Micro and Macro Effects of UI Policies: Evidence from Missouri *

Fatih Karahan
Central Bank of the Republic of Türkiye

Kurt Mitman
CEMFI, Institute for International Economic Studies, Stockholm University, Centre for Economic Policy Research, Institute for Labor Economics

Brendan Moore
Stanford University

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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. In our baseline specification, the data indicate that the cut in Missouri increased job-finding rates by 12% by raising firm vacancy creation and the search effort of unemployed workers. Both channels contribute roughly equally to the total effect.

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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. Those who lost their jobs during the COVID-19 recession received extended benefits through the summer of 2021, including intermittent supplemental benefits, sometimes resulting in replacement rates in excess of 100% (Ganong et al., 2020). With total payments approaching one percent of GDP during the last two pre-COVID recessions and over five percent during COVID, 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. Mitman and Rabinovich (2015) 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 helps illustrate these margins:

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

The first channel operates through labor supply by altering job search behavior s_{it}, which captures the search intensity and pickiness of unemployed workers. 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 a firm finding a worker. 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. The objective of this paper is to provide the first such decomposition.

We develop 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 H_t/V_t 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 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.[1] With these estimates at hand, we can then quantify the effect of benefit extensions on the job-finding rate and unemployment via the job-finding channel.

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.

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

(2018a) show, the optimal generosity of UI depends critically on the relative response of job search and vacancy creation.[2] 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.

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.[3]

The key challenge for estimating the effect of the policy change in Missouri is inferring the counterfactual dynamics of labor market outcomes in Missouri without the cut. We implement a difference-in-differences framework where the labor market outcomes of Missouri, as the treated unit, are compared to a group of control units with similar labor-market dynamics. We verify the robustness of our results to alternative specifications as well as newly-developed tools in the literature to bound our effects using pre-trends (Rambachan and Roth, 2023).

To implement this difference-in-differences 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.[4]

Our baseline results indicate that the UI cut in April 2011 led to an increase of 17.4% in the vacancy-unemployment ratio. The vacancy filling rate dropped by 5.6% due to the policy, 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.

We then implement our decomposition to infer the response of search effort and market tightness. We find that labor market tightness rose 12.4%. This estimate corresponds to an elasticity of market tightness with respect to benefit duration of around –0.50. Our preferred estimate infers that search effort rose in response to the UI cut as well, by around 5.0%, consistent with Johnston and Mas (2018).[5] The policy-induced tightening of the labor

[2]See also recent contributions from Herkenhoff (2019) and Braxton et al. (2020) that emphasize potential crowding out between public and private insurance for the unemployed.

[3]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 our paper to theirs, see Section 8.

[4]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 B. Hagedorn et al. (2013) constructed the data on unemployment benefit duration.

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

market and the increase in search effort led to a 11.8% higher job-finding rate in Missouri. Our baseline estimate attributes 58% of the increase in the job-finding rate to macro effects through tightness with the remaining part due to higher search effort. In a host of robustness exercises, we show that the macro effect always contributes between 20% and 80%. Finally, we show that changes in labor-market tightness and search effort can explain 60%-80% of the total decline in unemployment induced by the reform. The remaining 20%-40% of the decline is due to outflows from unemployment to non-participation.

A large literature beginning with Ham and Rea Jr (1987); Katz and Meyer (1990); Meyer (1990) has estimated negative effects on labor supply of varying magnitudes in response to UI extensions. Hagedorn et al. (2013) provided the genesis for a literature with a more aggregate (as opposed to individual) focus trying to identify the total effect of the policy (the sum of micro and macro effects) (Chodorow-Reich et al., 2018; Farber et al., 2015; Hagedorn et al., 2015) that has reached conflicting conclusions.[6] However, to the best of our knowledge, none of that literature has attempted to measure the macro and micro effects separately, 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 identifies both micro and macro effects of expanded benefits during the COVID recession. They find precisely estimated non-zero disincentive effects for the micro and macro channels, consistent with our findings.[7]

This paper contributes to a large literature that studies the labor market effects of unemployment benefit policies (see, for example, Schmieder and Von Wachter, 2016, for a recent review). 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 difference-in-differences 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 worth noting that they studied a policy change in Austria that 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.[8] See Hagedorn et al. (2016) for a more thorough review of recent quasi-experimental studies that measure

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.

[6] A separate, more structural literature evaluating UI policy's impact on the US business cycle has found conflicting answers. Mitman and Rabinovich (2024) find destabilizing effects of UI, whereas Kekre (2021) and Rendahl (2016) find 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.

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

[8] In that paper, the empirical analysis is based on the largest increase in potential benefit duration that is not due to a change in the benefits schedule. As a result, the changes in benefits are endogenous to labor market conditions, implying it is unclear whether the results represent responses to changes in policy. Our study overcomes this endogeneity issue by using an unexpected change in benefits, which may explain the differences in our findings.

the macro effects of UI benefit extensions.

The rest of the paper is organized as follows. Section 2 develops the methodology. Section 3 summarizes the institutional background. Section 4 discusses the data and empirical strategy. The main estimation results and our decomposition into micro and macro effects are reported in section 5. We discuss the implications of our findings for UI policy in Section 7. Section 6 includes a sensitivity analysis. Section 8 places our results in context of relevant literature. Section 9 concludes.

2 Measuring micro and macro effects

In this section, we develop our empirical methodology to measure micro and macro effects in two steps. First, we posit a relationship between vacancies, the unemployed, and hires consistent with equilibrium labor search theory. Second, we show how data on these three variables can be used to isolate micro and macro effects of policy.

We assume that new matches occur at time t in state s when vacancies (V_{st}) meet unemployed (U_{st}) individuals who search with s_{st} effective search units. Our main assumption is that the number of new hires (H_{st}) is equal to the number of matches and is given by a Cobb-Douglas matching function:

$$H_{st} = M(V_{st}, S_{st}) = \chi_{st} V_{st}^{\alpha} S_{st}^{1-\alpha}, \tag{1}$$

where χ_{st} is the efficiency of the matching function, $S_{st} = s_{st} \times U_{st}$ is aggregate effective search, and α is its elasticity with respect to vacancies.[9] The Cobb-Douglas form is consistent with empirical evidence documented in Petrongolo and Pissarides (2001). We make two further identifying assumptions:

Assumption 1. *The elasticity of the matching function, α, is constant and identical across states.*

Assumption 2. *The matching efficiency, χ_{st}, is orthogonal to UI durations.*

The Cobb-Douglas assumption implies that labor-market flows can be expressed solely as a function of *labor-market tightness*, $\theta_{st} = V_{st}/S_{st}$. The job-finding rate per effective unit of search is given by $f(\theta_{st}) = M(V_{st}, S_{st})/S_{st} = M(\theta_{st}, 1)$. The vacancy filling rate can be analogously expressed as: $q(\theta_{st}) = M(V_{st}, S_{st})/V_{st} = M(1, 1/\theta_{st})$. The goal is to identify the effect of changes in unemployment benefit duration on search effort, s_{st} (the micro effect) and labor-market tightness, θ_{st} (the macro effect). Both quantities, however, are not directly observable, as they depend on unobserved search effort. To make progress, we take logs of equation 1 and subtract $\log(V_{st})$ from both sides to obtain:

$$\log\left(\frac{H_{st}}{V_{st}}\right) = \log(\chi_{st}) - (1-\alpha)\log\left(\frac{V_{st}}{U_{st}}\right) + (1-\alpha)\log(s_{st}). \tag{2}$$

Intuitively, the equation expresses the vacancy-filling rate as a function of matching efficiency, the vacancy-unemployment ratio, and search effort. Our main empirical specification

[9] We further illustrate a linear log-log relationship between hires and vacancy-unemployment ratio in Appendix Figure A.10 in the U.S. during the time period of our sample.

compares the evolution of H_{st}/V_{st} and V_{st}/U_{st} in Missouri, which featured a plausibly exogenous cut in UI benefits, to states that did not experience a similar cut. Under our two assumptions, this difference is given by

$$\Delta_s \log\left(\frac{H_{st}}{V_{st}}\right) = (1-\alpha)\Delta_s \log\left(s_{st}\right) - (1-\alpha)\Delta_s \log\left(\frac{V_{st}}{U_{st}}\right) + \epsilon_{st}. \tag{3}$$

Here Δ_s denotes the difference operator between Missouri and non-treated states. Note that we observe V_{st}, H_{st}, and U_{st} directly in the data.[10] We then estimate two regressions of the form:

$$\begin{aligned}
\Delta \log\left(\frac{H_{st}}{V_{st}}\right) &= \beta_{H/V}\Delta_s D_{st} + \epsilon_{H/V,st} \\
\Delta \log\left(\frac{V_{st}}{U_{st}}\right) &= \beta_{V/U}\Delta_s D_{st} + \epsilon_{V/U,st},
\end{aligned}$$

where D_{st} denotes an indicator for the unexpected cut in UI in state s at time t (1 for Missouri and 0 for control states). Here, $\beta_{V/U}$ and $\beta_{H/V}$ are the effect of the policy change on the vacancy-unemployment ratio and vacancy filling rates, respectively (in section 4.2 we discuss how we obtain causal estimates of these quantities). Then, conditional on a value for α, we can measure the effect of the policy change on search effort, β_s, as a residual using equation (3), since ϵ_{st} is assumed to be orthogonal to the change in benefits.

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

Equation (4) 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 (4)).

The effect of policy on tightness β_θ can be inferred from its effect on the vacancy filling rate. Because 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

$$\beta_\theta = -\frac{\beta_{H/V}}{1-\alpha}. \tag{5}$$

[10] 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_{st} (where $V^*_{st} = e_{st}V_{st}$ represents effective vacancies) would yield:

$$\Delta \log\left(\frac{H_{st}}{V_{st}}\right) = (1-\alpha)\Delta \log\left(s_{st}\right) + \alpha\Delta \log\left(e_{st}\right) - (1-\alpha)\Delta \log\left(\frac{V_{st}}{U_{st}}\right).$$

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.

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

$$\beta_{\text{job-finding rate}} = \underbrace{\beta_s}_{\text{micro}} + \underbrace{\alpha\beta_\theta}_{\text{macro}} = \beta_{V/U} + \beta_{H/V} \tag{6}$$

Note that the effect on the job-finding rate is independent of the matching function elasticity. We use equation (6) to calculate and decompose this effect into the micro and macro components.

Before proceeding to the empirical analysis, we want to comment on our assumptions. First, the functional form for the matching function assumes a unitary elasticity of substitution between effective searches and vacancies. Outside of the Cobb-Douglas assumption, our methodology can be interpreted as identifying the local effect of UI on search efforts and vacancies.[11]

Second, we explicitly rule out matches coming from the currently employed (job-to-job switches), and we have abstracted from the participation margin. In reality, hires come from the employed as well as non-participants, and the decision of the unemployed to remain in the labor force may be influenced by the availability of UI. A more general specification would be $M(sU + \lambda_E E + \lambda_N N, V)$, where E, N represent the employed and non-participants, and λ_e, λ_N represent their passive search effort. Note that under this richer specification, the estimate and interpretation of $\beta_{H/V}$ and β_θ are unchanged, so the measurement of the macro effect is unbiased. The sum of $\beta_{H/V}/(1-\alpha) + \beta_{V/U}$ would now measure the effect on $\log(s) + \log(1 + (\lambda_E E + \lambda_N N)/(sU))$. Thus, our measure would also pick up movements in the relative number of effective searchers between the unemployed and employed/non-employed. Given the evidence (e.g., Hobijn and Şahin, 2021) showing that participation-related flows are unimportant for the cyclicality of the labor force participation rate, we think that this potential bias is small and unlikely to change our substantive conclusions about the relative contribution of micro and macro effects. For completeness, we present and discuss results on the effect of the policy on the size of the labor force in Section 7 since past work (Elsby et al., 2015) has documented that the cyclicality of the non-participation margin can be important for unemployment dynamics.

3 Institutional Background

Unemployment insurance in the U.S. is a joint state-federal program administered by individual states. While unemployed, eligible jobless workers ordinarily receive UI benefits for up to 26 weeks.[12]

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 who have exhausted their regular benefits. At the onset of the recession, half of the program's cost was paid for by the federal government, which included

[11] See Mukoyama et al. (2018) for a discussion of a generalized matching function with search effort and the empirical performance of the Cobb-Douglass assumption.

[12] Some states offer longer or shorter durations.

a set of triggers that the states could adopt. Initially, many states, including 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 four 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 that 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.

The cut triggered an additional 10-week reduction in emergency benefits because the EB and EUC programs calculate the federal benefit entitlement as a percentage of regular state UI 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 a change in program administration or search requirements.

4 Data and Empirical Strategy

4.1 Data Sources

We compile state-level data on unemployment, hires, vacancies, and unemployment benefit durations during the Great Recession. Data on number of unemployed residents come from the Local Area Unemployment Statistics (LAUS) provided by the Bureau of Labor Statistics.[13]

Data on hires are obtained from the Quarterly Workforce Indicators (QWI).[14] Specifically, we utilize the "new stable hires" variable which measures hires who last at least one full quarter with a given employer ("stable") and were not employed by the employer in any of the previous four quarters ("new"). 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 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.

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

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

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.[15] The data measure newly created vacancies in a given month as well as total vacancies—the sum of all openings, both extant and new. Our baseline results use "new vacancies," defined as listings that did not exist in the previous quarter and are newly created in a given quarter.[16] 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 TCB uniquely matches to a county of prospective employment.[17] 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 data using a Signal Extraction in ARIMA Time Series (SEATS) method to extract signal from the noise of yearly fluctuations. We default to seasonally-adjusting V/U and H/V as ratios, under the rationale that the standard unemployment rate is seasonally adjusted as a ratio, rather than separately seasonally adjusting the numerator of unemployment count and denominator of labor force. Further, search theory implies that the rations are the relevant quantities for labor-market dynamics. However, our main results and appendices show our results are robust to seasonally adjusting V, H, and U separately.

4.2 Causal Estimation

Our paper's primary goal is to identify the causal effect of a UI potential benefit duration cut on the aggregate labor market. Cross-state correlations of potential benefit durations and labor market outcomes may be plagued by substantial endogeneity concerns, not least because the UI trigger laws govern potential benefit duration as an increasing function of state unemployment. To obtain estimates that can be more credibly interpreted as causal, we leverage the drastic, unexpected benefit cut in Missouri described in section 3.

Specifically, we use a two-way fixed effect (TWFE) estimator to infer the causal effect of the cut in UI on labor market outcomes. As a baseline specification, we estimate the following model on a balanced, quarterly panel:

$$y_{st} = \lambda_s + \gamma_t + \beta \cdot D_{st} + \varepsilon_{st} \tag{7}$$

where y_{st} denotes an outcome for state s in year-quarter t. Our outcomes of interest are the log of the vacancy-unemployment ratio (V/U) and the vacancy filling rate (H/V). D_{st} is an indicator which equals 1 when s is Missouri and t is greater than or equal to 2011Q2, reflecting the enacted benefit cut. λ_s and γ_t are state and time fixed effects, respectively, which capture unobserved unit- and time-specific confounders.

Our preferred panel spans 2010Q1 to 2012Q3. We choose to begin our panel in 2010Q1,

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

[16] Appendix A.2, we show our results are robust to using the broader vacancy measure.

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

after the national unemployment rate (and most state unemployment rates) had peaked and were beginning to fall. Even though the Great Recession had differential incidence across place (Yagan, 2019), the recovery was broadly similar. Thus, starting the panel earlier than 2010 would risk the plausibility of the parallel trends assumption. We choose to study the effects of the policy through 2012Q3 because, as illustrated by Figure 1a, the policy's effect on the aggregate unemployment rate peaked in 2012Q3. In any case, we show our results are robust to choice of varying panel lengths (see Appendix A.1).

Our benchmark sample consists of 15 U.S. states, among which only one is treated with a UI potential benefit duration cut. We focus on a single case study to provide clean and credible estimates of this causal effect. Our set of comparison states had similar labor market trajectories before the cut in Missouri and had a similar fiscal situation for the unemployment insurance system as Missouri.[18] In particular, we select states whose unemployment rates peaked at roughly the same as Missouri and whose UI Trust Funds were insolvent at the end of 2010. In doing so, we compare Missouri's labor market outcomes to those of other states at the same point in their post-recession labor market recovery and were in a similar fiscal-political situation of insolvency, which helped trigger the Missouri cut. Our benchmark thus provides a set of well-matched states for inference. Appendix Table A.2 lists the identities of the 26 states reached peak unemployment within 2 months (plus or minus) of Missouri's peak and Appendix Table A.1 lists the states with insolvent UI trust funds at the end of 2010.

Because we study only Missouri's benefit cut, we do not have to be concerned about the TWFE estimator's performance in the presence of treatment effect heterogeneity and staggered treatment timing.[19] Our sample omits Massachusetts because QWI hires data are not available for the duration of the panel. It also excludes Arkansas, Florida, Georgia, Hawaii, Michigan, and South Carolina to avoid violations of our identifying assumptions. In addition to our benchmark comparison group, we present results using the full comparison group that includes 43 U.S. states that satisfy our identifying assumptions.[20]

One key identifying assumption of equation (7) is that, in the absence of Missouri's UI benefit cut, the average outcomes in Missouri and comparison states would have evolved along parallel trends. The coefficient β identifies the average treatment effect on the treated (ATT) of the potential benefit duration cut on labor market outcomes. The parallel trends assumption allows for selection bias – whereby states such as Missouri non-randomly select

[18] We thank an anonymous referee for suggesting this comparison group.

[19] Specifically, a voluminous literature (De Chaisemartin and d'Haultfoeuille, 2020; Borusyak et al., 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021) has attempted to sensibly aggregate TWFE estimates into an interpretable causal effect when treatments are staggered or heterogeneous.

[20] In Appendix Figure A.1, we plot a histogram of when each state (excluding the non-Missouri states that also cut UI benefit duration: AR, FL, GA, MI, SC; as well as MA for data availability reasons) first hit their peak unemployment rate during the Great Recession. Conveniently for our purposes, Missouri reached its peak unemployment in January 2010, the same month as 12 other states (the modal month in the histogram). The one state whose unemployment rate peaked after Missouri's policy change (the red dashed line of Figure A.1) is Hawaii. An essential assumption of our identification strategy is that labor market outcomes of treatment and control states are on parallel trends. The fact that Hawaii's labor market had not yet begun to mend when Missouri enacted its policy change makes it a poor candidate for a control state, as including it in estimation would risk violating the assumption of parallel trends. Thus, in all analyses (including our full sample), we exclude Hawaii from the group of control units.

into cutting UI based on characteristics that affect labor market outcomes – so long as the bias from selecting into treatment remains constant in the pre-treatment period as in the post-treatment period.

The other key identifying assumption of TWFE is that Missouri's benefit cut has no causal effect on its labor market prior to implementation. Without this "no anticipation" assumption, the estimates for Missouri's labor market outcomes could reflect not only the causal effect of the benefit cut but also the anticipatory effect just prior to implementation (Malani and Reif, 2015). The no-anticipation assumption is especially credible in our setting given the very short 5-day window between the first public reports of the policy and its implementation by Missouri's legislature. This sequence contrasts with other states that enacted similar policy changes during the Great Recession and which we omit from our analysis so we are left comparing Missouri to clean controls which don't experience treatment.[21] For example, in March 2011, Michigan's governor enacted a UI benefit duration cut similar to Missouri's. However, the law did not take effect until January 2012, ten months after its signing. In Michigan, it is plausible that firms reacted in anticipation of the impending law by changing vacancy-posting behavior (e.g., posting vacancies in anticipation of Michigan's diminished UI generosity). Including such states as treated units would risk violating the crucial no-anticipation identifying assumption.

The final key identifying assumption of TWFE is "no foresight." That is, Missouri legislators' decision to cut benefits was not due to advance knowledge of the future path of unemployment in Missouri. A possible threat to the no-foresight assumption would involve legislators being convinced to vote for the measure due to advanced knowledge about Missouri's economy being on a stronger path to recovery compared to other similar states. We believe this is implausible because the trajectory of Missouri's economic recovery was not markedly different from neighboring states at the time of the benefit cut. If legislators were acting based on economic forecasts, such forecasts likely did not indicate any substantial deviation in Missouri's economic outlook compared to nearby states or the national trend.[22] To verify the validity of the "no foresight" assumption, we contacted the initiator of the Missouri filibuster, former State Senator Jim Lembke, for the motivation for the cut in UI. He confirmed that it was not because of news of an improving Missouri labor market but to save Missouri businesses' money and encourage people to work.[23] Thus, we conclude that the "no foresight" assumption holds.

Under the no-anticipation, no-foresight, and parallel trends assumptions, the TWFE model allows us to rule out concerns that results may be driven by macroeconomic trends common to all states. Unemployment, vacancies, and hires all have strong cyclical components, but the model's year-quarter fixed effects will account for the considerable time variation in the factors that drive their cyclicality. Further, TWFE lets us rule out time-invariant differences between states in their labor market or broader economy. For example, labor market tightness and vacancy filling rates may be affected by factors such as educa-

[21] States that also cut their UI potential benefit duration around the same time as Missouri include Arkansas, Florida, Georgia, Michigan, and South Carolina.

[22] One example forecast, produced by the Bureau of Economic Research in Missouri (https://economicresearch.missouristate.edu/Economic-Forecast.htm), projected lower employment growth for Missouri than the U.S. as a whole.

[23] The communication is consistent with contemporaneous news reporting the motivation behind the cut.

tional attainment or demographic characteristics. But to the extent these factors are fixed over a reasonably short (11-quarter) time period, state fixed effects will account for the substantial cross-state variation in these variables.

In addition to equation (7), we also report results of the following dynamic TWFE ("event study") specification:

$$y_{st} = \lambda_s + \gamma_t + \sum_{k \neq 0} \beta_k \cdot D_{st}^k + \varepsilon_{st}, \tag{8}$$

where D_{st}^k are "event-time" dummies for Missouri's policy change relative to the period before enactment (when $k = 0$ in 2011Q1). This event-study specification allows us to visually check for pre-trends that may threaten the identification of a causal effect of the policy change.

As explained in section 2, we then use the estimated effect of the the UI cut in Missouri on the vacancy-unemployment ratio, $\hat{\beta}_{VU}$, and vacancy filling rate, $\hat{\beta}_{HV}$, in equation (6) to estimate the impact on job-finding rates.

4.2.1 Robust Inference for Parallel Trends

It is natural to worry about the plausibility of the assumption of parallel trends in our setting, particularly during periods such as the Great Recession. That is, it may be the case that various states are on different trends with respect to their aggregate labor market characteristics. We address this concern in four ways.

First, a shorter panel, which begins after the national unemployment rate peaked and most labor markets are mending, means it is reasonable to think Missouri's outcomes are on parallel trends with comparison group states (compared to estimating on a panel that began before the Great Recession). Second, we estimate a fully dynamic version of TWFE (equation (8)) and check for potential pre-trends. Third, we conduct robust inference by bounding our results using the tools developed by Rambachan and Roth (2023). Fourth, we consider two other restricted comparison groups with political characteristics similar to Missouri's. Fifth, while our baseline specification does not include control variables, we also control for other relevant state-level factors that might affect labor market conditions. This battery of sensitivity checks bolsters our findings about the effect of the UI benefit cut on labor market tightness, search effort, and the job-finding rate.

5 Estimation Results

5.1 Two-Way Fixed Effects Estimation

The TWFE model illustrates that the policy change in Missouri had a sizable effect on UI duration, the unemployment rate, the vacancy-unemployment ratio, and the vacancy filling rate. As shown by column 1 of Table 1, the maximum UI duration (aggregated to a quarterly frequency) in Missouri fell by more than 20 log points as a result of the policy change. This decline is similar to the actual cut in Missouri of $\log(73) - \log(57) \approx 24.7$ log points. Table 1 also documents the 16-week cut in Missouri triggered a full percentage point decline in the state unemployment rate (columns 2 and 3), consistent with Johnston and Mas (2018). In Figure 1a, we plot the point estimates from equation (8) for the dynamic effect on the unemployment rate. The figure illustrates the policy's impact on the unemployment rate takes full effect by 2012Q3.[24] Vacancies jump immediately following the cut, as shown in Figure 1b, where we plot the point estimates from equation (8) for the dynamic effect on vacancy posting. The event study analyses with all control units show the same basic response of vacancies and unemployment and are plotted in Figures 3a and 3b.

We next turn our attention to the policy's effects on our main labor market outcomes of interest. Figure 2a documents that the vacancy-unemployment ratio rises following the cut in UI durations. Over the year following the policy change, the increase is sizable: the ratio of vacancies to unemployed rises 23 log points. This change corresponds to an elasticity of about –0.92 ($\approx -23/24.7$). In contrast, the vacancy filling rate H/V falls precipitously in the first two quarters after the benefit cut (Figure 2b). In the first two post-policy quarters, the log of Missouri's vacancy filling rate dropped by nearly 9 and 13 log points, respectively, as a result of the cut, 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 0.4 ($\approx$ 10/24.7), the effect then stabilizes to an elasticity of 0.2 ($\approx$ 5/24.7). For completeness, we also plot the coefficients from the event study using all control units in Figures 3c and 3d. The effect on V/U is similar to the benchmark, albeit slightly smaller in magnitude. The effect on H/V exhibits the same precipitous drop on impact but noisier pre-period coefficients.

We now implement the methodology in Section 2 to estimate the effect of the UI cut on the job-finding rate using equation (6). This requires estimating the effect of the policy change on the vacancy-unemployment ratio and the vacancy-filling rates first. We do so by using the estimates from specification (7). We report both one-way standard errors clustered at the state-level and two-way standard errors clustered at the state-quarter level.

We use equation (7) to estimate a strong, positive effect of Missouri's UI benefit cut on the vacancy-unemployment ratio, reported in Panel A of Table 2. Our baseline specification suggests V/U increased by 17.4 log points. When we vary the composition of the comparison group or the method of seasonal adjustment (reflected in columns 2–4), the estimates lie between 11.3 and 17.4 log points. For all specifications of the vacancy-unemployment ratio, the effect is significant at a 0.1% level using two-way clustered standard errors.

Similarly, Panel B of Table 2 documents clear negative estimates for the effect on the vacancy filling rate. Our baseline estimate for $\hat{\beta}_{H/V}$ is –5.6 log points. The estimated effect ranges between –2.1 and –5.6 log points across different seasonal adjustment methods and comparison samples, all of which are significant at the 5% level. Moreover, both benchmark specifications, which compare Missouri to a stricter, more plausible set of control states, are significant at the 0.1% level. 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).

[24] In Appendix Figure A.6, we plot the estimates from a dynamic TWFE, which extends the post-period window to more clearly illustrate the causal effect of the benefit cut peaks in 2012Q3 and recedes considerably thereafter. Given the shape of Figure A.6's point estimates, we evaluate the policy's effect on the aggregate labor market through the "peak" unemployment effect in 2012Q3.

5.2 Implications for labor flows and unemployment

The estimates in Table 2 together with the decomposition in equation (6) allow us to calculate the effect on the job-finding rate due to policy change. Column 1 of Table 2 (below panel B) suggests the cut in UI durations resulted in a 11.8 log point increase in the job-finding rate, corresponding to an elasticity of -0.48 with respect to UI benefit duration. Different constructions of the sample agree on what happened to the availability of jobs in Missouri with the increase in the job-finding rate ranging between 9% and 12.2% (corresponding to elasticities of –0.36 to –0.49). Our estimates fall well within the range of U.S. studies of the effect of the potential benefit duration on job search, documenting job-finding elasticities between –0.1 and –0.41 (Katz and Meyer, 1990; Card and Levine, 2000; Landais, 2015).

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 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.[25] The flow balance unemployment rate is given by $u_{ss} = s/(s + f)$. The only difference between this and the counterfactual 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 2 shows that in this difference is 0.6–0.8 percentage points: the increased availability of jobs (higher θ) and an increased search effort (s) combined, lowered the unemployment rate by between six-tenths and eight-tenths of a percentage point. Note that this accounts for the vast majority of the full percentage point decline in the unemployment rate caused by the policy change in Missouri.

There are two other margins that can account for the remaining 20–40% 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 (Hartung et al., 2018). Thus, lowering benefit durations may reduce unemployment by reducing inflows into unemployment. We explore this channel in Section 6.2. 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 labor force participation fell in Missouri over this episode, and applications for disability insurance spiked. Consistent with those findings, we estimate a decline in the labor force four quarters after the cut in Appendix A.4 that can explain the remainder of the decline in the unemployment rate. We revisit the participation margin in Section 7.[26]

[25] These series are shown in Figure B.1 in Appendix B.

[26] 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.

5.3 Decomposing 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 (4) 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 our survey of the matching function estimates in Petrongolo and Pissarides (2001); Hall (2005); Mortensen and Nagypal (2007), we consider three alternative values for α: 0.50, 0.55, and 0.60.[27] 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 due to macro effects. Table 3 reports imputed values for our baseline TWFE estimates.

Table 3 shows the resulting decomposition. Our preferred estimate attributes an increase in market tightness θ of roughly 12.4% as a result of the policy (assuming an elasticity of the matching function $\alpha = 0.55$). Given that the cut in benefits was around 25 log points, these estimates imply that the elasticity of market tightness with respect to unemployment benefit duration of around –0.50. For our preferred formulation more broadly (depending on the choice of α), this elasticity of market tightness ranges between –0.45 and –0.56.

Workers reacted to the cut in UI durations by raising their search effort. In fact, we find an effect of comparable magnitude: 5.0% in our baseline estimate, corresponding to an elasticity of search effort of –0.20. For a choice of α between 0.5 and 0.6, the elasticity of search effort with respect to UI duration ranges from –0.14 to –0.25.

As we discussed before, combined with the effect on market tightness, these estimates imply a sizable increase in the job-finding rate of 11.8% (column 1 of Table 2). Of this increase, our decomposition implies that 58% is due to the macro effect when assuming $\alpha = 0.55$—a tightening of the labor market due to increased demand for labor in response to the UI cut. More broadly, this share ranges between 47–71% depending on a reasonable choice of the matching function elasticity.

Panels B, C, and D of Table 3 repeat the same exercise using the estimates from specifications 2, 3, and 4 from Table 2, respectively, which vary the method of seasonal adjustment or the composition of control units for the TWFE specification. These decompositions bolster our conclusion that a sizable portion of the increase in the job-finding rate after the benefit cut is attributable to macro effects. For a matching function elasticity of 0.55, for example, we attribute more than one-quarter and less than three-fifths (28% to 58%) 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 23% of the total effect, and as an upper bound, 71% of the effect for sufficiently high values of the matching function elasticity. Next, we explore the robustness of our findings.

[27] Petrongolo and Pissarides (2001) find the range of plausible estimates of the elasticity of the function is 0.3 to 0.5 using mostly non-U.S. data. Mortensen and Nagypal (2007) find an elasticity of 0.54 using U.S. data. Hall (2005) estimates a larger elasticity of 0.77. Given this range of 0.5–0.75 for the US, we take the lower bound of these values to be somewhat conservative in estimates for the role of macro effects.

6 Robustness and sensitivity analysis

We now explore the robustness of our findings to potential pre-trends, direct measurement of job-flows, and several choices of how we prepare the data for analysis.

6.1 Robust Inference and Pre-Trends Sensitivity

The canonical parallel trends assumption underlying our approach requires that Missouri's labor market outcomes would have evolved in parallel with the mean outcomes for all other states had its legislature not enacted the sudden benefit cut. One might be concerned that differential exposure to the severity of the Great Recession places states on different recovery trends (Yagan, 2019), even when comparing Missouri only to states whose unemployment rate peaked at the same time (i.e. columns 2 and 4 of Table 2). The dynamic specifications of TWFE at least partially alleviate such concerns, as Figures 2a and 2b exhibit flat trends in pre-policy point estimates. These suggest that our setting does not exhibit an obvious failure of the parallel trends assumption.

Nevertheless, it is possible that pre-treatment point estimates are flat even as actual pre-trends are not *exactly* parallel. Moreover, parallel pre-trends in labor market outcomes do not guarantee that the post-treatment parallel trends assumption is satisfied (Kahn-Lang and Lang, 2020). To remedy these concerns about pre-trends, Rambachan and Roth (2023) develop an approach for robust inference and sensitivity analysis when parallel trends may be violated. Specifically, their "bounds on relative magnitudes" approach assumes that the unobservable post-policy deviation in parallel trends is at most some multiple $\bar{M}$ of the deviation from parallel trends in the period before the policy. We implement this approach for our dynamic TWFE specifications to underscore the strength of our results even in the face of concerns about violating the pre-trends assumption.

Following the recommendation of the difference-in-differences literature review of Roth et al. (2023), Table 4 formally assesses the extent to which our conclusions are sensitive to violations in parallel trends by reporting the "breakdown" value according to Rambachan and Roth (2023).[28] Tables 2 and 4 are structured similarly in presenting our four primary specifications and transformations, with the exception that the latter necessarily reports breakdown values for dynamic TWFE equation (8) whereas the former presents results from a single post-period specification (7).

Our main specification's (column 1) finding of a positive effect on the log of vacancy-unemployment ratio in 2011Q2 (the first post-treatment period) is robust up to a value of $\bar{M} = 0.9$. This means to invalidate the conclusion of an initial positive effect of the policy change on $\log(V/U)$, we would need to allow for the 2011Q2 violation of parallel trends 0.9 times larger than the maximal pre-2011Q2 violation. Different specifications all yield relatively large breakdown values of at least 0.9 in the first period and as high as 1.5 when we use all states as control units. When we consider the overall effect in the post-policy (2011Q2–2012Q3) period, we can say that Missouri's policy had a significant effect (at the 5% level) on the vacancy-unemployment ratio unless we are willing to allow for post-UI cut differences in trends that were up to 0.6 times as large as the largest difference in trends prior

[28] For implementation, we use the `honestdid` package in Stata.

to April 2011. Various specifications place this breakdown value between 0.6–0.9, allowing us to be confident in an overall effect on the vacancy-unemployment ratio.

We find even stronger results for our finding of a negative effect on the log of the vacancy-filling rate in 2011Q2, with a breakdown value of 1.3 for our main specification after one post-treatment period (and between 1.0–1.7 depending on specification). For the overall effect, the breakdown value for $\log(H/V)$ ranges between 0.2 and 0.3, which is less surprising given the consistent pattern of variations of Figure 2b where we observe an initial strong effect which fades over time.

We next show that our estimates for the job-flows are robust to using direct measures of flows in the data.

6.2 Direct evidence on job flows

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. We regress the log of the quarterly average of the job-finding and separation rates on our standard TWFE model. Table 5 shows that the job-finding rate primarily responded to the policy change. 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. This is the case both when we compare the trends in Missouri to the baseline or all comparison states.

Interestingly, the coefficient on the job-finding rate from the TWFE estimator is very close to the implied effect on the job-finding rate reported in Table 2. The similarity between the measured impact on the job-finding rate across the two specifications lends further support to our two identifying assumptions and proposed decomposition in equation (6).

Appendix B provides further discussion of measured job flows for Missouri in the CPS.

6.3 Robustness to Alternate Control Group Composition

Throughout the main paper, our control groups are composed of either the subset of states whose unemployment rate peaked within two months of Missouri's peak and also had insolvent UI trust funds or all states (barring those who changed UI policies around the same time as Missouri). In Appendix A, we report the results of our TWFE regressions and decompositions where we use other plausible control groups.

First, as in the baseline, we concentrate on states from Table A.2 with a comparable labor market trajectory to Missouri. Instead of further restricting to states with insolvent trust funds (as in the baseline), we instead select states that match Missouri's political fundamentals. Missouri's 2011 policy change was passed by a Republican-controlled legislature and signed by a Democratic governor. Further, Missouri voted narrowly for the Republican candidate in the most recent presidential election (2008). We construct two distinct samples of states experiencing peak unemployment near the time of Missouri's that either (i) had Republican control of at least one chamber of the legislature in 2011[29] or (ii) voted for Obama within 10 points of Missouri's 2008 vote share. Such states also serve as plausible

[29] We choose to focus on Republican control of the legislature rather than Democratic governorship because typically such changes originate in the legislature, and Republicans are typically the party inclined to cut UI benefit generosity.

control units because they closely resemble 2011 Missouri – a state with Republican leanings recovering from the depths of the Great Recession a year prior – in both an economic and political sense. See Appendix A.1.2 for further details on sample construction.

When we compare Missouri's labor market outcomes to these states that match on political and economic fundamentals, our headline results remain largely unchanged. However, the effect on the vacancy-filling rate is slightly smaller than the baseline. Table 6 presents difference-in-difference results for these alternative sets of control states. The measured responses of the vacancy-unemployment ratio and vacancy-filling rate range from 11 to 14 log points and -2 to -3 log points, respectively, matching the magnitude of our baseline estimates. These responses imply that Missouri's benefit cut boosted the job-finding rate by 9 to 12 log points, identical to the baseline range in Table 2. Using our preferred matching function elasticity of $\alpha = 0.55$, Table 7 shows 23–43% of the increase in the job finding rate can be accounted for by macro effects. This range is strikingly similar to our baseline decomposition, which places this range at 28–44%.

Second, we compare Missouri to seven of its eight bordering states (we exclude Arkansas due to changing its UI generosity around the same time as Missouri).[30] Border states may serve as plausible control states due to facing more similar geographic and economic factors compared to states further away. The findings for the effects of the UI cut on the Missouri labor market – including the resulting decomposition – confirm our headline findings, whereby 41–61% of the increase in the job finding rate can be accounted for by macro effects. See Appendix A.1.3 for further details.

We present further robustness checks – including sensitivity to the length of panel chosen and alternate measurement of hires and vacancies – in Appendix A.

6.4 Inclusion of Covariates

Our baseline specification does not include any time-varying state-level controls. Given our relatively short panel of 11 quarters, it is reasonable to think many state-specific factors that influence labor market outcomes, such as availability of re-employment offices or attitudes towards work and nonemployment, are constant over the panel and thus absorbed in the state's fixed effect. Nevertheless, we measure the policy's effect on the key outcomes of interest while controlling for important time-varying economic factors which may have shifted at the height of the Great Recession, including industrial composition, home ownership rate, and the share of establishments gaining or losing jobs in Appendix A.1.5. Our key estimates of interest are largely similar to our benchmark estimates, although they are slightly attenuated, as expected, and have larger standard errors due to the inclusion of the controls. See Appendix A.1.5 for further details.

6.5 Sensitivity of Decompositions

Our baseline result is that macro effects can account for 58% of the policy's effect on the job-finding rate (row 2 of Panel A, Table 3). We establish this figure as our baseline because of, what we view, the sensible choices involved: (i) assuming the midpoint of our matching

[30]These states include Iowa, Illinois, Kentucky, Tennessee, Oklahoma, Kansas, and Nebraska.

function elasticity range, (ii) seasonally adjusting the outcomes of interest directly as ratios, and (iii) selecting the most plausible control group of states based on economic fundamentals. However, when there are many possible choices as to how raw data can be transformed to be analyzed, researchers may be more inclined to select the method that shows the largest effects.

For transparency, we present histograms of our estimated effects of the policy on search effort, market tightness, and the share attributable to the macro effects when we vary reasonable choices of seasonal adjustment, comparison sample composition, inclusion of covariates, and matching function elasticity. These yield 42 different estimates for the effects of the policy.[31] The findings in this section validate our conclusions about the effects of Missouri's policy change on the labor market and underscore we do not take measures to overstate the role of macro effects in our baseline results.

Figure 4 shows the histogram of the range of our estimates on the effect on search effort and market tightness. In both subfigures, the red vertical lines denote our baseline estimate that search effort increases by 5 log points and market tightness by 12.4 log points as a result of the policy. The effect on search effort and market tightness is always positive, with search effort ranging from 1 to 9.5 log points (Figure 4a) and tightness ranging from 3.4 to 14 log points (Figure 4b). These figures illustrate our baseline estimates are consistent with the broader range of possible responses one could have measured based on reasonable data transformation and elasticity parameter choices.

How do the decomposition results depend on these choices? To see this, we compute the share of the job-finding rate increase attributable to the macro effect for each estimate reported in the histogram on Figure 5. We always find that at least 19% of the increase in the job-finding rate is due to a higher availability of jobs, with a median estimate of 40%. We have argued that our baseline sample is the best match for Missouri to assess the causal effect of the UI cut. Across all plausible control groups, the UI cut significantly increased job-finding rate, search effort, and labor-market tightness. The relative contribution of micro and macro effects, however, does vary across specifications While our baseline suggests the two forces are of roughly equal magnitude, the other plausible control groups suggest a more conservative decomposition where vacancy channel contributes between one-third to one-half of the total effect.

7 Implications for Cyclical UI Policy

Our findings have important implications for designing optimal UI policies over the business cycle. In quantitative and theoretical work, Mitman and Rabinovich (2015) and Landais et al. (2018b) emphasize the importance of the relative size of the micro elasticity (ε^m) of search effort to benefit duration and the macro elasticity (ε^M) of unemployment to benefit duration for the cyclicality of optimal policy. In particular, Landais et al. (2018b) derive a sufficient statistic for the optimal cyclicality of policy that depends on the sign of the

[31](2 seasonal adjustments $\times 3$ matching function elasticities $\times 5$ sample compositions (benchmark, all, two political compoisitions and surrounding states)) + (2 seasonal adjustments $\times 3$ matching function elasticities $\times 2$ sample compositions (benchmark, all) with covariates in regressions) = 42.

elasticity wedge:

$$1 - \frac{\varepsilon^M}{\varepsilon^m}. \tag{9}$$

As explained by Landais et al. (2018a), the elasticity wedge is the effect of UI on labor-market tightness. A negative elasticity wedge means that a cut in UI increases labor market tightnes, and vice versa. As Landais et al. (2018a) explain, the sign of the elasticity wedge is sufficient to know the optimal cyclicality of UI policy. If the wedge is negative, then optimal UI should be lower, since it decreases unemployment. The basic idea is a trade-off between the consumption smoothing benefits of UI versus decreasing the total number of unemployed people. When the wedge is negative, the benefit of moving more people from unemployed to employed outweighs the loss in consumption smoothing of the unemployed.

To apply their sufficient statistic to our findings, note that their terminology is slightly different from the one adopted in this paper. Their macro elasticity is the general equilibrium effect of UI on unemployment, equivalent to our measured effects on the job-finding rate. The elasticity wedge is equivalent to what we call the macro effect or the effect on tightness. Our benchmark specification implies an elasticity of labor-market tightness to benefit duration of –0.5, implying that the elasticity wedge is negative. Our findings thus suggest that UI benefit duration should be pro-cyclical, as opposed to counter-cyclical as during the Great Recession.

One should be cautious, however, as the sufficient statistic formula is valid for small deviations from steady state, and in general would depend on the cyclicality of the elasticity wedge. The fact that the wedge was estimated to be negative in Missouri in 2011, implies that the cut in UI duration was welfare-improving. Our results are also consistent with the findings of Mitman and Rabinovich (2015) who solve the optimal Ramsey plan and find on average that the optimal UI benefit duration is pro-cyclical, though that there are potential non-linearities, whereby benefits are extended on impact when a recession hits, but then are reduced to hasten the subsequent recovery.

One final consideration for the cyclicality of UI policy is how it affects labor supply. The focus of literature on optimal UI cyclicality has generally neglected the participation margin and only focused on the search effort of unemployed people.[32] Our baseline empirical findings suggest that about 25% of the decline in unemployment induced by the UI cut was due to a contraction in the labor force. The fact that some unemployed drop out of the labor force when benefits are cut is fully consistent with standard equilibrium labor market theory (Krusell et al., 2017). When benefits are cut, the value of unemployment falls and the surplus of matching rises (otherwise labor market tightness would not increase). Some workers search harder, whereas others choose to drop out of the labor force. Unemployment unambiguously falls, the effect on employment and the labor force can be ambiguous as shown in Appendix Figure A.17. As the vast majority of the decrease in unemployment comes from increased labor market tightness the welfare implications are likely unchanged.

[32] Both Landais et al. (2018b) and Mitman and Rabinovich (2015) only consider two-state models with employed and unemployed individuals.

8 Comparison to Johnston and Mas (2018)

Given that Johnston and Mas (2018) (henceforth JM) 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.

Both papers identify the causal effect of the cut in UI on Missouri unemployment: JM using a synthetic control approach and our paper using a difference-in-difference specification. We find essentially the same effect, thus our paper essentially confirms the JM result on unemployment using a different identification strategy.

Where the papers differ, is in trying to identify effects to decompose what drives the total decline in unemployment. JM 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. The JM estimate measures the micro elasticity as defined in Landais et al. (2018b), namely, the change in search effort *keeping labor-market tightness constant* (the effect of labor-market tightness is differenced out by construction in JM, since the control and treatment workers face the same labor-market tightness). The strength of their approach is that they are able to isolate the pure micro elasticity. The downside of their approach is that they cannot estimate the effect of the cut on the search effort for individuals that are ineligible or non-claiming.[33] The JM sample only includes individuals who are eligible *and* claiming benefits. Many unemployed workers (both eligible and ineligible for UI) search for a job without receiving UI benefits, in no small part because take-up of the benefit in the U.S. is highly incomplete (Anderson and Meyer, 1997; Lachowska et al., 2023). Indeed, the JM sample covers less that one-third of the total unemployed in Missouri during this period (continued claims relative to total unemployed in 2010-2011).

Our decomposed micro effect captures the effect of changes in UI both on claimants and non-claimants, and thus is a different measure than the focus of the previous micro literature. Calculating the aggregate effect of a policy change on the labor market requires computing the change in search effort by all market participants, not simply those eligible and claiming benefits. The search behavior of ineligible workers and non-claimants can be impacted by the benefit cut even if the vacancy posting behavior of firms changes in response to the benefit cut. For example, imagine an ineligible individual is searching for a job to regain eligibility. If the value of eligibility goes down, then *ceteris paribus*, the search effort of that individual goes down. If, in addition, the vacancy posting behavior of firms changes, that provides an additional reason for individuals to change their search effort since the job-finding rate per unit of search changes. The downside of our approach is that we cannot separately identify the effects of search across the different groups of unemployed, only the combined effect.

Our new findings provide direct evidence of the effect of the UI cut on firms' vacancy posting rates. We show that vacancies, the vacancy-unemployment ratio, and the vacancy-filling rate all jump when benefits are cut in Missouri. By taking a more aggregate approach, considering all vacancies and hires, we can capture the total effect on search effect, including both from the eligible and ineligible and provide estimates on the participation margin. Finally, we also provide the estimates of the macro effect on job posting behavior by firms

[33]It also misses the behavior of separations and participation decisions as discussed in Section 5.2.

that JM do not consider.

We therefore see the findings in this paper complimentary to those of JM because they can help us distinguish the search effects across the different types of unemployed. Combining our results with theirs suggest a negative impact on the search effort of non-claimants, which we interpret as the value of a job to non-claimants declining due to a decrease in wages driven by the cut in UI (which lead to the increase in labor market tightness).[34]

9 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 developing a unified methodology that allows the joint measurement of the micro and macro effects. Our approach relies on minimal, standard assumptions 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 60–80% 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 roughly one-half of the total effect, and at least 20% according to our most conservative estimates.

Our findings have important implications for designing optimal UI policies over the business cycle. 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. Applying the sufficient static formula of Landais et al. (2018b) to our findings suggests that UI benefit durations should be pro-cyclical. More work is needed, however, to understand how the participation margin affects the optimal cyclicality of UI. Finally, our conclusions are based on the estimates of a single state at a particular time, and thus the policy prescription would be bolstered by addition evidence from more states and in response to different aggregate shocks. We leave this for future work.

10 Data Availability

Code replicating the tables and figures in this article can be found in Mitman et al. (2024) in the Harvard Dataverse `https://doi.org/10.7910/DVN/FHB4BK`. The vacancy data from

[34]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.

the HWOL are not publicly available. See the README in the replication files for the process of accessing the data.

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Table 1: Effects on Unemployment Rate and Unemployment Insurance Duration

	Log(Max Weeks UI)	Unemployment Rate	
	(1)	(2)	(3)
$\hat{\beta}_{MO\_post}$	-0.210	-1.019	-1.034
One-way clustered SE	(0.023)	(0.152)	(0.178)
Two-way clustered SE	(0.025)	(0.154)	(0.165)
Start Period	2010Q1	2010Q1	2010Q1
End Period	2012Q3	2012Q3	2013Q4
N	165	165	240

Note: Table reports the estimates from TWFE specification equation (7) for log of UI duration and the unemployment rate. Panel length varies between columns 2 and 3 as noted. The panel includes all states listed in Table A.1. Table reports one-way clustered standard errors at the state level and two-way clustered standard errors at the state-quarter level.

Table 2: Estimated Effects of UI Duration Cut on Missouri Labor Market

	(1)	(2)	(3)	(4)
A. $Vacancies/Unemployment$				
$\hat{\beta}_{MO\_post}$	0.174	0.115	0.171	0.113
One-way clustered SE	(0.031)	(0.016)	(0.031)	(0.016)
Two-way clustered SE	(0.030)	(0.015)	(0.030)	(0.015)
B. $Hires/Vacancies$				
$\hat{\beta}_{MO\_post}$	-0.056	-0.025	-0.049	-0.021
One-way clustered SE	(0.013)	(0.008)	(0.013)	(0.008)
Two-way clustered SE	(0.012)	(0.008)	(0.012)	(0.008)
Implied Effects on f and u_{ss}				
Δ log(f)	0.118	0.090	0.122	0.092
Δu_{ss} (percentage points)	-0.78	-0.59	-0.81	-0.60
Seasonal Adjustment as Ratios	✓	✓		
Seasonal Adjustment Separately			✓	✓
Full Comparison Group		✓		✓
N	165	473	165	473

Note: Hires and vacancies are new measures, as documented in HWOL and QWI respectively. The benchmark comparison group includes all states listed in Table A.1. The full comparison group includes all states except AR, FL, GA, HI, MA, MI, SC, and DC. Vacancies, hires, and unemployment are either seasonally adjusted independently or as ratios V/U and H/V. One-way clustered standard errors are clustered at the state level, and two-way are clustered at the state-quarter level. The table includes the imputed response of the job-finding rate and steady-state unemployment rate (p.p.) according to equation (6) and $u_{ss} = s/(s+f)$.

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. SA Ratio; Benchmark Control Units		
0.50	0.062	0.112	47%
0.55	0.050	0.124	58%
0.60	0.034	0.140	71%
	Panel B. SA Ratio; All Control Units		
0.50	0.065	0.050	28%
0.55	0.059	0.056	34%
0.60	0.052	0.062	42%
	Panel C. SA Separately; Benchmark Control Units		
0.50	0.073	0.098	40%
0.55	0.062	0.109	49%
0.60	0.049	0.122	60%
	Panel D. SA Separately; All Control Units		
0.50	0.071	0.042	23%
0.55	0.066	0.047	28%
0.60	0.060	0.052	34%

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 report the policy's estimated effect on the change from 2011Q1 to 2012Q3 in search effort (s_t) and market tightness (θ_t), respectively. Column 4 reports 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 2. Each panel reflects a different specification choice regarding the composition of the control group and the seasonal adjustment of labor market variables.

Table 4: Sensitivity to Parallel Trends Assumption: Specification Break Points

	(1)	(2)	(3)	(4)
A. *Vacancies/Unemployment*				
$\bar{M}$ First Post-Period	0.9	1.4	0.9	1.5
$\bar{M}$ All Post-Periods	0.6	0.9	0.6	0.8
B. *Hires/Vacancies*				
$\bar{M}$ First Post-Period	1.3	1.6	1.0	1.7
$\bar{M}$ All Post-Periods	0.3	0.3	0.2	0.2
Seasonal Adjustment as Ratios	✓	✓		
Seasonal Adjustment Separately			✓	✓
Benchmark Comparison Group	✓		✓	
Full Comparison Group		✓		✓

Note: Table reports "breakdown values" $\bar{M}$, according to Rambachan and Roth (2023), for the dynamic versions (equation (8)) of our four primary specifications from Table 2 with one-way robust standard errors clustered at the state level. Hires and vacancies are "new" measures, as documented in HWOL and QWI respectively. The benchmark comparison group includes all states listed in Table A.1. Full comparison group includes all states except AR, FL, GA, HI, MA, MI, SC and DC. Vacancies, hires, and unemployment are either seasonally adjusted independently or as ratios V/U and H/V.

Table 5: Estimated Effects of UI Duration Cut on Missouri Job-Finding & Separation Rates

	(1)	(2)
A. *Job Finding Rate*		
$\hat{\beta}_{MO\_post}$	0.261	0.264
One-way clustered SE	(0.023)	(0.014)
Two-way clustered SE	(0.036)	(0.021)
B. *Job Separation Rate*		
$\hat{\beta}_{MO\_post}$	-0.0055	-0.0054
One-way clustered SE	(0.0390)	(0.0203)
Two-way clustered SE	(0.0457)	(0.0234)
Full Comparison Group		✓
N	165	473

Note: Finding and separation rates are calculated from monthly UE and EU flows in the CPS, adjusted according to the method of Shimer (2012), and averaged to the quarterly frequency. Reported coefficient is the Missouri × Post-Period in a TWFE specification for year-quarter and state. Column (1) includes all control units exclude AR, FL, GA, HI, MA, MI, SC and DC for reasons cited in the text. Column (2) includes restricted control units listed in Table A.2. One-way clustered standard errors are clustered at the state level, and two-way are clustered at the worker-month level.

Table 6: Effects of UI Duration Cut on Missouri Labor Market, Political Controls

	(1)	(2)	(3)	(4)
A. $Vacancies/Unemployment$				
$\hat{\beta}_{MO\_post}$	0.115	0.139	0.116	0.139
One-way clustered SE	(0.034)	(0.029)	(0.034)	(0.029)
Two-way clustered SE	(0.031)	(0.026)	(0.031)	(0.026)
B. $Hires/Vacancies$				
$\hat{\beta}_{MO\_post}$	-0.030	-0.024	-0.026	-0.022
One-way clustered SE	(0.013)	(0.016)	(0.012)	(0.016)
Two-way clustered SE	(0.011)	(0.015)	(0.011)	(0.015)
Implied Effects on f and u_{ss}				
Δ log(f)	0.085	0.115	0.090	0.117
Δu_{ss} (percentage points)	-0.48	-0.66	-0.51	-0.68
Seasonal Adjustment as Ratios	✓	✓		
Seasonal Adjustment Separately			✓	✓
Control Units:				
Unemp Peak Near MO's	✓	✓	✓	✓
GOP House and/or Senate	✓		✓	
Obama Voteshare Near MO's		✓		✓
N	154	154	154	154

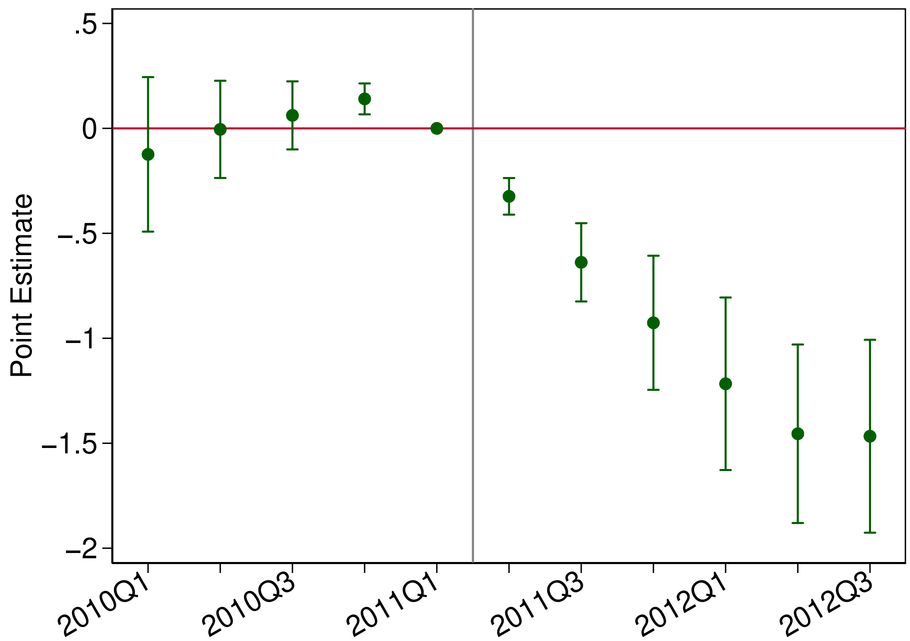
Note: Hires and vacancies are new measures, as documented in HWOL and QWI respectively. Vacancies, hires, and unemployment are either seasonally adjusted independently or as ratios V/U and H/V. The control units which experienced peak unemployment within 2 months of Missouri's peak are listed in Table A.2. Control units in this table are also required to resemble Missouri's political characteristics: having Republican control of at least one house of the state legislature, or having a 2008 Democratic presidential vote share with 10 percentage points (plus or minus) of Missouri's Democratic vote share. One-way clustered standard errors are clustered at the state level, and two-way are clustered at the state-quarter level. The table includes the imputed response of the job-finding rate and steady-state unemployment rate (p.p.) according to equation (6) and $u_{ss} = s/(s + f)$.

Table 7: Decomposition of Micro and Macro Effects, Political Controls

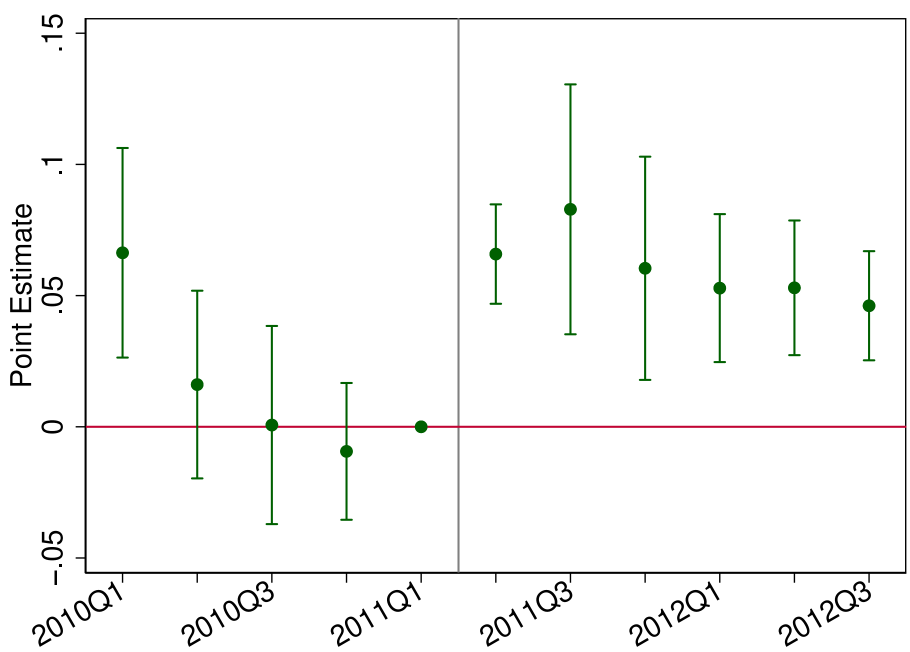
Matching function elasticity, α	Search effort Δs_t	Market tightness $\Delta\theta_t$	% Equilibrium effect in $\Delta \log(f)$
	A. SA Ratio; Unemp Near Peak; GOP House and/or Senate		
0.50	0.055	0.060	35%
0.55	0.048	0.067	43%
0.60	0.040	0.075	53%
	B. SA Ratio; Unemp Near Peak; Similar Obama '08 Vote		
0.50	0.091	0.048	21%
0.55	0.086	0.053	26%
0.60	0.079	0.060	31%
	C. SA Separate; Unemp Near Peak; GOP House and/or Senate		
0.50	0.064	0.052	29%
0.55	0.058	0.058	35%
0.60	0.051	0.065	43%
	D. SA Separate; Unemp Near Peak; Similar Obama '08 Vote		
0.50	0.095	0.044	19%
0.55	0.090	0.049	23%
0.60	0.084	0.055	28%

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 report the policy's estimated effect on the change from 2011Q1 to 2012Q3 in search effort (s_t) and market tightness (θ_t), respectively. Column 4 reports 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 6. Each panel reflects a different specification choice regarding the composition of the control group and the seasonal adjustment of labor market variables.

Figure 1: Dynamic Effect of UI Cut on Unemployment Rate and Vacancy Posting



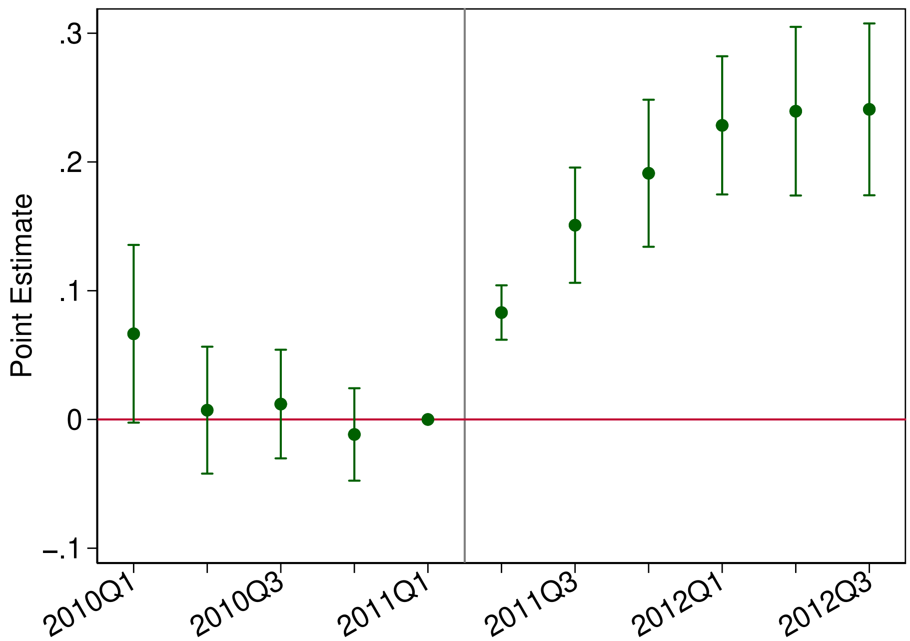
(a) Unemployment Rate



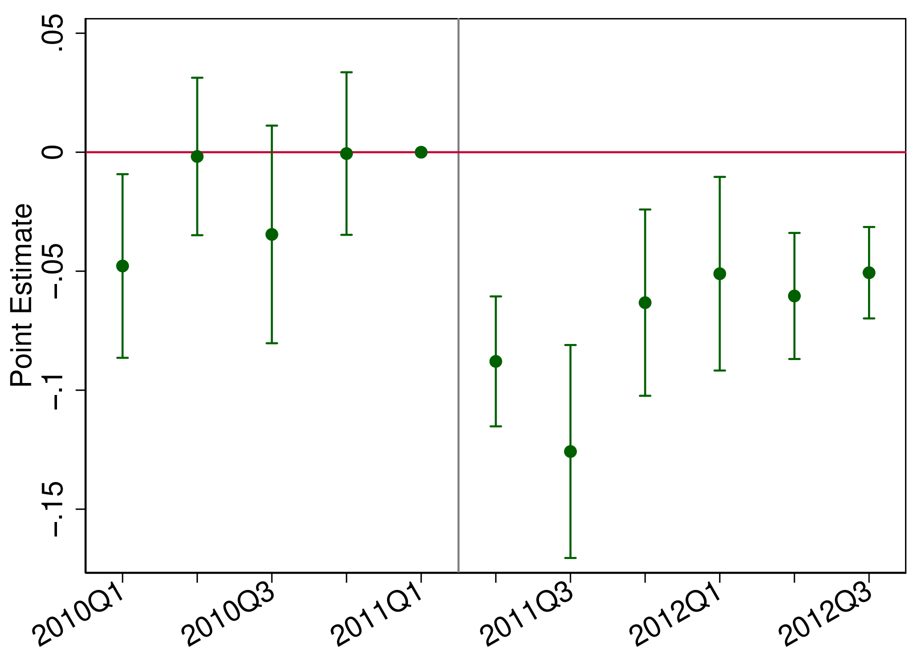
(b) Vacancies ($\log(V)$)

Note: These figures present the event-study plot constructed using a dynamic TWFE model (equation (8)) for the outcome variable of the seasonally-adjusted unemployment rate (panel a) and the log of new vacancies (panel b). The unemployment rate is averaged to quarterly frequency. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. The control group is comprised of "benchmark" states whose unemployment rates peaked near the time of Missouri's and had insolvent UI trust funds (listed in Table A.2). Bars represent 95 percent confidence intervals clustered at the state level.

Figure 2: Dynamic Effects of UI Duration Cut on Tightness and Vacancy-Filling



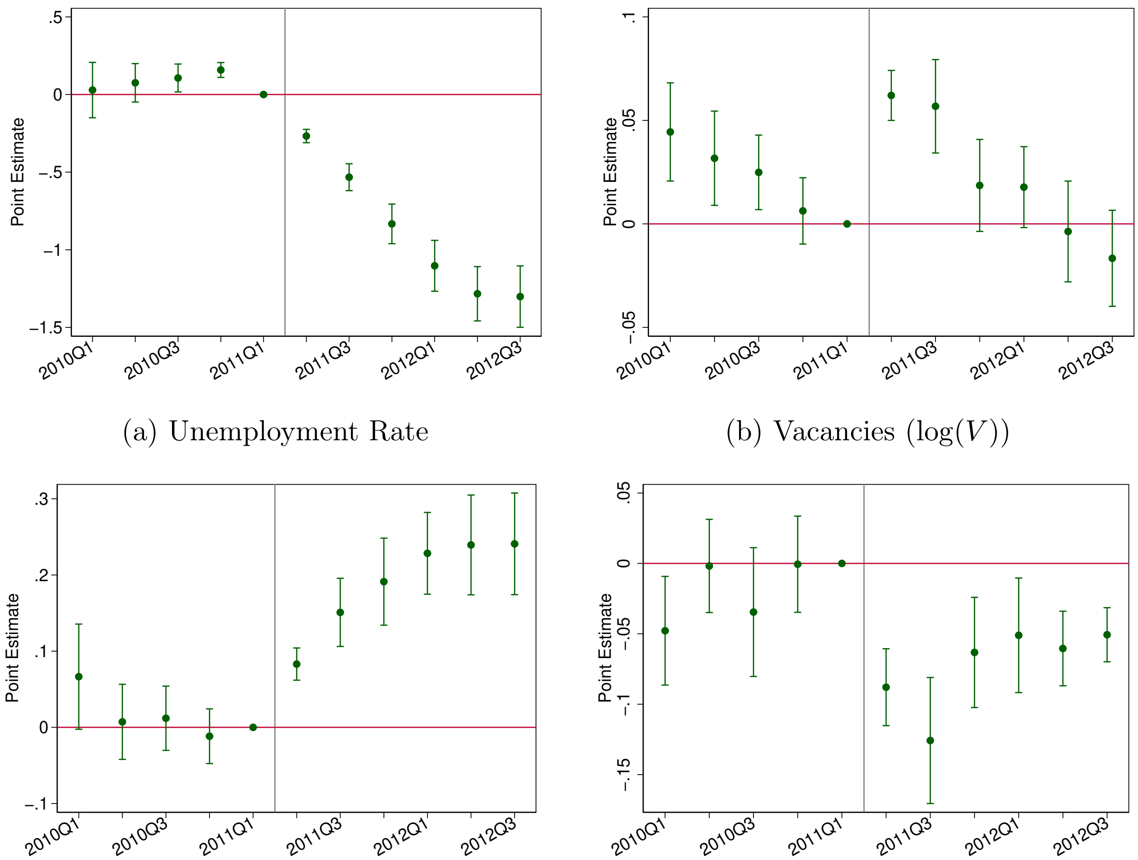
(a) Vacancy-unemployment ratio ($\log(V/U)$)



(b) Vacancy filling rate ($\log(H/V)$)

Note: This figure presents event-study plots constructed using a dynamic TWFE model (equation (8)) for the outcomes of the log of the vacancy-unemployment ratio and vacancy filling rate in panels (a) and (b), respectively. Measures are "new vacancies" and "new stable hires" from HWOL and QWI. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. The control group is comprised of states whose unemployed rate peaked around the time of Missouri's, listed in Table A.2. Bars represent 95 percent confidence intervals clustered at the state level.

Figure 3: Dynamic Effect of UI Cut on Missouri Labor Market, All Control Units



(a) Unemployment Rate

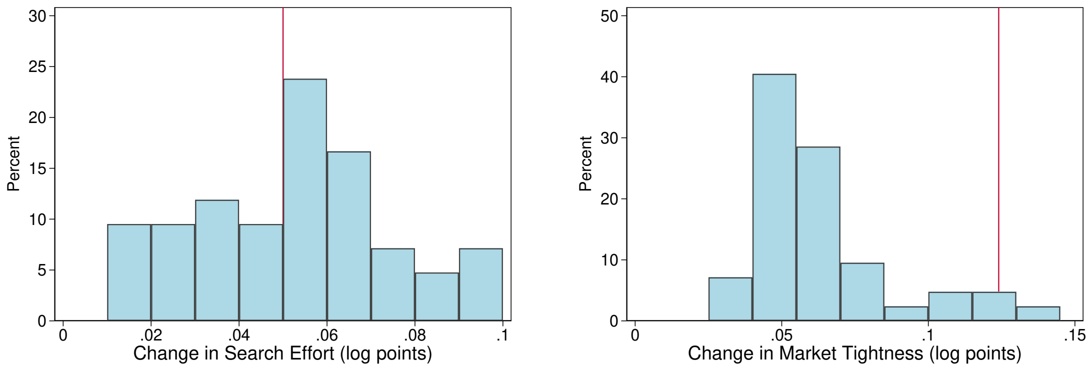
(b) Vacancies ($\log(V)$)

(c) Vacancy-unemployment ratio ($\log(V/U)$)

(d) Vacancy filling rate ($\log(H/V)$)

Note: These figures present the event-study plot constructed using a dynamic TWFE model (equation (8)) for the outcome variable of the seasonally-adjusted unemployment rate (panel a) and the log of new vacancies (panel b). The unemployment rate is averaged to quarterly frequency. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. AR, FL, GA, HI, MA, MI, SC, and DC are excluded from the control group. Bars represent 95 percent confidence intervals clustered at the state level.

Figure 4: Distribution of Estimated Policy Effect on Search Effort and Market Tightness

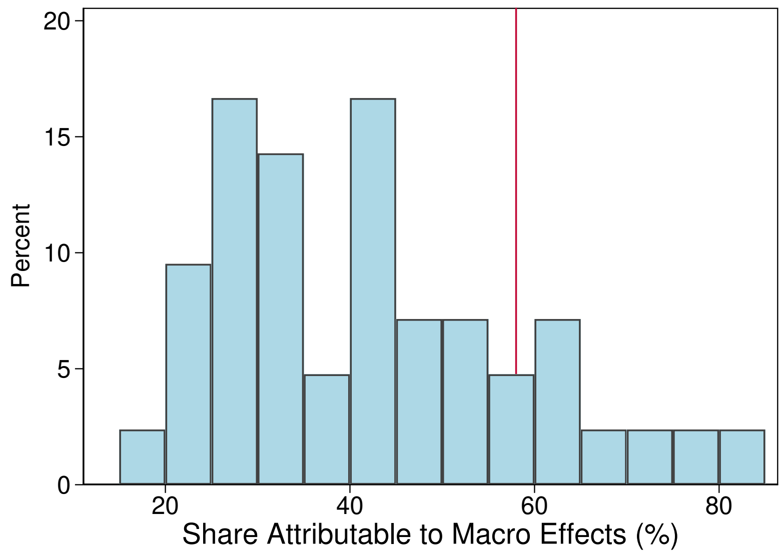


(a) Search Effort

(b) Market Tightness

Note: Panels represent histograms of imputed effect on search effort and market tightness as a result of the policy. Each observation represents a different imputation from Table 3 or a table in Appendix A.1 which conducts robustness on specification sensitivity. These specifications vary with the method the seasonal adjustment method, whether control variables are included in the estimation, the choice of the matching function elasticity, and the composition of the comparison group (benchmark, all states, political control units, surrounding states). The vertical line denotes our baseline estimate from Panel A of Table 3 for $\alpha = 0.55$.

Figure 5: Distribution of Estimated Share of job-finding Rate increase due to Macro Effect



Note: Histogram reports the distribution of the estimated share of the increase in the job-finding rate that is attributable to macro effects. Each observation represents a different imputation from Table 3 or a table in Appendix A.1 which conducts robustness on specification sensitivity. These specifications vary with the method the seasonal adjustment method, whether control variables are included in the estimation, the choice of the matching function elasticity, and the composition of the comparison group (benchmark, all states, political control units, surrounding states). The vertical line denotes our baseline estimate from Panel A of Table 3 for $\alpha = 0.55$.

Online Appendix—Not for Publication

A Further Results: Difference-in-Difference

This section includes several difference-in-difference results not provided in the main text as well as their resulting decomposition using different sample selections.

A.1 Specification Sensitivity

In this subsection, we subject our baseline results to a battery of sensitivity tests regarding the composition of the comparison sample of states, the beginning and end date of analysis, and the inclusion of control variables in TWFE estimation.

A.1.1 Comparison Group of States with Similar Unemployment Rate Peaks

In this section, we relax one of our benchmark conditions about UI trust fund debt and focus on a broader control group of states whose unemployment rate peaked around the time of Missouri's. This control group serves as an intermediate between our benchmark and using all possible states. In Appendix Figure A.1 we plot a histogram of when each state[35] first hit their peak unemployment rate during the Great Recession. Conveniently for our purposes, Missouri reached its peak unemployment in January 2010, the same month as 12 other states (the modal month in the histogram). The one state whose unemployment rate peaked after Missouri's policy change (the red dashed line of Figure A.1) is Hawaii. An essential identifying assumption of our identification strategy is that labor market outcomes of treatment and control states are on parallel trends. The fact Hawaii's labor market had not yet begun to mend when Missouri enacted its policy change makes it a poor candidate for a control state, as including it in estimation would risk violating the parallel trends assumption. Thus, in all analyses (including our full sample in Table 2), we exclude Hawaii from the group of control units.

Using this subset of comparison group states that reached peak unemployment within 2 months (plus or minus) of Missouri's peak, we find dynamic effects which mirror our baseline effects for our main labor market outcomes (see Figures A.2 and A.3) according to our event study specification (equation (8)).

A.1.2 Comparison Group Based on Labor Market and Political Environment

We further tailor control groups to Missouri's particular circumstances. Missouri's 2011 policy change was passed by a Republican-controlled House of Representative and Senate and signed by a Democratic governor. In the most recent presidential election (2008), Missouri narrowly voted for the Republican candidate by 0.13%. We construct two distinct samples:

1. States in which (i) peak unemployment rate within 2 months of Missouri's peak and (ii) Republicans control *at least one* chamber of the legislature in 2011. These restrictions yields 13 control states.

[35] Excluding the non-Missouri states that also cut UI benefit duration: AR, FL, GA, MI, SC; as well as MA for data availability reasons.

2. States in which (i) peak unemployment rate within 2 months of Missouri's peak and (ii) the 2008 Obama voteshare was within 10 points (plus or minus) of Missouri's voteshare. These restrictions yields 13 control states.

These states for each group are listed in Table A.3. Note that we choose to focus on the Republican control of the legislature rather than the Democratic governorship because typically such changes originate in legislature and Republicans are typically the party inclined to cut UI benefit generosity, so Republican control is what is more relevant.

The difference-in-difference estimates for the effect of the policy on the vacancy-unemployment ratio and vacancy-filling rate are presented in the main body in Table 6. Table 7 presents the resulting decomposition. We discuss the results in the main body.

Figure A.4 plots the point estimates for our main outcomes from a dynamic TWFE specification where control units are states where unemployment peaked near the time of Missouri's peak and where Republicans controlled either the state House or Senate in 2011. Reassuringly, we find Figure A.4 exhibits patterns very consistent with Figure 2. Namely, observe a marked increase in $\log(V/U)$ after the policy change and a sharp decrease in $\log(H/V)$ in the first few quarters post-policy before the effect eventually fades. Overall, using this restricted set of control states that match on political and economic characteristics confirms our headline findings.

A.1.3 Comparison Group Based on Surrounding States

A natural control group for Missouri's labor market could simply be the set of states with which Missouri shares a geographic border. These states – Iowa, Illinois, Kentucky, Tennessee, Oklahoma, Kansas, and Nebraska – face similar geographic and economic circumstances by nature of their location.[36] In this section, we report the results of our TWFE specifications (standard and dynamic) as well as the resulting decomposition when we only include these border states as the control group.

Panel A of Table A.4 presents point estimates for the effect of the policy on the vacancy-unemployment ratio, which increases roughly 8 log points as a result. The point estimates are only slightly smaller in magnitude to our baseline estimates for the vacancy-unemployment ratio in Table 2 (11–15 log points). However, due to a much smaller sample size, we can only reject a null of zero effect at the 5% level.

Panel B of Table A.4 presents point estimates for the effect of the policy on vacancy-filling, which decreases by 2-3 log points. The point estimates match the magnitude to our baseline estimates for the effects on the vacancy-filling rate in Table 2 (between -2 and -4 log points). However, the smaller sample size renders our estimates less precise, with p-values around 0.2.

The final part of Table A.4 characterizes the implied effects of the policy on the labor market, suggesting the job finding rate increase by 5–6 log points, depending on seasonal adjustment method. This magnitude is roughly half the implied value from Table A.4 of 9–11 log points. For a matching function elasticity of $\alpha = 0.55$, we find that search effort increased by 2–4 log points and tightness increased by 4–6 log points. This compares to 6–7 log points

[36] We exclude analyzing Arkansas as a border state due to changing its UI generosity around the same time as Missouri.

and 5–9 log points in our baseline decomposition for $\alpha = 0.55$ in Table 3, respectively. While the magnitudes of the responsiveness of search and tightness are somewhat less responsive when we use border states as control units, it is reassuring to note the general patterns confirm our headline findings.

When we use border states as control units, our decomposition finds that macro effects can account for between 41% and 61% of the total effect on the job finding rate. This range is similar to Table 3, which places this range at 28–58% when the matching function elasticity is 0.55. Overall, a decomposition using border states alone as control units confirms our headline findings.

Lastly, A.5 plots the point estimates from a dynamic TWFE specification where border states are the control units and we find patterns consistent with Figure 2. Specifically, point estimates for $\log(V/U)$ gradually increase by over 10 log points after a year after the policy change (similar to Figure 2a) and point estimates for $\log(H/V)$ sharply decreased by 8 log points for several quarters before fading to zero (similar to Figure 2b). However, a key difference is that the point estimates from Figure A.5 are less precisely estimated due to a smaller sample size.

A.1.4 Start & End Dates of Panel

As a sensitivity check, we show that our baseline choices for start and end date for equation (7) do not yield outlier estimates for the effect of the policy on $\log(V/U)$ or $\log(H/V)$ or the resulting imputations. Specifically, in Table A.5 we apply equation (7) to panels with earlier start dates (2007Q1, 2008Q1, 2009Q1) and a later end date (2013Q1). In the main body we discuss the perils with an earlier beginning of the panel, namely that it becomes more likely comparison unit states are on different labor market trends than Missouri due to the onset of the Great Recession. Further, our choice to end our analysis in 2012Q3 is driven by the fact the policy appears to have had its "maximal impact" on the unemployment rate in Missouri by 2012Q3, seen clearly in Figure A.6. Moreover, we avoid analyzing time periods beyond 2013Q1 in Table A.5 because the EUC program which extended UI benefit duration to (up to) 99 weeks for most of our comparison states expired at the end of 2013. Analyzing outcomes beyond 2013 would mean the comparison units all received a separate type of treatment later in the panel.

Nevertheless, we find our estimates on all outcomes – $\log(V/U)$, $\log(H/V)$, imputed job finding, search effort, market tightness, and macro effects – remarkably stable, especially for panels that begin in 2008 or 2009 (as opposed to our 2010 baseline). Table A.5 provides confidence our results are not driven by arbitrarily picking a time window of analysis that is favorable to our conclusions.

A.1.5 Inclusion of Controls

Such controls are included in the $\boldsymbol{X_{st}}$ in the TWFE equation (10):

$$y_{st} = \lambda_s + \gamma_t + \beta \cdot D_{st} + \boldsymbol{X_{st}\theta} + \varepsilon_{st} \tag{10}$$

as well as its dynamic version (11), both of which we estimate.

$$y_{st} = \lambda_s + \gamma_t + \boldsymbol{X_{st}\theta} + \sum_{k \neq 0} \beta_k \cdot D_{st}^k + \varepsilon_{st} \tag{11}$$

Table A.6 reports the estimated effects on our key outcomes of interest. The estimates for the vacancy-unemployment ratio and the vacancy filling rate are largely similar to our baseline results in Table 2, although they are slightly attenuated towards zero and, as expected, have larger standard errors due to the inclusion of controls. In our preferred sample construction (column 1), inclusion of covariates lowers the estimate of the policy's effect on the vacancy-unemployment ratio from 11.5 log points down to 10.2 log points. Effects on the vacancy-unemployment ratio are still highly robust to the inclusion of controls, with strong significance across all specifications. Figure A.7a illustrates the slight attenuation of our estimates for the vacancy-unemployment ratio. On the other hand, it shows that the inclusion of controls flattens the pre-trends.

We also continue to estimate a negative effect of the policy on the vacancy-filling rate when controls are included, in line with our framework's prediction of greater job availability. However, the estimated negative effect drops from 2-4 log points in the baseline to 1-3 log points in Table A.6. Due to attenuated point estimates and significantly larger standard errors, we cannot reject the null when clustering at the state-quarter level using equation (10).[37] However, Figure A.7b, which estimates the dynamic effects using equation (11), shows a clear sharp and statistically significant jump in $\log(H/V)$ in 2011Q2-Q3 which is robust to the inclusion of covariates. This is consistent with the idea vacancies are a jump variable and respond immediately to market fundamentals.

We also show that our findings about the effect on the job-finding rate and steady state unemployment rate are robust to the inclusion of state-level controls. Specifically, Table A.6 suggests Missouri's policy boosted the job-finding rate by 8–9 log points and lowered the steady state unemployment rate by 0.5–0.6 percentage points. These results are essentially the same as in the baseline estimates of Table 2.

Finally, Table A.6 uses the point estimates to repeat the decomposition into micro and macro effects. For $\alpha = 0.55$, we infer an increase of search effort of 4–6 log points. These changes in search effort imply that at 25–53% of increase in the job-finding rate is attributable to the macro effect (compared to 28–44% in our baseline decomposition in Table 3).

Lastly, while our baseline specification does not include control variables, in Appendix A.1.5, we also control for other relevant state-level factors that might affect labor market conditions. Specifically, in Table A.6, we control for state industrial composition, home ownership rates, and the share of establishments expanding or contracting, all of which may vary in important ways by state during this period at the peak of the Great Recession.

[37] Several estimates for $\log(H/V)$ are significant at the 5% level when including controls when clustering at the state level only, although we believe it is likely that errors are correlated by state and over time, and thus two-way clustering is preferred.

A.2 Alternate Vacancy & Hire Measures

In this section, we examine the sensitivity of our estimates to the coverage of the vacancy and hire counts. Specifically, our data sources offer various measures of vacancies and hires. In our baseline analysis, we use new vacancies rather than all vacancies in HWOL because it is intuitive that newly-created job postings would be more responsive to a policy change than the stock of all vacancies (new and existing). Likewise, we choose to measure how "new stable hires" in the QWI react to the policy change because we want to exclude hires that do not last a full quarter.

Figure A.8 demonstrates that our estimates for the effect on the vacancy-unemployment ratio are robust to both alternate seasonal adjustments and measures of vacancies. Our estimates for the effect on $\log(V/U)$ – whether calculated using all or only new vacancies, and regardless of applying seasonal adjustments to the time series as ratios or to their individual components – consistently show similar patterns. These estimates are flat and statistically insignificant before the policy change, and increase significantly, by about 10–15 log points, one year following the policy change.

Figure A.9 illustrates that our estimates for the effect on the vacancy-filling rate are generally insensitive to seasonal adjustment and hire measurement choice. Point estimates for the permutations follow the same basic pattern of decreasing sharply in the first two post-policy periods before slowly fading to smaller values. In all permutations, pre-trends are sufficiently flat.

As in the main text, we then take the estimated effects on the vacancy-unemployment rate and vacancy-filling rate and use equation (4) to infer the response on search effort and market tightness. The results of this decomposition (analogous to Table 3) are listed in Table A.8. Choosing a broader definition of hires (stable and non-stable hires) and different seasonal adjustments do not materially change the degree to which the policy's effect on job finding is mediated through equilibrium effects. For reasonable ranges of the matching function elasticity α, Table A.8 reports effects on search effort of 3–8 log points (compared to 3–7 in Table 3's baseline results), on market tightness of 4–14 log points (compared to 4–14 in baseline), and shares attributable to the equilibrium effect of 18–72% (compared to 23–71% in baseline).

Overall, we conclude the choice of seasonal adjustments and broadness of specific labor market measurements do not meaningfully matter for our results on the policy's effect on search effort and market tightness.

A.3 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 A.10 plots the population-weighted average of the vacancy-unemployment ratio to that of hires (both logged) over our sample period.

A.4 Other Outcomes and Raw Time Series

A.4.1 Hiring, Labor Force, Employment

While we report our difference-in-difference specifications for the most relevant outcomes for our framework in the body of the paper, we also report the direct effects of the policy change on hiring in Missouri. Figure A.11 shows the event study plot for equation (8) with the log of new hires as the dependent variable. Regardless of the composition of control units, our estimates for the effect of the policy on hiring are imprecise and centered around zero on impact and then rise and become positive toward the end of the sample. As discussed in section 7 and illustrated in figure A.17, when the UI cut has labor supply and demand effects, the effect on hires can be ambiguous even if the impact on labor market tightness is unambiguous. From the point of view of the theory, the H/V ratio is the model relevant measure for labor market tightness which is why we report those measures in the main text.

Additionally, in Figure A.12 we regress the log of labor force size on the event study specification to measure the impact of the policy change on the stock of unemployed and employed workers together. The figure illustrates that there are negative labor supply effects in addition to positive labor demand effects of the UI benefit cut, as discussed in 7 and illustrated in figure A.17.

Lastly, in Figure A.13 we regress the log of employment on the event study specification to measure the impact of the policy change on the stock of employed workers. We generally find positive effects of the UI benefit cut (the coefficients are positive significant at the 10% level for the first year), consistent with the foregoing discussion of positive labor demand effects and negative labor supply effects.

A.4.2 Raw Time Series

Lastly, we show the evolution of our paper's key labor market measures – (log of) unemployment, vacancies, hires, tightness, and the vacancy-filling rate – in Missouri over the relevant time period, normalized to their 2010Q1 level. The constituent components are presented in Figure A.14a and the key ratios of tightness and vacancy-filling are presented in Figure A.14b. While we believe the dynamic TWFE plots better represent the causal effect of the policy change on these outcomes, it is reassuring to observe a similar pattern in the raw data after 2011Q1 whereby $\log(V/U)$ in Missouri rises gradually and $\log(H/V)$ precipitously drops, as in Figure 2.

Further, we show how vacancies as a share of the labor force evolved in Missouri (normalized to their 2010Q1 level) in Figure A.15. Consistent with our broader findings, we find that vacancies increase markedly soon after the reform in 2011Q1.

Lastly, we show that the ratio of hires to employment in Missouri generally increased relative to its 2010Q1 level and generally did not break from the trend pre- and post-benefit cut in Figure A.16.

Table A.1: UI Trust Fund Debt by State in August 2010

State	**UI Debt (in millions)**
MO	722
AL	283
AZ	59
CA	7700
CO	251
CT	498
DE	13
FL	1600
GA	416
ID	202
IL	2200
IN	1700
KS	88
KY	795
MD	134
MA	387
MI	3800
MN	487
NV	490
NJ	1700
NY	3200
NC	2300
OH	2300
PA	3000
RI	225
SC	887
TX	1300
VT	33
VI	16
VA	347
WI	1400

Note: Table lists the set of "benchmark" control group states which had elevated levels of debt in their UI trust fund as of August 2010.

Table A.2: List of States Whose Unemployment Rate Peaked Near Missouri's Peak

State	Peak Month	Peak Unemployment Rate (%)
NE	Nov 2009	4.9
NH	Nov 2009	6.7
NY	Nov 2009	9.0
OK	Nov 2009	7.0
AK	Dec 2009	8.4
AL	Dec 2009	11.1
AZ	Dec 2009	10.6
IL	Dec 2009	11.4
ME	Dec 2009	8.7
OH	Dec 2009	11.1
CA	Jan 2010	12.6
DE	Jan 2010	9.1
MD	Jan 2010	8.1
MO	Jan 2010	9.7
MS	Jan 2010	10.5
MT	Jan 2010	7.5
NJ	Jan 2010	10.0
UT	Jan 2010	8.0
VA	Jan 2010	7.6
VT	Jan 2010	6.7
WA	Jan 2010	9.3
WV	Jan 2010	9.0
WY	Jan 2010	7.6
PA	Feb 2010	8.5
RI	Feb 2010	11.9
CT	Mar 2010	9.7
NM	Mar 2010	7.9

Note: Table lists the set of control group states which experienced peak unemployment within two months (plus or minus) of Missouri's peak in January 2010. Reported is each state's peak unemployment rate during the Great Recession and the month it first reached its peak value (some states remained at their peak for more than one month). Table excludes states which cut UI benefit duration around the same time as Missouri.

Table A.3: List of Control Group States with Similar Economic and Political Fundamentals

State	Unemployment Peak Near MO	GOP Controls One or More Chambers	Within +/− 10 p.p. MO Obama '08 vote share
AK	✓	✓	
AL	✓	✓	
AZ	✓	✓	✓
ME	✓	✓	✓
MS	✓		✓
MT	✓	✓	✓
NE	✓		✓
NH	✓	✓	✓
NJ	✓		✓
NM	✓		✓
NY	✓	✓	
OH	✓	✓	✓
OK	✓	✓	
PA	✓	✓	✓
VA	✓	✓	✓
UT	✓	✓	
WA	✓		✓
WV	✓		✓
WY	✓	✓	

Note: Table lists the control group states used in a robustness exercise (e.g. Table 6) which compares Missouri to states which both experienced peak unemployment within two months (plus or minus) of Missouri's peak in January 2010 and also have similar political fundamentals. GOP Controls One or More Chambers requires Republicans control either the state legislature's house or senate in 2011. Table excludes states which cut UI benefit duration around the same time as Missouri.

Table A.4: UI Duration Cut Effects on Missouri Labor Market: Border State Control Group

	(1)	(2)
A. *Vacancies/Unemployment*		
$\hat{\beta}_{MO\_post}$	0.078	0.080
One-way clustered SE	(0.034)	(0.034)
Two-way clustered SE	(0.030)	(0.031)
B. *Hires/Vacancies*		
$\hat{\beta}_{MO\_post}$	-0.026	-0.020
One-way clustered SE	(0.020)	(0.020)
Two-way clustered SE	(0.019)	(0.018)
Implied Effects on Labor Market ($\alpha = 0.55$)		
Δ log(f)	0.052	0.060
Δs_t	0.020	0.036
$\Delta \theta_t$	0.058	0.044
Share Attributable to Macro	61%	41%
Seasonally Adjusted as Ratio	✓	
Seasonally Adjusted Separately		✓
N	88	88

Note: All specifications follow column 1 of Table 2, with measures adjusted either as ratios (column 1) or separately (column 2). Control units include IA, IL, KS, KY, NE, OK, and TN. One-way clustered standard errors are clustered at the state level, and two-way are clustered at the state-quarter level. The table includes the imputed response of the job finding rate, market tightness, search effort, and the share attributable to macro effects according to (6) assuming $\alpha = 0.55$.

Table A.5: UI Duration Cut Effects on Missouri Labor Market: Varying Start/End Dates

	(1)	(2)	(3)	(4)
A. *Vacancies/Unemployment*				
$\hat{\beta}_{MO\_post}$	0.174	0.166	0.198	0.284
One-way clustered SE	(0.031)	(0.039)	(0.038)	(0.047)
Two-way clustered SE	(0.030)	(0.037)	(0.037)	(0.046)
B. *Hires/Vacancies*				
$\hat{\beta}_{MO\_post}$	-0.056	-0.050	-0.066	-0.117
One-way clustered SE	(0.013)	(0.015)	(0.013)	(0.019)
Two-way clustered SE	(0.012)	(0.019)	(0.018)	(0.022)
Implied Effects on Labor Market ($\alpha = 0.55$)				
Δ log(f)	0.118	0.116	0.132	0.167
Δs_t	0.050	0.055	0.051	0.024
$\Delta\theta_t$	0.124	0.111	0.147	0.260
Share Attributable to Macro	58%	53%	61%	86%
Beginning Panel	2010Q1	2009Q1	2008Q1	2007Q1
End Panel	2012Q3	2013Q1	2013Q1	2013Q1
N	165	255	315	375

Note: All specifications follow column 1 of Table 2, with measures adjusted as ratios and using the "full comparison group" which includes all states except AR, FL, GA, HI, MA, MI, SC and DC. One-way clustered standard errors are clustered at the state level, and two-way are clustered at the state-quarter level. The table includes the imputed response of the job finding rate, market tightness, search effort, and the share attributable to macro effects according to (6) assuming $\alpha = 0.55$.

Table A.6: Estimated Effects of UI Duration Cut on Missouri: Including Control Variables

	(1)	(2)	(3)	(4)
A. *Vacancies/Unemployment*				
$\hat{\beta}_{MO\_post}$	0.102	0.102	0.107	0.104
One-way clustered SE	(0.031)	(0.018)	(0.030)	(0.018)
Two-way clustered SE	(0.040)	(0.015)	(0.044)	(0.020)
B. *Hires/Vacancies*				
$\hat{\beta}_{MO\_post}$	-0.026	-0.012	-0.018	-0.010
One-way clustered SE	(0.016)	(0.011)	(0.016)	(0.011)
Two-way clustered SE	(0.025)	(0.019)	(0.034)	(0.019)
Implied Effects on f and u_{ss}				
Δ log(f)	0.076	0.090	0.089	0.094
Δu_{ss} (percentage points)	-0.49	-0.59	-0.58	-0.62
Include Control Vector	✓	✓	✓	✓
Seasonal Adjustment as Ratios	✓	✓		
Seasonal Adjustment Separately			✓	✓
Full Comparison Group		✓		✓
N	165	473	165	473

Note: Hires and vacancies are new measures, as documented in HWOL and QWI respectively. Balanced panel includes all states except AR, FL, GA, HI, MA, MI, SC and DC. Vacancies, Hires, and Unemployment are either seasonally adjusted independently or as ratios V/U and H/V. One-way clustered standard errors are clustered at the state level, and two-way are clustered at the state-quarter level. The table includes the imputed response of the job-finding rate and steady-state unemployment rate (p.p.) according to equation (6) and $u_{ss} = s/(s+f)$.

Table A.7: Decomposition of Micro and Macro Effects: Specifications with Control Variables

Matching function elasticity, α	Search effort Δs_t	Market tightness $\Delta\theta_t$	% Equilibrium effect in $\Delta \log(f)$
	Panel A. SA Ratio; Benchmark Control Units		
0.50	0.027	0.066	55%
0.55	0.019	0.073	67%
0.60	0.010	0.082	83%
	Panel B. SA Ratio; All Control Units		
0.50	0.064	0.034	21%
0.55	0.061	0.038	25%
0.60	0.056	0.042	31%
	Panel C. SA Separately; Benchmark Control Units		
0.50	0.044	0.059	40%
0.55	0.037	0.066	49%
0.60	0.029	0.074	61%
	Panel D. SA Separately; All Control Units		
0.50	0.058	0.043	27%
0.55	0.053	0.048	33%
0.60	0.047	0.054	41%

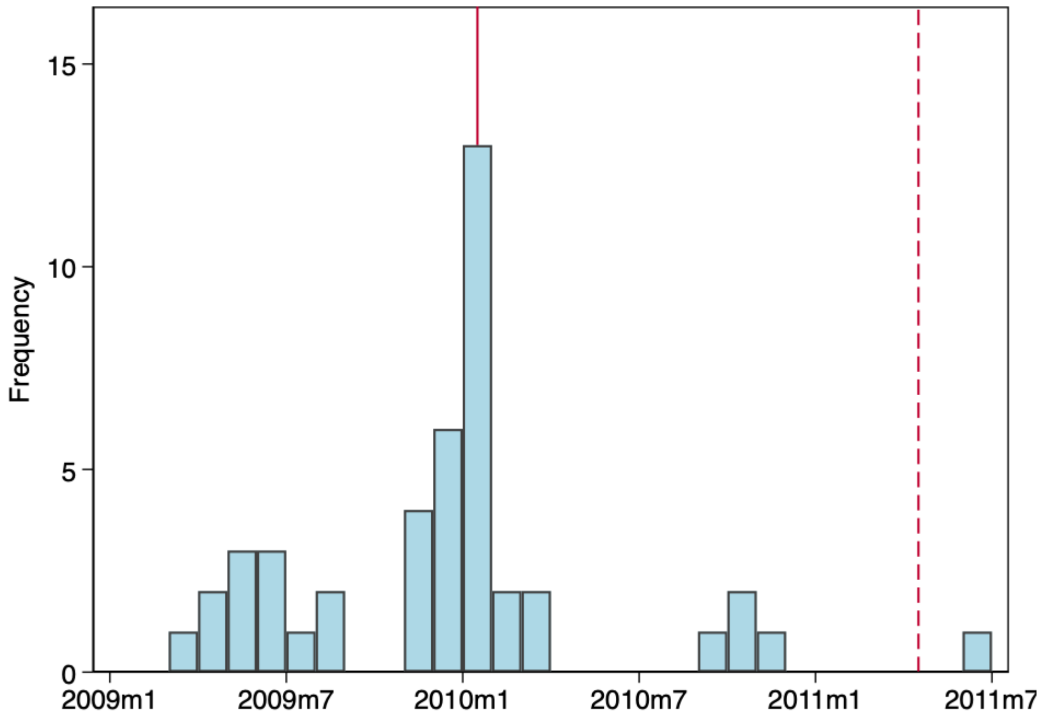
Note: This table decomposes the effect of the UI cut into micro and macro effects for different matching function elasticities α. Columns 2 and 3 report the policy's estimated effect on the change from 2011Q1 to 2012Q3 in search effort (s_t) and market tightness (θ_t), respectively. Column 4 reports 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 A.6, in which all specifications include a vector of covariates (equation (10)). Each panel reflects a different specification choice regarding the composition of the control group and the seasonal adjustment of labor market variables.

Table A.8: Decomposition of Micro and Macro Effects: Broader Hire Measures

Matching function elasticity, α	Search effort Δs_t	Market tightness $\Delta\theta_t$	% Equilibrium effect in $\Delta\log(f)$
	Panel A. SA Ratio; Benchmark Control Units		
0.50	0.079	0.095	38%
0.55	0.068	0.106	46%
0.60	0.055	0.119	57%
	Panel B. SA Ratio; All Control Units		
0.50	0.080	0.035	18%
0.55	0.077	0.039	22%
0.60	0.072	0.044	27%
	Panel C. SA Separately; Benchmark Control Units		
0.50	0.060	0.110	48%
0.55	0.048	0.123	58%
0.60	0.033	0.138	72%
	Panel D. SA Separately; All Control Units		
0.50	0.062	0.051	29%
0.55	0.057	0.056	35%
0.60	0.050	0.063	43%

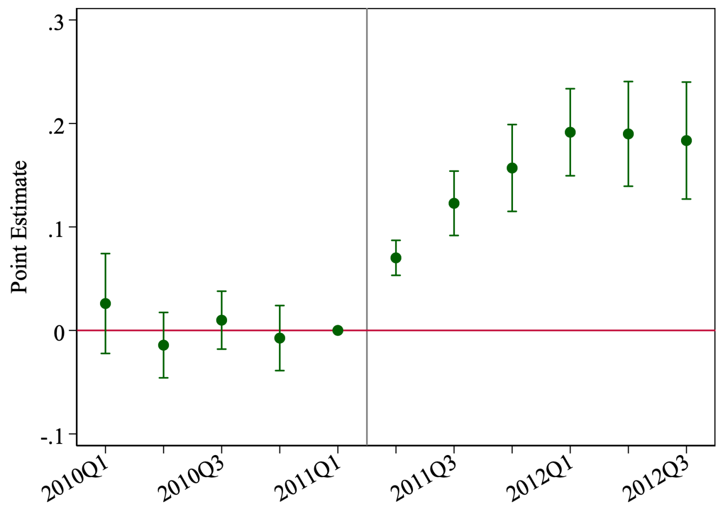
Note: This table decomposes the effect of the UI cut into micro and macro effects for different matching function elasticities α, similar to Table 3, but using a broader measure of "new hires" which includes stable and non-stable hires from QWI. Columns 2 and 3 show the estimated change in search effort, 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 2. Each panel reflects a different specification choice regarding the seasonal adjustment of labor market variables.

Figure A.1: Month of Peak Unemployment during Great Recession for U.S. States



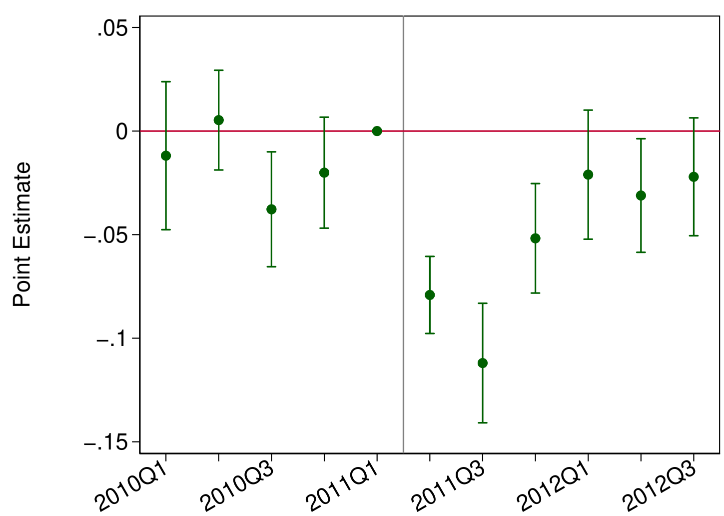
Note: Histogram plots the temporal distribution of the first month U.S. states reached their peak unemployment rate during the Great Recession. Each bin represents a single month. Missouri's unemployment rate peaked in January 2010 (denoted with a solid red line). Missouri's policy change occurred in April 2011 (denoted with a dashed red line). HI is the only state whose unemployed rate peaked after Missouri's benefit cut (peaked in June 2011). States excluded from histogram are AR, FL, GA, MI, SC, and MA.

Figure A.2: Effect on Vacancy-Unemployment Ratio with Unemployment Rate Control Group



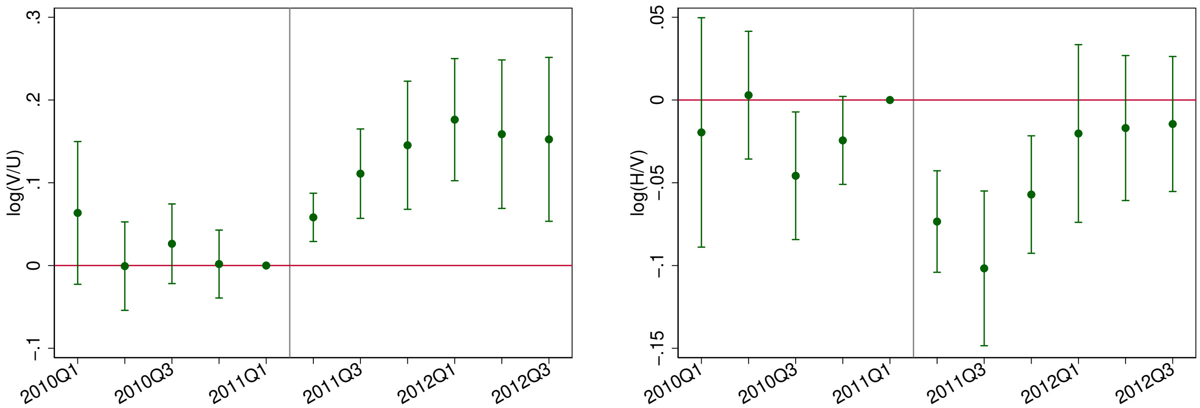
Note: Plot represents point estimates for dynamic TWFE regression of log(vacancies/unemployed). The omitted category for these specifications is 2011Q1. Measures are "new vacancies" and "new stable hires" from HWOL and QWI, respectively. Control group states (listed in Table A.2) are limited to those states whose unemployment rate peaked within two months of Missouri's peak in January 2010. Bars represent 95 percent confidence intervals clustered at the state level.

Figure A.3: Effect on Vacancy Filling Rate with Unemployment Rate Control Group



Note: Plot represents point estimates for dynamic TWFE regression of log(hires/vacancies). The omitted category for these specifications is 2011Q1. Measures are "new vacancies" and "new stable hires" from HWOL and QWI, respectively. Control group states (listed in Table A.2) are limited to those states whose unemployment rate peaked within two months of Missouri's peak in January 2010. Bars represent 95 percent confidence intervals clustered at the state level.

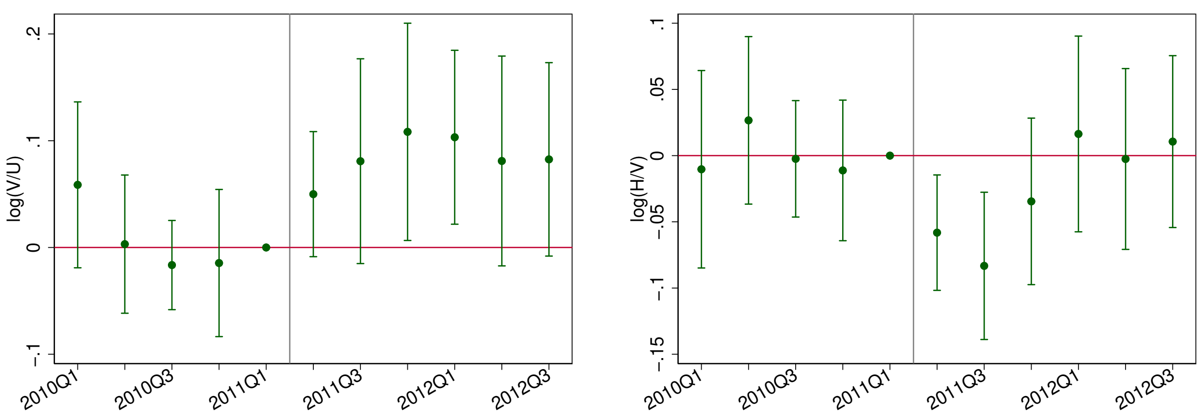
Figure A.4: Dynamic Effects of UI Duration Cut on Missouri: Control Units Matched for Economic and Political Characteristics



(a) Vacancy-unemployment ratio ($\log(V/U)$) (b) Vacancy filling rate ($\log(H/V)$)

Note: This figure presents event-study plots constructed using a dynamic TWFE model for the outcomes of the log of the vacancy-unemployment ratio and vacancy filling rate in panels (a) and (b), respectively. Measures are "new vacancies" and "new stable hires" from HWOL and QWI. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. Control units are the following states where unemployment peaked near the time of Missouri's peak and where Republicans controlled either the state House or Senate in 2011 (similar to Missouri): AL, AK, AZ, ME, MT, NH, NY, OH, OK, PA, UT, VA, and WY. Bars represent 95 percent confidence intervals clustered at the state level.

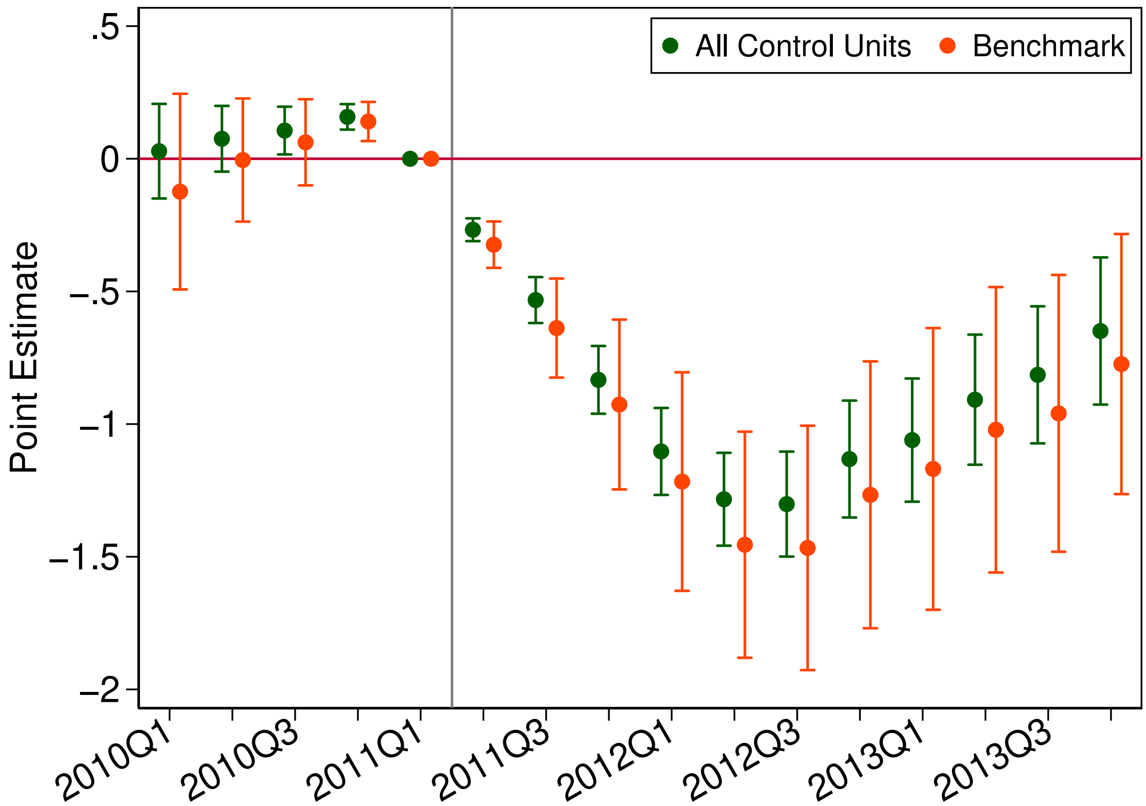
Figure A.5: Dynamic Effects of UI Duration Cut on Missouri: Border State Control Units



(a) Vacancy-unemployment ratio ($\log(V/U)$) (b) Vacancy filling rate ($\log(H/V)$)

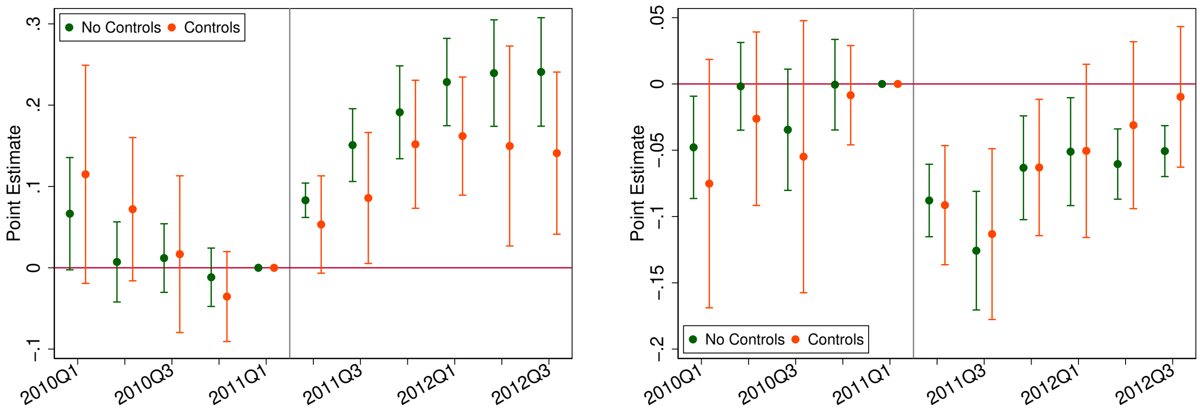
Note: This figure presents event-study plots constructed using a dynamic TWFE model for the outcomes of the log of the vacancy-unemployment ratio and vacancy filling rate in panels (a) and (b), respectively. Measures are "new vacancies" and "new stable hires" from HWOL and QWI. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. Control units include Missouri's border states of IA, IL, KS, KY, NE, OK, and TN. Bars represent 95 percent confidence intervals clustered at the state level.

Figure A.6: Dynamic Effect of UI Cut on Unemployment Rate through 2013Q4



Note: This figure presents the event-study plot constructed using a dynamic TWFE model for the outcome variable of the seasonally-adjusted unemployment rate. Distinct from Figure 1a, we measure the dynamic effect of the policy through 2013Q4 rather than 2012Q3. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. For the "all control units" comparison group, the following states are excluded for reasons cited in the text: AR, FL, GA, HI, MA, MI, SC and DC. The "benchmark" states had unemployment rates which peaked near the time of Missouri's and insolvent UI trust funds, and are listed in Table A.2. Bars represent 95 percent confidence intervals clustered at the state level.

Figure A.7: Dynamic Effects of UI Duration Cut on Missouri: Including Control Variables

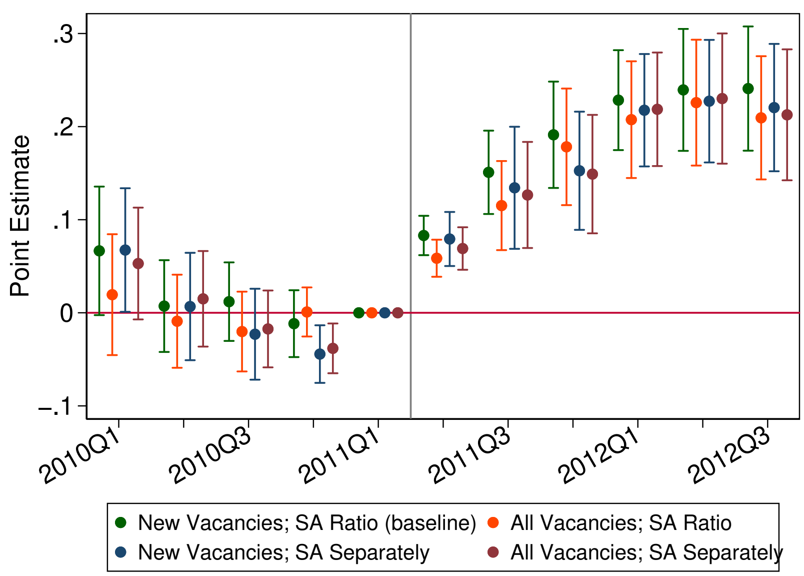


(a) Vacancy-unemployment ratio ($\log(V/U)$)

(b) Vacancy filling rate ($\log(H/V)$)

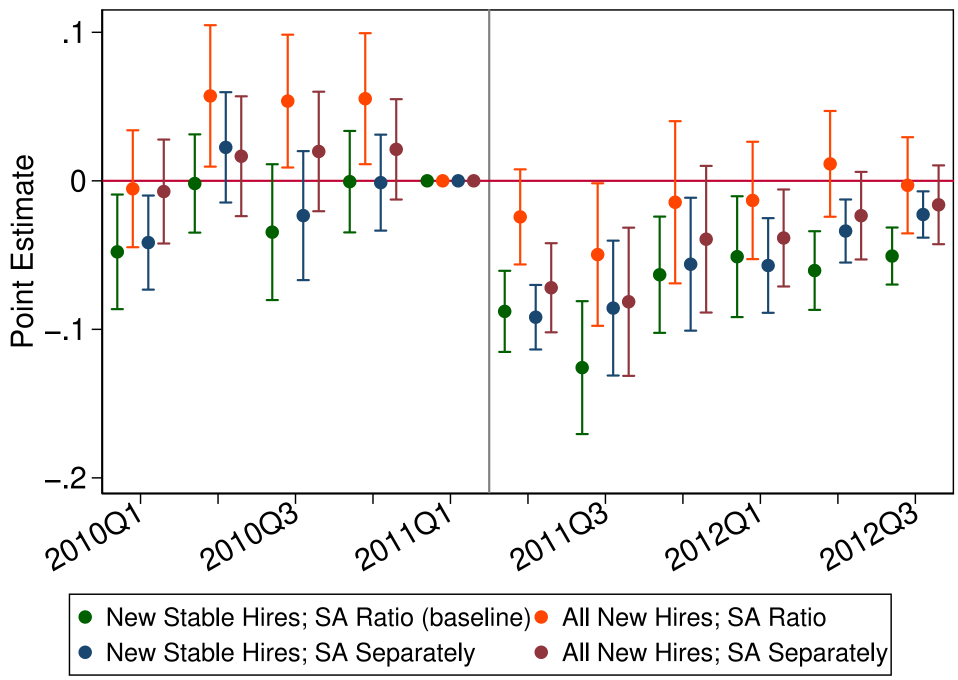
Note: This figure presents event-study plots constructed using a dynamic TWFE model with controls (equation (11) in orange) and without controls (equation (8) in green) for the outcomes of the log of the vacancy-unemployment ratio and vacancy filling rate in panels (a) and (b), respectively. Measures are "new vacancies" and "new stable hires" from HWOL and QWI. We consider 2011Q1 to be the time of treatment given the policy change affected outcomes in nearly all of 2011Q2. AR, FL, GA, HI, MA, MI, SC and DC are excluded from the control group. Bars represent 95 percent confidence intervals clustered at the state level.

Figure A.8: Effect on Vacancy/Unemployment: Alternate Vacancy Measure/Seasonal Adjust



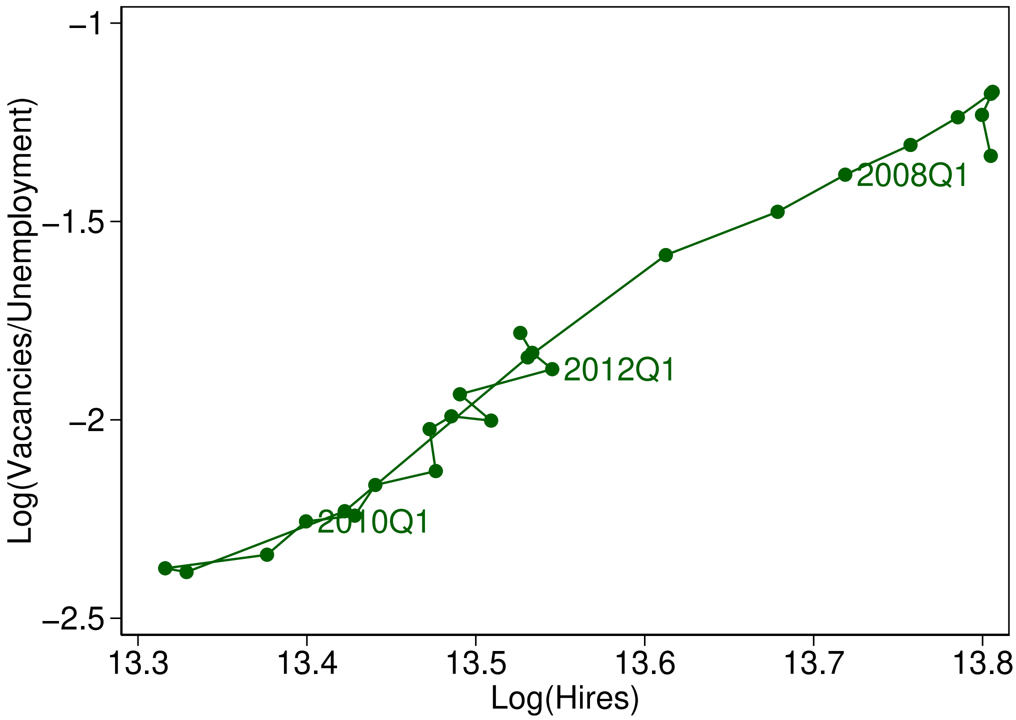
Note: Plot presents point estimates for log(vacancies/unemployed) regressed on equation (8). Our preferred measure is presented in green. We vary (i) the seasonal adjustment method and (ii) the HWOL measure of either new vacancies or "all" vacancies (i.e. new and existing). The omitted category for these specifications is 2011Q1. Bars represent 95 percent confidence intervals clustered at the state level.

Figure A.9: Effect on Vacancy-Filling Rate: Alternate Hire Measure/ Seasonal Adjust



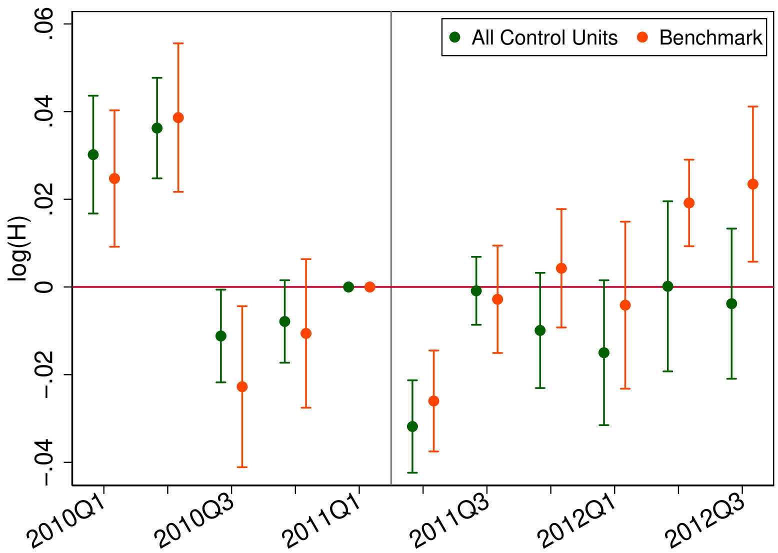
Note: Plot presents point estimates for log(hires/vacancies) regressed on equation (8). Our preferred measure is presented in green. We vary (i) the seasonal adjustment method and (iii) the QWI measure of either new hires or new stable hires, where the latter omits hires that don't last the full quarter. The omitted category for these specifications is 2011Q1. Bars represent 95 percent confidence intervals clustered at the state level.

Figure A.10: Vacancies-Unemployment vs. Hires, All States, 2006Q3–2012Q3



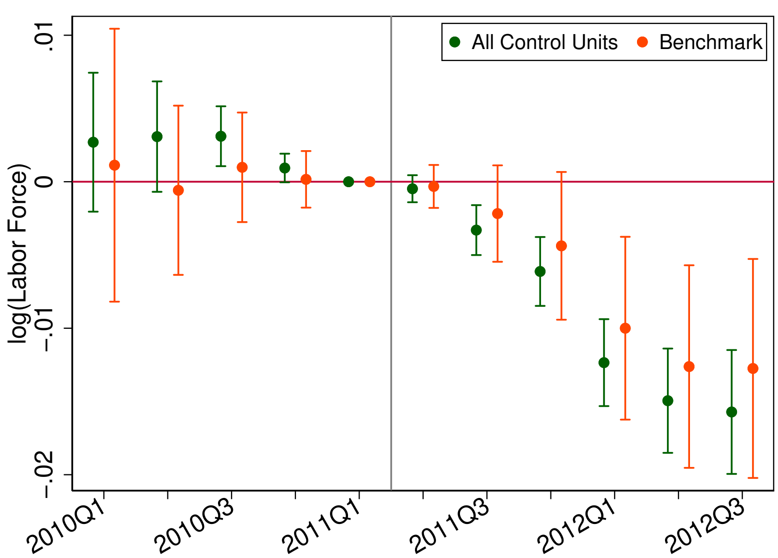
Note: Vacancies, unemployment, and hires are calculated as population-weighted averages of all 50 states. Next, the vacancy measure is divided by the unemployment measure. Last, we take logs of V/U and H measures.

Figure A.11: Dynamic Effect of UI Cut on Hiring



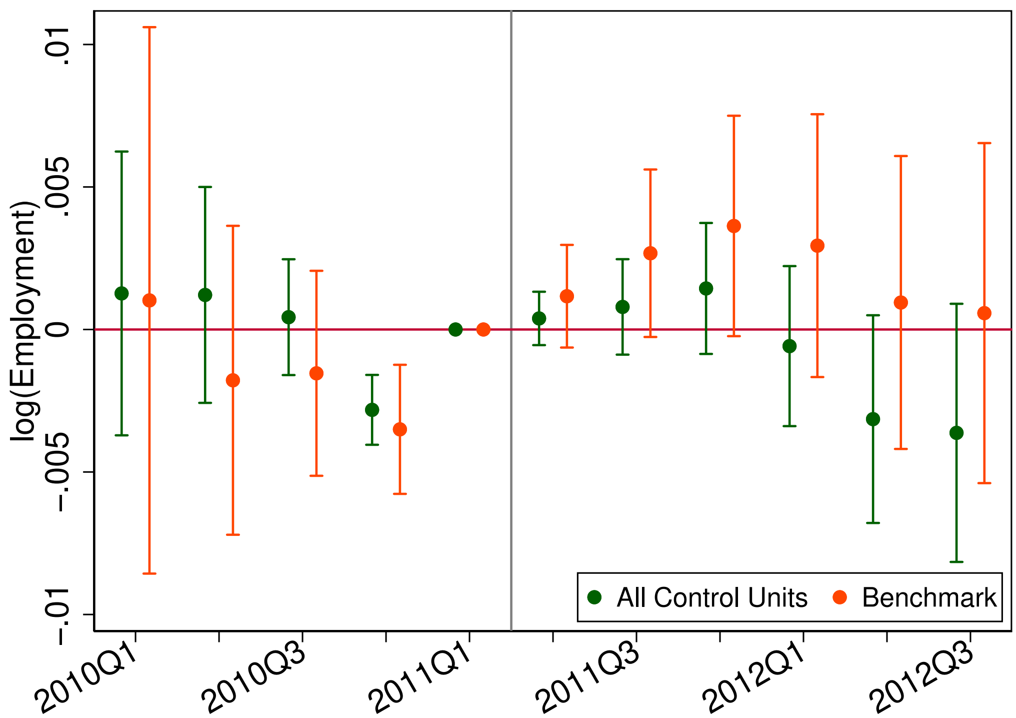
Note: These figures presents the event-study plot constructed using a dynamic TWFE model (equation (8)) for the outcome variable of the log of new hires. For the "all control units" comparison group, the following states are excluded for reasons cited in the text: AR, FL, GA, HI, MA, MI, SC and DC. The "benchmark" states had unemployment rates which peaked near the time of Missouri's and insolvent UI trust funds, and are listed in Table A.2. Bars represent 95 percent confidence intervals clustered at the state level.

Figure A.12: Dynamic Effect of UI Cut on Labor Force



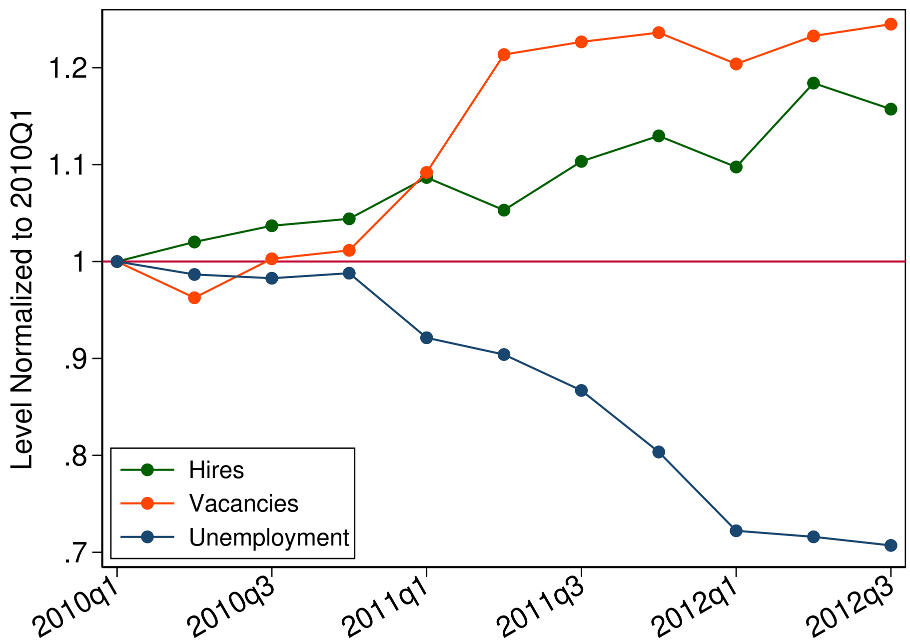
Note: These figures presents the event-study plot constructed using a dynamic TWFE model (equation (8)) for the outcome variable of the log of labor force ($E + U$). For the "all control units" comparison group, the following states are excluded for reasons cited in the text: AR, FL, GA, HI, MA, MI, SC and DC. The "benchmark" states had unemployment rates which peaked near the time of Missouri's and insolvent UI trust funds, and are listed in Table A.2. Bars represent 95 percent confidence intervals clustered at the state level.

Figure A.13: Dynamic Effect of UI Cut on Employment

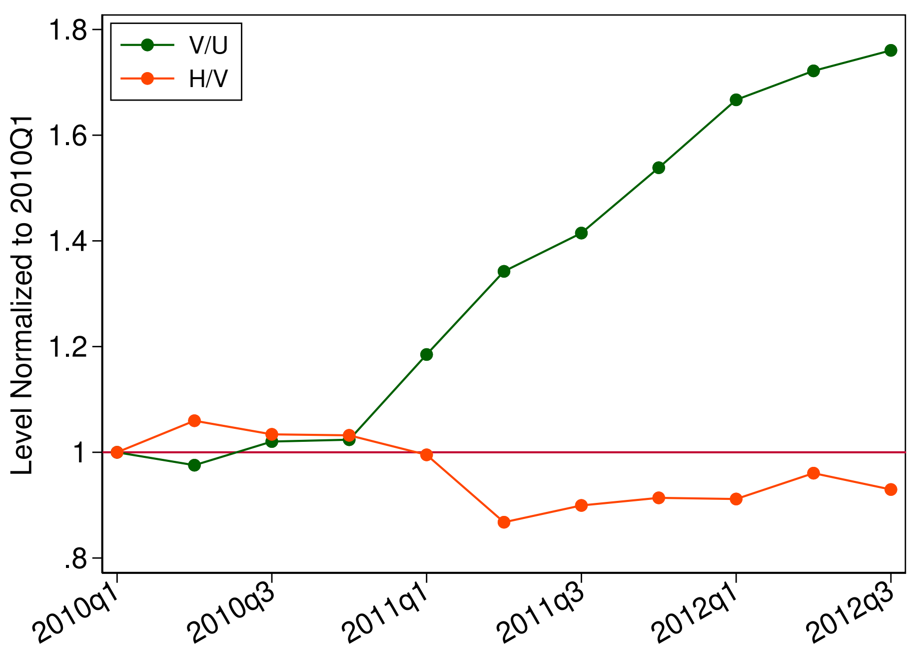


Note: These figures presents the event-study plot constructed using a dynamic TWFE model (equation (8)) for the outcome variable of the log of employment. For the "all control units" comparison group, the following states are excluded for reasons cited in the text: AR, FL, GA, HI, MA, MI, SC and DC. The "benchmark" states had unemployment rates which peaked near the time of Missouri's and insolvent UI trust funds, and are listed in Table A.2. Bars represent 95 percent confidence intervals clustered at the state level.

Figure A.14: Evolution of Key Labor Market Variables in Missouri



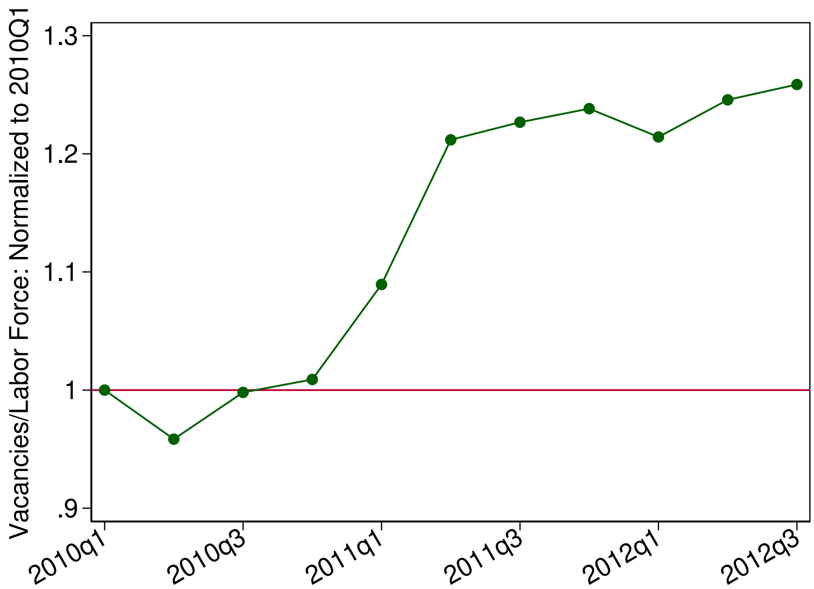
(a) Normalized H, V, U



(b) Normalized V/U, H/V

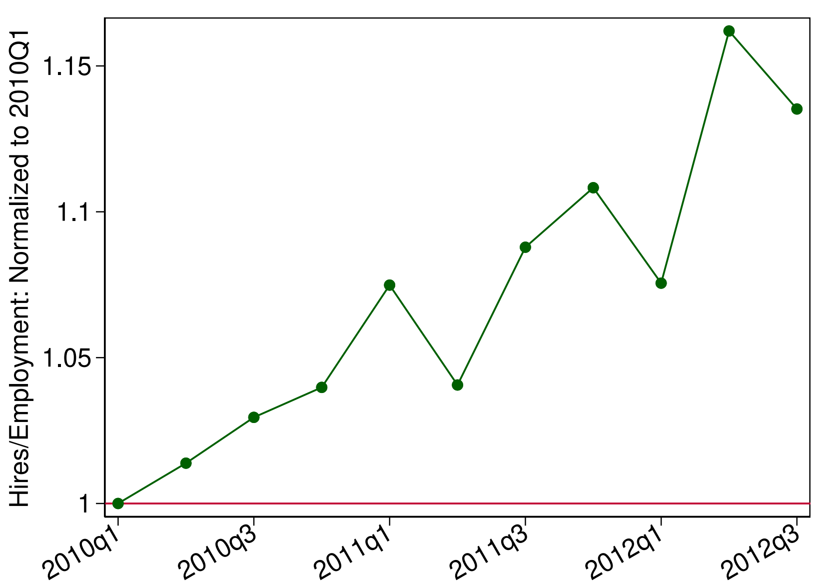
Note: This figure plots the raw time series of Missouri's hires, vacancies, and unemployment in panel (a) and labor market tightness (V/U) and the vacancy-filling rate (H/V) in panel (b). Series are normalized to their level in 2010Q1. Measures are "new vacancies" and "new stable hires" from HWOL and QWI, respectively.

Figure A.15: Evolution of Vacancies-to-Labor Force Ratio in Missouri

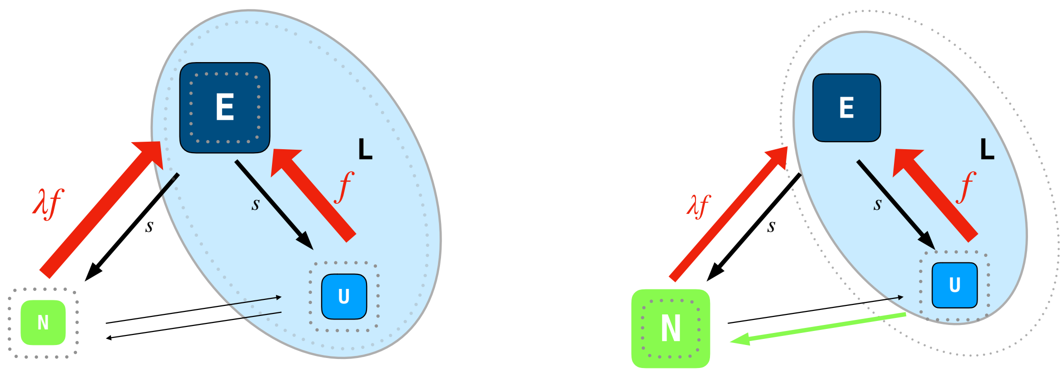


Note: This figure plots the raw time series of Missouri's new vacancies divided by its labor force, normalized to its 2010Q1 level.

Figure A.16: Evolution of Hires-to-Employment Ratio in Missouri



Note: This figure plots the raw time series of Missouri's new stable hires divided by its employment, normalized to its 2010Q1 level.



(a) **UI Benefit Cut: Labor demand effects**
UI Benefit Cut $\Rightarrow$ Vacancy Creation $\uparrow$ $\Rightarrow$
Job Finding $\uparrow$
Net effect: $E \uparrow$, $L \uparrow$

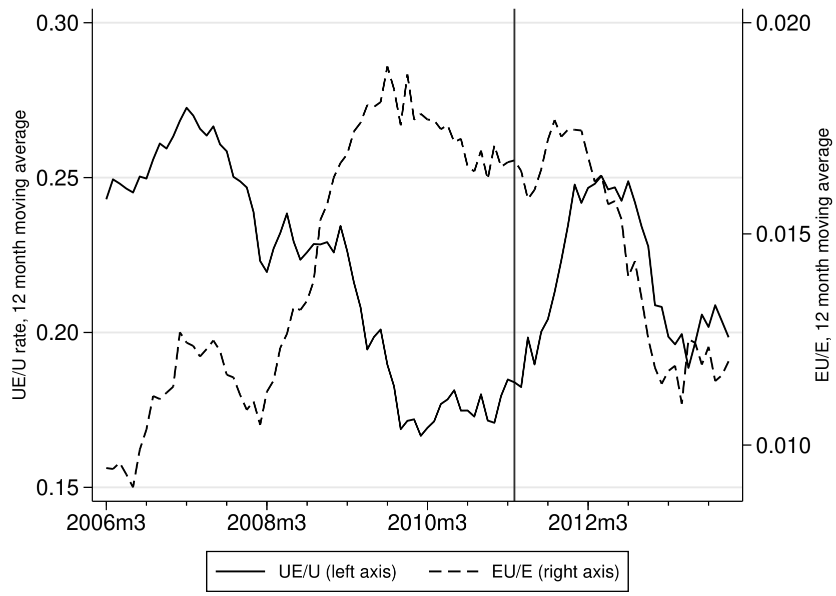
(b) **UI Benefit Cut: Labor supply effects**
UI Benefit cut $\Rightarrow$ Search Effort $\uparrow$,
Dropping out $(U \rightarrow N) \uparrow$
Net effect: $E \uparrow\downarrow$, $L \downarrow$

Figure A.17: Labor supply and demand effects of UI.

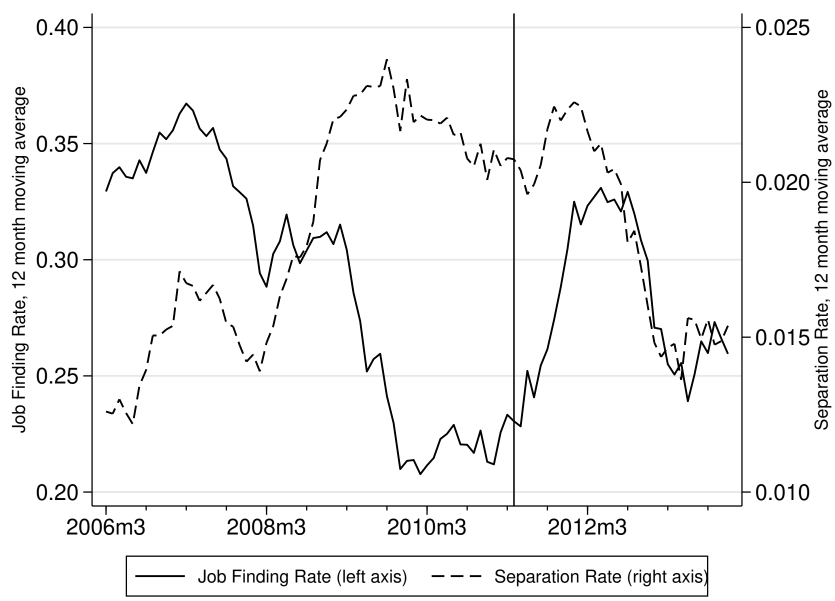
B Labor Market Flows in the CPS

In this section we show the labor market flow rates computed using the panel structure of the CPS. The top panel of Figure B.1 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 B.1: 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.