the "luck" associated with initial market conditions, suggesting systematic irrational behavior in the labor market. This phenomenon, where even economists do not fully compensate for factors such as alphabetical ordering, may also extend to a failure to account for the effect of labor market conditions on first jobs (Oyer 2006). In Forsythe (2022a), a partially equilibrium model predicts that employers restrict hiring to more experienced workers when labor markets are slack.

2.2 Anticipated Effect from Changes in Labor Demand

If labor demand increases but labor supply stays constant, I expect *upward* pressure on wages as companies compete for a more limited labor supply pool. Similarly to what is experienced from increasing labor supply, only in reverse, inexperienced workers would be more sensitive to these changes as they have no prior experience for which to bargain. Therefore, I expect that an increase in labor demand, holding labor supply constant, would cause initial wages for inexperienced workers to also increase. However it is unclear whether this effect would persist. A spot labor market (Beaudry and Dinardo 1991) would predict that these changes go away in the next period, as employers cut costs after regaining negotiating power. Job mismatch (Kahn 2010) suggests that inexperienced individuals would sort to better (or near perfect) matches during this period and experience their full productive potential, so to speak. This would predict a sustained elevated effect for this cohort if the overall population did not enjoy this benefit at the start of their career. Job search friction (Oreopoulus et al 2012) suggests that these individuals may find really good jobs and similarly not move from these roles over time, possibly to prevent spoiling their good fortunes. Additionally, if labor markets are tight, employers may find it more costly to restrict job applicant pools, thus increasing opportunities for those otherwise restricted (Forsythe 2022b). Guo (2022) finds that workers with multiple offers enjoy a persistent wage premium of over 10% for about nine years.

There also could be a differential effect between different measures of labor market demand or tightness. In particular, there could be difference in variables like the state-level job openings rate and the state-level quits rate. Companies can decide to hire for two reasons. The primary reason is expansion. The employer wants to scale their operations and requires more employees to meet that goal. The other reason is to cover loss productivity from workers leaving. The latter situation, which is measured by the quits rate, is potentially more problematic and urgent for the employer. A company can delay expansion, and thus continue to be restrictive with whom they hire, much easier than they are able to recover loss productivity from workers quitting. If an employer is unable to meet the expected needs of its current customers, it can cease to exist. This suggests when the quits rate is rising, employers have less negotiating leverage. Therefore, the wage gains from a rising quits rate may be more substantial than the gains from a rising job openings rate. The effectiveness of quits in determining wages is also likely influenced by firm monopsony power. Webber (2023) finds labor supply elasticity to the firm declined significantly since the late 1990s, leading to a drop in earnings for the average worker.

3 Data

My primary data source is the 2001-2022 Current Population Survey, Annual Social and Economic Supplement, or the CPS-ASEC (Flood et al. 2023). These data provide basic demographic information like state, gender, age, race, and educational attainment. Using current year, age, and educational attainment, I impute potential experience[4] and approximate job-market-entry year[5] for each individual in the sample[6]. I then limit my sample to workers ages 16-39 with one to fifteen years of potential experience. My primary outcome variable is annual earnings, or the pre-tax wage and salary income from the previous calendar year, but I also examine hourly wages, hours worked per week, weeks employed last year, and food stamp receipt in the last year. Annual earnings and hourly wages are normalized to 2000 dollars using the Consumer Price Index for All Urban Consumers[7].

Similar to prior scarring literature (Oreopoulos et al. 2012; Schwandt and von Wachter 2019; Mask 2021), I aggregate outcomes to the level of current state of residence, job-market-entry year, gender, race, and educational attainment before conducting my analysis. Table 1 shows that after aggregating my sample to this identifying level of variation, I have 127,180 observations in order to conduct my analysis. The sample is 49.65% female, 74.87% caucasian, 25.29% high school graduates, and 30.37% college graduates. In Table 2, I provide averages for annual earnings, hourly wages, hours worked, weeks worked, and food stamp receipt across the entire sample. I also show how these averages differ across gender, race, and educational attainment.

For my main analysis, I also use 2001-2022 state-level Job Openings, Layoffs, and Turnover Survey data[8] along with 2001-2022 Local Area Statistics data[9] as the source of my treatment variables. JOLTS data were previously only regional and too noisy to identify labor demand effects. However, a recent implementation of a Extended Composite Synthetic Model[10] on regional data provides consistent and plausible estimates of state-level data for the 2000-2019

[4]Potential experience = age - years of education - 6

[5]Job-market-entry year = current year - potential experience

[6]Similar to the imputation method used in Genda et. al (2010) and Schwandt and Von Wachter (2019)

[7]https://fred.stlouisfed.org/series/CPIAUCSL

[8]https://www.bls.gov/jlt/jlt_statedata.htm

[9]https://www.bls.gov/lau/

[10]https://www.bls.gov/jlt/jlt_statedata_methodology.htm

period (Skopovi et al 2021 and Forsythe 2022b) with subsequent periods measured directly. Using these dis-aggregated data, I construct treatment variables for the unemployment rate, unemployed-to-job-openings ratio, job openings rate, and V/U ratio for each state-year combination between 2001 and 2022. These state-year combinations are then merged with the CPS-ASEC data according to each state-by-job-market-entry year combination.

4 Empirical Strategy

The primary challenge with using CPS-ASEC data to measure outcomes from initial economic conditions is that these data contain no variable that identifies the year nor state of job market entry. However, Schwandt and Von Wachter (2019) show that potential experience and current state-of-residence in the CPS-ASEC provide great approximations of the entry-year and entry-state. They test their estimates against historical graduation trends and state-to-state migration trends and show that the bias for estimates are negligible and towards zero. In Section 6.1, I replicate these tests to show that this method also works well when using alternative treatments like unemployed-to-job-openings ratio, job openings rate, or V/U ratio.

Following Schwandt and Von Wachter (2019) and Mask (2021), I use the following specification for Tables 3-12:

Specification 1: $y_{istge} = \alpha + \beta T_{0s} + \delta(T_{0s} \times \Phi_e) + \gamma X_{ist} + \Phi_s + \Phi_t + \Phi_g + \Phi_e + \varepsilon_{istge}$

This specification estimates the initial treatment effect, β, from an increase in the treatment variable, T_{0s}. The treatment variable, T_{0s}, is either the state-level unemployment rate, unemployed-to-job-opening-ratio, job openings rate, or V/U ratio from the imputed state and year of job market entry. The identifying assumption is that increases in these variables are exogenous to the outcome of the individual, which is plausible given the inability of one person or even a group of people to influence state-level aggregate data. δ measures how this effect changes as potential experience, Φ_e, increases. X_{ist} are controls for education, race, and gender. I also control for state fixed effects, Φ_s, contemporaneous year fixed effects[11], Φ_t, job-market-entry-year fixed effects, Φ_g, and potential experience, Φ_e. I weight my regressions using the ASECWT variable from IPUMS[12], which adjusts the sample to represent the civilian noninstitutionalized population of the United States. This ensures that my estimates are representative of the target population and not biased by the sampling design. Following Schwandt and Von Wachter (2019), all standard errors for regressions used in Tables 3-12 are clustered at the state-by-job-market-entry-year level. [13]

For Figure 3, I relax the functional form assumption in Specification 1 and measure how the treatment effect directly varies from potential experience year to potential experience year. To accomplish this, I use the following specification:

Specification 2:

$$\bar{y}_{istge} = \alpha + \sum_{j=1}^{15} \lambda_j (T_{0s} \times \Phi_e) + \gamma X_{ist} + \Phi_s + \Phi_t + \Phi_g + \Phi_e + \varepsilon_{istge}$$

Specification 2 is similar to Specification 1 except that the coefficient for the treatment effect, λ_j, is stratified across 1 to 15 years of potential experience. For every percentage point (or point) increase in T_{0s}, λ_1 represents wage losses/gains for workers with 1 year of experience, λ_2 represents wage losses/gains for workers with 2 years of experience, et cetera.

[11] Contemporaneous year fixed effects are used to control for contemporaneous economic conditions. In Section 6.5, I substitute contemporaneous year fixed effects for the contemporaneous state-of-residence-by-year unemployment rate to check how my estimates change when I control for more direct measures of contemporaneous economic conditions.

[12] Flood et al. (2023)

[13] In Section 6.2, I discuss how clustering my errors at the state level instead the state-by-job-market-entry-year level does little to change statistical significance of my estimates.

5 Results

5.1 Effect from Alternative Treatments

In Table 3, I estimate how an increase in the local unemployment rate, unemployed-to-job-opening ratio, job openings rate, and V/U ratio at the beginning of one's career affects annual earnings for 15 years. In Table 3, column 1, I show that a one percentage point increase in the job-market-entry year state unemployment rate initially reduces annual earnings by 3.01%, similar to estimates found in Schwandt and von Wachter (2019). This disparity is then reduced by 0.2% for each experience year, suggesting the initial scarring effect persists for approximately 13.09 years. In Table 3, column 2, I show a one point increase in the job-market-entry year state unemployed-to-job-openings ratio initially reduces annual earnings by 4.45% and then improves by 0.33% with each experience year, suggesting a 13.48 year persistence. In Table 3, column 3, I estimate that a one percentage point increase in the job-market-entry year state job openings rate initially *increases* annual earnings by 8.18%. This increase is then reduced by 0.73% for each experience year suggesting that this effect persists for 11.21 years. Finally, in Table 3, column 4, I estimate that a one point increase in the job-market-entry year state V/U ratio initially increases annual earnings by 17.16%[14]. This increase is then reduced by 1.72% for each year of experience which suggests that this effect also persists for nearly 11 years.

In Table 4, I evaluate the effect of initial labor market tightness on log annual earnings by considering different factors such as the job-to-job transition rate, quits rate, job finding rate, and labor-leverage ratio. Moscarini and Postel-Vinay (2017) proposed that labor demand is primarily transmitted to wage growth through Employer-to-Employer (EE) transitions. The more frequent EE transitions, the higher the pace of reallocation towards high wage jobs, and the higher the average wage growth. In Table 4, column 1, I show that a

[14] Note from Figure 2 that volatility in the V/U ratio is relatively small, usually between 0-0.5 in a non-recession year.

one percentage point increase in the job-to-job transition rate[15] initially increases log annual earnings by 2.31%. This effect then diminishes by 0.17% for each experience year. In Table 4, column 2, I find that a one percentage point increase in the quits rate initially boosts log annual earnings by 13.79% and then reduces by 1.32% with each experience year. In Table 4, column 3, I estimate that a one percentage point increase in the job finding rate initially enhances log annual earnings by 19.58%, with a subsequent reduction of 2.00% for each experience year. Finally, in Table 4, column 4, I estimate that a one point increase in the labor-leverage ratio[16] initially raises log annual earnings by 12.20%, with a reduction of 1.35% for each year of experience.

In Tables 5-8, I analyze the effect of each treatment on alternative outcomes: hourly wages, weeks worked, hours worked, and food stamp receipt. In Table 5, I show that increases in the state-level unemployment rate initially reduce hourly wages by 1.00%, weeks worked by 1.12%, and hours worked by 0.90%, and increase food stamp receipt by 0.52%. These disparities are then reduced by each additional potential experience year by 0.04% for hourly wages, 0.11% for weeks worked, and 0.08% for hours worked. In Table 6, I demonstrate how these outcomes vary with increases in the state-level unemployed/job openings ratio. A point increase in the unemployed/job openings ratio initially reduces hourly wages by 1.54%, weeks worked by 1.39%, and hours worked by 1.51%, and increases food stamp receipt by 0.62%. For each subsequent potential experience year, these effects are reduced by 0.08% for hourly wages, 0.11% for weeks worked, and 0.14% for hours worked.

[15] The job-to-job transition rate is not directly observed in JOLTS or Local Area Statistics data. I first estimate the state-by-job-market-entry-year job-to-job transition level by subtracting levels of layoffs/discharges, other separations, and unemployment inflow from the level of total separations (Job-to-Job transition level = Total Separations - Layoffs - Other Separations - Unemployment inflow). The level of layoffs/discharges and other separations level are both observed, but I must calculate the level of unemployment inflow from the previous period by taking the difference in unemployment levels between this year and last year. The resulting job-to-job transition level is then divided by the total employment level and multiplied by 100 to get the job-to-job transition rate.

[16] Sojourner and DiVito (2022)

In Tables 7 and 8, I show how changes in labor market tightness affect long run outcomes. In Table 7, I reveal that a percentage point increase in the job openings rate increases hourly wages by 2.24%, weeks worked by 2.57%, and hours worked by 3.73%, and decreases food stamp receipt by 1.09%. For each subsequent potential experience year, these effects are reduced by 0.23% for hourly wages, 0.16% for weeks worked, and 0.34% for hours worked. In Table 8, I show how changes in the state-level V/U ratio affect these alternate outcomes. For every point increase in the state-level V/U ratio at job market entry, hourly wages initially increase by 3.90%, weeks worked by 7.16%, and hours worked by 6.44%, while food stamp receipt decreases by 2.78%. These increases are then reduced by 0.27% for hourly wages, 0.76% for weeks worked, and 0.68% for hours worked for each subsequent potential experience year.

5.2 Heterogeneity between Education, Gender, and Race

The literature has analyzed how changes in the unemployment rate at labor-market entry affect individuals with college degrees (Kahn 2010; Oreopoulos et al. 2012; Altonji, Kahn, and Speer 2016), high school degrees (Hershbein 2012), youth (Forsythe 2022a, 2022b), gender (Choi et al. 2020), and race (Schwandt and Von Wachter 2019). Alhaif (2022) finds differential effects from labor tightening on the Black-White wage gap. In an effort to connect my estimates with the broader literature, for Tables 9-12, I show how my estimates differ across educational attainment, gender, and race.

In Table 9, columns 1 and 2, I show that changes in the local unemployment rate at labor market entry don't produce different estimates, -2.81% versus -2.86%, between individuals with high school degrees versus those with college degrees, and the effect dissipates at a rate of 0.18% for high school graduates and 0.22% for college graduates. However, in Table 9, columns 3 and 4, I show there is a larger initial decrease for men versus women, -3.78% versus -2.34%, and that the disparity dissipates slightly faster for men, 0.26% versus 0.20%, for each potential experience year. Finally, in Table 9, columns 5 and 6, I show this effect differs between whites and non-whites, -2.95% decrease versus -3.35%, and the disparity dissipates at a rate of 0.22% for whites and 0.28% for non-whites for each potential experience year.

In Table 10, I show how changes in the state-level unemployed/job openings ratio affect annual earnings between education, gender, and race. In Table 10, columns 1 and 2, I show there is a difference in the initial effect between high school graduates and college graduates, a decrease of -5.01% versus -3.72%. This disparity dissipates by 0.29% for both high school graduates and college graduates for each potential experience year. In Table 10, columns 3 and 4, I show there is also an initial difference between men and women, a decrease of -5.59% versus -3.37%, that dissipates by 0.39% and 0.27%, respectively, for each potential experience year. Finally, in Table 10, columns 5 and 6, I show a decrease in the unemployed/job openings ratio at labor market entry produces a decrease of -4.58% for whites and -4.25% for non-whites, that dissipates by 0.32% and 0.39%, respectively, for each potential experience year.

In Table 11, I show how changes in the state-level job openings rate at labor market entry affect annual earnings across education, gender, and race. I find substantial initial differences between high school graduates and college graduates (increases of 10.52% versus 5.76%), and men and women (increase of 9.24% versus 7.20%). However, in Table 11, columns 5 and 6, I see there is a large initial effect for whites, 10.36%, but I find no statistically significant effect for non-whites, 2.30%. The point estimate of 2.30% suggests there is some effect of the job-market-entry year state-level job opening rate for non-whites' annual earnings, but it is much less than the advantage enjoyed by whites. Across all groups, the initial increase in annual earnings for an elevated job-market-entry year state-level job openings rate is decreased by 0.63%-0.84% for each potential experience year.

Finally, in Table 12, I show how a change in the job-market-entry year state-level V/U (vacancy/unemployment) ratio affects annual earnings across education, gender, and race. I find substantial differences across each group. In Table 12, columns 1 and 2, I show that a one percentage point increase in the local V/U ratio increases initial annual earnings by 16.92% for high school graduates and 15.16% for college graduates. In Table 12, columns 3 and 4, I show that men enjoy an 18.56% initial increase in annual earnings versus only a 16.30% initial increase for women. In Table 12, columns 5 and 6, I show that whites enjoy an initial 20.40% increase in annual earnings from an increase in the job-market-entry year state-level V/U ratio, but non-whites only see a 7.76% statistically insignificant increase. Across all groups, the initial increase in annual earnings for an elevated job-market-entry year state-level V/U ratio is decreased by 1.42%-1.85% for each potential experience year.

6 Robustness Checks

6.1 Testing for Migration and Graduation Trends

The primary identification concern with this study is that the CPS-ASEC data do not contain any information on the year or state of job market entry. Following Schwandt and Von Wachter (2019), I estimate the year of job market entry based on self-reported age and educational attainment. This introduces a selection concern because past job market entrants could have delayed their labor market entry (such as staying an extra year in college) based on initial economic conditions. My estimates of labor market entry year conditions are therefore potentially based on the outcomes of both those who delay labor market entry from bad economic conditions and those who have no choice. If better advantaged groups, like college graduates, can delay labor market entry in a systemic way, then my estimates of the effect of local labor market conditions at entry would be based more on highly disadvantaged groups, potentially biasing my estimates away from zero and overstating the effect. The second identification concern is that the CPS-ASEC data does not contain a variable for the state that a person first entered the labor market. I impute the state of job market entry as being the same state as a person resides when they answer the CPS-ASEC survey. However, it is possible that a CPS-ASEC respondent has moved between the time they first entered the labor market and when they answer the survey. Furthermore, this decision to move could have been affected by the local labor market conditions one faced when they decided to enter. This concern would likely bias estimates towards zero, or understate the effect, as individuals harmed by adverse economic conditions from one state might migrate to another in response.

To test these concerns, I follow Schwandt and Von Wachter (2019) and use the 2000 decennial census data along with the 2001-2021 American Community Survey data (ACS)[17] to construct three alternative measures. The first measure, Census-Mincerian, tests the effect of each state-level treatment, the unemployment rate, unemployed/job openings ratio, job openings rate, and the V/U ratio, using Specification 1 from Section 4 on census and ACS data instead of the CPS-ASEC data. The second measure, Census-using state of birth, takes advantage of available birthplace information in the census and ACS data to measure this effect based on state of birth rather than current state. Finally, the third measure, Census-double weighted by age, is a prediction of the state-level unemployment rate at job-market-entry after accounting for both state-to-state migration trends between each cohort and historical graduation trends for high school and college graduates. This measure is much noisier but is especially useful for assessing bias away from zero, or overstating the effect, because it accounts for timing of labor market entry based on economic conditions.

In Figure 5, I show the results of these alternate measures. The first line, CPS-Mincerian, are the original estimates of the different treatment effects using the original CPS-ASEC data (Figure 3). This line serves as a baseline for the alternate measures. The goal is to assess whether there is bias and/or direction of bias within my original estimates from

[17] Ruggles et al 2022

incorrectly assuming when and where CPS-ASEC respondents first entered the labor market. The second measure, Census-mincerian, shows estimates from changes in the state-level unemployment rate, unemployed/job openings ratio, job openings rate, and V/U ratio at job-market-entry using Census/ACS data instead of CPS-ASEC data yield similar results across a 15-year career. Similarly, I find that estimates using the third measure, Census-using state of birth, also yield similar results despite the designated entry state being the state-of-birth instead of current state of residence. Finally, using the fourth measure, Census-double weighted by age, I find similar results for estimates when using the state-level unemployment rate and unemployed/job openings ratio treatments. However, for changes in the state-level job openings rate and V/U ratio, I find evidence to suggest that my main estimates, CPS-Mincerian, may be biased towards zero, or understating the effect. The double weighted measure accounts for both state-to-state migration and historical graduation trends, so I would only be concerned that I was overstating the effect if this measure was less than what I measure in CPS-Mincerian. While only one piece of evidence, the effect from an increasing job openings rate and V/U ratio may be more substantial than my estimates in Figure 3.

6.2 Clustering Errors by State

Column 1 of Tables 13-16 tests the robustness of the main results when clustering errors by state instead of by state-by-job-market-year. This methodological adjustment takes into account the possibility of correlated errors within individual states, reflecting shared economic, demographic, or policy characteristics. By clustering the standard errors at the state level, the analysis recognizes the potential non-independence of observations within the same state. This approach is critical in ensuring that the confidence intervals are appropriately widened to reflect this intra-state correlation, and more conservative statistical inference. The fact that the main results change little under this alternative clustering method lends further credence to the study's conclusions.

6.3 Dropping Pandemic Years

In column 2 of Tables 13-16, I drop the post-pandemic years (2019-2023) to assess whether the unique economic conditions during this time have any significant impact on the main findings. The global Covid-19 pandemic brought about unprecedented labor market conditions, which could potentially skew the general patterns identified in the study. By excluding these years, I find that the observed relationships are not artifacts of these extraordinary circumstances but rather reflect more typical labor market dynamics. The robustness of the results to this exclusion confirms the study's relevance and applicability beyond a specific context.

6.4 Fixed Effects for State-by-Year and Education-by-Year

In Tables 13-16, Column 3 incorporates state-by-year and education-by-year fixed effects, building upon Specification 1 detailed in Section 4. This inclusion refines the analysis by accounting for potential time-related variations within states and education levels. By addressing these trends, we can better mitigate unobserved fluctuations that might systematically affect states or educational levels over time. Notably, introducing these controls diminishes the size of my estimates, indicating that these trends could introduce some bias and thus should be considered.

6.5 Replacing Year Fixed Effects with Unemployment Rate

In column 4 of Tables 13-16, I again change Specification 1 detailed in Section 4 by replacing the contemporaneous year fixed effects with the contemporaneous year unemployment rate. This substitution allows my estimates to vary based on contemporaneous macroeconomic conditions, rather than controlling for all time-specific effects through year fixed effects. The stability of the results under this alternative specification provides evidence that the observed relationships are not sensitive to these variables.

6.6 Restricting the Sample Based on Labor Market Entry

Columns 5 and 6 in Tables 13-16 divide the sample between individuals who entered the labor market before and after the 2009 global financial crisis, respectively. This division provides a nuanced view of how economic conditions at entry affect different cohorts. By comparing these two distinct periods, this approach helps isolate the impact of general economic conditions from the specific effects related to the financial crisis. While there are notable differences in statistical power between the estimates in Columns 5 and 6 of Tables 13-16, there aren't any qualitative differences in estimates derived from pre-2007 only data and post-2007 only data.

7 Conclusion

In this paper, I provide evidence that changes in labor demand at the start of one's career can lead to substantial changes in earnings and employment. For every percentage point increase in the state-level job openings rate, initial annual earnings increase by 8.18%. This increase reverts back to the mean at a rate of 0.73% per year of experience, suggesting that this effect can last over a decade. For every percentage point increase in the state-level V/U ratio, initial annual earnings increase by 17.16%. Likewise, this increase diminishes linearly at a rate of 1.72%, suggesting this effect can last over a decade. These results provide evidence that long run outcomes from initial economic conditions are highly sensitive to changes in labor demand. The literature should therefore consider the inclusion of JOLTS state-level data when analyzing these effects.

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8 Figures

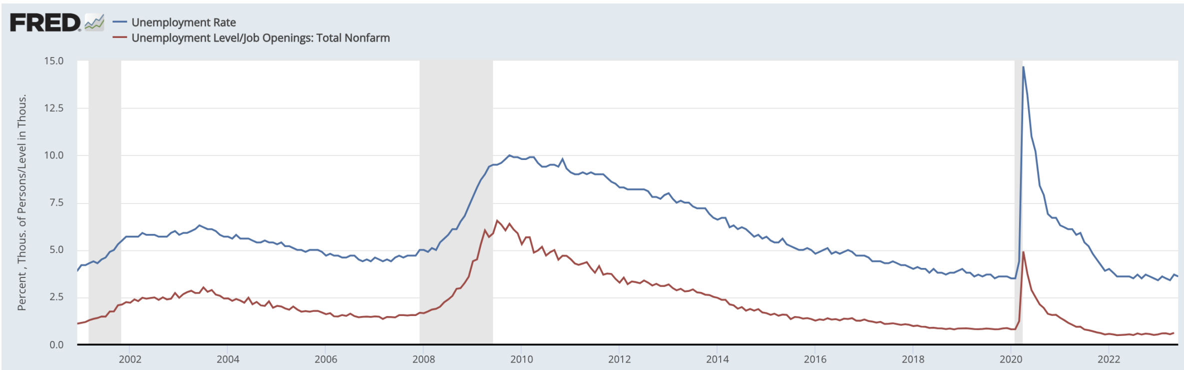

Figure 1: Changes in Unemp. Rate and Unemployed/Job Opening Ratio (2000-2023)[18]

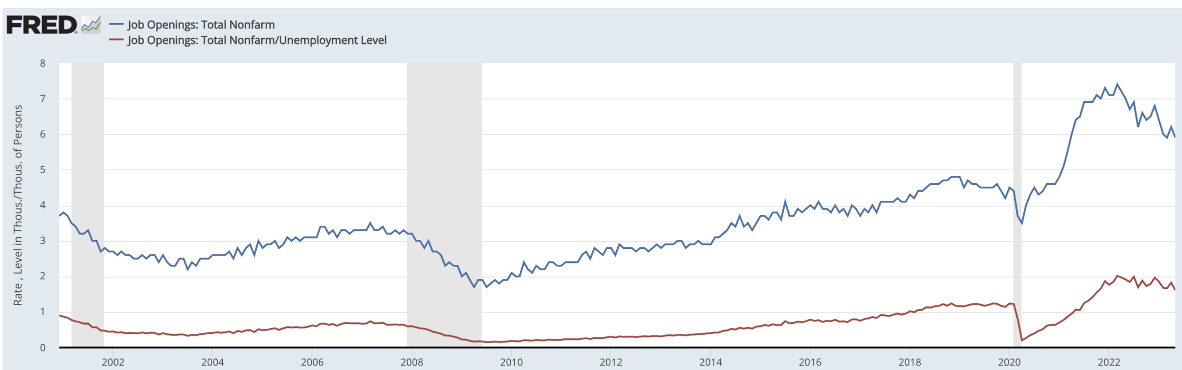

Figure 2: Changes in Job Openings Rate and Vacancy/Unemployment Ratio (2000-2023)[19]

[18]In Figure 1, I compare changes in the national Unemployment rate and Unemployed/Job Openings ratio between 2000-2023, the years covered in my main analysis (Table 3). Figure 2 offers a similar multi-decade comparison of changes in the national Job Openings rate and Vacancy/Unemployment ratio. Although the respective variables in each figure are correlated, there are notable differences in the trajectory of both variables, particular around recessions (shaded regions).

Data and graphs are courtesy the Federal Reserve Bank of St. Louis, or FRED: `https://fred.stlouisfed.org/series/UNRATE`, `https://fred.stlouisfed.org/series/JTSJOR`, `https://fred.stlouisfed.org/series/UNEMPLOY`, `https://fred.stlouisfed.org/series/JTSJOL`, July 29, 2023.

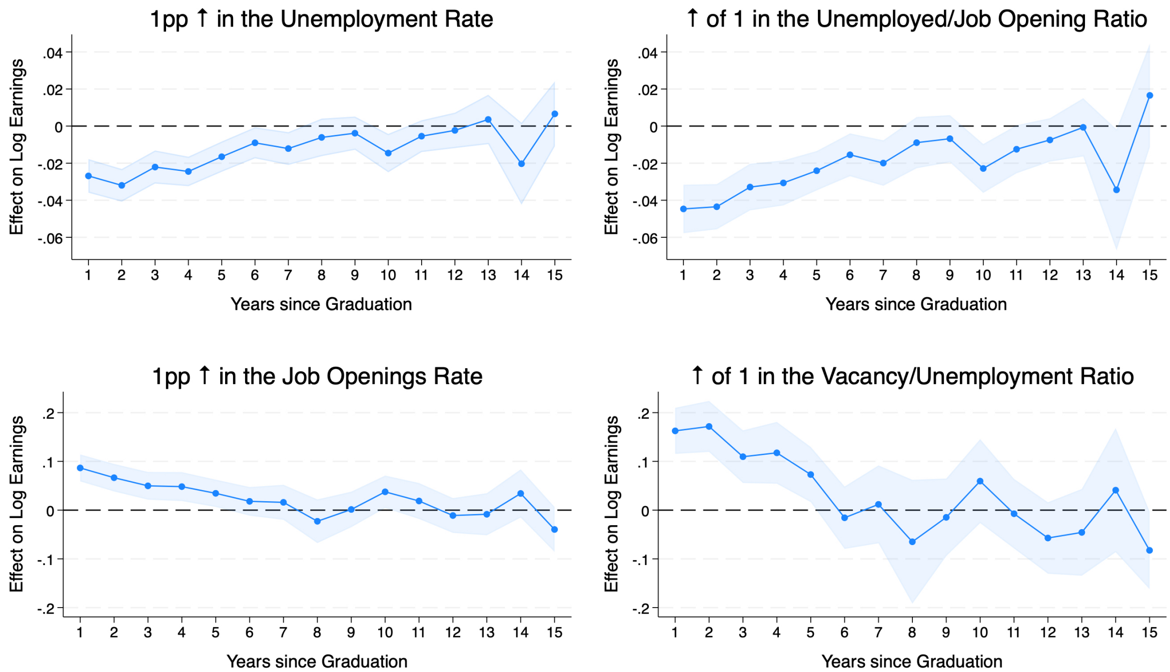

Figure 3: How Initial Labor Market Conditions Affect Career Earnings (Main Results)

[20]This paper aims to understand how changes in various labor market indicators at the onset of a career, beyond the unemployment rate, can influence outcomes 15 years down the line. Leveraging data from current and potential experience, state-level labor market trends, and Job Openings and Labor Turnover Survey (JOLTS), I calculate the local unemployment rate, the ratio of unemployed individuals to job openings, the job openings rate, and the vacancy-to-unemployment ratio for each Current Population Survey Annual Social and Economic Supplement (CPS-ASEC) respondent at the time they entered the job market between 2001-2022. I then estimate the effect of changes in these initial economic conditions on annual earnings over the course of a 15-year career.

Figure 4: Initial Labor Market Tightness and Career Earnings: Alternate Measurements

[21]Similar to Figure 3, in Figure 4 I estimate how changes in four alternate state-level measures of initial labor market tightness, the job-to-job transition rate, quits rate, job finding rate, and the labor-leverage ratio, can affect annual earnings over a 15-year career.

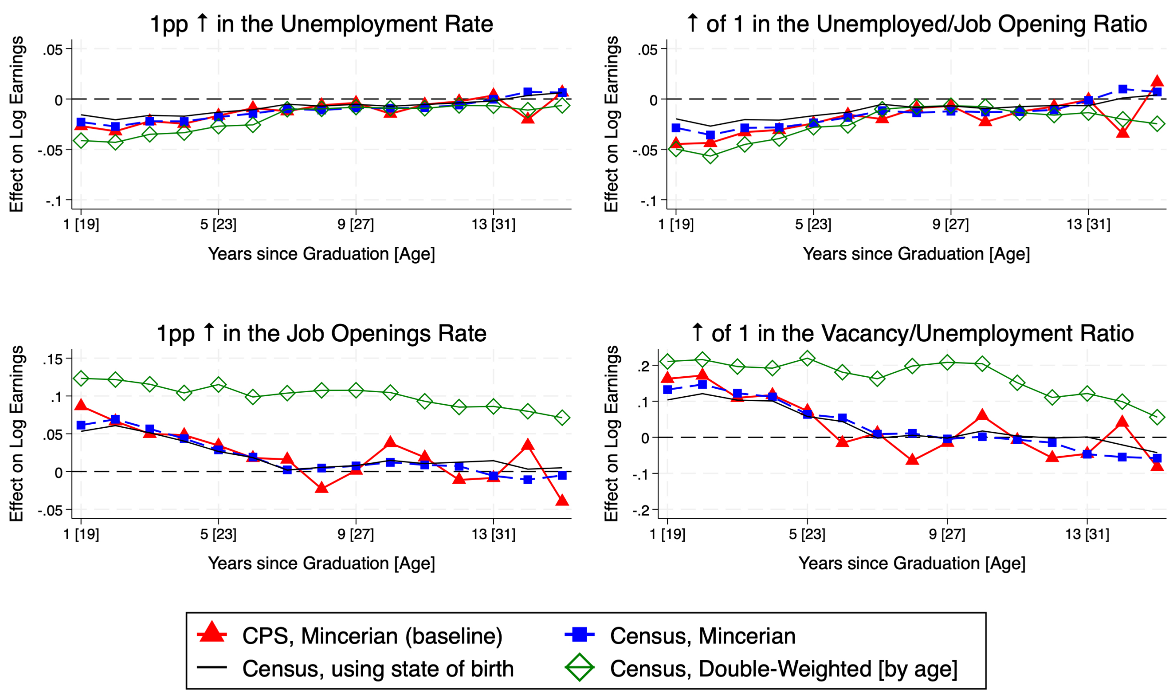

Figure 5: Robustness Test for Timing and Migration

[22]This figure serves as robustness check for the estimates derived in Figure 3. I do not observe state-of-job-market-entry and year-of-job-market-entry directly in the CPS ASEC, so I must rely on a method developed by Schwandt and Von Wachter (2019) to impute these variables from state-of-residence and potential years of experience. The CPS-Mincerian estimates, represented in red, are directly derived from Figure 3 and act as a reference point for the other three measures used in the figure. This comparison allows us to identify any potential biases from incorrect assumptions about the year and state of job-market entry in the CPS-ASEC data.

The Census-Mincerian estimates, indicated in blue, are a replication of the original estimates, but using data from the 2000 US Census and 2001-2021 American Communicty Survey (ACS) rather than the CPS-ASEC. The Census-using state of birth estimates, denoted in black, also replicate the original, but use the state-of-birth instead of the current state of residence as the state for job-market entry.

The final measure, Census-double weighted by age, depicted in green, factors in both historical state-to-state migration and graduation trends. Age is paired with the average number of years of experience, and these pairs are marked with square brackets on the x-axis. This measure addresses both migration and graduation trends, and it provides the upper limit for the estimates. The magnitude of this measure is significantly larger than my main estimates for both job openings rates and vacancy/unemployment ratio, suggesting that my primary results in Figure 3 may potentially underestimate the overall effect of increased labor market tightness.

9 Tables

Table 1: **Sample Summary Table**

	Mean
% Female	49.65
% Caucasian	74.87
% High School Graduates	25.29
% College Graduates	30.37
Observations	127180

Table 2: **Sample Summary Table by Outcomes**

	Mean Annual Earnings	Mean Hourly Wages	Mean Hours Worked	Mean Weeks Worked	Food Stamp Receipt
Full Sample	\$ 18,198.26	\$ 13.77	35.58	32.76	11.53%
Men	\$ 20,808.53	\$ 14.17	37.03	34.26	9.58%
Women	\$ 15,551.74	\$ 13.36	34.09	31.24	13.50%
White	\$ 18,791.54	\$ 13.73	35.60	33.84	9.43%
Non-White	\$ 16,430.92	\$ 13.92	35.51	29.53	17.77%
High School	\$ 11,984.36	\$ 10.27	36.62	32.01	18.27%
College	\$ 34,867.01	\$ 20.95	40.37	42.34	2.62%

[23]Summary descriptions of the 2001-2022 CPS ASEC data used in my analysis. Similar to prior scarring literature (Oreopoulos et al. 2012; Schwandt and von Wachter 2019; Mask 2021), data are aggregated to the level of current state of residence, job- market-entry year, gender, race, and educational attainment before conducting my analysis. After aggregation, I have 127,180 observations in order to conduct my analysis.

Table 3: **Main Results**

	(1) Log Annual Earnings	(2) Log Annual Earnings	(3) Log Annual Earnings	(4) Log Annual Earnings
Unemployment Rate	-0.0301***			
	(0.0039)			
Unemployment Rate × exp	0.0023***			
	(0.0004)			
Unemployed/Job Opening		-0.0445***		
		(0.0054)		
Unemployed/Job Opening × exp		0.0033***		
		(0.0006)		
Job Openings Rate			0.0818***	
			(0.0131)	
Job Openings Rate × exp			-0.0073***	
			(0.0013)	
Vacancy/Unemployment				0.1716***
				(0.0213)
Vacancy/Unemployment × exp				-0.0172***
				(0.0026)
Observations	113444	113444	113444	113444
Adjusted R^2	0.703	0.703	0.703	0.703

Standard errors clustered at the state-by-job-market-entry-year level.

+ 0.1, * 0.05, ** 0.01, *** 0.001

[24] This table serves as a complement to Figure 3 and provides precise estimates of my main results. For each labor market indicator, unemployment rate, unemployed/job opening ratio, job openings rate, and vacancy/unemployment ratio, the first estimate represents the initial effect on log annual earnings of a one percentage point (or one point for ratios) increase in the variable at labor market entry. The second estimate, indicator X exp, represents the linear decay of the effect for each year of experience.

Table 4: **Effect of Initial Labor Market Tightness on Log Annual Earnings:**

	(1) Log Annual Earnings	(2) Log Annual Earnings	(3) Log Annual Earnings	(4) Log Annual Earnings
Job-to-Job Transition Rate	0.0231***			
	(0.0031)			
Job-to-Job Transition Rate × exp	-0.0017***			
	(0.0003)			
Quits Rate		0.1379***		
		(0.0179)		
Quits Rate × exp		-0.0132***		
		(0.0017)		
Job Finding Rate			0.1958***	
			(0.0239)	
Job Finding Rate × exp			-0.0200***	
			(0.0023)	
Labor-Leverage Ratio				0.1220***
				(0.0227)
Labor-Leverage Ratio × exp				-0.0135***
				(0.0024)
Observations	113444	113444	113444	113444
Adjusted R^2	0.703	0.703	0.703	0.703

Standard errors clustered at the state-by-job-market-entry-year level.

+ 0.1, * 0.05, ** 0.01, *** 0.001

[25]This table serves as a complement to Figure 4 and provides precise estimates. For each labor market tightness indicator, job-to-job transtion rate, quits rate, job finding rate, labor-leverage ratio (Sojourner and DiVito 2022), the first estimate represents the initial effect on log annual earnings of a one percentage point (or one point for ratios) increase in the variable at labor market entry. The second estimate, indicator X exp, represents the linear decay of the effect for each year of experience.

Table 5: **Effect of Unemployment Rate on Alternate Outcomes**

	(1) Log Hourly Wages	(2) Log Weeks Worked	(3) Log Hours Worked	(4) Food Stamp Receipt
Unemployment Rate	-0.0100***	-0.0112***	-0.0090***	0.0052***
	(0.0023)	(0.0024)	(0.0018)	(0.0009)
Unemployment Rate × exp	0.0004	0.0011***	0.0008***	0.0000
	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Observations	113444	114292	114292	127180
Adjusted R^2	0.542	0.418	0.525	0.190

Standard errors clustered at the state-by-job-market-entry-year level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 6: **Effect of Unemployed-to-Job-Opening Ratio on Alternate Outcomes**

	(1) Log Hourly Wages	(2) Log Weeks Worked	(3) Log Hours Worked	(4) Food Stamp Receipt
Unemployed/Job Opening	-0.0154***	-0.0139***	-0.0151***	0.0062***
	(0.0031)	(0.0035)	(0.0025)	(0.0014)
Unemployed/Job Opening × exp	0.0008**	0.0011**	0.0014***	0.0001
	(0.0003)	(0.0003)	(0.0002)	(0.0001)
Observations	113444	114292	114292	127180
Adjusted R^2	0.542	0.417	0.525	0.190

Standard errors clustered at the state-by-job-market-entry-year level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

[26] In Tables 5-8, Column 1 represents hourly wages, which is calculated from CPS ASEC data by dividing annual earnings by (usual hours worked last year*weeked employed in the last year). Column 2 represents usual hours worked last year. Column 3 represents of weeks employed in the last year. Finally, column 4 represents an indicator that is 1 if a household member has received SNAP benefits in the past year.

Table 7: **Effect of Job Openings Rate on Alternate Outcomes**

	(1) Log Hourly Wages	(2) Log Weeks Worked	(3) Log Hours Worked	(4) Food Stamp Receipt
Job Openings Rate	0.0224*	0.0257**	0.0373***	-0.0109***
	(0.0088)	(0.0079)	(0.0056)	(0.0032)
Job Openings Rate × exp	-0.0023**	-0.0016*	-0.0034***	-0.0004
	(0.0007)	(0.0008)	(0.0006)	(0.0003)
Observations	113444	114292	114292	127180
Adjusted R^2	0.542	0.417	0.525	0.190

Standard errors clustered at the state-by-job-market-entry-year level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 8: **Effect of Vacancy/Unemployment Ratio on Alternate Outcomes**

	(1) Log Hourly Wages	(2) Log Weeks Worked	(3) Log Hours Worked	(4) Food Stamp Receipt
Vacancy/Unemployment	0.0390**	0.0716***	0.0644***	-0.0278***
	(0.0148)	(0.0142)	(0.0109)	(0.0060)
Vacancy/Unemployment × exp	-0.0027+	-0.0076***	-0.0068***	-0.0000
	(0.0016)	(0.0014)	(0.0011)	(0.0007)
Observations	113444	114292	114292	127180
Adjusted R^2	0.542	0.417	0.525	0.190

Standard errors clustered at the state-by-job-market-entry-year level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 9: **Effect of Unemployment Rate on Annual Earnings by Education, Gender, and Race**

	(1) HS	(2) College	(3) Male	(4) Female	(5) White	(6) Non-White
Unemployment Rate	-0.0281***	-0.0286***	-0.0378***	-0.0234***	-0.0295***	-0.0335***
	(0.0066)	(0.0045)	(0.0054)	(0.0051)	(0.0048)	(0.0097)
Unemployment Rate × exp	0.0018**	0.0022***	0.0026***	0.0020***	0.0022***	0.0028**
	(0.0007)	(0.0004)	(0.0005)	(0.0005)	(0.0005)	(0.0009)
Observations	47012	66432	57410	56034	70320	43124
Adjusted R^2	0.628	0.555	0.737	0.707	0.752	0.585

Standard errors clustered at the state-by-job-market-entry-year level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 10: **Effect of Unemployed/JO Ratio on Annual Earnings by Education, Gender, and Race**

	(1) HS	(2) College	(3) Male	(4) Female	(5) White	(6) Non-White
Unemployed/Job Opening	-0.0501***	-0.0372***	-0.0559***	-0.0337***	-0.0458***	-0.0425**
	(0.0100)	(0.0061)	(0.0076)	(0.0067)	(0.0062)	(0.0133)
Unemployed/Job Opening × exp	0.0029***	0.0029***	0.0039***	0.0027***	0.0032***	0.0039***
	(0.0009)	(0.0006)	(0.0007)	(0.0006)	(0.0006)	(0.0011)
Observations	47012	66432	57410	56034	70320	43124
Adjusted R^2	0.628	0.555	0.738	0.707	0.752	0.585

Standard errors clustered at the state-by-job-market-entry-year level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

[27] In Tables 9-12, column 1 represents estimates for high school graduates, column 2 represents estimates for college graduates, column 3 represents estimates for male respondents, column 4 represents estimates for female respondents, column 5 represents estimates for white respondents, and column 4 represents estimates for non-white respondents.

Table 11: **Effect of Job Openings Rate on Annual Earnings by Education, Gender, and Race**

	(1) HS	(2) College	(3) Male	(4) Female	(5) White	(6) Non-White
Job Openings Rate	0.1052***	0.0576***	0.0924***	0.0720***	0.1036***	0.0230
	(0.0198)	(0.0157)	(0.0191)	(0.0164)	(0.0142)	(0.0292)
Job Openings Rate × exp	-0.0068**	-0.0064***	-0.0084***	-0.0063***	-0.0077***	-0.0065*
	(0.0021)	(0.0013)	(0.0017)	(0.0015)	(0.0014)	(0.0026)
Observations	47012	66432	57410	56034	70320	43124
Adjusted R^2	0.628	0.555	0.737	0.707	0.752	0.585

Standard errors clustered at the state-by-job-market-entry-year level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 12: **Effect of VU Ratio on Annual Earnings by Education, Gender, and Race**

	(1) HS	(2) College	(3) Male	(4) Female	(5) White	(6) Non-White
Vacancy/Unemployment	0.1692***	0.1516***	0.1856***	0.1630***	0.2040***	0.0776
	(0.0354)	(0.0260)	(0.0321)	(0.0311)	(0.0233)	(0.0606)
Vacancy/Unemployment × exp	-0.0169***	-0.0142***	-0.0185***	-0.0165***	-0.0178***	-0.0162**
	(0.0047)	(0.0026)	(0.0033)	(0.0030)	(0.0028)	(0.0053)
Observations	47012	66432	57410	56034	70320	43124
Adjusted R^2	0.628	0.555	0.737	0.707	0.752	0.585

Standard errors clustered at the state-by-job-market-entry-year level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 13: **Robustness Checks for Unemployment Rate**

	(1) Cluster Errors by State	(2) Drop Pandemic (2019-2023)	(3) Control for State/Educ Trends	(4) Control for Current Unemp.	(5) Restrict gradyear to 2000-2007	(6) Restrict gradyear to 2008-2022
Unemployment Rate	-0.0301***	-0.0324***	-0.0151***	-0.0198***	-0.0160+	-0.0308***
	(0.0039)	(0.0043)	(0.0039)	(0.0038)	(0.0094)	(0.0056)
Unemployment Rate × exp	0.0023***	0.0028***	0.0014***	0.0020***	0.0027**	0.0022***
	(0.0005)	(0.0005)	(0.0004)	(0.0004)	(0.0009)	(0.0006)
Observations	113444	91039	113444	113444	57892	55552
Adjusted R^2	0.703	0.710	0.704	0.704	0.677	0.716

Standard errors clustered at the state level for column 1.
Standard errors cluster at state-by-job-market-entry-year level for columns 2-6.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 14: **Robustness Checks for Unemployed/Job Opening Ratio**

	(1) Cluster Errors by State	(2) Drop Pandemic (2019-2023)	(3) Control for State/Educ Trends	(4) Control for Current Unemp.	(5) Restrict Unemp. 2000-2007	(6) Restrict gradyear to 2008-2022
Unemployed/Job Opening	-0.0445***	-0.0464***	-0.0240***	-0.0303***	-0.0257+	-0.0447***
	(0.0051)	(0.0061)	(0.0053)	(0.0051)	(0.0155)	(0.0068)
Unemployed/Job Opening × exp	0.0033***	0.0039***	0.0020***	0.0028***	0.0046**	0.0033***
	(0.0006)	(0.0007)	(0.0005)	(0.0005)	(0.0014)	(0.0008)
Observations	113444	91039	113444	113444	57892	55552
Adjusted R^2	0.703	0.710	0.704	0.704	0.677	0.716

Standard errors clustered at the state level for column 1.
Standard errors cluster at state-by-job-market-entry-year level for columns 2-6.
+ 0.1, * 0.05, ** 0.01, *** 0.001

[28]Tables 13-16 present a series of robustness checks for the main estimates provided in Table 3. In these tables, the first column offers estimates obtained by clustering errors at the state level, rather than at the more preferred level of state-by-job-market-entry-year. The second column provides estimates derived from data limited to the years preceding the Covid-19 pandemic. In the third column, estimates are adjusted to account for state-by-contemporaneous-year and education-by-contemporaneous-year fixed effects. The fourth column replaces the preferred contemporaneous year fixed effect with controls for the contemporaneous year state unemployment rate. Columns 5 and 6 provide insights into which business cycle periods have the most substantial influence on my estimates. Column 5 narrows the data to graduates from the 2000-2007 period, a time frame encompassing a recession in 2000-2001 followed by a period of economic expansion until 2007. Column 6 constrains the data to graduates from the 2008-2022 period, which includes a recession in 2008-2009, economic expansion up to 2020, and another recession triggered by the Covid-19 pandemic.

Table 15: **Robustness Checks for Job Openings Rate**

	(1) Cluster Errors by State	(2) Drop Pandemic (2019-2023)	(3) Control for State/Educ Trends	(4) Control for Current Unemp.	(5) Restrict gradyear to 2000-2007	(6) Restrict gradyear to 2008-2022
Job Openings Rate	0.0818***	0.0754***	0.0511***	0.0601***	0.0245	0.0938***
	(0.0155)	(0.0152)	(0.0126)	(0.0122)	(0.0193)	(0.0198)
Job Openings Rate × exp	-0.0073***	-0.0079***	-0.0046***	-0.0060***	-0.0062**	-0.0082***
	(0.0016)	(0.0016)	(0.0011)	(0.0011)	(0.0019)	(0.0025)
Observations	113444	91039	113444	113444	57892	55552
Adjusted R^2	0.703	0.709	0.704	0.704	0.677	0.716

Standard errors clustered at the state level for column 1.
Standard errors cluster at state-by-job-market-entry-year level for columns 2-6.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 16: **Robustness Checks for Vacancy/Unemployment Ratio**

	(1) Cluster Errors by State	(2) Drop Pandemic (2019-2023)	(3) Control for State/Educ Trends	(4) Control for Current Unemp.	(5) Restrict gradyear to 2000-2007	(6) Restrict gradyear to 2008-2022
Vacancy/Unemployment	0.1716***	0.1842***	0.0935***	0.1243***	0.0828*	0.1662***
	(0.0295)	(0.0273)	(0.0241)	(0.0234)	(0.0356)	(0.0335)
Vacancy/Unemployment × exp	-0.0172***	-0.0211***	-0.0111***	-0.0148***	-0.0152***	-0.0185***
	(0.0037)	(0.0030)	(0.0024)	(0.0023)	(0.0038)	(0.0055)
Observations	113444	91039	113444	113444	57892	55552
Adjusted R^2	0.703	0.709	0.704	0.704	0.677	0.716

Standard errors clustered at the state level for column 1.
Standard errors cluster at state-by-job-market-entry-year level for columns 2-6.
+ 0.1, * 0.05, ** 0.01, *** 0.001