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 one faces at the beginning of their career to measure how initial economic conditions affect long-run outcomes. However, the unemployment rate moves in response to changes in labor supply or labor demand. Using JOLTS State Estimates for job openings, hires, and separations along with Local Area Unemployment Statistics, I test how changes in more direct measures of demand at labor market entry 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 4.53% and remain depressed for 13 years. Conversely, I find that a one percentage point increase in the local job openings rate or the local quits rate, *increases* initial annual earnings by 8.15% and 14.23%, respectively.

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

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

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1 Introduction

It is well established in the labor economics literature that increased unemployment at the beginning of one's career can depress outcomes like employment, health, and earnings[1]. For some outcomes, like annual earnings, these effects can also persist for a long time. It is unclear, however, how much these changes reflect changes in labor supply and labor demand. The unemployment rate is best thought to be a measure of slack and reflects changes in both supply and demand, particularly in highly volatile situations like recessions. This presents a missing variables problem if the source of change is only one side of the labor market. Forsythe (2022b) shows that an unusual increase in labor market tightness, as measured by the stock of vacancies divided by the stock of unemployed, during the Covid-19 recession explains why youth unemployment was less severe than in past recessions. Following work by Forsythe (2022b), in addition to a method developed by Schwandt and Von Wachter (2019), this paper aims to further bridge this gap by taking advantage of recently developed state-level estimates of variables within the Job Openings and Labor Turnover Survey[2]. In addition to measuring how annual earnings respond to changes in the state-level unemployment rate and unemployment-to-job openings ratio at labor market entry, I also analyze how pure changes in labor demand like the job openings rate and quits rate affect these outcomes.

In Figure 1, I show how national measures of the unemployment rate, unemployed-to-job openings ratio, job openings rate, and quits rate change over time. During a recession, the unemployment rate and unemployed-to-job-openings ratio rise while the job openings rate and quits rate typically fall. In Figure 2, I show the main results of my analysis. In line with the previous literature, I show that as the job market entry state-level unemployment rate and unemployed/job openings ratio rise, annual earnings in the first year of a career are depressed by 3-5%. As people gain experience, this disparity dissipates. Conversely, focusing

[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.bls.gov/jlt/

on pure labor demand changes, I show that increases in the job market entry state-level job openings rate and quits rate 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.

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 have been laid off. These shifts 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. 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. In Forsythe (2022a), a partially equilibrium model predicts that employers restrict hiring to more experienced workers when labor markets are slack. However, 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).

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 find really good jobs and similarly would not move from these roles over time, preventing them from spoiling their good fortunes.

There also could be a differential effect between my two primary labor demand variables, 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.

3 Data

My primary data source is the 2001-2021 Current Population Survey, Annual Social and Economic Supplement, or the CPS-ASEC (Ruggles et al. 2022). 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[3] and approximate job-market-entry year[4] for each individual in the sample. 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, and weeks employed last year. Annual earnings and hourly wages are normalized to 2000 dollars using the Consumer Price Index for All Urban Consumers[5].

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 118,858 observations in order to conduct my analysis. The sample is 49.63% female, 74.98% caucasian, 25.15% high school graduates, and 29.92% college graduates. In Table 2, I provide averages for annual earnings, hourly wages, hours worked, and weeks worked across the entire sample. I also show how these averages differ across gender, race, and educational attainment.

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

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

[5]`https://fred.stlouisfed.org/series/CPIAUCSL`

For my main analysis, I also use 2001-2020 state-level Job Openings, Layoffs, and Turnover Survey data[6] along with 2001-2020 Local Area Statistics data[7] 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[8] on regional data provides consistent and plausible estimates of state-level data for the 2000-2019 period (Skopovi, S. et al 2021 and Forsythe 2022b). Using these dis-aggregated data, I construct treatment variables for the unemployment rate, unemployed-to-job-openings ratio, job openings rate, and quits rate for each state-year combination between 2001 and 2020. 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 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, 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 quits rate.

Following Schwandt and Von Wachter (2019) and Mask (2021), I use the following specification for Tables 3-11:

[6] `https://www.bls.gov/jlt/jlt_statedata.htm`
[7] `https://www.bls.gov/lau/`
[8] `https://www.bls.gov/jlt/jlt_statedata_methodology.htm`

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 quits rate 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, year fixed effects, Φ_t, job-market-entry-year fixed effects, Φ_g, and potential experience, Φ_e.

For Figure 2, 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.

5 Results

5.1 Effect from Alternative Treatments

In Table 3, I estimate how a change in the local unemployment rate, unemployed-to-job-opening ratio, job openings rate, and quits rate 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 3.11 percent, similar to estimates found in Schwandt and von Wachter (2019). This disparity is then reduced by 0.24 percent for each experience year, suggesting the initial scarring effect persists for approximately 13 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 4.53 percent and then improves by 0.34 percent with each experience year, also suggesting a 13 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 8.15%. This increase is then reduced by 0.75% for each experience year suggesting that this effect lasts nearly 11 years. Finally, in Table 3, column 4, I estimate that a one percentage point increase in the job-market-entry year state quits rate initially *increases* annual earnings by 14.23%[9]. This increase is then reduced by 1.35% for each year of experience which suggests that this effect also persists for nearly 11 years.

In Tables 4-7, I analyze the effect of each alternate treatment on alternative outcomes: hourly wages, weeks worked, and hours worked. Similar to estimates found in Schwandt and Von Wachter (2019), I show in Table 4 that increases in the state-level unemployment rate initially reduce hourly wages by 1-1.5%, weeks worked by 1-1.5%, and hours worked by 0.5-1%. These disparities are then reduced by each additional potential experience year by 0.04% for hours worked, 0.11% for weeks worked, and 0.08% for hours worked. In Table 5,

[9]Note from Figure 1 that volatility in the quits rate is relatively small, usually between 0.1-0.3% in a non-recession year.

I show 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 1.63%, weeks worked by 1.37%, and hours worked by 1.52%. For each subsequent potential experience year, these effects are reduced by 0.09% for hourly wages, 0.11% for weeks worked, and 0.14% for hours worked.

In Tables 6 and 7, I show how pure changes in labor demand affect long run outcomes. In Table 6, I show that a percentage point increase in the job openings rate increases hourly wages by 2.6%, weeks worked by 2.17%, and hours worked by 3.80%. For each subsequent potential experience year, these effects are reduced by 0.28% for hourly wages, 0.12% for weeks worked, and 0.34% for hours worked. In Table 7, I show how changes in the state-level quits rate affects these alternate outcomes. For every percentage point increase in the state-level quits rate at job market entry, hourly wages initially increase by 3.29%, weeks worked by 5.30%, and hours worked by 5.26%. These increases are then reduced by 0.43% for hourly wages, 0.41% for weeks worked, and 0.49% 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 affects individuals with college degrees (Kahn 2010; Oreopoulus et al. 2012; Altonji, Kahn, and Speer 2016), high school degrees (Hershbein 2012), youth (Forsythe 2022a, 2022b), gender (Choi, E. J. et al 2020), and race (Schwandt and Von Wachter 2019). In an effort to connect my estimates with the broader literature, for Tables 8-11, I show how my estimates differ across educational attainment, gender, and race.

In Table 8, columns 1 and 2, I show that changes in the local unemployment rate at labor market entry don't produce different estimates, 2.96% versus 2.94%, between individuals with high school degrees versus those with college degrees, and the effect dissipates at an identical 0.21% rate for both groups. However in Table 8, columns 3 and 4, I show there is a larger initial decrease for men versus women, 4.04% versus 2.26%, and that the disparity dissipates slightly faster for men, 0.29% versus 0.19%, for each potential experience year. Finally, in Table 8, columns 5 and 6, I show this effect differs between whites and non-whites, 2.94% decrease versus 3.82%, and the disparity dissipates at a rate of 0.22% for whites and 0.31% for non-whites for each potential experience year.

In Table 9, I show how changes in the state-level unemployed/job openings ratio affect annual earnings between education, gender, and race. In Table 9, columns 1 and 2, I show there is a difference in the initial effect between high school graduates and college graduates, a decrease in annual earnings of 4.97% versus 3.88%. This disparity dissipates by 0.32% for high school graduates and 0.29% for college graduates for each potential experience year. In Table 9, columns 3 and 4, I show there is also an initial difference between men and women, a decrease of 5.81% versus 3.27%, that dissipates by 0.43% and 0.25%, respectively, for each potential experience year. Finally, in Table 9, columns 5 and 6, I show a point increase in the state-level unemployed/job openings ratio at labor market entry produces no statistically meaningful difference between whites and non-whites. This is unexpected as increases in the unemployment rate show a large effect for non-whites versus whites (Table 8, columns 5-6).

In Table 10, 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 10.32% versus 6.15%) and men and women (increase of 9.81% versus 6.53%). However, in Table 10, columns 5 and 6, I see there is a large initial effect for whites, 10.23%, but I find no statistically significant effect for non-whites. The point estimate of 2.60% 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 0.65%-0.93% for each potential experience year.

Finally, in Table 11, I show how a change in the job-market-entry year state-level quits rate affects annual earnings across education, gender, and race. I find substantial differences across each group. In Table 11, columns 1 and 2, I show that a one percentage point increase in the local quits rate increases initial annual earnings by 17.11% for high school graduates and 12.12% for college graduates. In Table 11, columns 3 and 4, I show that men enjoy a 17.68% initial increase in annual earnings versus only a 10.45% initial increase for women. In Table 11, columns 5 and 6, I show that whites enjoy an initial 14.96% increase in annual earnings from an increase in the job-market-entry year state-level quits rate, but non-whites only see a 12.85% increase. However, this effect is substantially larger for non-whites than the effect observed for an increase in the job openings rate. This is possible if increases in the quits rate reduce employer bargaining power while an increases in the job openings rate do not. Employers would be less able to discriminate in the latter situation. Across all groups, the initial increase in annual earnings for an elevated job-market-entry year state-level quits rate is decreased by 1.19%-1.55% for each potential experience year.

6 Testing for Migration and Graduation Trends

The primary identification concern with this study is that the CPS-ASEC data does 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 1980, 1990 and 2000 decennial census data along with the 2001-2019 American Community Survey data (ACS)[10] 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 quits rate, 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

[10]Ruggles et al 2022

the effect, because it accounts for timing of labor market entry based on economic conditions.

In Figure 3, 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. 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 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 quits rate 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 yeild 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 quits rate, 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 quits rate may be more substantial than my estimates in Figure 2.

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 8.15%. This increase reverts back to the mean at a rate of 0.75% per year of experience, suggesting that this effect can last over a decade. For every percentage point increase in the state-level quits rate, initial annual earnings increase by 14.23%. Likewise, this increase reverts back to the mean at a rate of 1.35%, 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

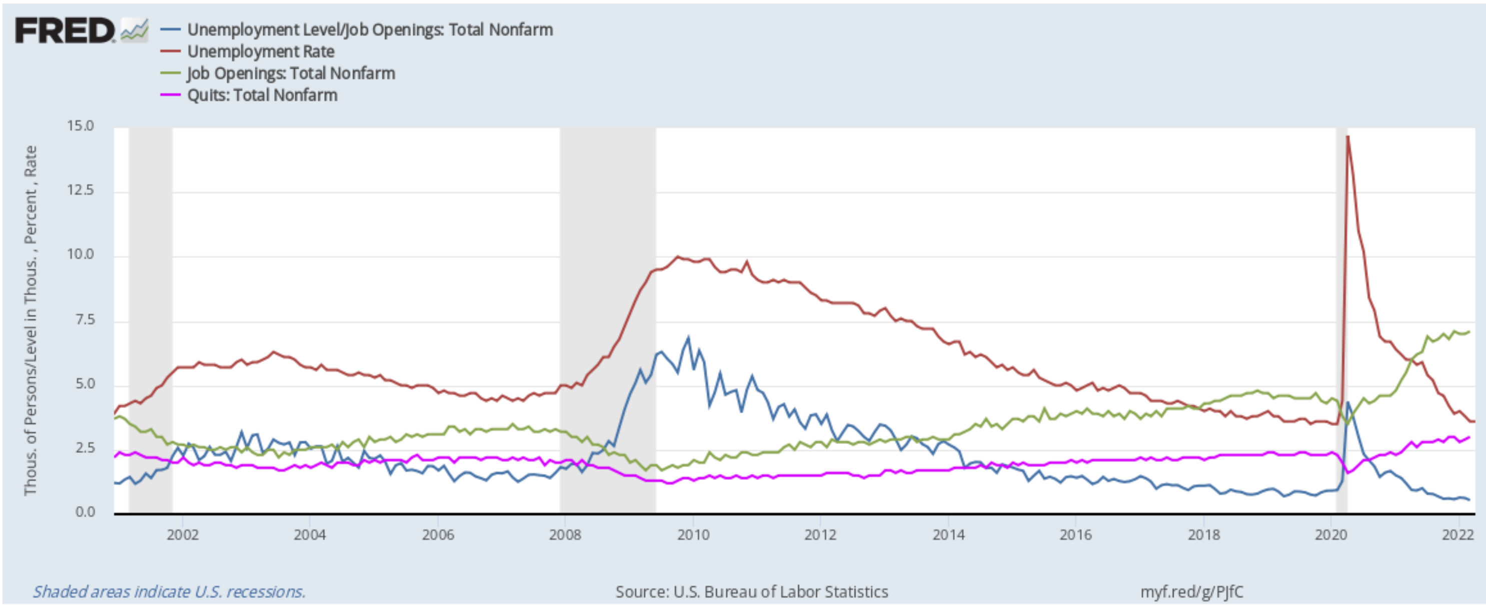

Figure 1: Changes in Unemployment, Unemployed/Job Openings, Jobs, and Quits (2000-2022)[11]

[11]U.S. Bureau of Labor Statistics, retrieved from FRED, Federal Reserve Bank of St. Louis; `https://fred.stlouisfed.org/series/LNU03000000`; `https://fred.stlouisfed.org/series/JTUJOL`; `https://fred.stlouisfed.org/series/UNRATE`; `https://fred.stlouisfed.org/series/JTSJOR`; `https://fred.stlouisfed.org/series/JTSQUR`, May 22, 2022.

Figure 2: Main Results

[11]Using potential experience and current state combined with historical state unemployment, job openings, and quits data, I impute the change in treatment at job-market-entry for each CPS-ASEC respondent. I then estimate how changes in four different state-level treatments, the unemployment rate, unemployed/job openings ratio, job openings rate, and quits rate, affect annual earnings over a 15-year career.

Figure 3: Test for Timing and Migration

[11]CPS-Minccrian is based on estimates from Figure 2. These estimates represent a baseline for the other three measures in order to assess the existence and/or direction of bias that could arise from incorrectly assuming the year and state of job-market entry in the CPS-ASEC. Census-Mincerian replicates estimates using Census and ACS data instead of the CPS-ASEC data. Census-using state of birth replicates estimates using the state-of-birth rather than current state of residence as the job-market-entry state. Finally, Census-double weighted by age, replicates estimates by accounting for both historical state-to-state migration and graduation trends.

9 Tables

Table 1: **Sample Summary Table**

	Mean
% Female	49.63
% Caucasian	74.98
% High School Graduates	25.15
% College Graduates	29.92
Observations	118858

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

	Mean Annual Earnings	Mean Hourly Wages	Mean Hours Worked	Mean Weeks Worked
Full Sample	$ 17,914.96	$ 13.49	35.47	32.58
Men	$ 20,491.26	$ 13.92	36.94	34.04
Women	$ 15,299.97	$ 13.04	33.95	31.09
White	$ 18,512.62	$ 13.50	35.49	33.66
Non-White	$ 16,123.44	$ 13.46	35.40	29.32
High School	$ 11,876.18	$ 10.22	36.55	31.94
College	$ 34,563.67	$ 20.66	40.38	42.26

Table 3: **Main Results**

	(1) Log Annual Earnings	(2) Log Annual Earnings	(3) Log Annual Earnings	(4) Log Annual Earnings
Unemployment Rate	-0.0311***			
	(0.0039)			
Unemployment Rate × exp	0.0024***			
	(0.0005)			
Unemployed/Job Openings		-0.0453***		
		(0.0055)		
Unemployed/Job Openings × exp		0.0034***		
		(0.0006)		
Job Openings Rate			0.0815***	
			(0.0135)	
Job Openings Rate × exp			-0.0075***	
			(0.0014)	
Quits Rate				0.1423***
				(0.0193)
Quits Rate × exp				-0.0136***
				(0.0018)
Observations	106078	106078	106078	106078
Adjusted R^2	0.705	0.705	0.705	0.705

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

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

Table 4: **Effect of Unemployment Rate on Alternate Outcomes**

	(1) Log Hourly Wages	(2) Log Weeks Worked	(3) Log Hours Worked
Unemployment Rate	-0.0115***	-0.0107***	-0.0088***
	(0.0023)	(0.0025)	(0.0018)
Unemployment Rate × exp	0.0004+	0.0011***	0.0008***
	(0.0002)	(0.0003)	(0.0002)
Observations	106078	106839	106839
Adjusted R^2	0.546	0.421	0.527

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

Table 5: **Effect of Unemployed-to-Job-Opening Ratio on Alternate Outcomes**

	(1) Log Hourly Wages	(2) Log Weeks Worked	(3) Log Hours Worked
Unemployed/Job Openings	-0.0163***	-0.0137***	-0.0152***
	(0.0032)	(0.0037)	(0.0025)
Unemployed/Job Openings × exp	0.0009**	0.0011**	0.0014***
	(0.0003)	(0.0004)	(0.0002)
Observations	106078	106839	106839
Adjusted R^2	0.546	0.421	0.528

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

Table 6: **Effect of Job Openings Rate on Alternate Outcomes**

	(1) Log Hourly Wages	(2) Log Weeks Worked	(3) Log Hours Worked
Job Openings Rate	0.0260**	0.0217**	0.0380***
	(0.0087)	(0.0082)	(0.0060)
Job Openings Rate × exp	-0.0028***	-0.0012	-0.0034***
	(0.0007)	(0.0008)	(0.0006)
Observations	106078	106839	106839
Adjusted R^2	0.545	0.421	0.528

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

Table 7: **Effect of Quits Rate on Alternate Outcomes**

	(1) Log Hourly Wages	(2) Log Weeks Worked	(3) Log Hours Worked
Quits Rate	0.0329**	0.0530***	0.0526***
	(0.0115)	(0.0117)	(0.0087)
Quits Rate × exp	-0.0043***	-0.0041***	-0.0049***
	(0.0009)	(0.0010)	(0.0008)
Observations	106078	106839	106839
Adjusted R^2	0.545	0.421	0.528

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

Table 8: **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	-0.0296***	-0.0294***	-0.0404***	-0.0226***	-0.0294***	-0.0382***
	(0.0067)	(0.0045)	(0.0055)	(0.0052)	(0.0049)	(0.0097)
Unemployment Rate × exp	0.0021**	0.0021***	0.0029***	0.0019***	0.0022***	0.0031***
	(0.0007)	(0.0005)	(0.0006)	(0.0005)	(0.0005)	(0.0008)
Observations	44166	61912	53629	52449	65709	40369
Adjusted R^2	0.630	0.558	0.740	0.709	0.754	0.587

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

Table 9: **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 Openings	-0.0497***	-0.0388***	-0.0581***	-0.0327***	-0.0458***	-0.0456***
	(0.0103)	(0.0063)	(0.0078)	(0.0069)	(0.0064)	(0.0137)
Unemployed/Job Openings × exp	0.0032***	0.0029***	0.0043***	0.0025***	0.0032***	0.0042***
	(0.0009)	(0.0006)	(0.0007)	(0.0007)	(0.0007)	(0.0011)
Observations	44166	61912	53629	52449	65709	40369
Adjusted R^2	0.630	0.558	0.740	0.709	0.754	0.587

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

Table 10: **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	0.1032***	0.0615***	0.0981***	0.0653***	0.1023***	0.0260
	(0.0214)	(0.0167)	(0.0197)	(0.0174)	(0.0147)	(0.0303)
Job Openings Rate × exp	-0.0073**	-0.0065***	-0.0093***	-0.0057***	-0.0076***	-0.0076**
	(0.0023)	(0.0014)	(0.0017)	(0.0016)	(0.0015)	(0.0026)
Observations	44166	61912	53629	52449	65709	40369
Adjusted R^2	0.630	0.557	0.740	0.709	0.754	0.586

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

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

	(1) HS	(2) College	(3) Male	(4) Female	(5) White	(6) Non-White
Quits Rate	0.1711***	0.1212***	0.1768***	0.1045***	0.1496***	0.1285**
	(0.0325)	(0.0218)	(0.0280)	(0.0258)	(0.0216)	(0.0458)
Quits Rate × exp	-0.0119***	-0.0140***	-0.0155***	-0.0119***	-0.0135***	-0.0143***
	(0.0029)	(0.0016)	(0.0023)	(0.0020)	(0.0020)	(0.0032)
Observations	44166	61912	53629	52449	65709	40369
Adjusted R^2	0.630	0.558	0.740	0.709	0.754	0.587

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