STAFF REPORTS

NO. 905
DECEMBER 2019

REVISED
APRIL 2022

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FEDERAL RESERVE BANK *of* NEW YORK

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Federal Reserve Bank of New York Staff Reports, no. 905
December 2019; revised April 2022
JEL classification: E24, J63, J64, J65

Abstract

We develop a method to jointly measure the response of worker search effort (micro effect) and vacancy creation (macro effect) to changes in the duration of unemployment insurance (UI) benefits. To implement this approach, we exploit an unexpected cut in UI durations in Missouri and provide quasi-experimental evidence on the effect of UI on the labor market. The data indicate that the cut in Missouri significantly increased job finding rates by both raising the search effort of unemployed workers and the availability of jobs. The latter accounts for at least one half of the total effect.

Key words: unemployment insurance, unemployment, vacancies, search

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1 Introduction

In nearly every post-War recession, U.S. policymakers have increased the potential maximum duration of unemployment insurance (UI) benefits. After the unemployment rate eclipsed 10% in late 2009, UI benefits were extended to an unprecedented 99 weeks. Those who lost their jobs during the COVID-19 recession received extended benefits through the summer of 2021, including intermittent supplemental benefits of $300 or $600 per week—sometimes resulting in replacement rates in excess of 100% (Ganong et al., 2020). With total payments approaching one percent of GDP during the past two recessions, UI is one of the most prominent and commonly-used automatic stabilizers in the United States.

Not surprisingly, the dramatic policy responses in the last two recessions renewed interest in studying the effects of UI benefits and the mechanisms through which they operate. Early Great Recession studies (Rothstein, 2011) followed the classic labor literature in focusing on identifying the search responses of individuals in response to changes in benefits. Hagedorn et al. (2013) pointed out that equilibrium labor market theory implies that vacancy creation decisions of firms, in addition to worker search behavior, respond to changes in UI benefits. A simple decomposition they provide helps illustrate these margins:

$$\text{Job finding rate}_{it} = \underbrace{s_{it}}_{\text{search behavior}} \times \underbrace{f(\theta_t)}_{\text{job finding rate per unit of search}}$$

The first channel operates through labor supply by altering job search behavior s_{it}, which captures the search intensity and pickiness of unemployed workers. A large literature has estimated negative effects on labor supply of varying magnitudes in response to UI extensions. We label this channel as the micro effect. The second captures changes in labor demand for all workers. Firms reduce labor demand if workers search less, because lower search effort implies a lower probability of finding a worker for the firm. Moreover, benefit extensions generate upward pressures in wages, reduce profits and therefore reduce the demand for labor in equilibrium. We label this channel that alters the job finding rate of all workers in the same market as the macro effect. A complete evaluation of the effect of UI policies requires measurement of both margins.

Hagedorn et al. (2013) provided the genesis for a large literature trying to identify the total effect of the policy (Chodorow-Reich et al., 2018; Farber et al., 2015; Hagedorn et al., 2015) that has reached conflicting conclusions.[1] However, to the best of our knowledge, none

[1] A separate, more structural literature that has evaluated the impact of UI policy over the US business cycle has found conflicting answers. Mitman and Rabinovich (2019) find destabilizing effects of UI, whereas Kekre (2021) finds that UI reduced aggregate unemployment during the Great Recession, and McKay and Reis (2016) find results in between— that UI contributed negligibly to aggregate volatility.

of that literature has attempted to separately measure the macro and micro effects, only the sum of the total effect of UI on labor market outcomes. One notable exception is the recent innovative work by Ganong et al. (2021) that identify both micro and macro effects of expanded benefits during the COVID recession. They find precisely estimated non-zero disincentive effects both for the micro and macro channels, consistent with our findings.[2]

Understanding the relative contribution of the micro and macro effects is essential for the normative evaluation of UI policies. As Mitman and Rabinovich (2015) and Landais et al. (2018a) show, the optimal generosity of UI depends critically on the relative response of job search and vacancy creation. If vacancy creation responds more to a change in UI relative to worker search effort, labor market tightness will fall, implying that UI generosity should be pro-cyclical, and vice-versa. Therefore, the design of an appropriate policy response to adverse macroeconomic shocks relies on understanding the quantitative relevance of these two channels. The objective of this paper is to provide the first such decomposition.

We start by developing a methodology for decomposing the total effect of UI policies on job finding rates into micro and macro effects by imposing minimal assumptions. We assume that hires are determined by a matching function that combines a given number of vacancies V_t and aggregate search $S_t = s_t U_t$ into H_t hires. If the matching function exhibits constant returns to scale, the (log) change of the vacancy filling rate can be expressed as a weighted sum of the changes in search effort s_t and the vacancy-unemployment ratio V_t/U_t. Our two-step decomposition strategy first measures the effect of UI extensions on the vacancy filling rate H_t/V_t and the vacancy-unemployment ratio separately, and then uses the relationship derived from the matching function to infer the response of search effort and market tightness.[3] With these estimates at hand, we can then quantify the effect of benefit extensions on the job finding rate and to unemployment via the job finding channel.

To this end, we follow the innovative work by Johnston and Mas (2018) and exploit an unexpected 6-week cut in the maximal UI duration in Missouri in 2011. That reduction in state-funded UI triggered an additional 10-week cut in federally-financed benefits from the Emergency Unemployment Compensation Act of 2008. Importantly, as Johnston and Mas (2018) document, this policy change was sudden and unanticipated, and therefore provides a quasi-experimental setting to study the labor market implications of UI extensions.[4]

[2]Our study in general complements a nascent literature that has used structural models to study positive and normative questions about UI during the pandemic (Fang et al., 2020; Kapicka and Rupert, 2020; Birinci et al., 2020; Mitman and Rabinovich, 2021, e.g.,).

[3]We also provide a discussion of how firm search effort or *recruiting intensity* would affect our decomposition.

[4]Johnston and Mas (2018) focus on identifying the effect of worker search effort using a regression discontinuity design (RDD) by comparing individuals claiming benefits that were laid off before and after the policy change. We see our approach as complementary to theirs. For a more thorough discussion comparing

The key challenge for estimating the effect of the policy change in Missouri is inferring the counterfactual dynamics of labor market outcomes in Missouri in the absence of the cut. We follow the synthetic control method of Abadie and Gardeazabal (2003) and Abadie et al. (2010) and construct a synthetic control for Missouri, given by a weighted average of other states. During the Great Recession, states varied in the timing, magnitude and types of shocks they faced. The weights are chosen optimally so that the control state mimics Missouri in the period leading up to the unexpected UI cut in April 2011. We verify the robustness of our results to alternative implementations of the synthetic control methodology and an average of all other U.S. states excluding Missouri.

To implement this synthetic control approach, we construct a quarterly state-level dataset of hires from the Quarterly Workforce Indicators (QWI), vacancies from the Help Wanted Online (HWOL) and the unemployment rate from the Local Area Unemployment Statistics (LAUS), which we supplement with data on the maximum duration of unemployment benefits for states during the Great Recession constructed using weekly trigger reports published by the Bureau of Labor Statistics.[5]

Our baseline results indicate that the UI cut in April 2011 led to a gradual increase of 13% in the vacancy-unemployment ratio. The vacancy filling rate dropped immediately by 5% relative to the synthetic control—signifying a tightening of the labor market in Missouri, as the vacancy filling rate is inversely related to labor market tightness. In standard labor market theory, vacancies are a jump variable, so we would expect tightness to jump immediately, even if unemployment as a stock variable evolves more slowly in response to the policy change. Our findings are quantitatively consistent with Hagedorn et al. (2013): Assuming that the cut in Missouri lasts until the end of the Emergency Unemployment Compensation program (EUC) at the end of 2013, the elasticity of the vacancy-unemployment ratio with respect to a one-quarter cut in UI duration of –0.1 estimated in Hagedorn et al. (2013) implies an increase in the vacancy-unemployment ratio of around 16%, which is consistent with the evidence in this paper.

We then implement our decomposition to infer the response of search effort and market tightness. We find that labor market tightness rose 23%. This estimate corresponds to an elasticity of market tightness with respective to benefit duration of around –0.9. Our preferred estimate infers that search effort rose in response to the UI cut as well, by around 4.7%, consistent with Johnston and Mas (2018).[6] The tightening of the labor market and

our paper to theirs, see Section 3.7.

[5] We verify that the dynamics of unemployment at the state level in LAUS are consistent with those measured in micro data from the Current Population Survey (CPS). See Appendix C. The data on unemployment benefit duration were constructed by Hagedorn et al. (2013).

[6] Johnston and Mas (2018) find a reduction in unemployment duration of 17.2% due to the policy. How-

the increase in search effort led to a higher job finding rate in Missouri. We find that this increase induced by the policy change was 10.5%. Under our most conservative estimate of the increase in the job finding rate, we conclude that at least half is due to macro effects through tightness and the remaining part is due to higher search effort.

This paper contributes to a large literature that studies the labor market effects of unemployment benefit policies (see, for example, Feldstein, 1978; Ham and Rea Jr, 1987; Katz and Meyer, 1990; Meyer, 1990; Card and Levine, 2000). Despite the importance of separating the micro and macro effects for optimal design, we know little about the relative magnitudes of these channels with a few notable exceptions. Johnston and Mas (2018) identify the effect on worker search effort directly using a regression discontinuity design, whereas we use a synthetic control approach to measure the effect on market tightness and measure the effect on search effort as a residual. Lalive et al. (2015) argue that unemployment insurance policies create sizable market externalities, whereby extensions of UI durations raise the job finding rate of workers not eligible for UI. It is worthwhile to note that they study a policy change in Austria which effectively served as a bridge early-retirement program for workers in the steel industry. Finally, Marinescu (2017) uses state-level variation in potential UI durations and finds that UI extensions lead to lower search at the state level and no change in the number of vacancies. See Hagedorn et al. (2016) for a more thorough review of recent quasi-experimental studies on the effects of UI benefit extensions.

The rest of the paper is organized as follows. Section 2 develops the methodology, Section 3 discusses the data and the findings, and Section 4 concludes.

2 Measuring micro and macro effects

We assume that matches between vacancies and unemployed workers are formed via a constant returns to scale matching function $M(V_t, S_t)$, where V_t is vacancies and $S_t = s_t \times U_t$ is the effective units of search.[7] The number of hires per period is given by the number of matches, $H_t = M(V_t, S_t)$.

The job-finding rate per effective unit of search is given by

$$f(\theta_t) = \frac{M(V_t, S_t)}{S_t} = M(\theta_t, 1),$$

where $\theta_t = V_t/S_t$ is the labor market tightness. The vacancy filling rate can be analogously expressed as:

ever, only 30% of unemployed were collecting UI, suggesting that in the aggregate search effort would have increased $\approx 17.2\% \times 0.3 = 5.2\%$, consistent with our estimates.

[7] s_t is the average search effort per unemployed worker.

$$q(\theta_t) = \frac{M(V_t, S_t)}{V_t} = M\left(1, \frac{1}{\theta_t}\right) = \frac{f(\theta_t)}{\theta_t}.$$

The constant returns assumption implies that labor market tightness is also given by the ratio of the job finding rate per unit of search to the vacancy filling rate: $\theta_t = f(\theta_t)/q(\theta_t)$. It is exactly this relationship that we exploit to infer the response of search effort. Consistent with empirical evidence documented in Petrongolo and Pissarides (2001), we assume that the matching function has a Cobb-Douglass form $M(V, S) = \chi V^{\alpha} S^{1-\alpha}$, where χ is the efficiency of the matching function and α is its elasticity with respect to vacancies.[8] Taking logs of the θ_t expression and recognizing that $q_t = H_t/V_t$, we obtain:

$$\log\left(\frac{H_t}{V_t}\right) = \log(\chi) - (1-\alpha)\log\left(\frac{V_t}{U_t}\right) + (1-\alpha)\log(s_t). \tag{1}$$

Our main empirical specification compares the evolution of H_t/V_t and V_t/U_t in Missouri, which featured a plausibly exogenous cut in UI benefits, to a synthetic control that did not experience a similar cut. Assuming the elasticity of the matching function is common across states and that the (potentially state-specific) matching efficiency is invariant to UI durations, this difference is given by

$$\Delta\log\left(\frac{H_t}{V_t}\right) = (1-\alpha)\Delta\log(s_t) - (1-\alpha)\Delta\log\left(\frac{V_t}{U_t}\right). \tag{2}$$

Here Δ denotes the difference operator between Missouri and the synthetic control. Note that we observe V_t, H_t, and U_t directly in the data. Thus, conditional on a value for α, we can measure the effect of the policy change on search effort as a residual using equation (2).[9] Specifically,

$$\beta_s = \frac{1}{1-\alpha}\beta_{H/V} + \beta_{V/U}. \tag{3}$$

Here, β_s, $\beta_{V/U}$ and $\beta_{H/V}$ are the effect of the policy change on job search effort, the vacancy-

[8] We further illustrate a linear log-log relationship between hires and vacancy-unemployment ratio in Appendix Figure B.4 in the U.S. during the time period of our sample.

[9] Davis et al. (2013) have recently highlighted the importance of *recruiting effort* or firm search effort in explaining aggregate fluctuations in the labor market. Gavazza et al. (2018) show quantitatively that this channel was important for labor market dynamics during the Great Recession. Adapting our methodology to accommodate recruiting intensity e_t (where $V_t^* = e_t V_t$ represents effective vacancies) would yield:

$$\Delta\log\left(\frac{H_t}{V_t}\right) = (1-\alpha)\Delta\log(s_t) + \alpha\Delta\log(e_t) - (1-\alpha)\Delta\log\left(\frac{V_t}{U_t}\right).$$

Thus, what we attribute to search effort can be interpreted as the combination of worker search and recruiting intensity. Theory implies that both move in the same direction in response to a change in UI. Thus the sign of the empirical response is indicative for both worker effort and recruiting intensity, and the magnitude can be interpreted as the total "search effort" (by both workers and firms) response.

unemployment ratio and the vacancy filling rates, respectively. Equation (3) exploits the fact that the vacancy filling rate is only a function of market tightness, whereas unemployment is also a function of search effort. Therefore, we infer a change in search effort to the extent that the vacancy-unemployment ratio responds by more than the vacancy filling rate (scaled appropriately as in equation (3)).

The effect of policy on tightness β_θ can be inferred from its effect on the vacancy filling rate. Because the matching efficiency does not respond to policy, changes in the vacancy filling rate are driven only by tightness; i.e. $\Delta \log \theta = \Delta \log(H/V)/(\alpha - 1)$. It follows that

$$\beta_\theta = -\frac{\beta_{H/V}}{1-\alpha}. \tag{4}$$

Finally, we can calculate the impact of the policy on the job finding rate as the sum of the micro and equilibrium effects.

$$\beta_{\text{job finding rate}} = \underbrace{\beta_s}_{\text{micro}} + \underbrace{\alpha\beta_\theta}_{\text{macro}} = \beta_{V/U} + \beta_{H/V} \tag{5}$$

Note that the effect on the job finding rate is independent of the matching function elasticity. We use equation (5) to calculate and decompose this effect into the micro and macro components.

3 Empirical analysis

We begin this section by giving a brief overview of the sudden cut in UI durations in Missouri. Next we describe the data sources followed by the details of the empirical analysis.

3.1 Institutional background

Unemployment insurance in the U.S. is a federally-regulated program administered by the individual states. Eligible jobless workers ordinarily receive UI benefits for up to 26 weeks while unemployed.[10]

During the Great Recession, two programs provided extended benefits: Extended Benefits (EB) and Emergency Unemployment Compensation (EUC). EB allows for 13 to 20 extra weeks of benefits to workers that have exhausted their regular benefits. At the onset of the recession, half of the cost of the program was paid for by the federal government, which included a set of triggers that the states can adopt. Initially, many states including

[10]Some states can offer longer durations.

Missouri adopted high triggers. As a result of the American Recovery and Reinvestment Act, which made EB fully federally funded through December 2013, Missouri (and other states) enacted legislation that would increase EB duration from 13 to 20 weeks. EUC, on the other hand, was federally funded from the onset. The program eventually had 4 tiers, providing potentially 53 weeks of additional benefits. The availability of each tier depended on state unemployment rates.

Four Missouri state senators filibustered the receipt of additional funds through the EB program. To end the filibuster, the legislature brokered a compromise which would cut regular benefits from 26 to 20 weeks in exchange for the state accepting federal funds and maintaining extended benefits for the long-term unemployed. Effectively, Missouri instituted shorter UI-durations in the long run while allowing extended benefits for the already-long term unemployed. As Johnston and Mas (2018) describe, the unanticipated legislation was passed and took effect a mere five days after media first reported of a compromise including potential cuts to regular benefits.

Because federal regulations calculate federal benefits administered during times of high unemployment relative to regular state UI benefits, the cut triggered an additional 10-week reduction in emergency benefits. Thus, claimants approved for UI by April 13, 2011 could receive benefits for a maximum of 73 weeks. Those approved after April 13 were only eligible for a maximum of 57 weeks. Johnston and Mas (2018) note that the shortened potential UI duration did not coincide with any other change in the state's UI system, such as change in program administration or search requirements.

3.2 Data

We compile state level data on unemployment, hires, vacancies and unemployment benefit durations for the period 2005-2013. Data on number of unemployed residents come from the Local Area Unemployment Statistics (LAUS) provided by the Bureau of Labor Statistics.[11]

Data on hires are obtained from the Quarterly Workforce Indicators (QWI).[12] Specifically, we utilize the "new hires" variable which measures hires who were not employed by that employer in any of the previous four quarters. The new hires measure excludes recalls, which is useful for our purposes because hiring through a recall likely does not operate through the same matching process as in our framework (Fujita and Moscarini, 2017). Unlike monthly unemployment counts, hires data is only available at a quarterly frequency. The QWI is constructed using micro data from the Longitudinal Employer-Household Dynamics (LEHD), which covers over 95% of U.S. private sector jobs via a partnership between state

[11] https://download.bls.gov/pub/time.series/la/

[12] https://lehd.ces.census.gov/data/qwi/

labor market information agencies and the Census Bureau. The QWI supplies data for all states since at least 2010, although some states entered the partnership as early as 1990.

We obtain vacancy data from the Help Wanted OnLine (HWOL) dataset provided by The Conference Board (TCB). This monthly series covers the universe of unique vacancies advertised on around 16,000 online job boards and online newspaper editions.[13] The data, which begin in May 2005, measure newly created vacancies in a given month as well as total vacancies—the sum of all openings, both extant and new. Each observation in the HWOL database refers to a unique online advertised vacancy. Our analysis is based only on approximately 98% of all online vacancies that are uniquely matched by TCB to a county of prospective employment.[14] One advantage of the HWOL compared to the Job Openings and Labor Turnover Survey is geographic granularity—while JOLTS is aggregated to four broad Census regions, vacancies are documented at the county level by HWOL (Şahin et al., 2014).

Due to the frequency of the QWI, monthly data on the employed and unemployed stocks from the LAUS and vacancies from HWOL must be aggregated to a quarterly frequency. We then seasonally adjust these series, along with the hires data, using a Signal Extraction in ARIMA Time Series (SEATS) method.

3.3 Causal inference

We take two complementary approaches to make inference on the causal effect of the cut in UI on labor market outcomes. First, we use a synthetic control approach to show the effect of the policy on key labor market variables. Second, to obtain quantitative estimates of the effect, we employ a difference-in-difference estimator.

Synthetic control approach. We implement the synthetic control method developed by Abadie and Gardeazabal (2003) and extended by Abadie et al. (2010). The method compares the outcomes between a treated unit—in our case, Missouri subjected to a UI potential duration cut—and otherwise similar but unaffected units by constructing a synthetic counterfactual that will serve as a better control group than any single unit alone. The weights are assigned to each state to minimize the mean squared prediction error between the treatment and control groups prior to the benefit cut. The resulting "synthetic Missouri" provides a good approximation of how its outcomes of interest—V/U and H/V—would have developed if no intervention had taken place.

[13] Duplicate postings are identified and removed by TCB.

[14] We do not use approximately 2% of HWOL vacancies that are coded as "nationwide."

Our baseline synthetic counterfactual is constructed from state-specific weights selected to match the pre-treatment values of the outcome variable. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. Our beginning time period is 2006Q3. We exclude from the donor pool states which cut UI duration around the time of Missouri's policy change, as the synthetic control must be a weighted average of untreated units.[15] We list the resulting weights for individual states that comprise Missouri's synthetic V/U and H/V counterfactuals in Appendix Table A.1.

We conduct sensitivity analyses by varying (i) the pre-treatment time period, (ii) the frequency with which lagged outcome variables are matched, and (iii) the choice to instead use covariates, economic variables that have predictive power for explaining the dependent variable (see figures in Appendix A). In all versions of the synthetic control method, we find that Missouri's policy change boosted the vacancy-unemployment ratio and lowered the vacancy filling rate, thereby increasing market tightness.

Difference-in-differences. We obtain point estimates for the effect on V/U and H/V using a two-way fixed effects linear regression on the same balanced panel as the synthetic control method:

$$y_{st} = \lambda_s + \gamma_t + \beta \cdot D_{st} + \varepsilon_{st} \tag{6}$$

Here, s denotes the state and t denotes time. D_{st} is an indicator which equals 1 when s is Missouri and t is greater than or equal to 2011Q2. λ_s and γ_t are state and time fixed effects, respectively, which capture unobserved unit- and time-specific confounders. We estimate equation (6) on panels of differing lengths, starting in either 2006Q3 or 2005Q3 (the earliest available quarter of HWOL data). Our baseline results seasonally-adjust V, H, and U separately. In Appendix B.1, we provide sensitivity checks by seasonally adjust V/U and H/V as ratios and varying the beginning and end date of the panel.

3.4 Results

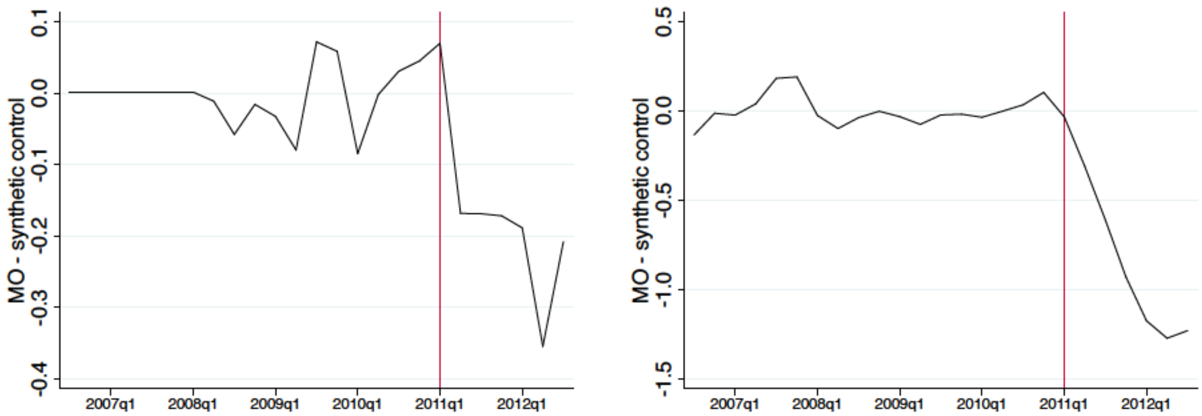
3.4.1 Synthetic control

The synthetic control method illustrates the policy change in Missouri had a sizable effect on UI duration, the unemployment rate, the vacancy-unemployment ratio and the vacancy filling rate. Following this policy change, the UI duration in Missouri fell by more than 20

[15] These states are Arkansas, Florida, Georgia, Michigan, and South Carolina. Massachusetts is also excluded from the donor pool because they did not begin sharing administrative records with the Census Bureau for purposes of the QWI until 2010, while our time-varying outcomes used in synthetic control begin in 2006.

log points relative to the synthetic control by 2012. This decline is similar to the actual cut in Missouri of $\log(73) - \log(56) \approx 0.265$ log points (Figure 1a). As shown by Johnston and Mas (2018), which we replicate in Figure 1b, the 16-week cut in Missouri triggered a full percentage point decline in the unemployment rate in Missouri.

Figure 1: Effects on Unemployment Rate and Unemployment Insurance Duration



(a) Maximum Weeks of UI Benefits

(b) Unemployment Rate (LAUS)

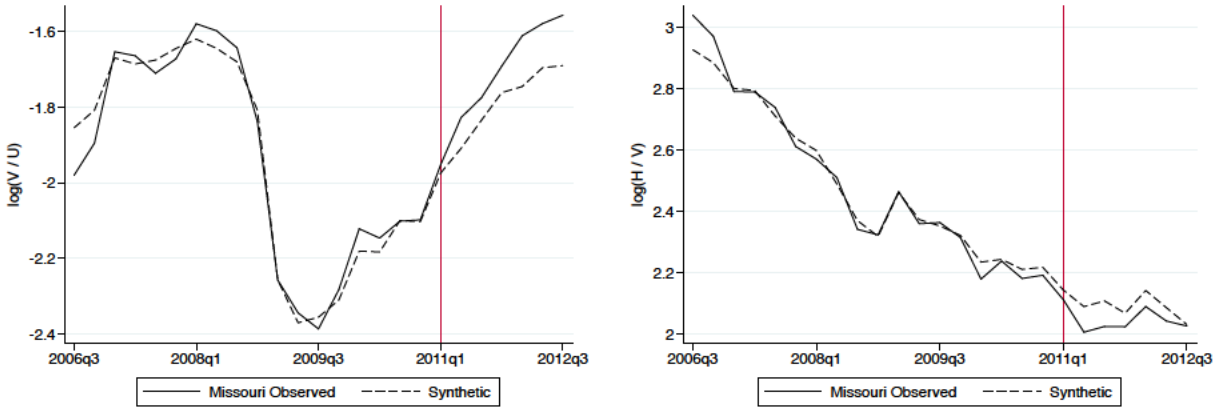
Note: Panels (a) and (b) plot the difference between Missouri's observed log of average duration of unemployment insurance benefits or unemployment rate and those of its synthetic control, respectively. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. Both graphs use lags of the outcome variable as the predictor (every fourth observation from 2007Q1 to 2011Q1).

The vacancy-unemployment ratio picks up gradually following the cut in UI durations (Figure 2a). Over the several quarters following the policy change, the increase is sizable: the ratio of vacancies to unemployed rises nearly 15 log points. This change corresponds to an elasticity of about –0.6 ($\approx -15/26.5$). In contrast, the vacancy filling rate H/V falls precipitously in the first quarter and remains depressed for much of 2012 (Figure 2b). In the first post-policy quarter, H/V drops by nearly 10 log points in Missouri relative to the synthetic state, indicating a tightening of the labor market. The immediate adjustment in the labor market is consistent with equilibrium search models, where market tightness is a jump variable and responds immediately to fundamentals. After the initial jump, where the implied elasticity is just below 0.4 ($\approx 10/26.5$), the effect slowly fades out.

3.4.2 Difference-in-differences estimation

We now implement the methodology in Section 2 to estimate the effect of the UI cut on the job finding rate using equation (5). This requires estimating the effect of the policy change on the vacancy-unemployment ratio and the vacancy filling rates first. We do so by running

Figure 2: Effects on Vacancy-Unemployment Ratio and the Vacancy Filling Rate



(a) V/U

(b) H/V

Note: Plot represents Missouri's observed and synthetic log(vacancies/unemployed) (panel (a)) and log(hires/vacancies) (panel (b)). Synthetic controls are constructed using pre-period (2006Q3-2011Q1) observations of the variable of interest. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. Measures are "new vacancies" and "new hires" from HWOL and QWI, respectively.

specification (6). In our baseline results, our dependent variables are constructed with data on new vacancies in HWOL. These are defined as listings that did not exist in the previous quarter and are newly created in a given quarter. We report both one-way standard errors clustered at the worker-level and two-way standard errors clustered at the worker-quarter level.

We use equation (6) to estimate the effect of Missouri's UI benefit cut on the vacancy-unemployment ratio to be roughly 30 log points, as reported in Panel A of Table 1. Our baseline specification suggests V/U increased by 27.8 log points. When we vary the start period of the panel or the method of seasonal adjustment (reflected in columns 2–4), the estimates lie between 27.5 and 31.8 log points. For all specifications, the effect is significant at a 0.1% level using two-way clustered standard errors.

For robustness on statistical significance, we implement a percentile rank test, whereby equation (6) is estimated on a sample consisting of all states except Missouri and where one of the states is assumed to be treated as the placebo. Additionally, we allow the placebo timing to vary among the quarters 2011Q1, 2011Q2 and 2011Q3. This yields 129 placebo estimates for $\hat{\beta}_{V/U}$ with which we can compute a percentile rank for the estimate based on the actual treatment of Missouri.[16] Our baseline estimate for the effect on the vacancy-

[16]There are 129 placebo estimates because there are 3 placebo treatment periods and 43 potential placebo treated states, as the placebo sample omits AR, DC, FL, GA, MA, MI, SC, and by necessity, MO. Thus,

Table 1: Estimated Effects of UI Duration Cut on Missouri Labor Market

	(1)	(2)	(3)	(4)
A. $Vacancies/Unemployment$				
$\hat{\beta}_{MO\_post}$	0.278***	0.275***	0.318***	0.315***
One-way clustered SE	(0.032)	(0.032)	(0.035)	(0.035)
Two-way clustered SE	(0.030)	(0.030)	(0.033)	(0.033)
Percentile Rank	9.3%	9.3%	9.3%	9.3%
B. $Hires/Vacancies$				
$\hat{\beta}_{MO\_post}$	-0.173***	-0.169***	-0.220***	-0.217***
One-way clustered SE	(0.016)	(0.017)	(0.018)	(0.019)
Two-way clustered SE	(0.016)	(0.017)	(0.019)	(0.019)
Percentile Rank	4.7%	4.7%	4.7%	4.7%
Implied Effects on f and u_{ss}				
Δf (actual $f_{MO} = 0.31$)	0.105	0.106	0.098	0.098
Δu_{ss} (percentage points)	-0.60	-0.61	-0.56	-0.56
Start Period	2006Q3	2006Q3	2005Q3	2005Q3
End Period	2012Q3	2012Q3	2012Q3	2012Q3
Seasonally Adjust V, H, U Separately	✓		✓	
Seasonally Adjust as Ratios		✓		✓
N	1,100	1,100	1,276	1,276

Note: Hires and Vacancies are new measures, as documented in HWOL and QWI respectively. Balanced panel includes all states except AR, FL, GA, MA, MI, SC and DC. Vacancies, Hires, and Unemployment are either seasonally adjusted independently or as ratios V/U and H/V. Percentile Rank is based on the percentage of permutations (where treatment is designated as beginning 2011Q2 in a given state) which yield a placebo treatment coefficient larger (smaller) than Missouri's estimate for V/U (H/V). Placebo tests are calculated with Missouri dropped from the comparison group sample. One-way clustered standard errors are clustered at the worker level, and two-way are clustered at the worker-quarter level. The table includes the imputed response of the job finding rate and steady-state unemployment rate (p.p.) according to equation 5 and $u_{ss} = s/(s+f)$. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

unemployment ratio, listed in column (1) of panel A, is larger in magnitude than all but 9.3% of the placebo estimates. For V/U, all four specifications yielded a percentile rank of less than 10%, providing further support for the finding that the increase in Missouri's vacancy-unemployment ratio upon the policy change was statistically significant.

Similarly, Panel B of Table 1 reports the estimates for the effect on the vacancy filling rate. Our baseline estimate for $\hat{\beta}_{H/V}$ is -17.3 log points. And the estimated effect ranges between -16.9 and -22 log points across different samples and seasonal adjustment methods. Moreover, the percentile rank for all of the vacancy filling rate estimates are below 5%. These results show that the policy change significantly reduced the vacancy filling rate in Missouri. As section 2 illustrated, a lower H/V indicates a tighter labor market (higher θ). We now explore the implications of our estimates for the job finding rate of workers and unemployment.

3.4.3 Implications for labor flows and unemployment

The estimates in Table 1 together with the decomposition in equation (5) allow us to calculate the change in the job finding rate as a result of the policy change. Listed below panel B, column (1) of Table 1, our calculations suggest that the cut in UI durations resulted in a 10.5% increase in the job finding rate, corresponding to an elasticity of -0.42 with respect to the UI benefit duration. Different constructions of the panel (start times, seasonal adjustment methods) agree on what happened to the availability of jobs in Missouri with the increase in the job finding rate ranging between 9.8% and 10.6% (corresponding to elasticities of -0.39 to -0.42).

How economically meaningful is the increase in the job finding rate? To answer this question, we quantify the impact on unemployment by computing a "flow-balance" unemployment rate for Missouri with the policy change (i.e. with actual data) and a counterfactual one in which the Missouri does not experience a cut in UI duration. This flow-balance unemployment rate is simply the rate at which the flows in and out of unemployment balance each other. To calculate it, we compute monthly employment-to-unemployment and unemployment-to-employment transition rates using monthly data from the CPS and seasonally adjust them. We follow Elsby et al. (2015) in adjusting these transition rates for time aggregation to obtain continuous time inflow and outflow rates, which we refer to as the separation (s) and job finding rates (f). We take quarterly averages of these rates prior to the policy change; i.e. over the three months in 2011Q1.[17] The flow balance unemployment rate is given by $u_{ss} = s/(s + f)$. The only difference between this and the counterfactual

$43 \times 3 = 129$.

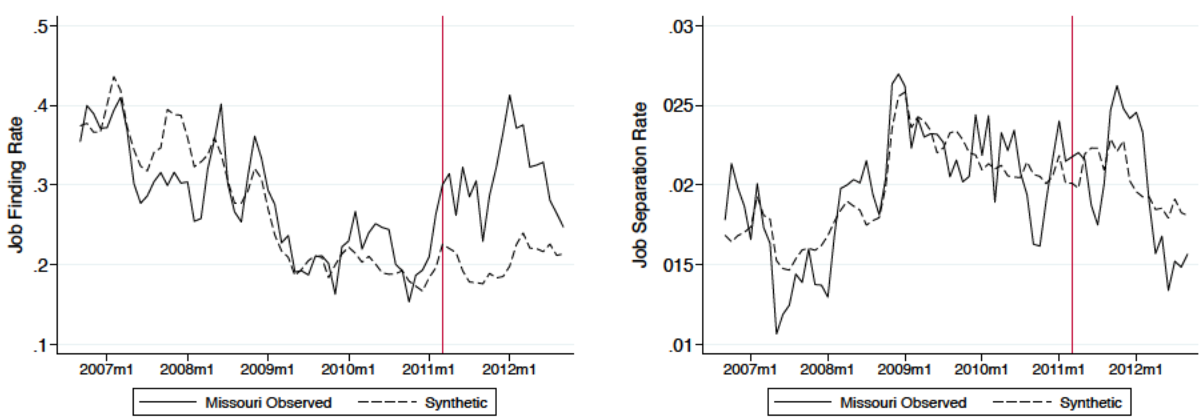
[17]These series are shown in Figure C.5 in Appendix C.1.

unemployment rate u_c is that in the counterfactual, the job finding rate is lower because the policy change is not enacted: $u_c = s/(s + (1 - \beta_f)f)$. The bottom row of Table 1 shows that in our baseline this difference is 0.6 percentage points: the increased availability of jobs (higher θ) and an increased search effort (s) combined, lowered the unemployment rate by 0.6 percentage points. Note that this accounts for 60% of the full percentage point decline in the unemployment rate caused by the policy change in Missouri (Figure 1b).

There are two other margins that can account for the remaining 40% decline in unemployment. The first is separations into unemployment. If making UI less generous raises job creation by raising profits, it would be plausible to also expect lower separations into unemployment. Thus, lowering benefit durations may reduce unemployment through reducing inflows into unemployment. We explore this channel in the next section. The second is the participation margin. When UI becomes less generous, some unemployed workers may leave the labor force, which further reduces the unemployment rate. In fact, Karahan and Mercan (2019) provide evidence that over this episode, labor force participation fell in Missouri and applications for disability insurance spiked. While potentially important, quantifying this margin is beyond the scope of this paper.[18]

3.4.4 Direct evidence on job flows

Figure 3: Effects on Job Finding and Separation Rate



(a) Job Finding Rate (b) Job Separation Rate

Note: Plots represents Missouri's observed and synthetic job finding and separation rates. Synthetic controls are constructed using pre-period (2006m7–2011m3) observations of the variable of interest. Finding and separation rates are calculated from monthly UE and EU flows in the CPS, adjusted according to the method of Shimer (2012), and smoothed to a three-month moving average.

[18] See Hagedorn et al. (2019) for a discussion on how cuts in UI affect labor supply and demand in a three-state (N-U-E) labor market model.

We provide direct evidence that the labor market effects of the UI duration cut operate through the job finding channel and not the job loss channel. To do so, we run the synthetic control on the job finding and separation rates of all states (calculated as discussed in the previous section). Figure 3 shows that it was primarily the job finding rate that responded to the policy change. The gap in the job finding rate of Missouri and the synthetic control opens up following the policy, and the separation rate shows no discernible change. Additionally, we estimate the effect using the difference-in-differences specification, analogous to those in Table 1, on the job finding and separation rates. While the treatment estimate is positive and highly significant for the job finding rate, the effect is statistically indistinguishable from zero on the separation rate (Table 2). Interestingly, the coefficient on the job finding rate from the difference-in-differences estimator is nearly identical to the implied effect on the job finding rate reported in Table 1. The similarity between the measured impact on the job finding rate across the two specifications lends further support to our proposed decomposition in equation (5).

Table 2: Estimated Effects of UI Duration Cut on Missouri Job Finding & Separation Rates

	(1)	(2)
A. *Job Finding Rate*		
$\hat{\beta}_{MO\_post}$	0.102***	0.106***
One-way clustered SE	(0.007)	(0.006)
Two-way clustered SE	(0.007)	(0.006)
B. *Job Separation Rate*		
$\hat{\beta}_{MO\_post}$	-0.0005	0.0004
One-way clustered SE	(0.0003)	(0.0003)
Two-way clustered SE	(0.0003)	(0.0003)
Start Period	Jul 2006	Jul 2005
End Period	Oct 2012	Oct 2012
N	6,556	7,084

Note: Finding and separation rates are calculated from monthly UE and EU flows in the CPS and adjusted according to the method of Shimer (2012). Reported coefficient is the Missouri $\times$ Post-Period in a two-way fixed effects specification for year-month and state. Balanced panel includes all states except AR, FL, GA, MA, MI, SC and DC. One-way clustered standard errors are clustered at the worker level, and two-way are clustered at the worker-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3.5 Decomposing the aggregate effect in to micro and macro effects

Using the estimated effects on the vacancy-unemployment ratio and the vacancy filling rate (cumulative effect through 2012Q3), we can now use equation (3) to infer the response of search effort β_s and market tightness β_θ that justify the joint behavior of the vacancy-unemployment ratio and the vacancy filling rate. To do so, we need to pick a value for the matching function elasticity α. Based on a survey of matching function estimates in Petrongolo and Pissarides (2001), we consider three alternative values for α: 0.25, 0.30, and 0.35. For each of these values, we report the imputed effects on market tightness, search effort and the share of the change in job finding rate that is due to macro effects in Table 3.

Table 3: Decomposition of Micro and Macro Effects

Matching function elasticity, α	Search effort Δs_t	Market tightness $\Delta\theta_t$	% Equilibrium effect in $\Delta \log(f)$
	Panel A. Specification 1		
0.25	0.047	0.231	55%
0.30	0.031	0.247	71%
0.35	0.012	0.266	89%
	Panel B. Specification 2		
0.25	0.050	0.225	53%
0.30	0.034	0.241	68%
0.35	0.015	0.260	86%
	Panel C. Specification 3		
0.25	0.025	0.293	75%
0.30	0.004	0.314	96%
0.35	-0.020	0.338	121%
	Panel D. Specification 4		
0.25	0.026	0.289	74%
0.30	0.005	0.310	95%
0.35	-0.019	0.334	119%

Note: This table decomposes the effect of the UI cut into micro and macro effects for different matching function elasticities α. Columns 2 and 3 show the estimated change in search effort, s_t, and market tightness, θ_t, in Missouri between 2011q1 and 2012q1, respectively. Column 4 shows the relative contribution of the macro effect ($\Delta\theta_t$) to the change in job finding rate. These effects are calculated using the methodology described in Section 2 given the estimated effect of the policy on V/U and H/V from Table 1.

Panel A of Table 3 shows the results. We find an increase in market tightness θ ranging from 23.1% to 26.6%, depending on the elasticity of the matching function. Given that the cut in benefits was around 26 log points, these estimates imply that the elasticity of market tightness with respect to unemployment benefit duration ranges between -0.89 and -1.02.

Workers reacted to the cut in UI durations by raising their search effort. In fact, we find an effect of comparable magnitude—ranging from 1.2% to 4.7% in our baseline in Panel A. These estimates correspond to elasticities of search effort of -0.05 and -0.18, respectively.

As we discussed before, combined with the effect on market tightness, these estimates imply a sizable increase in the job finding rate of 10.5%. Of this increase, our decomposition implies that between 55% to 89% is due to the macro effect—a tightening of the labor market due to increased demand for labor in response to the UI cut.

Panels B, C, and D of Table 3 repeat the same exercises using the estimates from specifications 2, 3, and 4 from Table 1, respectively, which vary the start period of the panel or the method of seasonal adjustment for the difference-in-difference estimates. These decompositions bolster our conclusion that the majority of the increase in the job finding rate after the benefit cut is attributable to macro effects. For a matching function elasticity of 0.25, for example, we attribute between 53% and 75% of the total impact to macro effects.

We conclude that in response to the unexpected cut in UI durations, job finding rates improved in Missouri. This improvement reflects contributions both from changing search effort as well as macro effects due to labor demand with the latter accounting for at least one half of the total effect, and as an upper bound, nearly the entire effect for sufficiently high values of the matching function elasticity.

3.6 Robustness

We now explore the robustness of our findings to several choices in the analysis.

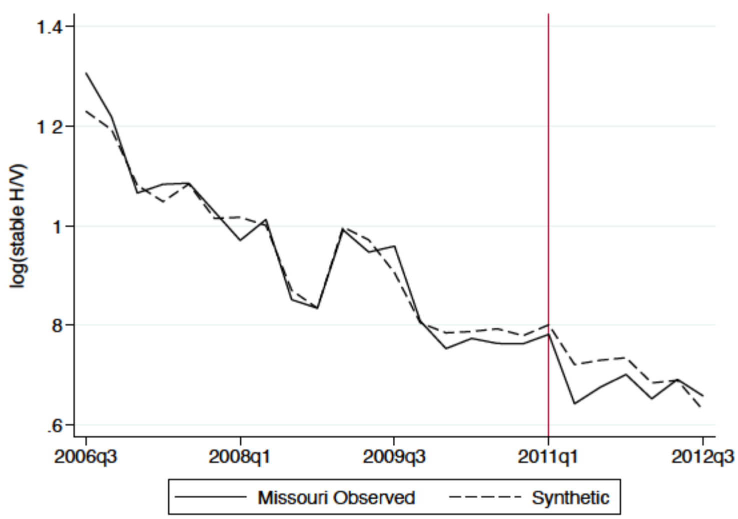
Results using stable hires. Our main results use a measure of hires from the QWI that counts all workers who start a job with an employer in a given quarter. This number may include hires that do not last a full quarter. These temporary hires may be hired differently, without being subject to search frictions in the labor market. To assess if this distinction matters for our results, we repeat the analysis using "stable hires." QWI defines a stable hire as someone who starts a job that lasts at least one full quarter with a given employer. More specifically, a hire is counted as a stable hire in the second of three consecutive quarters when an individual first receives earnings from the same employer.[19] This contrasts with our baseline measure, which considers a hire to occur in the first quarter of positive earnings with a new employer when an individual had no earnings with that employer in the previous

[19]See https://lehd.ces.census.gov/doc/QWI_101.pdf.

quarter. To distinguish the relative contribution of recalls, the QWI reports both "total stable hires" and "new stable hires" using the same criteria as the primary measure, with the difference between the two being stable recall hires. We focus on new stable hires, as recalls are unlikely to go through traditional recruiting (Fujita and Moscarini, 2017).

Figure 4 presents the results of the synthetic control for the vacancy filling rate using new stable hires. It is evident that the substantive conclusions do not change: Missouri's UI benefit cut lowered the vacancy filling rate and increased tightness of the labor market. Table 4 estimates the effect of the policy on the vacancy-unemployment ratio (unchanged from Table 1) and the stable vacancy filling rate, H_{stable}/V. The stable vacancy filling rate increases by 16–21 log points, depending on the length of the panel and the method of seasonal adjustment. This range is virtually identical to that estimated in Table 1 (17-22 log points). All estimates are significant at the 10% level and withstand a permutation test, being larger than more than 95% of placebo effects.

Figure 4: Effect on Vacancy Filling Rate using "New Stable Hires" Measure



Note: Synthetic control is constructed using pre-period (2006Q3-2011Q1) observations of interest. We consider 2011Q1 to be the time of treatment given the policy change. Measures are "new vacancies" and "new stable hires" from HWOL and QWI, respectively.

Table 4 also reports the labor market effects of the policy change under the alternative hires measure. The policy boosted the job finding rate by 0.11 to 0.12 (from a base job finding rate of 0.31) and lowered the steady state unemployment rate by 0.6–0.7 percentage points. These results are essentially the same as in the baseline.

Finally, we repeat the decomposition of the effect into micro and macro effects with the stable hires measures (Table 5). Depending on the matching function elasticity and the

Table 4: Estimated Effects of UI Duration Cut on Missouri Labor Market using Stable Hires Measure

	(1)	(2)	(3)	(4)
A. $Vacancies/Unemployment$				
$\hat{\beta}_{MO\_post}$	0.278***	0.275***	0.318***	0.315***
One-way clustered SE	(0.032)	(0.032)	(0.035)	(0.035)
Two-way clustered SE	(0.030)	(0.030)	(0.033)	(0.033)
Percentile Rank	9.3%	9.3%	9.3%	9.3%
B. $StableHires/Vacancies$				
$\hat{\beta}_{MO\_post}$	-0.156***	-0.156***	-0.203***	-0.205***
One-way clustered SE	(0.015)	(0.015)	(0.018)	(0.018)
Two-way clustered SE	(0.015)	(0.015)	(0.018)	(0.018)
Percentile Rank	4.7%	4.7%	4.7%	4.7%
Implied Effects on f and u_{ss}				
Δ log(f)	0.122	0.119	0.115	0.110
Δu_{ss} (percentage points)	-0.71	-0.69	-0.66	-0.63
Start Period	2006Q3	2006Q3	2005Q3	2005Q3
End Period	2012Q3	2012Q3	2012Q3	2012Q3
Seasonal Adjustment as Ratios		✓		✓
N	1,100	1,100	1,276	1,276

Note: Vacancies and Stable Hires are new measures, as documented in HWOL and QWI respectively. Balanced panel includes all states except AR, FL, GA, MA, MI, SC and DC. Vacancies, Hires, and Unemployment are either seasonally adjusted independently or as ratios V/U and H/V. Percentile Rank is based on the percentage of permutations (where treatment is designated as beginning 2011Q2 in a given state) which yield a placebo treatment coefficient larger (smaller) than Missouri's estimate for V/U (H/V). Placebo tests are calculated with Missouri dropped from the comparison group sample. One-way clustered standard errors are clustered at the worker level, and two-way are clustered at the worker-quarter level. The table includes the imputed response of the job finding rate and steady-state unemployment rate (p.p.) according to equation 5 and $u_{ss} = s/(s+f)$. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Decomposition of Micro and Macro Effects using Stable Hires Measure

Matching function elasticity, α	Search effort Δs_t	Market tightness $\Delta \theta_t$	% Equilibrium effect in $\Delta \log(f)$
	Panel A. Specification 1		
0.25	0.070	0.208	43%
0.30	0.055	0.223	55%
0.35	0.038	0.240	69%
	Panel B. Specification 2		
0.25	0.067	0.208	44%
0.30	0.052	0.223	56%
0.35	0.035	0.240	71%
	Panel C. Specification 3		
0.25	0.047	0.271	59%
0.30	0.028	0.290	76%
0.35	0.006	0.312	95%
	Panel D. Specification 4		
0.25	0.042	0.273	62%
0.30	0.022	0.293	80%
0.35	-0.000	0.315	100%

Note: This table decomposes the effect of the UI cut into individual and market-level effects for different matching function elasticities α. Columns 2 and 3 show the estimated change in search effort, s_t, and market tightness, θ_t, in Missouri between 2011q1 and 2012q1, respectively. Column 4 shows the relative contribution of the market-level effect ($\Delta\theta_t$) to the change in job finding rate. These effects are calculated using the methodology described in Section 2 given the estimated effect of the policy on V/U and H/V from Table 4.

specification, we infer an increase of search effort up to 7 log points. These changes in search effort imply that at least 43% of the increase in the job finding rate is attributable to the macro effect. The upper bound for the estimate is 100% for the case of $\alpha = 0.35$ using specification 4 (Panel D).

Robustness to other choices. When running the synthetic control on different outcome variables, we re-estimate the weights for each variable, which implies that the synthetic control to which we compare Missouri can potentially differ across outcomes.[20] One might be concerned that the effects across different margins cannot be compared to each other since each corresponds to a different control. To investigate this issue, in Appendix A.2 we check the sensitivity of our estimates to using a fixed set of weights. More specifically, we fix the weights of the synthetic control for a given outcome variable (H/V and V/U) and construct a control state using the weights corresponding to it for all the other outcomes of interest. The findings in this section validate our conclusions about the effects of Missouri's policy change on labor market tightness and the vacancy filling rate.

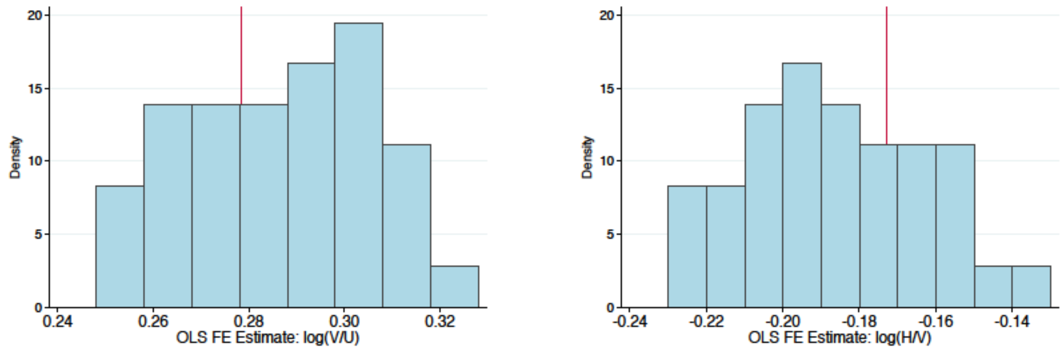
Another issue is how to pick the pre-period length in the construction of the synthetic control, the frequency with which the predictor variable was matched, and whether to use covariates in the matching. We explore the implications of our baseline choices in Appendix A.3. These various ways of visualizing the labor market effect of the policy further corroborate our main findings.

Lastly, we explore the robustness of the difference-in-difference estimates of the effects of policy on the labor market. Here, we consider varying the choice of the sample period and the seasonal adjustment method. We vary the starting period of the panel from 2005Q3 to 2006Q4 and the last period from 2012Q2 to 2012Q4. These choices combined with different seasonal adjustment methods yield 36 point estimates for each outcome variable. Figure 5 shows the histogram of these estimates. The red vertical line denotes our baseline estimate from Table 1. The effect on the vacancy-unemployment ratio is always positive, ranging from 0.25 to 0.33 (Figure 5a), and the effect on hires per vacancies are always negative (Figure 5b). These histograms also show that our baseline estimate is in line with estimates from other permutations.

How do the decomposition results depend on these choices? To see this, we fix an elasticity for the matching function α and compute the share of the job finding rate increase attributable to the macro effect for each estimate reported in the histogram on Figure 6. For $\alpha = 0.25$, we find that at least 40% of the increase in the job finding rate is due to a higher availability of jobs. Appendix B.1 presents the results for $\alpha = 0.2$ and 0.3, reaching similar

[20] We report the states and their relative weights for each outcome variable in Appendix A.1.

Figure 5: Histograms for Estimated Policy Effect on Vacancy-Unemployment Ratio and the Vacancy Filling Rate



(a) $\log(V/U)$ (b) $\log(H/V)$

Note: Panels represent histograms of estimated $\hat{\beta}$ from equation (6), the effect of Missouri's UI benefit cut on log(vacancies/unemployed) and log(hires/vacancies), estimated on panels of various lengths and data series seasonally adjusted using differing methods. Point estimates are results of regressions with varying start periods (2005Q3-2006Q4), end periods (2012Q2-2012Q4), and seasonal adjustment methods (adjusting V/U and H/V as ratios or adjusting their components separately). These combinations yield 36 point estimates. The vertical line denotes our baseline estimate from Table 1. Measures used are "new vacancies" and "new hires" from HWOL and QWI, respectively.

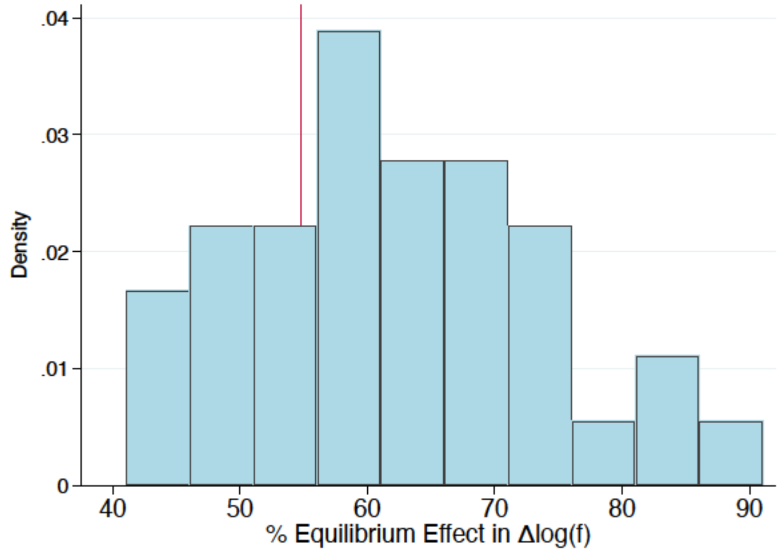
conclusions.

3.7 Comparison to Johnston and Mas (2018)

Given that Johnston and Mas (2018) exploit the same policy reform in Missouri to identify the micro effect of the cut in UI on worker search behavior, it is instructive to clarify the differences and complementarities between their approach and ours. Johnston and Mas (2018) focus on the search behavior of individuals receiving unemployment insurance by exploiting the policy discontinuity for individuals laid off just before and just after the cut. They estimate the change in hazard for leaving unemployment in that sample. They apply that hazard to initial unemployment claims after April 2011 to generate a counterfactual unemployment prediction. While valuable, that analysis is incomplete for measuring the total state-wide change in unemployment: it omits the behavior of unemployed households not receiving benefits.[21] Non-claimants account for the majority of the unemployed in Missouri in our sample. Our decomposition of the micro effect captures the average effect between claimants and non-claimants. That allows us to identify the macro effect, which Johnston and Mas (2018) difference out by construction. Our findings combined with theirs suggest a

[21] It also misses the behavior of separations and participation decisions as discussed in Section 3.4.3.

Figure 6: Histogram of the share of job finding rate increase due to macro effect, $\alpha = 0.25$



Note: Histograms of estimated share of the increase in the job finding rate that is attributable to equilibrium (market-level) effects. Imputations assume α=0.25. Estimates result from regressions with varying start periods (2005Q3-2006Q4), end periods (2012Q2-2012Q4), and seasonal adjustment methods (adjustment of V/U and H/V as ratios or U, V, and H as separate components). These combinations yield 36 point estimates. The vertical line denotes our baseline estimate from Panel A of Table 3.

negative impact on the search effort of non-claimants, which we interpret as the value of a job to non-claimants declining due to an increase in market tightness.[22]

4 Conclusions

Given the prominent role of UI benefit extensions as an automatic stabilizer, it is critical for policymakers and economists to understand their effects and the channels through which they operate. While the micro labor literature historically has primarily focused on the worker search effort channel, Hagedorn et al. (2013) demonstrated that the job creation decisions of firms can also potentially respond to changes in UI policies, affecting the job prospects of all workers within a market. Despite the sizable literature that emerged in response to that paper, to the best of our knowledge, no paper has provided a simultaneous investigation of both the micro and macro channels.

This paper fills that gap by implementing a unified methodology that allows the joint measurement of the micro and macro effects. Our approach relies on minimal, standard as-

[22]Gałecka-Burdziak et al. (2021) use a similar design to Johnston and Mas (2018) to study an unexpected cut in UI benefit duration in Poland. They also find non-trivial effects on both search effect and aggregate unemployment, further reinforcing our findings in a different context.

sumptions and requires data on vacancies, unemployment and hires. Following Johnston and Mas (2018), we apply this method to Missouri, which experienced a large and unanticipated cut in potential UI durations in April 2011. We find that about 50% of the decline in unemployment following this policy change is attributable to higher exits from unemployment into jobs. Importantly, we estimate macro effects to be sizable—accounting for at least 50% of the total effect according to our most conservative estimates.

Our findings have important implications for designing optimal UI policies over the business cycle. Landais et al. (2018b) emphasize the importance of the relative size of the micro elasticity of search effort to benefit duration and the macro elasticity of tightness for the cyclicality of optimal policy. The results presented in this paper suggest a negative elasticity wedge during this episode—namely that the unemployment benefit duration of 73 weeks in Missouri in 2011 was too generous from the point of view of a utilitarian planner. Because our analysis pertains to a single state at a particular time, more work is needed—both empirically and theoretically—to make more general statements about the optimal response of unemployment benefit duration to different aggregate shocks.

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Online Appendix—Not for Publication

A Further Results: Synthetic Control

This section includes several results related to synthetic control that are not provided in the main text. In Section A.1, we report the states and their weights that are assigned a positive weight for H/V and V/U. Because the state-specific weights resulting from the matching differ across these two outcomes, in Section A.2 we construct synthetic controls using a fixed set of weights. In Section A.3, we test the sensitivity of the baseline results in Figure 2 to alternative ways of constructing the synthetic control.

A.1 State weights

The synthetic control method used in Section 3 assigns weights to non-treated states to construct a "synthetic Missouri," which allows us to assess the effect of the policy. Below we identify the states used to construct the synthetic control and proceed with robustness exercises. We exclude from the donor pool states that likewise cut UI benefit duration during the period of our analysis.[23] Further, the QWI—derived from a partnership between labor market information divisions of state governments and the Census Bureau Local Employment Dynamics—supplies data for all states since at least 2010 (although most entered much earlier). Because Massachusetts entered the QWI only in 2010, we drop it from the donor pool as well. Lastly, we exclude Washington, D.C.

Table A.1 presents the weights assigned to different states in the synthetic control for V/U and H/V in Figure 2. With the exception of North Carolina, which is heavily weighted in both, the set of states which comprise the synthetic vacancy-unemployment ratio is largely disjoint from those which comprise the synthetic vacancy-filling rate.

[23] These states are Arkansas, Florida, Georgia, Michigan, and South Carolina.

Table A.1: State weights in the baseline synthetic control

State	V/U	H/V
Alaska	0	0.262
Kentucky	0.198	0
Mississippi	0.156	0
Nevada	0	0.112
New Hampshire	0	0.023
North Carolina	0.208	0.477
North Dakota	0.035	0
Ohio	0	0.076
Oklahoma	0.055	0
South Dakota	0	0.051
Vermont	0.328	0
Wisconsin	0.021	0

Note: This table presents the state-specific weights used to construct a synthetic control for V/U and H/V in our baseline specification, represented in Figure 2.

Table A.2: Synthetic control weights using a different time frame (Figure A.2)

State	V/U	H/V
Alaska	0.054	0.079
Hawaii	0	0.048
Kentucky	0.147	0
Mississippi	0.023	0
New Hampshire	0	0.209
North Carolina	0.160	0.137
North Dakota	0.053	0
Ohio	0	0.048
Oklahoma	0.233	0.479
Tennessee	0.266	0
West Virginia	0.064	0

Note: This table presents the state-specific weights used to construct a synthetic control for V/U and H/V in our alternate specification using a time frame which begins in 2005Q3, represented in Figure A.2.

Table A.3: Synthetic control weights when matching on every fourth observation (Figure A.3)

State	V/U	H/V
Alaska	0.008	0.256
Delaware	0	0.147
Kentucky	0.550	0
Nebraska	0.181	0
North Carolina	0	0.316
Ohio	0	0.128
Oklahoma	0.010	0
Tennessee	0	0.153
Vermont	0.251	0

Note: This table presents the state-specific weights used to construct a synthetic control for V/U and H/V in our alternate specification matching on every fourth pre-period observation (instead of every observation), represented in Figure A.3.

Table A.4: Synthetic control weights using additional predictors in matching (Figure A.4)

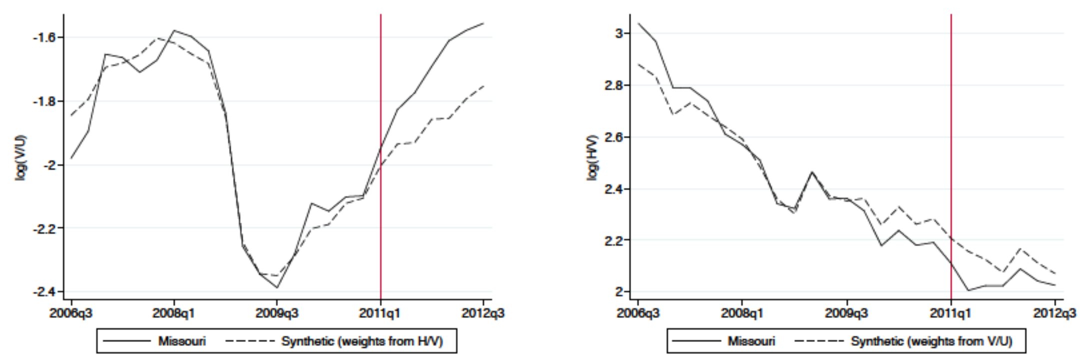
State	V/U	H/V
Alaska	0.174	0.157
Connecticut	0.039	0
Delaware	0.133	0.160
Hawaii	0.004	0.039
Iowa	0.030	0.061
Maine	0.023	0.028
Mississippi	0.155	0.127
Nevada	0.013	0
Ohio	0.315	0.357
Rhode Island	0.063	0.056
South Dakota	0.052	0.009
Vermont	0	0.007

Note: This table presents the state-specific weights used to construct a synthetic control for V/U and H/V in our alternate specification using explicit sector and other economic predictors, represented in Figure A.4.

A.2 Results using fixed state weights

As the previous section has shown, the state weights differ quite a bit across the two labor market outcomes (V/U and H/V). To ensure our results are not driven by the different composition of the synthetic Missouri, we adopt a "weight fixing" approach. Here, we use a set of weights that we fix across outcomes when constructing the synthetic control. Specifically, the weights are derived from the synthetic control of *either* $\log(V/U)$ or $\log(H/V)$ and are used to construct a control group for both measures. The results are essentially the same as the baseline for the vacancy-unemployment ratio and the vacancy filling rate: i) the difference in the vacancy-unemployment ratio increases more in Missouri relative to the control and this happens gradually after the policy change. ii) the vacancy filling rate drops precipitously in Missouri (Figure A.1).

Figure A.1: Effects on V/U and H/V using fixed weights



(a) $\log(V/U)$

(b) $\log(H/V)$

Note: Plot represents Missouri's observed and synthetic log(vacancies/unemployed) (panel (a)) and log(hires/vacancies) (panel (b)), where synthetic controls are constructed using weights resulting from Figure 2 for $\log(H/V)$ and $\log(V/U)$. Weights are listed in Table A.1. Pre-period covers 2006Q3-2011Q1. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. Measures are "new vacancies" and "new hires" from HWOL and QWI, respectively.

A.3 Alternative specifications

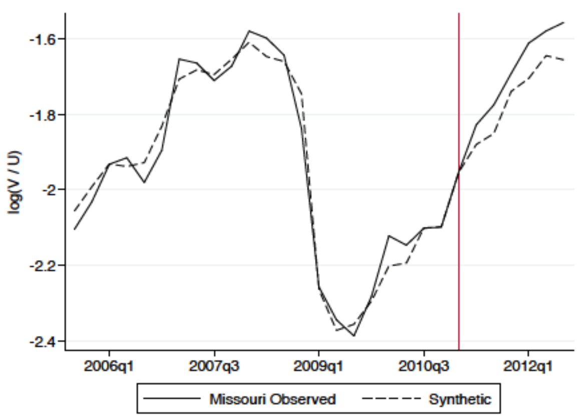
To operationalize the synthetic control method, we made choices regarding the matching period, the frequency of matching on pre-treatment values, and whether or not to use other predictor variables. In this section, we consider alternative choices to test the sensitivity of our baseline findings of the effect of Missouri's UI cut on market tightness.

Figure A.2 applies the synthetic control method to V/U and H/V in the same manner as Figure 2 but designates 2005Q3 (the earliest period for which we have vacancy data from

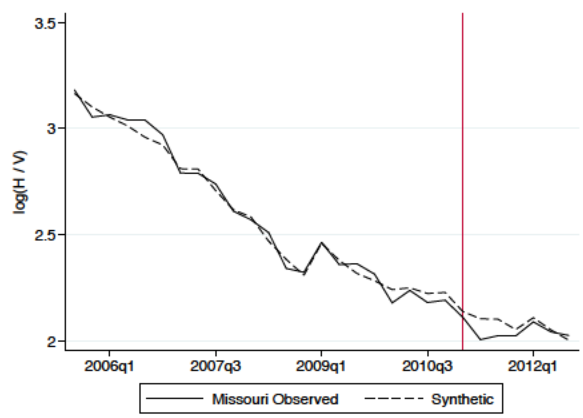
HWOL) as the beginning period. Figure A.3 alters our baseline approach by instead constructing the synthetic control using every fourth observation for the outcome of interest, rather than every observation. Lastly, Figure A.4 constructs a counterfactual using other predictor variables as in Johnston and Mas (2018). Time-varying state-level predictor variables for the pre-period (2006Q3-2011Q1, every fourth observation) are the unemployment rate, home ownership rate, and the share of workers employed in manufacturing, education and health, leisure and hospitality, and finance, insurance, and real estate. Time-invariant predictors include the percent of the state's population in rural areas and housing price growth from 1999–2006 to 2007–2010.

These results support the main conclusions in the paper: the sudden cut to potential UI durations had a clear, discernible effect on the vacancy-unemployment ratio and the vacancy-filling rate.

Figure A.2: Synthetic Control Results with Longer Pre-Treatment Period



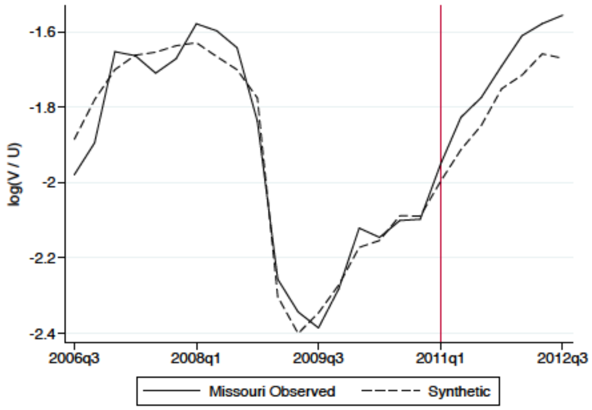
(a) $\log(V/U)$



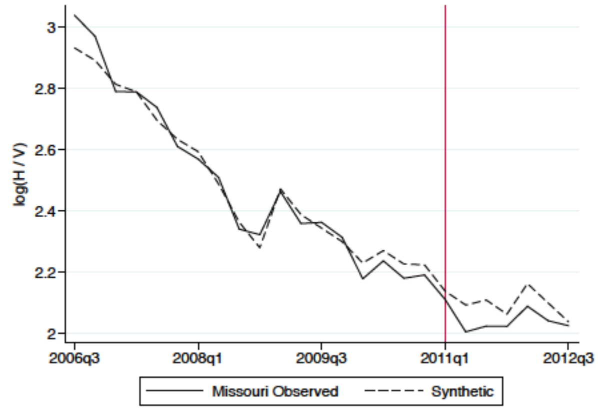
(b) $\log(H/V)$

Note: Panels (a) and (b) plot Missouri's observed and synthetic log(vacancies/unemployed) and log(hires/vacancies), respectively. Synthetic controls are constructed using observations between 2005Q3 (the earliest quarter available for vacancy data) and 2011Q1.

Figure A.3: Synthetic Control Results with Lower Frequency of Pre-Period Matching



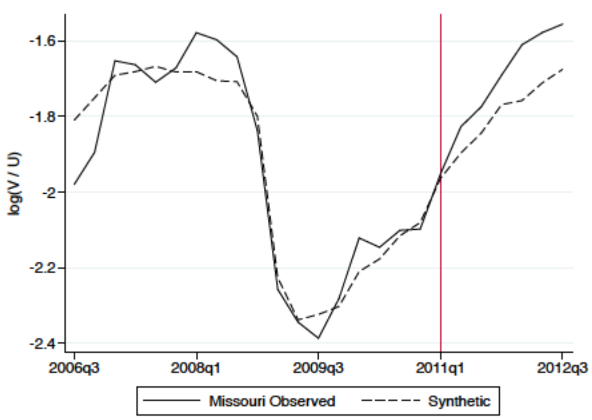
(a) $\log(V/U)$



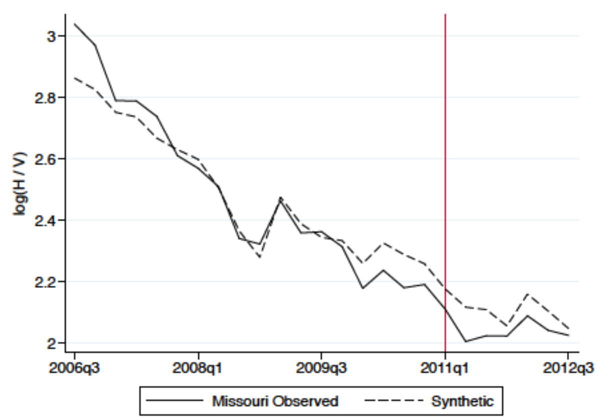
(b) $\log(H/V)$

Note: Panels (a) and (b) plot Missouri's observed and synthetic log(vacancies/unemployed) and log(hires/vacancies), respectively. Synthetic controls are constructed using observations between 2006Q3 and 2011Q1. The predictor is matched on every fourth pre-treatment observation, rather than every observation as in the baseline.

Figure A.4: Synthetic Control Results with Predictor Variables



(a) $\log(V/U)$



(b) $\log(H/V)$

Note: Panels (a) and (b) plot Missouri's observed and synthetic log(vacancies/unemployed) and log(hires/vacancies), respectively. Synthetic controls are constructed using the following state-level predictor variables: the unemployment rate, home ownership rate, the change in housing prices from 1999-2006 and 2007-2010, the percent of the state's population in rural areas, and the share of workers employed in manufacturing, education and health, leisure and hospitality, and finance, insurance, and real estate.

B Further Results: Difference-in-Difference

This section includes several difference-in-difference results not provided in the main text, their resulting decompositions, and histograms of point estimates with using different sample selections.

B.1 Sensitivity Checks for Difference-in-Difference Equation (6)

As a reminder, the difference-in-difference regression underlying our main empirical findings is equation (6):

$$y_{st} = \lambda_s + \gamma_t + \beta \cdot D_{st} + \varepsilon_{st}$$

where D_{it} is an indicator which equals 1 when s is Missouri and t is greater than or equal to 2011Q2. λ_s and γ_t are state and time fixed effects. We estimate this specification starting in either 2006Q3 or 2005Q3 (the earliest quarter of HWOL data) and ending in 2012Q3. Our baseline results seasonally-adjust V, H, and U separately.

As a sensitivity check, we show that our baseline choices for start date, end date, and seasonal-adjustment method for equation (6) do not yield outlier estimates for the effect of the policy on $\log(V/U)$ or $\log(H/V)$. Specifically, we apply equation (6) to panels with 6 different start dates (2005Q3, 2005Q4, 2006Q1, 2006Q2, 2006Q3, 2006Q4), 3 different end dates (2012Q2, 2012Q3, 2012Q4), and 2 different methods of seasonal adjustment (adjusting V, U, and H separately or adjusting V/U and H/V as ratios). The cross-product of these three sets of preparing the data yield 36 potential panels on which we can estimate the relationship between Missouri's UI policy and its labor market effects.

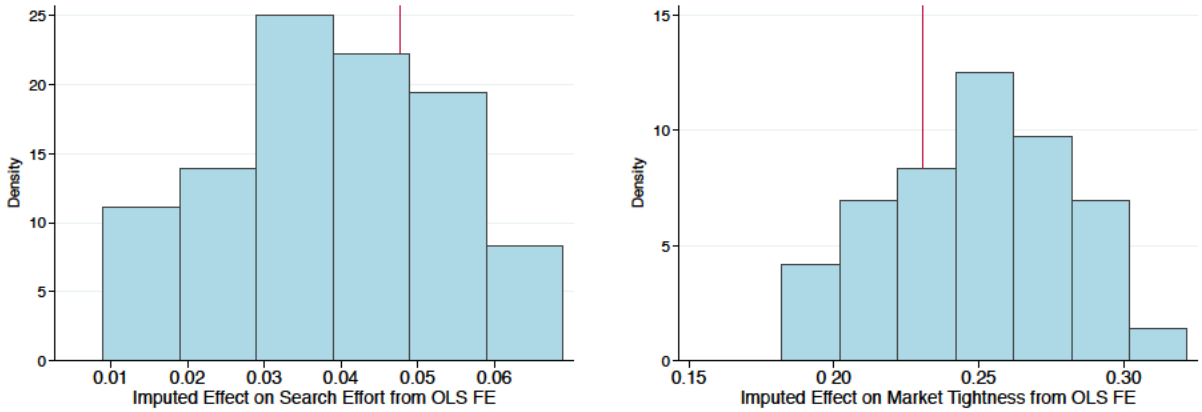
Figures B.1, B.2, and B.3 apply the histogram representation (results from 36 possible panel datasets where the vertical red line indicates our preferred estimate) to our decomposition of individual and market-level effects for matching function elasticities of $\alpha = 0.25$, 0.30, and 0.35, respectively. For each elasticity, the imputed search effort s (panel a of the figures) and imputed market tightness θ (panel b) which result from differently-specified panels follow a normal distribution. For example, when $\alpha = 0.25$, our preferred imputed effects on search effort s of 0.047 log points and market tightness θ of 0.231 log points (both from Table 3, Panel A) are very close to the modal Δs and $\Delta\theta$ in the histograms of Figure B.1. The same goes for our preferred estimates from Table 3, Panel A and the histograms in Figures B.2 and B.3 for greater elasticities of the matching function.

The findings from Figure B.1 yields Figure 6 in the main body, which shows the histogram for the imputed share of the job finding rate's increase that is attributed to equilibrium (or market-level) effects when $\alpha = 0.25$. The possible shares range between 41% and 89%, with our preferred share estimate of 55% from Table 3, Panel A (the vertical red line) is very close to the modal bin in the histogram, which covers the 60% mark. This histogram illustrates that our estimated contribution of market-level effects is not the result of sample selection to yield a particularly high or low estimate.

B.2 Matching Function Elasticity

We justify our choice of matching function by illustrating a clear linear relationship between the log of hires and vacancy-unemployment ratio for our sample period across all U.S. states. Specifically, Figure B.4 plots the population-weighted average of the vacancy-unemployment ratio to that of hires (both logged) over our sample period.

Figure B.1: Histograms for Effect on Search Effort and Market Tightness, $\alpha = 0.25$

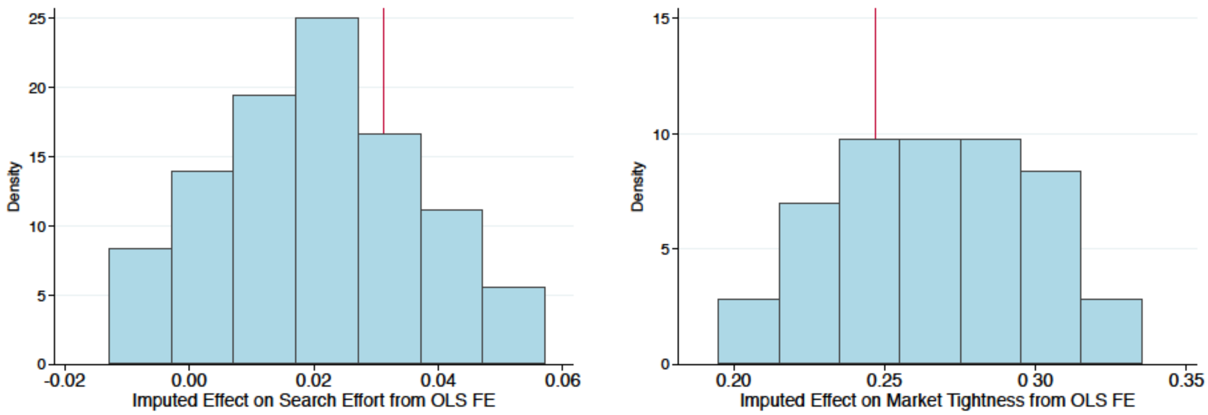


(a) Imputed Search Effort, s

(b) Imputed Market Tightness, θ

Note: Histograms of imputed search effort and market tightness responses to the policy change using estimated $\hat{\beta}_{V/U}$, $\hat{\beta}_{H/V}$, and $\hat{\beta}_{JFR}$ through the decomposition provided by equation (5). Imputations assume α=0.25. Estimates result from regressions with varying start periods (2005Q3-2006Q4), end periods (2012Q2-2012Q4), and seasonal adjustment methods (adjustment of V/U and H/V as ratios or U, V, and H as separate components). These combinations yield 36 point estimates. The vertical line denotes our baseline estimate from Panel A of Table 3.

Figure B.2: Histograms for Effect on Search Effort and Market Tightness, $\alpha = 0.30$

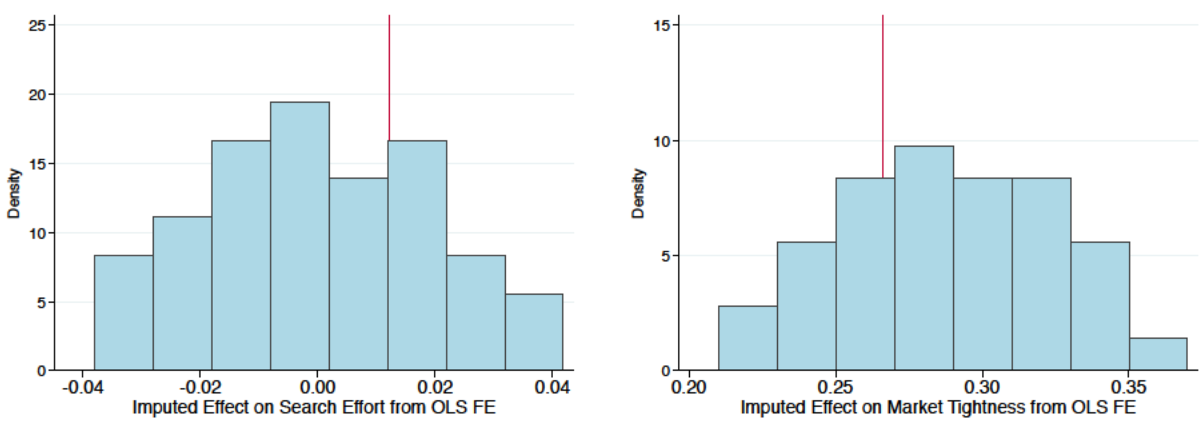


(a) Imputed Search Effort, s

(b) Imputed Market Tightness, θ

Note: Histograms of imputed search effort and market tightness responses to the policy change using estimated $\hat{\beta}_{V/U}$, $\hat{\beta}_{H/V}$, and $\hat{\beta}_{JFR}$ through the decomposition provided by equation (5). Imputations assume α=0.30. Estimates result from regressions with varying start periods (2005Q3-2006Q4), end periods (2012Q2-2012Q4), and seasonal adjustment methods (adjustment of V/U and H/V as ratios or U, V, and H as separate components). These combinations yield 36 point estimates. The vertical line denotes our baseline estimate from Panel A of Table 3.

Figure B.3: Histograms for Effect on Search Effort and Market Tightness, $\alpha = 0.35$

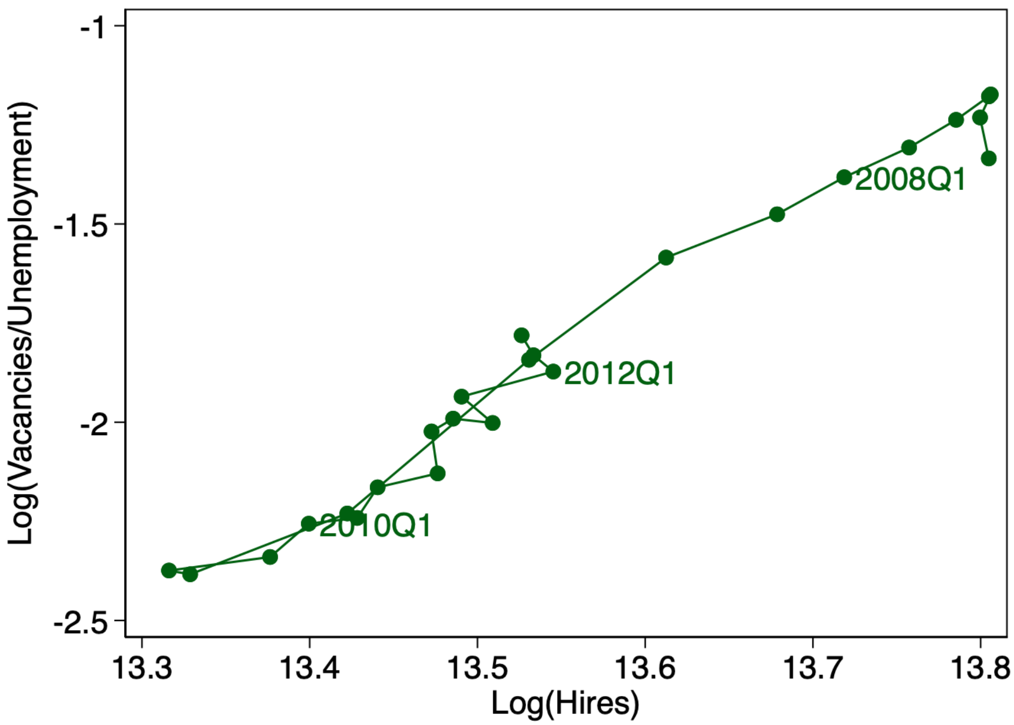


(a) Imputed Search Effort, s

(b) Imputed Market Tightness, θ

Note: Histograms of imputed search effort and market tightness responses to the policy change using estimated $\hat{\beta}_{V/U}$, $\hat{\beta}_{H/V}$, and $\hat{\beta}_{JFR}$ through the decomposition provided by equation (5). Imputations assume α=0.35. Estimates result from regressions with varying start periods (2005Q3-2006Q4), end periods (2012Q2-2012Q4), and seasonal adjustment methods (adjustment of V/U and H/V as ratios or U, V, and H as separate components). These combinations yield 36 point estimates. The vertical line denotes our baseline estimate from Panel A of Table 3.

Figure B.4: Vacancies-Unemployment vs. Hires, All States, 2006Q3–2012Q3



Note: Vacancies, unemployment, and hires are calculated as population-weighted averages of all 50 states. Next, the vacancy measure is divided by the unemployment measure. Last, we take logs of V/U and H measures.

C Measures in LAUS and CPS

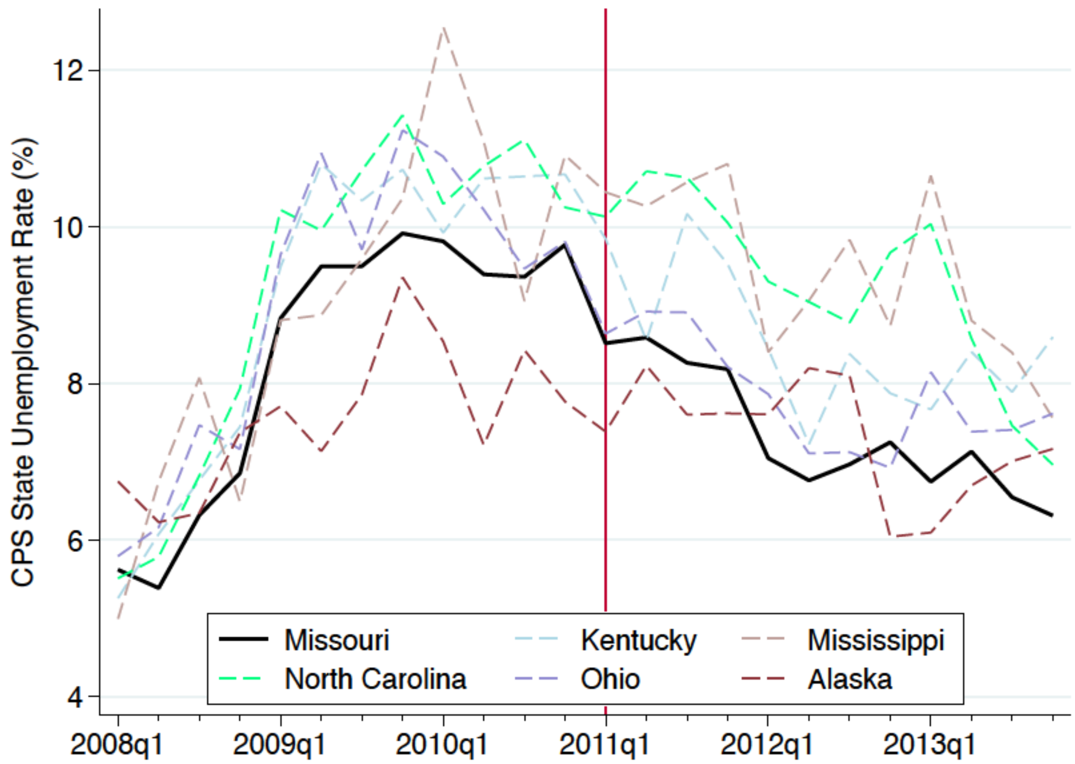
In this section we first show how measures of unemployment rates constructed from the the CPS compare to the measure used to construct the vacancy-unemployment ratio, which come from the BLS Local Area Unemployment Statistics (LAUS). To this end, we compare these two measures in the states that get positive weights in the synthetic control (Section C.1). We then show the labor market flow rates computed using the panel structure of the CPS (Section C.2).

C.1 Unemployment Rate

Because the CPS is not designed to be representative at the state level, the LAUS program estimates state-level unemployment and employment by drawing on a variety of data sources such as quarterly census of payroll employment data from administrative records, including the universe of UI claims. While our main analysis relies on unemployment data from LAUS, we can measure a state-level unemployment rate (albeit noisily) using the survey-based CPS.

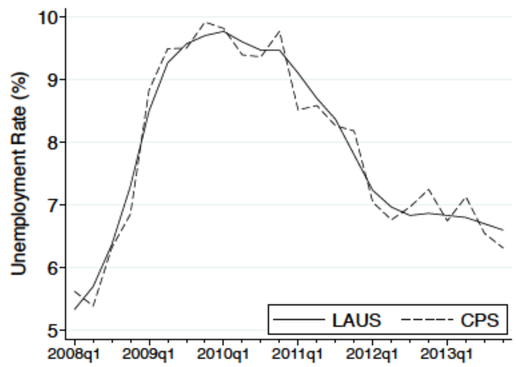
Figure C.1 plots the quarterly unemployment rate according to the CPS for Missouri and five of the states with the heaviest weights used for synthetic control for the vacancy-to-unemployment ratio or the vacancy-filling rate. Consistent with Johnston and Mas (2018), Missouri's unemployment rate declined sharply relative to states used for synthetic control in the quarters following the UI cut. Figures C.2 and C.3 compare the unemployment rates according to LAUS and CPS for Missouri and the 10 states that were assigned non-zero weights in our synthetic control analysis.

Figure C.1: Unemployment Rates for Missouri and Control States (Derived from CPS)

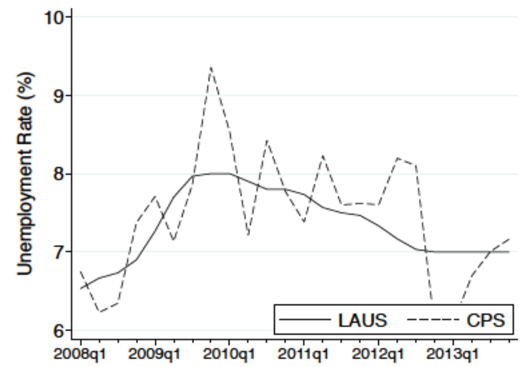


Note: Plot shows unemployment rates calculated from the Current Population Survey (CPS) for Missouri and five of the states which are assigned positive weights to construct synthetic control for a given construction of either H/V or V/U, with North Carolina being weighted to construct multiple synthetic measures. The unemployment rate for each state is calculated as a simple quotient of the count of unemployed and the count of those in the labor force.

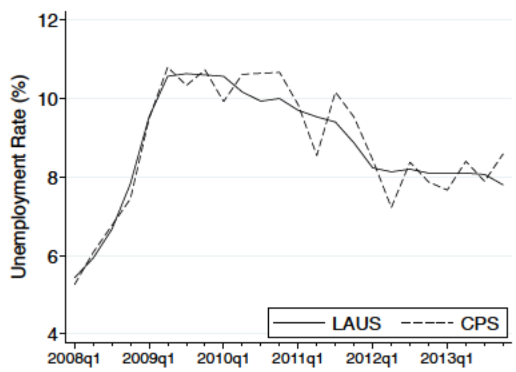
Figure C.2: State Unemployment Rates: LAUS and CPS Comparisons



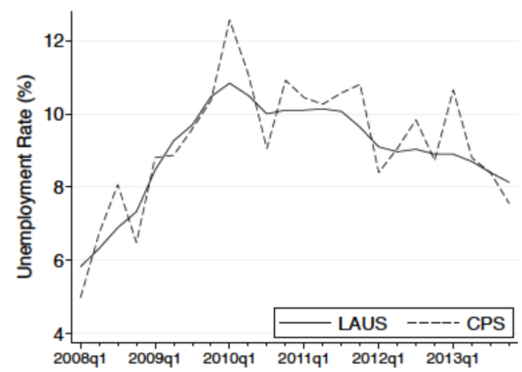
(a) Missouri



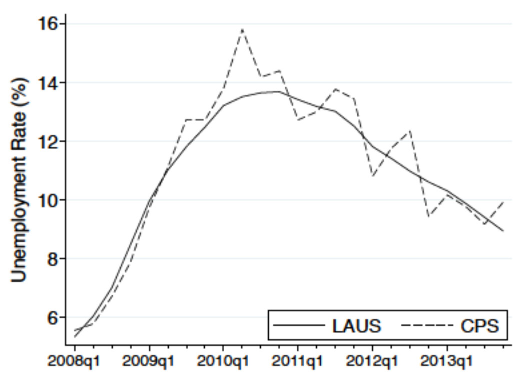
(b) Alaska



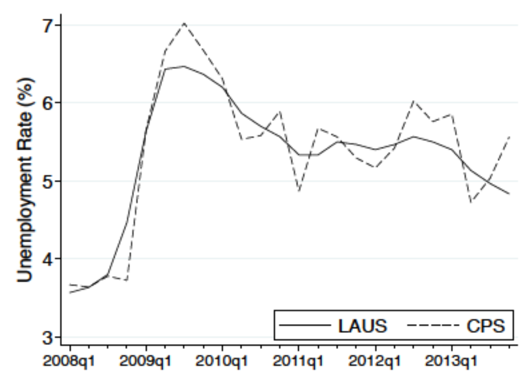
(c) Kentucky



(d) Mississippi



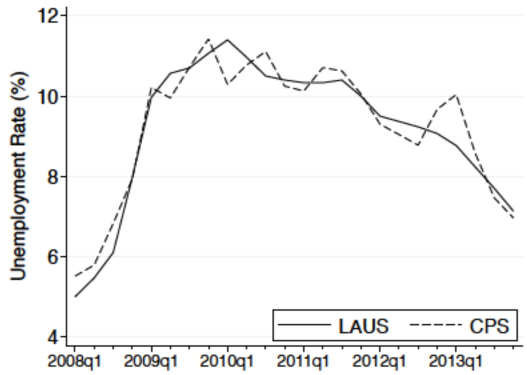
(e) Nevada



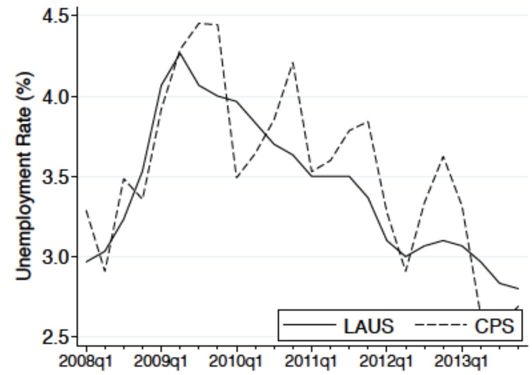
(f) New Hampshire

Note: Figure presents unemployment rates for states that are weighted to construct a synthetic Missouri (for either V/U or H/V). Solid lines report the official unemployment rate as reported by LAUS, while the dashed line is the unemployment rate as constructed from the monthly CPS alone.

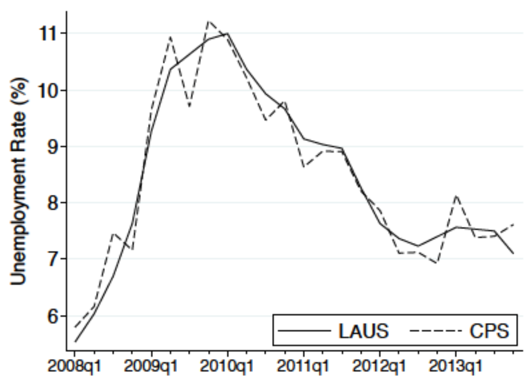
Figure C.3: State Unemployment Rates: LAUS and CPS Comparisons



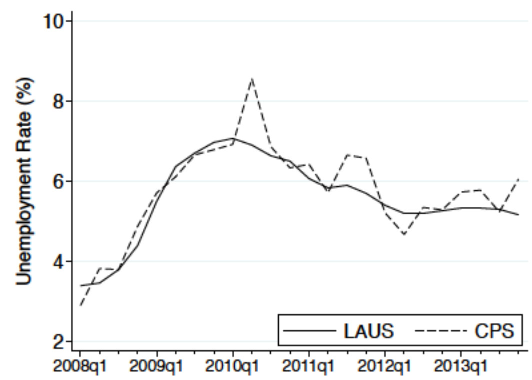
(a) North Carolina



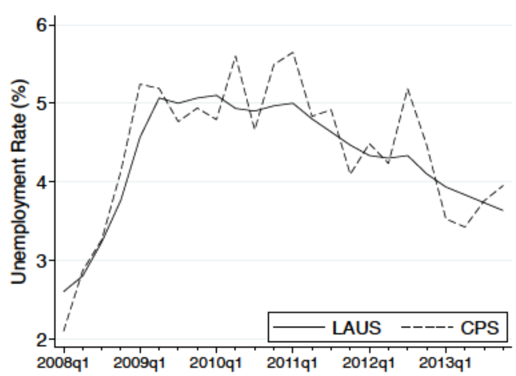
(b) North Dakota



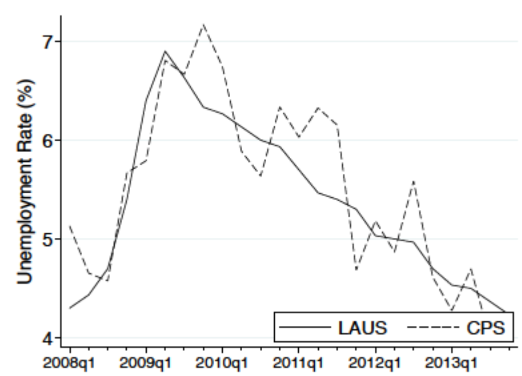
(c) Ohio



(d) Oklahoma



(e) South Dakota

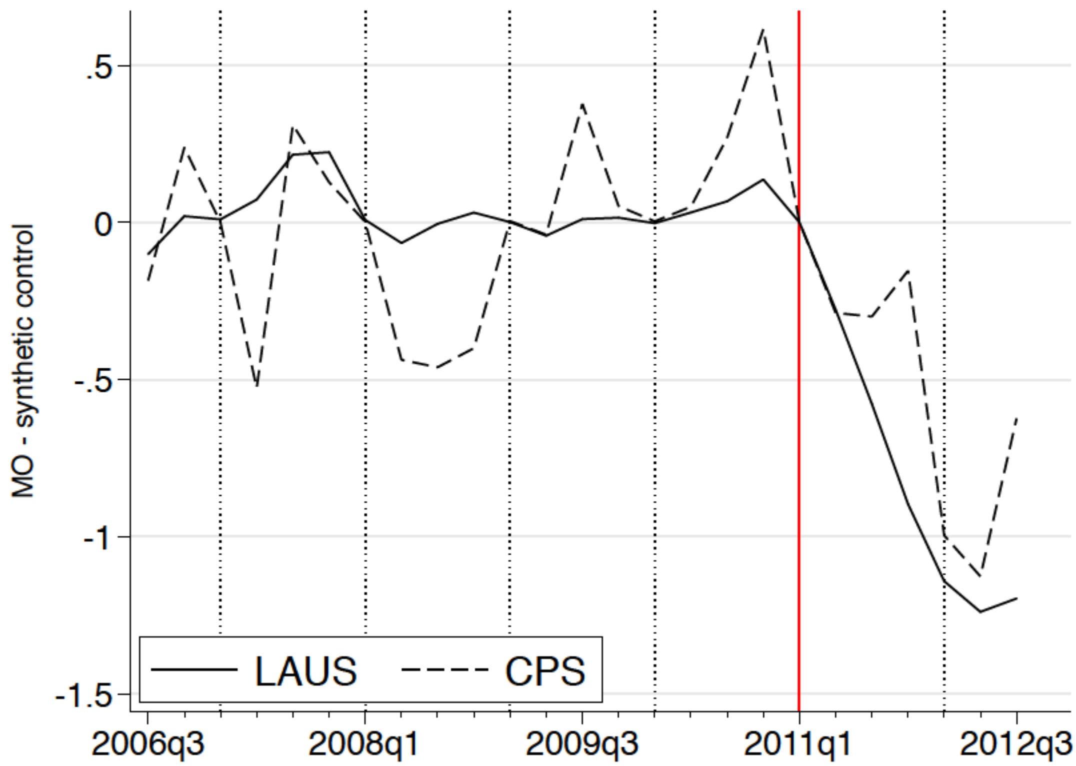


(f) Vermont

Note: Figure presents unemployment rates for states that are weighted to construct a synthetic Missouri (for either V/U or H/V). Solid lines report the official unemployment rate as reported by LAUS, while the dashed line is the unemployment rate as constructed from the monthly CPS alone.

Figure C.4 plots the results of synthetic control approach for Missouri's quarterly unemployment rate according to LAUS (result from Figure 1b) and the CPS, showing the effect of the duration cut on unemployment is similar regardless of the measure.

Figure C.4: Synthetic Control for Missouri Unemployment Rate: LAUS and CPS

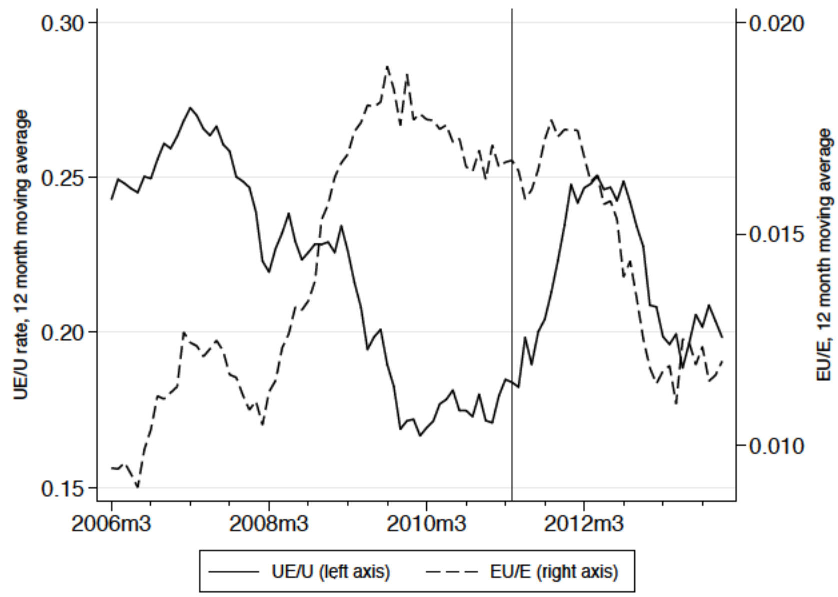


Note: Graphs plots the results of synthetic control approach (difference between Missouri's observed and synthetic values) for two different measures of Missouri's unemployment rate: the official reported unemployment rate from the BLS's Local Area Unemployment Statistics (LAUS, solid line) and the computed unemployment rate from the Current Population Survey, which equals the unemployed count divided by the number in the labor force. States which cut UI benefits around the time of Missouri's policy change are excluded from the donor pool.

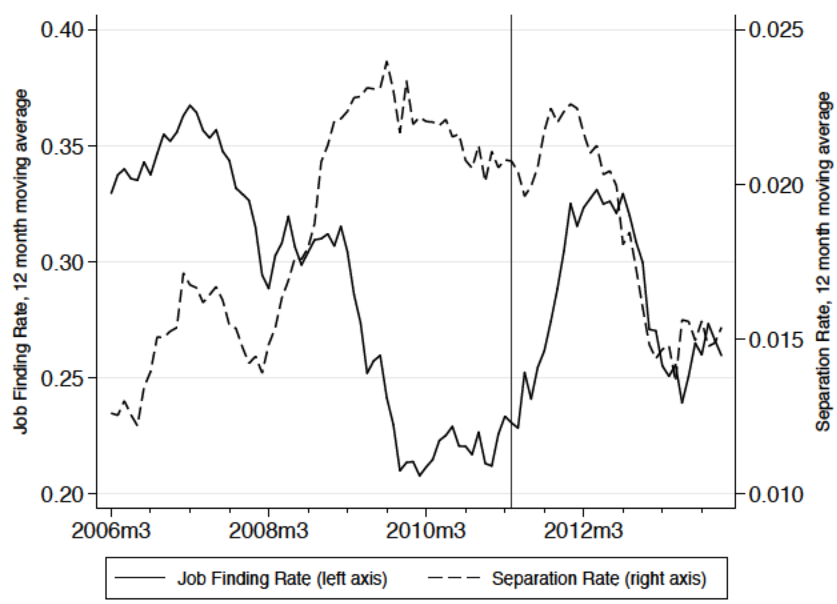
C.2 Labor Market Flows

Top panel of Figure C.5 shows monthly labor market transition rates for Missouri from the CPS between 2009 and 2013. We adjust these transition rates for time aggregation to obtain continuous time inflow and outflow rates, which we refer to as job finding (f) and separation (s) rates (Shimer, 2012; Elsby et al., 2015). These rates are plotted in the bottom panel.

Figure C.5: Labor Market Flows in Missouri from CPS



(a) UE and EU flow rates



(b) s and f rates

Note: Graph plots the 12-month moving averages of labor market flow rates in Missouri from the Current Population Survey. Panel (a) plots raw UE/U and EU/E as measured in the CPS monthly files. Panel (b) plots the job finding rate (f) and separation rate (s) after adjustment for time-aggregation bias as outlined by Shimer (2012). All data are seasonally adjusted at a monthly frequency. Vertical line denotes time of treatment.