Job Rationing in Recessions: Evidence from Work-Search Requirements

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November 11, 2014

Job Market Paper

Abstract

The last decade has seen a flurry of changes to unemployment insurance (UI) systems as policymakers have tried to shorten the unemployment rolls and support weak UI budgets. Many states have increased the work-search requirements for UI claimants in an effort to raise the disutility of claiming and match claimants with employers. However, the effects of these changes are initially unclear: canonical search-and-matching models suggest increased search effort will reduce unemployment, while job-rationing models suggest that the number of jobs is limited in weak labor markets. I compile novel data on work-search rules and use changes in them as plausibly exogenous variation in worker search effort. I show that increases in state UI search requirements are associated with increased search effort as measured by a number of proxies. However, these increased efforts translate into only marginally shorter unemployment durations on average. Using a number of different identification strategies, I show that there is heterogeneity in the effects across labor market conditions and that search requirements are relatively less effective in weak labor markets. The results suggest that the effectiveness of job-search policies may be limited by the rationing of jobs in recessions.

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1 Introduction

In recent years, policymakers in many states have dramatically increased the requirements for unemployment insurance (UI) claimants to show that they are actively searching for work. Requiring evidence of work search is an attractive option for policymakers facing UI budgets that have remained weak and unemployment rolls that have remained long in the wake of the Great Recession. There is initially good reason to believe that the changes should be effective: randomized experiments examining the effects of UI job search requirements have shown that they lead to faster reemployment (Johnson and Klepinger 1994; Klepinger, Johnson, and Joesch 2002). However, a growing literature indicates that negative externalities may limit the benefits of reemployment policies in general equilibrium (Davidson and Woodbury 1993; Lise, Seitz, and Smith 2004; Crépon et al. 2013). Further, recent job-rationing models of the labor market suggest that a substantial fraction of unemployment in recessions is driven not by matching frictions but by wage rigidities (Michaillat 2012; Landais, Michaillat, and Saez 2010). Because search requirements are largely aimed at increasing the rate of matches, their efficacy will be limited if other rigidities are the primary drivers of unemployment. In weak labor markets, search requirements may lead UI claimants to simply compete harder for the same number of job openings. It initially remains unclear whether search requirements can improve the functioning of the labor market and lead to faster reemployment when implemented at the state level.

In this paper, I show that while there is some evidence that work search requirements lead to faster reemployment for UI claimants, the equilibrium effects of work search policies depend on the strength of the labor market. I exploit increases in search requirements in a number of states over the last decade using fixed effects models in state and time. I test whether the requirements measurably affect search intensity and whether any additional search efforts pay off in the form of faster reemployment.
Although stronger requirements are associated with measurable increases in a number of search proxies, I find that the increased search requirements have only small impacts on the rate at which UI claimants find employment. Using two search-and-matching models of the labor market, I show that the efficacy of search requirements is likely to depend on ability of the market to create additional jobs. If jobs are limited (“rationed”) in downturns, then search requirements may be ineffective at alleviating unemployment in recessions. Evidence on the effects of search requirements across labor market conditions shows that they do seem to be less effective when the labor market is weak.

Although job search requirements have returned to near-ubiquity in UI systems, they are only minimally documented and studied in the existing literature. The prototypical job search requirement in the US requires claimants to contact a minimum number of prospective employers each week. The details of the number of contacts required each week vary across states and over time. I catalog the changes in state-level search requirements since the turn of the century using UI handbooks and forms provided to claimants over time. On the whole, the documented policy changes suggest a strong reversal of the slackening in search rules during the 1980s and 1990s, with a number of states implementing more stringent requirements over the last decade.

I compare these policy codings to the actual number of contacts reported by claimants when audited by their states’ Benefit Accuracy Measurement (BAM) programs. First, this analysis shows that claimants respond directly to rising search requirements by reporting more contacts to UI authorities. Second, it shows that many of these contacts are genuine: the number of contacts found by auditors to be acceptable also rises. This latter finding suggests that the additional contacts are reflective of actual search and are not bogus contacts invented by claimants.

I use within-state changes in search requirements to identify the effects of the policies. Using fixed effects models in state and time, I control for time-invariant state characteristics and national trends over time. As it is possible that the policy changes are correlated with time-varying state and claimant characteristics, I control for a number of variables that are likely to be drivers of individual search effort and labor market
outcomes.

The results show that likely UI claimants increase some measures of observed search effort when search requirements go up. I show this using measures of search methods as reported in the Current Population Survey (CPS) as in Shimer (2004). Despite this increase in measured effort, there is only slight and statistically undetectable evidence of more rapid reemployment. Reemployment hazard models using monthly CPS data reveal positive but small and statistically insignificant impacts of search requirements on the rate of reemployment. Published statistics on weeks claimed during claimants’ benefit years suggest mixed and statistically insignificant impacts.

I interpret the effects of an increase in search requirements in the context of two search-and-matching models. I show that under standard assumptions, search requirements are likely to bind when the labor market is weak. If the market faces no rigidities beyond matching frictions, search requirements will lead workers to find faster reemployment, while firms also open additional vacancies to take advantage of the additional search effort. General equilibrium effects should compound the effectiveness of search requirements. However, if wages are rigid as in recent job-rationing models, a limit on the number of available jobs in downturns may stunt the effectiveness of search requirements. I test for these business cycle differentials and find that search requirements are relatively less effective in weak labor markets when using both lagged unemployment and an industry shift-share measure to proxy for labor market strength.

This paper builds on the experimental results of the search policy literature exemplified by Klepinger, Johnson, and Joesch (2002) and Ashenfelter, Ashmore, and Deschênes (2005) by examining effects in general equilibrium. It also extends search policy analysis in other contexts as in Borland and Tseng (2007), McVicar (2008), and McVicar (2010) to the United States environment by examining a variety of different policies in the US unemployment system. A number of these studies (Ashenfelter, Ashmore, and Deschênes 2005; McVicar 2008, 2010) vary the incidence of job search monitoring, which may have different effects from changing job search requirements under a stable monitoring regime.
More generally, this paper directly examines the general equilibrium effects of labor market policies in the spirit of Davidson and Woodbury (1993) and Lise, Seitz, and Smith (2004). Those papers, however, primarily consider search externalities exerted by a treated group on untreated groups, while this paper largely focuses on the mechanisms for crowd-out even if everyone is treated.\footnote{To my knowledge, it is also the first paper to interpret unemployment policy changes in the context of a job-rationing model as in Michaillat (2012). While Landais, Michaillat, and Saez (2010) suggest that some labor market policies may be less effective during downturns, this paper directly tests the implication.} To my knowledge, it is also the first paper to interpret unemployment policy changes in the context of a job-rationing model as in Michaillat (2012). While Landais, Michaillat, and Saez (2010) suggest that some labor market policies may be less effective during downturns, this paper directly tests the implication.

The paper proceeds as follows. Section 2 describes the relevant institutional features of UI and details the way in which the policies studied in this paper are identified. Section 3 discusses the general empirical strategy employed in much of the paper. Section 4 presents the estimated effects of search policies on observable effort and reemployment. Section 5 examines the predicted effects of a search requirement in a standard search-and-matching model. Section 6 presents evidence on the efficacy of search requirements across labor market conditions. Section 7 discusses and concludes.

\section{Institutional Setting}

Search requirements are an important feature of UI systems even though they receive limited attention in the existing literature. Many states have implemented dramatic changes in these requirements over the past decade. I document the recent changes in search requirements using the documentation available to UI claimants at the time of their claims. I compare my coding of the policies to data on the number of job contacts actually reported by claimants who are audited and find a strong relationship between the published rules and the reported contacts.\footnote{The effects of these policies on nonclaimants or search-exempt workers is an important topic for future work.}
2.1 Search Requirement History

Unemployment insurance, while governed by a set of federal guidelines under the Federal Unemployment Tax Act (FUTA), differs across states. In general, it provides compensation to full-time, permanent workers who lose their employment through no fault of their own. To be initially eligible, workers must meet thresholds for quarters of employment and earnings in a set period before a job separation. Benefit levels are determined by earnings over the covered period. In general, benefits can be paid for approximately 26 weeks, but the benefit duration is increased through both automatic and ad hoc extensions during periods of high unemployment via the Extended Benefits (EB) and Extended Unemployment Compensation (EUC) programs.

Unemployment insurance is affected by a well-known moral hazard problem: higher unemployment benefits raise the relative value of remaining unemployed, lowering the incentive to search for and accept new employment. Conversely, UI allows workers to smooth consumption over employment shocks (Gruber 1997). While a second-best result can be reached by setting benefits to balance moral hazard against consumption-smoothing benefits (Chetty 2006, 2008), policymakers have historically attempted to mitigate the problem by requiring demonstrable search effort and job offer acceptance from claimants.

Search requirements of one form or another have been an important part of UI systems since at least the 1980s. While the original UI system established in the Social Security Act of 1935 did not specifically require search from claimants, later additions to federal law called for claimants to demonstrate active search. In particular, federal law first demanded these requirements for claimants receiving benefits under the EB program (Anderson 2001). Over the last few decades of the twentieth century, states variously implemented and eliminated search requirements for claimants of regular UI benefits (Klepinger, Johnson, and Joesch 2002). The 2000s and 2010s have been characterized by a steady increase in the strength of search requirements across many states. While most states already had wording in their statutes requiring that claimants be
“able, available, and actively searching” for work, this language was added to the United States Code as a condition for state UI funding as part of the Middle Class Tax Relief and Job Creation Act of 2012.

The prototypical search requirement calls for claimants to contact some minimum number of potential employers each week. In some cases, these contacts are required to be in person, while some more general requirements simply disallow phone calls. The most general requirements simply ask claimants to contact employers in the way that is customary for their professions. In general, the same employer cannot be contacted again for a minimum number of weeks or unless there is reason to believe that another position has become available. Currently, virtually all states require claimants to at least track their contacts in a diary or work search log. Blank search logs are often provided to claimants with the rest of their UI documentation. However, there is considerable variation in how often these logs are checked by state workforce agencies. A steeper and increasingly common requirement is that claimants must report the details of their employer contacts at the time of making their weekly or biweekly claims. Claimants who are found to have not fulfilled their work search requirements will be deemed ineligible for the week and, potentially, disqualified from receiving future benefits.

While search requirements have become common across states, they do not apply to all workers. In particular, most states exempt claimants who find work through a union hiring hall. They are not required to make regular contacts with other employers, though a weekly minimum may be set on the number of times a claimant must contact the hiring hall. Workers who are on layoff and awaiting recall can also be exempt, though they must often have a definite recall date within a set number of weeks. Claimants who are participating in agency-approved training programs may also be exempt.
2.2 Existing Studies

Characteristics of UI systems are studied extensively in the existing literature, but search requirements themselves receive somewhat less attention. Borland and Tseng (2007) is the only other study of which I am aware that examines broad changes in work-search requirements outside the specific context of an experiment. The authors examine a job-search diary program in Australia shortly after implementation in the late 1990s. Due to a labor dispute involving unemployment caseworkers, some benefit offices did not enforce the requirement that claimants keep a job-search diary satisfying a particular number of employer contacts.\(^2\) Those who kept the job-seeker diaries experienced shorter unemployment durations. However, part of the identification is generated from variation in the level of implementation between geographic regions and the study does not specifically try to estimate the implied effect of the policy under universal application. Further, workers with relatively poorer labor market options, as inferred from their recent unemployment histories, did not exhibit faster reemployment under the job-search diary regime. This finding in the Australian context is consistent with the idea that work search requirements are not effective in generating reemployment when workers are already constrained in their job market opportunities.

The two studies most relevant to the examination of search requirements in the US are Johnson and Klepinger (1994) and Klepinger, Johnson, and Joesch (2002). Johnson and Klepinger (1994) describes the Washington Alternative Work Search Experiment, in which the 9,634 eligible UI claimants who applied for benefits in Tacoma between July 1986 and July 1987 were randomly assigned to different work search treatments. The first group was exempted from search requirements and was not even required to file biweekly continuing claims forms. This resulted in a 3.34 week increase in UI durations over the reference group’s 14.48 average weeks. A second treatment group was assigned individualized work search requirements based on their circumstances. A third treatment group participated in an intensive job-search training workshop early

\(^2\)In general, this requirement was for eight contacts every two weeks.
in their employment spells. The individualized requirement group saw no change in benefits drawn while the workshop group drew, on average, half a week fewer benefits. The dramatic increase in UI durations for the first treatment group suggests that, in this context, work search requirements decrease UI claim durations. However, because the sweeping treatment effectively removed all costs of continuing to claim UI, the results likely overstate the effects of the search requirement alone.

The Maryland Unemployment Insurance Work Demonstration, as detailed in Klepinger, Johnson, and Joesch (2002), provides a sharper test of the direct experimental effects of work search requirements. During 1994, all new claimants at six randomly-selected Maryland UI offices were enrolled in the study. The experiment included treatment groups that faced increased search requirements, decreased search requirements, required participation in a job search workshop, and monitored work search. Both informed and uninformed control groups were included to test for Hawthorne effects. The simple changes in work search requirements are of greatest interest for the purposes of this paper. The decreased search treatment required no contacts as compared to Maryland’s standard of two, while the increased treatment required four weekly contacts. The zero-contact group saw an increase of 0.36 weeks claimed over the control mean of 11.94, and the four-contact group saw weeks claimed fall by 0.72. While the results are less striking than those seen with the sweeping treatment in Washington, they suggest a distinct slope in required contacts. In the case of both experiments, though the effects are well-estimated in partial equilibrium, there is little scope for considering the general equilibrium effects of search policies. Even if the entire labor market were randomized into the experiments, which arguably happened in Tacoma or at each site in Maryland, only some of the claimants saw their work requirements change, and they were always counteracted by a treatment group changing in the opposite direction.

As these studies do not address general equilibrium issues, it may be important to consider them as in Davidson and Woodbury (1993), Lise, Seitz, and Smith (2004), and Crépon et al. (2013). Both Davidson and Woodbury (1993) and Lise, Seitz, and Smith (2004) consider the general effects of some manner of reemployment bonus. In both
cases, a model that allows for general equilibrium effects is calibrated using the partial equilibrium results of an experiment. Davidson and Woodbury (1993) then discuss the implied equilibrium effects of the policy, while Lise, Seitz, and Smith (2004) compare the model’s predictions to an out-of-sample group before using the results to identify feedback in the policy. In both cases, partial equilibrium experimental results are at least partially reversed when implemented in general. Crépon et al. (2013) endeavors to explicitly measure spillover effects of a French job search assistance program through a two-level randomization process. Municipalities were first randomized into groups that would vary the share of unemployed that would be treated with the program and then the appropriate percentage of individuals within each municipality were randomly treated. They find spillover effects in markets where the treated individuals compete for jobs mostly with other similar individuals and when labor market conditions are poor. Overall, this literature indicates that there is scope for analysis of policies in which general effects should be considered.

Another strand of the literature examines the effects of differential monitoring under constant search requirements. In two papers, McVicar (2008, 2010) uses the refurbishment of unemployment benefit offices in Northern Ireland as a source of variation in the monitoring of claimant search requirements. On the whole, his findings show that eliminating monitoring reduces unemployment exit hazard and job entry hazard and increases the stock of claimants. If we interpret these findings as being equivalent to moving between a no-search requirement (when claimants are not monitored) and a standard search requirement (under standard monitoring), we would expect changes in search requirements to induce changes in unemployment exit and job entry in other contexts. However, experimental evidence from four US states in Ashenfelter, Ashmore, and Deschênes (2005) suggests that UI claimants complied with work-search requirement even under very limited monitoring regimes. Performing additional verification of reported work-search did not induce meaningful changes in the duration of unemployment claims.
2.3 Policy Identification

Although state-specific search requirements are quite common, documentation of the policies is not readily available at most points in time. The DOL reported state search requirements in its annual *Comparison of State Unemployment Insurance Laws* through 1999, but it was determined that there was not enough cross-state variation at the time to justify continuing to include search requirements. Around the same time, Anderson (2001) performed a cross-sectional review of the standing search requirements, notably finding that some workforce agencies’ publications suggested different rules from those listed in the DOL report. Until search requirements were added back into the *Comparison of State Unemployment Insurance Laws* in 2012, information on policies is largely limited to O’Leary (2004), who provided a cross-section of rules as reported in a survey of state workforce agencies. A handful of other papers mention the search requirements for individual states at various points in time over this period.

I overcome the lack of existing information on search policies by constructing new data from state workforce agency publications. All states make some documentation available to claimants, whether through standalone UI claimant handbooks or “frequently asked questions” brochures and web pages. Many of these instruct claimants on the exact search requirements they are expected to fulfill. Agency-provided work search logs, which have become more common in recent years, also often detail the rules that claimants are supposed to follow. Although most of these are not directly available online, many are archived through Internet crawler caches and others are available through university and state government libraries. Through these sources, I have collected all relevant and available documents published by workforce agencies between 2001 and 2014. I examine these documents for information on work search requirements and track within state changes via document publication dates.

The rules implied by the agency publications generally correspond with the other available sources, but there are some discrepancies with DOL’s *Comparison of State Unemployment Insurance Laws* and O’Leary (2004). For the sake of consistency, I defer
to the agency publications throughout. I also argue that the rules as they are described to claimants in this information are the most relevant for measuring the policies. However, I cannot rule out that workforce agency staff provide different information in person. In Sections 2.4 and 4.1, I compare the rules recorded from agency publications to actual numbers of employer contacts reported by claimants in BAM data.

### 2.4 Changes in Policies

I group search policies by the number required weekly employer contacts. During the 2000s, various states have specifically required claimants to make between zero and five contacts. States that do not make these specific requirements fall into two groups. First, many states do not indicate an exact number of employer contacts. In general, these are the states listed as “no specific number” in *Comparison of State Unemployment Insurance Laws* and recorded in O’Leary (2004) as states in which claimants are instructed to follow the customs of hiring in their profession. Throughout this paper, I refer to these policies as “nonspecific” search requirements. A second group of six states provides search requirements that are in some way individualized for claimants. My research suggests that Arkansas, Colorado, Idaho, Missouri, Texas, and West Virginia have all had such “directed” search policies at some point in the 2000s.\(^3\) I exclude these states from analysis both because the determinants of the individual requirements are not always clear and because they generate cross-Sectional within-state heterogeneity that makes analysis at the state level difficult.

Figure 1 displays the number of states with each policy from zero to five required employer contacts over the ten years from 2004 through 2013.\(^4\) While there are a few state movements over the first three quarters of the displayed period, most of the increases in requirements take place in 2011 or later. The increases are also concentrated

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\(^3\)Much of the documentation for Ohio appears to indicate that it should also fall into this group because workers’ requirements have been individually communicated to claimants at the time of claiming. However, various unemployment information websites, newspaper articles, and the BAM data suggest that there is no cross-sectional variation in Ohio’s requirements for those who are instructed to search. It appears that the individualized information is just whether claimants are subject to work search.

\(^4\)Analogous graphs of share of claimants under each policy and total claimants under each policy appear in the appendix.
at the high end of the requirement distribution. While six states required at least three weekly employer contacts in 2004, 16 required three or more contacts by the end of 2013. There are 14 instances of increasing search requirements in the sample examined in this paper, 12 of which took place under the administrations of Republican governors. Anecdotally, search requirements have been raised to get the unemployed back to work through increasing labor force attachment and more or less explicitly raise the burden of drawing UI benefits.

Figure 2 displays the number of employer contacts reported to the BAM program by audited UI claimants. The BAM program is designed to identify the sources of overpayments in the UI system. It accomplishes this task by randomly selecting UI claimants and thoroughly checking their monetary and nonmonetary eligibility for a given week. As part of the audit, claimants are asked to record their requisite employer contacts for the week, even if they are not generally required to explicitly report contacts. While the question of whether the recorded contacts satisfy the actual requirements is of interest, my first goal with this data is to see how many contacts claimants think they should be reporting. The seven different search policies examined in this paper are listed along the horizontal axis of Figure 2, with the number of state-months for each listed below. The bars above each policy number show the discrete empirical PDF of contacts reported by the 159,613 claimants who were audited by BAM between 2001 and 2013 and were not union- or layoff-attached. In general, the patterns suggest a strong relationship between the policies I have recorded and the BAM reports. For each of the specific policies, the required number makes up a plurality of the reports. It is easier to explain apparent overreports than underreports because claimants have an incentive to list extra contacts if they are concerned that some of their contacts will be deemed ineligible. Thus, the most striking deviations are for states three-contact policies, which have over 40 percent of claimants reporting zero or one contacts.5

5Further examination of this pattern indicates that almost half of these observations are from Massachusetts. Bay Staters almost exclusively reported zero contacts over the vast majority of the sample period even though Massachusetts’ UI claimant handbooks very clearly require three contacts throughout. As several other sources also suggest a three-contact coding in Massachusetts, it appears that there is some idiosyncrasy with the state’s BAM codings. Efforts to learn more about this from Massachusetts workforce
the whole, however, the BAM data are supportive of the policies as they are coded.

The relationship between BAM contact reports and state changes is displayed in Figure 3. For each of the 12 states with a policy change during the period available in the BAM data, the graphs indicate the policy (as indicated by the thick, dashed line) and the monthly average of reported contacts (the thin, solid line). Analogous graphs for other states are displayed in the appendix. No dashed line is displayed for periods during which a nonspecific policy prevailed. A number of the changes appear only at the very end of the available BAM data, with Mississippi, Tennessee, and Rhode Island having only brief available data after the implementation of search policies. Florida, Hawai‘i, and South Carolina’s graphs all suggest reasonably close adherence to their new policies after these states switched away from nonspecific rules. Likewise, North Dakota, Ohio, Pennsylvania, and Utah all generally follow changes in their explicit policies. While the data do seem to reflect the more recent change in Louisiana’s requirements, they follow the earlier increased period somewhat less closely. The single policy change in Maine does not initially seem to show up in the data, but this is because deviations away from three contacts were fairly balanced between increases and decreases, having little effect on the average.\(^6\)

### 3 Primary Empirical Strategy

The primary analysis in this paper involves the estimation of regressions on aggregate and individual outcomes with state fixed effects and time fixed effects. In particular, I consider individual-level models of the form

$$y_{ist} = \alpha_0 + \alpha_1 D_{st} + \alpha_2 x_{ist} + \alpha_3 X_{st} + \gamma_s + \delta_t + \epsilon_{ist} \quad (1)$$

where \(y_{ist}\) is an outcome for individual \(i\) in state \(s\) and month \(t\), \(D_{st}\) is a vector measuring the prevailing search policy. Controls for individual characteristics are included in the

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\(^6\)The distribution of reported contacts over time is reported for Maine and all other states in the appendix.
vector $x_{ist}$ and aggregate state-level controls are included in the vector $X_{st}$. State and month fixed effects are given by $\gamma_s$ and $\delta_t$, respectively, and $\epsilon_{ist}$ is an idiosyncratic error term.

The coefficient vectors of interest are given by $\alpha_1$. These coefficients estimate the difference in outcomes between state-months with differing search requirements. The estimates are generated off of within-state variation in policies over time and between-state variation within months. In general, I parameterize the policies in two different ways. First, I estimate the effect of moving through the zero- to five-contact policies with a continuous linear measure of the number of required contacts. While this specification is simple in that it provides a single estimate of the effects of an additional contact, it constrains that effect to be constant as the number of contacts increases. In these regressions, I include nonspecific policies as a separate indicator and assign them a zero in the continuous measure. The coefficient estimate on the indicator indicates the estimated difference between zero-contact and nonspecific policies. Second, I estimate the effects of each policy individually by including a full set of indicators, one for each policy with zero-contacts omitted. This specification is clearly more flexible, but suffers from relatively few observations for the zero and four-or-more contact policies.

No matter the exact policy coding, consistent estimation relies on the assumption that there is no correlation between search policies and outcomes for the sampled respondents, conditional on the fixed effects and covariates. A primary source of concern is the possibility of policy endogeneity: states with deteriorating labor markets may implement policy increases in response. To test for this, I regress monthly state unemployment rates on indicators for months until a search requirement increase and state and time fixed effects. The indicators show no pretreatment rise in unemployment immediately before policy changes. Estimated leads a year in advance of policy changes.

\footnote{Florida is the only state to implement a five-contact requirement, so I group it with the four-contact policies for the purposes of these indicators.}

\footnote{I choose the zero-contact policies as the omitted group for these specifications because it seems most straightforward to compare more stringent policies to a simple zero-contact requirement. Using other omitted groups would generally reduce the statistical significance implied by the stars in the paper's tables. In the absence of another natural omitted group, I forgo providing tests of all pairwise comparisons and generally rely on the linear specification as a summary of the average differences.}
do show significantly higher unemployment rates, but these are followed by a downward trend to the policy change.

4 Evidence on Policy Effects

In this section, I examine the estimated effects of search requirements on a number of different outcomes using models broadly of the form described in Section 3. I first examine measures of search effort using data from the BAM program and the CPS. I then test for effects of search requirements on reemployment hazard as measured through Cox proportional hazard models using CPS data. Finally, I examine the estimated effects on average UI claim durations from administrative claims data.

4.1 Evidence on Search Requirements Affecting Search Effort

Table 1 displays the estimates from regressions of the form of equation (1). The first two columns regress measures of search policy on the number of contacts reported in the BAM data, quantifying the effects shown in Figure 3. Columns (3) through (6) use different measures of genuine, acceptable employer contacts as the dependent variables. Columns (7) through (10) extend the analysis to more general measures of search methods and effort in the CPS.

4.1.1 Employer Contacts Reported to BAM

The number of employer contacts reported is regressed initially on a linear measure of required contacts with an indicator for nonspecific policies. Claimants living under nonspecific policies are assigned a zero in the linear term. Thus, the estimate in the first row indicates the number of additional reported contacts associated with a one-contact increase in the requirement. The estimate in the last row indicates the conditional expectation of the difference between reports under a nonspecific policy and a zero-search policy.

A single additional required contact is estimated to increase reported contacts by 0.564 and claimants under nonspecific policies are estimated to report 0.848 more contacts than claimants under zero-search policies. The estimates reflect a number of
patterns apparent in Figures 2 and 3. First, nonspecific policies are associated with some variation in number of reported contacts, but the average level is greater than that of the lowest specified search policies. Second, the estimated effect of increasing a search policy by one is significantly less than one, though much greater than zero. This likely reflects both measurement differences between the BAM data and the policies and less-than-full adjustment to policies by claimants. Note that this latter effect does not imply anything about noncompliance. Because the share of claimants exceeding the requirement decreases as the policy increases, the estimated effect of the policy should be attenuated below one.

The second column of Table 1 makes some of the source of this difference clear. When estimated with individual indicators, the number of reported contacts generally increases in search requirement, though nonmonotonically. Here, the relatively low levels of reported contacts associated with the three-contact policy are apparent.

The estimates in columns (1) and (2) indicate that higher requirements are generally associated with more reported contacts. However, it is initially unclear whether these reports reflect actual contact with employers or fake contacts by claimants trying to appear as though they have satisfied the requirement. To separate these two possibilities, I use data on the results of BAM audits themselves. As part of determining claimant eligibility, BAM auditors attempt to verify that the employer contacts were made and were legitimate under state rules. Ultimately, employer contacts are identified as acceptable, unacceptable, or not verified. A large portion of the contacts (42%) fall into this last category, indicating that there was not enough information to rule the contact as acceptable or unacceptable. Of those that can be verified, approximately 88% of the contacts in the sample are found to be acceptable.

Columns (3) and (4) of Table 1 repeat the same specifications as the first two columns but use the number of contacts that are not found to be unacceptable as the dependent variable. This measure is an upper bound on the number of contacts

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9See discussion of discrepancies in Section 2.4.
10For the purposes of determining eligibility for UI benefits, the audits generally treat these contacts as acceptable. Payments are not ruled as improper because of unverified contacts.
that would be found to be acceptable if all contacts could be verified one way or the other. Columns (5) and (6) use a lower bound: the dependent variable is the number of reported contacts that are found to be acceptable. In either case, the coefficient on the linear measure remains positive and significant. While only half of contacts are unverifiable, the linear estimate for the verified, acceptable contacts in column (5) is attenuated rather more than half as compared to column (1). This suggests that, on average, the additional contacts provided under higher search requirements are found to be unacceptable at a higher rate, but the total measured search effort is increasing regardless.

4.1.2 CPS Search Effort Proxies

A limitation of the analysis based on reported contacts is that, even if all contacts are genuine, the differences across policies may simply represent differences in reporting alone. That is, the above results could be generated by all claimants actually contacting well more than the requirement and only reporting the required number. Therefore, I turn to the CPS for additional measures of search effort that are not driven by directly by reporting requirements.

Columns (7) and (8) of Table 1 demonstrate that search requirements broadly increase the number of job-search methods used by basic monthly CPS respondents between 2003 and 2013. Unemployed CPS respondents are asked to indicate what job-search methods they have used in the past four weeks, with twelve possible methods. These regressions estimate the policy effects on the total number of indicated methods among those respondents who indicate they were laid off from or otherwise lost their most recent job. This is intended to capture the population of the likely-UI-eligible unemployed. It is important to note that the population differs from that sampled by the BAM data, which is UI claimants. Measuring the usage of the 12 search methods described in the CPS provides a reasonable proxy for search effort. Because job-seekers are likely to diversify the ways in which they look for jobs as they put more effort into search, these measures provide readily-available proxies for effort. Table 2 reports
the prevalence of each job search method in the sample. While contacting employers and sending out resumes are each used by approximately 40 percent of respondents, no individual method is used by more than half of the sample, and there is considerable variation in the prevalence of each method.

On a sample average of approximately 1.8 methods, the linear estimate suggests that an additional required contact increases the number of search methods used by approximately 0.04. The individual policy indicators show that this effect is largely concentrated in a jump between a zero-search policy and a one-contact policy. The estimated effects increase monotonically up to four required contacts, though the estimates are not statistically different from each other.

Columns (9) and (10) of Table 1 display the effects on whether respondents made use of one particular search method: directly contacting or interviewing with employers. This method was chosen because it appears to correspond most directly to the definitions of employer contacts in search requirements. The linear estimate is again positive, indicating that an additional contact requirement raises the probability of contacting employers directly by 1.3 percentage points. The specification with indicators shows that the effect is largely driven by difference between zero-search policies and other policies. All of the estimates, including the one for nonspecific policies, put the effect between 0.06 and 0.12. In general, this result should not be surprising. If respondents in states with one-contact policies are following the requirements and making at least one employer contact, there should not be much effect on this probability from adding additional contacts.

4.2 Evidence on Search Requirements Affecting Reemployment

I next show that search requirements have no impact on the duration of unemployment spells, which are outcomes of more fundamental interest to economists and policymakers. I first show that search requirements have small effects on reemployment hazard, which is one of the most important outcomes for the overall welfare of UI claimants. I estimate Cox proportional hazard models with the same general form of controls as the
linear models of the previous Section. I estimate the models on a subset of monthly CPS respondents who can be linked across months. For the unemployed each month, failure is defined as reemployment the following month. I allow the hazard to vary proportionally in month, year, state, education, occupation, industry, race, ethnicity, and sex. Estimates from these models are reported in Table 3. The estimates are all greater than one, indicating faster reemployment, and are largely increasing across the increasing search requirements, but they are never statistically significant. The linear specification suggests that the hazard of leaving unemployment each month is about one percent higher for each additional required contact. While the estimates for some of the individual policies are larger, it is difficult to distinguish them from no effect.

4.3 Evidence on Search Requirements Affecting UI Claim Durations

I next turn to measures of the duration of UI claims as reported in administrative data. The data are generated from DOL quarterly reports of the total weeks paid over the previous year divided by the number of first payments over that time period. Though this definition makes the outcome a moving average, the measure is advantageous because it is more directly comparable to the estimated effects of search requirements from the experimental literature. Given the aggregate nature of the outcome, I simply regress these average durations on the policies and fixed effects in state and time. I lag the measured policy changes by two quarters in an effort capture the point at which approximately half of the new claimants and weeks drawn for each observation are under the prior policy and the new policy.

The estimated effect from the linear specification is the first estimate reported in Table 4. It suggests that an increase in one required contact lowers average number of weeks drawn by one eighth of a week. The estimates from the indicators in the second column exhibit large standard errors, though all the point estimates are negative. The point estimates themselves are, in some cases, of quite meaningful magnitude. The large standard errors, emphasized by the rejection of joint F-tests, highlight the limitations of this data for this estimation.
In principle, the number of weeks claimed can be estimated from the BAM data as well. Unfortunately, because BAM only samples ongoing spells and not completed spells, the average length of completed spells must be inferred from the distribution of randomly-sampled ongoing spells. This can be solved through a nonlinear least squares problem, but despite the many observations in BAM, the resulting estimates are also extremely imprecise. In ongoing work, I am attempting to make this procedure more efficient and seeking microdata better suited to estimating claim durations. Ultimately, it is difficult to say much with certainty about the exact effects on unemployment claim durations. Regardless, the amount of time people actually spend unemployed, as shown in the CPS proportional hazard models, is estimated relatively precisely and shown to be affected very little on average.

5 A Model of Search Requirements

While there are some basic empirical results apparent from testing the effects of search requirements on search effort and unemployment duration, I turn to a general search-and-matching model to interpret how we might expect search requirements to operate in equilibrium more generally. The model includes a measure of technology, which in general could be used to drive business cycles. However, for the purposes of this exposition, I assume technology is fixed and I examine comparative statics across different equilibria. Thus, from the perspective of the model’s agents, there is no uncertainty over technology, wages, firm size, or aggregate measures of the labor market.

I begin by presenting the general features of the model. I then find the equilibrium conditions under two alternative wage-setting mechanisms. The first allows wages to be flexibly bargained. The second imposes wage rigidity that prevents the market from clearing under some circumstances. In particular, it allows the market to ration jobs when productivity is low and to exhibit unemployment beyond that caused by search frictions (Michaillat 2012).
5.1 Environment

The model takes place in discrete time. At the beginning of each period, unmatched workers search for jobs and hires are made. Production then takes place and wages are paid. Matches are then exogenously destroyed and the period ends. Firms and workers both have discount factor $\beta$. At the end of each period, a share $\lambda$ of existing employment matches are exogenously destroyed. Matches occur between the measure of unemployed workers, $u$, who exert average search effort $s$, and posted vacancies, $v$, according to a constant returns to scale (CRS) matching function

$$m = m(su, v)$$

(2)

which is increasing in both of its arguments. Given the CRS assumption, matches per vacancy can be expressed as a function of average search effort, $s$, and labor market tightness, $\theta = \frac{v}{u}$:

$$\frac{m}{v} = m(s \frac{u}{v}, 1) = q(s, \theta).$$

(3)

An individual worker exerting $s_i$ efficiency units of search is providing $\frac{s_i}{su}$ of total search effort and receives that fraction of the matches:

$$\frac{s_i}{su}m = \frac{s_i}{s}m(s, \frac{v}{u}) = \frac{s_i}{s}f(s, \theta).$$

(4)

As in Pissarides (2000), I consider only symmetric Nash equilibria in which all workers exert average search effort ($s_i = s$). Thus, the transition probability for the representative worker is given by $f(s, \theta)$. Workers take the variables $s$ and $\theta$ as given, so the derivative with respect to an individual’s search effort is $\frac{1}{s}f(s, \theta)$. The vacancy and worker transition probabilities are related through $f(s, \theta) = \theta q(s, \theta)$. In equilibrium, flows into and out of unemployment are balanced, defining the standard Beveridge curve:

$$u = \frac{\lambda}{\lambda + \theta q(s, \theta)}.$$ 

(5)
5.1.1 Workers

The value of being employed at the time of production is given by

\[ W = w(N, a) + \beta[(1 - \lambda)W' + \lambda S'], \]  

(6)

where \( w(N, a) \) is the wage, which is potentially a function of technology and the number of workers employed the firm, and \( S \) is the value of being an unemployed job-seeker at the beginning of a period. The value of being unemployed during the time of production is given by

\[ U_t = b + \beta S', \]  

(7)

where \( b \) is the flow payment received by the unemployed. The value of searching for a job at the beginning of a period is given by

\[ S = -\psi(s_i) + \frac{\psi}{s} f(s, \theta)W + (1 - \frac{\psi}{s} f(s, \theta))U, \]  

(8)

where \( \psi \) is the disutility of search effort, which is assumed to be increasing and convex in its argument. If workers are free to choose search intensity, then their choice will satisfy the first order condition given by

\[ \psi'(s) = \frac{f(s, \theta)}{s}[W - U]. \]  

(9)

Combining equations (6) and (7), we have

\[ W - U = w(N, a) - b + \beta[(1 - \lambda)(W' - S')]. \]  

(10)

Using the value of \( S \) as indicated by equation (8) evaluated at symmetric equilibria where \( s_i = s \), this worker surplus becomes

\[ W - U = w(N, a) - b + \beta(1 - \lambda)\left[\psi(s') + (1 - f(s', \theta'))[W' - U']\right]. \]  

(11)
5.1.2 Firms

Firms use inputs of labor, \( N \), and technology, \( a \), to produce output via the production function \( F(N,a) \). The production function is initially quite general, though I later parameterize it to have diminishing marginal product of labor, which is a necessary condition for the model to exhibit job rationing (Michaillat 2012). The value of a firm entering a period with \((1 - \lambda)N_{-1}\) employees remaining from the previous period is given by

\[
\Pi((1 - \lambda)N_{-1}) = \max_{N} F(N,a) - \frac{ca}{q(s,\theta)} [N - (1 - \lambda)N_{-1}] + \beta\Pi((1 - \lambda)N),
\]

(12)

where \( N - (1 - \lambda)N \) is hires made during the matching period. Firms make hires by posting vacancies at cost \( ca \). Each of these vacancies yields \( q(s,\theta) \) hires. Thus, the firm posts \( \frac{1}{q(s,\theta)} \) vacancies to make one additional hire, and the cost of that hire is given by \( \frac{ca}{q(s,\theta)} \).

The first order condition on \( N \) is

\[
F_N(N,a) - \frac{ca}{q(s,\theta)} + \beta(1 - \lambda)\Pi_N((1 - \lambda)N) = 0.
\]

(13)

The marginal value of an additional worker carried into the next period is given by

\[
\Pi_N((1 - \lambda)N) = \frac{ca}{q(s',\theta')},
\]

(14)

so the first order condition in equation (13) can be written as

\[
w(N,a) + W_N(N,a)N + \frac{ca}{q(s,\theta)} = F_N(N,a) + \beta(1 - \lambda)\frac{ca}{q(s',\theta')}.\]

(15)

That is, firms hire until the costs equal the benefits. The costs include the marginal worker’s wage, the change in wages for all other workers, and the cost of posting the marginal vacancies. The benefits include the marginal production of the worker and savings on the following period’s hiring costs.
5.2 Equilibrium Under Bargained Wages

I first consider flexible wages determined by Stole and Zwiebel (1996) bargaining as in Elsby and Michaels (2013) and Michaillat (2012). In this bargaining environment, after firms and workers have matched, they bargain over the marginal surplus produced by the match. I denote the firm’s marginal surplus after hiring costs are sunk by $J(N)$, where

$$J(N) = F_N(N, a) - w(N, a) - w_N(N, a)N + \beta(1 - \lambda) \frac{ca}{q(s', \theta')}. \quad (16)$$

If the firm sets its choice of employment optimally, as in equation (15), then this marginal surplus is also equal to the cost of making a hire:

$$J(N) = \frac{ca}{q(s, \theta)}. \quad (17)$$

The Stole and Zwiebel (1996) game implies that, for a worker’s bargaining weight $\eta$, the wage satisfies

$$(1 - \eta)[W - U] = \eta J(N). \quad (18)$$

If wages are assumed to be bargained the same way in all periods, then by equations (17) and (18),

$$W' - U' = \frac{\eta}{1 - \eta q(s', \theta')} \frac{ca}{q(s, \theta')}, \quad (19)$$

which can be plugged into equation (11) to obtain

$$W - U = w(N, a) - b + \beta(1 - \lambda) \left[ \psi(s') + (1 - f(s', \theta')) \frac{\eta}{1 - \eta q(s', \theta')} \frac{ca}{q(s, \theta')} \right]. \quad (20)$$

Using the surpluses as in equations (16) and (20) in the wage condition of equation (18) gives the wage in the form of a differential equation:

$$w(N, a) = \eta \left[ F_N(N, a) - w_N(N, a)N + \beta(1 - \lambda)ca\theta' \right] + (1 - \eta) \left[ b - \beta(1 - \lambda)\psi(s') \right]. \quad (21)$$

Assuming the production function is given by $F(N, a) = aN^\alpha$, the wage equation
becomes

\[
 w(N, a) = \eta \left[ \frac{aaN^{\alpha-1}}{1 - \eta(1-\alpha)} + \beta (1 - \lambda)ca\theta' \right] + (1 - \eta) \left[ b - \beta (1 - \lambda)\psi(s') \right]. \tag{22}
\]

This can be plugged into the equation (15), to remove wages from the firm’s employment decision condition:

\[
 (1 - \eta) \left[ \frac{aaN^{\alpha-1}}{1 - \eta(1-\alpha)} - b + \beta (1 - \lambda)\psi(s') \right] - \eta \beta ca\theta' - \frac{ca}{q(s, \theta)} + \beta (1 - \lambda) \frac{ca}{q(s', \theta')} = 0. \tag{23}
\]

In equilibrium, the wage condition of equation (18) and the firm’s marginal surplus in equation (17) can be used to rewrite the condition on optimal search effort from equation (9):

\[
 s\psi'(s) = \frac{\eta}{1 - \eta} \theta ca. \tag{24}
\]

Firms open vacancies, increasing \( N \) and \( \theta \), until equation (23) is satisfied at an \( s-\theta \) pair that also satisfies equation (24). The steady state equilibrium is defined by the point where this job creation curve intersects the Beveridge curve given by equation (5). Such an equilibrium is illustrated by the solid lines in Figure 4. For lower levels of technology, the value of vacancies is lower for any given level of unemployment and worker search effort because filled jobs are less productive. Thus, equation (23) is satisfied at a lower level of vacancies and the job creation curve rotates down, as in the dashed line of Figure 4.\(^\text{11}\)

### 5.3 Rigid Wages

I compare the flexible wage-setting environment of the previous section to one in which rigid wages are set via a reduced form schedule as in Michaillat (2012) and Blanchard and Galí (2010). That is, I assume that wages follow a schedule given by

\[
 w(N_t, a) = \omega a^\gamma \tag{25}
\]

\(^{11}\)I assume throughout that even under low technology levels, marginal productivity never falls below \( b - \beta (1 - \lambda)\psi(s') \). That is, there is always surplus to be gained from employment.
where $\omega \in [0,1]$ is a measure of wage flexibility. Under this assumption and Cobb-Douglas production, the firm’s employment condition simplifies to

$$\omega a^\gamma - 1 + \frac{c}{q(s, \theta)} = \alpha N^\alpha - 1 + \beta(1 - \lambda)\frac{c}{q'(s', \theta')}.$$  \hfill (26)

Workers’ chosen search intensity can be determined using equations (9) and (11). While next period’s search intensity and job finding rate can use to create a sufficient statistic for next period’s worker surplus ($W' - U'$), it is more convenient to consider steady state equilibria, in which $W - U = W' - U'$ and $s = s'$. In steady state, optimal search intensity satisfies

$$s\psi'(s) = f(s, \theta)\frac{\omega a^\gamma - b + \beta(1 - \lambda)\psi(s)}{1 - \beta(1 - \lambda)(1 - f(s, \theta))}.$$  \hfill (27)

If technology is high enough, the rigidity of the wage schedule does not affect employment. If marginal productivity at $N = 1$ exceeds the wage, then the job creation curve will intercept the origin. This is again as in the solid upward-sloping line of Figure 4. For some lower levels of technology, workers beyond some employment level will be less productive than the wage. Thus, the wage rigidity moves the job creation curve away from the origin: employment would not increase beyond some level even if the cost of vacancy-posting were eliminated. This scenario is illustrated by the dotted job creation curve at the right of Figure 4.

### 5.4 Partial Restrictions on Search Effort

I first discuss the effects of increasing the search effort of an atomistic worker or group of workers. That is, what is the effect of exogenous increases in search intensity that do not move the economy to a new equilibrium? In particular, suppose an unemployed worker is constrained to set an individual search effort higher than the economy-wide equilibrium, $s_i > s$. The effect on unemployment duration is straightforward, as it will decrease by $\frac{s}{s_i}$:

$$\frac{s}{s_i f(s, \theta)} < \frac{1}{f(s, \theta)}.$$  \hfill (28)
This operationalization is meant to approximate the effects of raising the search requirement for a small group of workers in a randomized controlled trial. Such searchers can find faster reemployment at the (possibly imperceptible) expense of other searchers in the market.

5.5 Equilibrium Restrictions on Search Effort

In considering the effects of a search effort restriction on the equilibrium, it is first important to note when such a restriction will bind. Under either wage-setting regime, optimal search effort, given by equations (24) and (27) can be shown to be an increasing function of labor market tightness.\footnote{Shimer (2004) presents a model in which search effort is instead countercyclical and presents supporting evidence using a measure of CPS search methods. Gomme and Lkhagvasuren (2013) review the research on this topic and argue both that controlling for spell duration reverses this result and that, more generally, the weight of the evidence is behind procyclical search effort.} Therefore, as shown in Figure 5, in u-v space, search is constant along rays from the origin and is increasing as the rays rotate to the northwest. If an economy-wide floor is put on search effort at $s$, it will affect behavior below some ray $\theta(s)$.

At points below this ray, search is no longer defined by equation (24) or equation (27) and is set at $s$. Because this is an increase in search effort, the Beveridge curve shifts in as displayed in Figure 6. Any job creation curves also shift up under either wage-setting regime. This is apparent from equations (23) and (26), as $q(s, \theta)$ rises for any given $\theta$. The additional search effort on the part of workers lowers the cost of filling a vacancy, and more vacancies are posted.\footnote{It is also the case that the additional disutility of search lowers a worker’s threat point under the flexible wage regime and decreases the bargained wage the firm must pay.}

While the effects on equilibrium depend on a number of factors, some potential outcomes are illustrated in Figure 7. In all cases, the firm-side response to increased search effort should serve to further lower unemployment beyond what would be expected if firms did not respond endogenously. However, the impact on unemployment depends on the relative positions of the Beveridge and job creation curves and on the ability of the economy to absorb new jobs. If a search requirement is implemented in a market where the Beveridge and job creation curves are flat, then small horizontal movements...
can bring about large changes in unemployment, as illustrated in the “Weak Economy, Flexible Wages” curves in Figure 7. If however, there is little scope to increase employment because of wage rigidities, then a search requirement may have little impact on equilibrium unemployment, as in rigid wage curves of Figure 7. On the whole, the predictions for how search requirements should affect the functioning of a slack labor market are initially unclear.

6 Differential Effects by Market Conditions

The theory in Section 5 suggests that the efficacy of search requirement policies may vary considerably across labor market conditions. If unemployment is always driven by matching frictions, then search requirements may be very effective in reducing unemployment during recessions due to all the slack in the labor market. If, on the other hand, jobs are rationed in recessions because of wage rigidities, then search requirements will have little ability to reduce unemployment. Therefore, at the level of the labor market, I test whether search requirements are effective at reducing unemployment.\footnote{Effects of the same sign are generated by performing similar tests at the individual level using the CPS duration data. However, the results are relatively imprecise and the theory suggests that an analysis of the labor market as a whole is reasonable.}

One strategy for examining differential effects across market conditions would be to simply interact the policy measures with the unemployment rate, a commonly-chosen measure of labor market strength. However, in the current analysis, it is also the outcome of interest. Therefore, I use two plausibly-exogenous measures of market strength. First, I separate labor markets by lagged unemployment, testing to see whether the implementation of a search requirement differentially impacts the equilibrium in labor markets that were weaker before the policy change. Second, I use a shift-share measure of employment-by-industry to proxy local labor market demand with national employment trends.
6.1 Equilibrium Effects by Lagged Unemployment

Within states that implement changes to their search requirements, I explicitly examine differences across labor markets by their conditions before implementation. If search requirements are more effective when the labor market is weak, then unemployment should fall relatively more for markets with initially high unemployment. If search requirements are not as effective in weak markets, then unemployment rates should fall relatively more in the initially low unemployment locations. I implement the test via a specification of the form

\[ u_{mt} = \alpha L_m 1(t > t^*) + \gamma_m + \delta_t + \varepsilon_{mt}, \]  

(29)

where \( u_{mt} \) is the unemployment rate in market \( m \) at time \( t \), \( L_m \) is an indicator for being a low-unemployment market prior to implementation, which is multiplied by an indicator function for time \( t \) being post-implementation. Market and time fixed effects are given by \( \gamma_m \) and \( \delta_t \). Estimates of the coefficient \( \alpha \) indicate the average change in unemployment differential between high- and low-unemployment markets following implementation of a search requirement increase. Negative estimates of \( \alpha \) show that low-unemployment markets had relatively even lower unemployment after implementation. Positive estimates of \( \alpha \) show that low-unemployment markets had relatively higher unemployment after implementation. The former suggests that search requirements are relatively more effective in low unemployment markets, while the latter suggest that they are relatively more effective in high unemployment markets.

I estimate regressions of the form of (29) using data from the Local Area Unemployment Statistics (LAUS) program. LAUS data are useful in that they provide monthly estimates of unemployment and labor force participation at relatively disaggregated levels. A disadvantage of the data are that they are partially constructed by BLS models that combine data from a number of different sources. Unemployment estimates at the level of the labor market are constructed from data on current and past UI claims
and estimates of new entrants and reentrants into the labor force.\textsuperscript{15} Entrants and reen- 
trants are estimated using state-level estimates of these groups modeled from current 
and past CPS data, which are then divided into market areas based on the relative 
age distributions of the markets. Each market is assigned new entrants in proportion 
to its share of the state’s age 16–19 population. Reentrants are assigned based on the 
market’s share of the population ages 20 and over.

In essence, the process combines high-quality data on local unemployment claims, 
which are not always otherwise available, with an averaged apportioning of entrants. 
The estimates may be biased if state policy changes are correlated with changes in the 
number of state entrants and reentrants and these groups are differently assigned to 
markets defined as high- or low-unemployment before implementation. As it is difficult 
to explicitly control for these concerns without the exact models used by the BLS, I 
proceed using the LAUS data as they are published.

Table 5 reports the regression results using data from 2001 to 2014 and four states 
that have increased their search requirements: Florida, Ohio, Pennsylvania, and Utah. 
Within each state, I divide the micropolitan and metropolitan statistical areas by their 
unemployment rates pre-implementation using two measures. The first row of results 
are generated defining low-unemployment areas as those that had average unemploy-
ment rates below the state median between 2001 and the implementation of the policy 
change. The second row defines low-unemployment areas as those that had average un-
employment below the state median in the year before the policy change. The former 
identifies areas that are consistently low in unemployment, while the latter identifies 
those that may have been transitively so. In both rows of Table 5, the estimates are 
universally negative, indicating that low-unemployment markets do relatively better 
after a search requirement increase under either definition. It is striking that the es-
timates are more negative when using the transitory definition (second row), as one 
might expect that are briefly low-unemployment to regress to the mean over time.

\textsuperscript{15}This description draws heavily on the LAUS estimation methodology details found at 
6.2 Equilibrium Effects by Industry Shift-Share

I next proxy for labor market strength using a shift-share measure as in Bartik (1991). This method uses changes in the national distribution of employment-by-industry to proxy for local labor demand in individual markets with different industry mixes. Intuitively, if employment in manufacturing declines nationally, one would expect markets that have larger shares of workers in manufacturing to see larger employment declines. If the national movements in industry employment are exogenous to an individual market’s local labor conditions, then the measure is a plausibly exogenous measure of local labor demand.

In practice, I calculate the proxy using predicted employment in market $m$ at time $t$ as

$$\hat{E}_{mt} = \sum_k \frac{N_{kt}}{N_{kb}} E_{mkb},$$

(30)

where $b$ is a chosen baseline date and $k$ indexes industries. $N_{kt}$ is national employment in industry $k$ at time $t$, $N_{kb}$ is national employment in the industry at baseline, and $E_{mkb}$ is employment in market $m$ in industry $k$ at baseline. Thus, $\hat{E}_{mt}$ is predicted using baseline employment in each industry ($E_{mkb}$) multiplied by national growth in that industry since the baseline ($\frac{N_{kt}}{N_{kb}}$) and summed across industries. For the purposes of the regressions that follow, I use predicted employment growth,

$$\hat{G}_{mt} = \frac{\hat{E}_{mt} - E_{mb}}{E_{mb}},$$

(31)

which has the advantage of being the same scale for all markets.

While this measure can be used as an instrument, I interpret it directly and estimate reduced-form regressions given by

$$u_{mt} = \alpha_0 + \alpha_1 \hat{G}_{mt} + \alpha_3 D_{st} + \alpha_4 D_{st} \hat{G}_{mt} + \gamma_m + \delta_t + \epsilon_{mt}.$$  

(32)

The estimates of $\alpha_4$ indicate the differential effect of a search policy when predicted employment growth is 100 percent higher. Negative estimates for these coefficients
suggest that search policies do more to lower unemployment when labor demand is relatively stronger.

I calculate the shift-share proxy using the Quarterly Workforce Indicators (QWI) for all available micropolitan and metropolitan statistical areas. The QWI provide quarterly estimates of various employment stocks and flows using Longitudinal Employer-Household Dynamics (LEHD) microdata. The QWI is sourced with high-quality administrative data from a number of sources, but have some noise infused to protect individual confidentiality. For the purposes of constructing the shift-share measure, I use employment counts in two-digit North American Industry Classification System (NAICS) sectors at the level of the statistical areas. I use a baseline of the first quarter of 2004 because it is before most of the policy changes of interest, but is at the point at which almost all states appear in the QWI.\textsuperscript{16} The following estimates are robust to other choices of baseline quarters, including using each quarter’s lag as its baseline.\textsuperscript{17}

The first row of Table 6 indicates a strong relationship between the shift-share measure and the local unemployment rate, again taken from LAUS data. In these results, both the unemployment rate and $\hat{G}_{mt}$ are scaled as shares of one (e.g., the unemployment rate is 0.05 and $\hat{G}_{mt}$ is 1.01). While there is some variability in the interaction estimates, the trend is summarized by the coefficient on the linear interaction term in the second row of column (1). A one percentage-point increase in predicted growth strengthens the decrease in the unemployment rate from each additional required contact by 0.0003. Alternatively, increasing from one contact to five should lower unemployment by 0.15 percentage points for each additional percentage point of expected growth. While the pattern in column (2) is not monotonic, the general trend across the first three policies tells the same story. The relative outlier estimate on the interaction with four-plus policies (0.016) turns out to be driven by the five-contact policy in Florida. Dropping Florida from this analysis results in a negative estimate for

\textsuperscript{16}Not all geographies are available in the QWI data. Massachusetts is missing entirely and no estimates are available for New England City and Town Areas (NECTAs) in other states. Washington, DC is not available until 2005 and is also excluded from the analysis.

\textsuperscript{17}I do not present results using individual lags because of concerns that industry mixes and employment could be affected by the policies of interest when they change.
the four-plus policies of the same magnitude as that for the three-contact policies. As one would expect, dropping this nonmonotonic outlier from the regression in column (1) increases both the magnitude and the significance of the linear estimate. The reason for Florida appearing as an outlier is not clear. In the absence for a compelling reason to treat it differentially, I leave it included in the main results presented here.

7 Conclusion

The wave of increases in search requirements for UI claimants does not appear to have dramatically increased the speed at which claimants return to work. Although some measures of search intensity suggest that stronger requirements have effects on search effort, there is only small evidence on the extent to which this is translated to faster reemployment in microdata. I motivate additional analysis of the labor market with a general search-and-matching model. In some cases, such a model will predict that the general equilibrium effects of search requirements will compound the search effort effects alone: firms will open more vacancies as searchers exert more effort. However, if the market is not fully flexible, as in rigid-wage, job-rationing models, search requirements may have little scope to improve labor market conditions. In particular, short-term partial equilibrium experiments may not generalize to fully-implemented and permanent policies in general equilibrium.

The limitations of some active labor market policies in recessions are strongly suggested in the job-rationing analyses of Michaillat (2012) and Landais, Michaillat, and Saez (2010), particularly those that aim to counter search-and-matching frictions. The evidence in this paper provides some empirical support for those limitations. Although UI search requirements may be unambiguously effective for small groups of workers in partial equilibrium, their effectiveness appears muted in general equilibrium in weak labor markets. While policymakers may wish to continue raising search requirements as a way of increasing the burden of UI claiming, these changes are unlikely to improve outcomes for UI systems through faster reemployment in recessions.
References


Table 1: Evidence on Efficacy of Search Requirements

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Number of Reported Contacts (upper bound)</th>
<th>Acceptable Contacts (lower bound)</th>
<th>Number of Methods</th>
<th>Contacted Employers?</th>
</tr>
</thead>
<tbody>
<tr>
<td># contacts</td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td># contacts</td>
<td>0.564*** (0.121)</td>
<td>0.559*** (0.129)</td>
<td>0.218*** (0.071)</td>
<td>0.037*** (0.008)</td>
</tr>
<tr>
<td>1 contact</td>
<td>0.651*** (0.182)</td>
<td>0.544*** (0.194)</td>
<td>1.332*** (0.135)</td>
<td>0.108*** (0.037)</td>
</tr>
<tr>
<td>2 contacts</td>
<td>1.610*** (0.324)</td>
<td>1.438*** (0.359)</td>
<td>1.680*** (0.217)</td>
<td>0.117** (0.051)</td>
</tr>
<tr>
<td>3 contacts</td>
<td>1.108*** (0.100)</td>
<td>0.988*** (0.106)</td>
<td>1.534*** (0.042)</td>
<td>0.138*** (0.018)</td>
</tr>
<tr>
<td>4+ contacts</td>
<td>3.086*** (0.371)</td>
<td>2.947*** (0.407)</td>
<td>2.171*** (0.224)</td>
<td>0.204*** (0.069)</td>
</tr>
<tr>
<td>Nonspecific</td>
<td>0.848** (0.420)</td>
<td>0.698*** (0.165)</td>
<td>0.903*** (0.443)</td>
<td>0.637*** (0.168)</td>
</tr>
<tr>
<td>N:</td>
<td>126,857</td>
<td>126,857</td>
<td>126,844</td>
<td>126,844</td>
</tr>
<tr>
<td>Sample Mean:</td>
<td>1.819</td>
<td>1.819</td>
<td>1.686</td>
<td>1.686</td>
</tr>
<tr>
<td>F-stat, all=0:</td>
<td>14.8</td>
<td>47.6</td>
<td>12.3</td>
<td>46.5</td>
</tr>
<tr>
<td>p-value:</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
</tbody>
</table>

Huber/White/sandwich standard errors clustered at the state level appear below estimates in parentheses. Columns (1) and (2) display the estimated effects of search policies on the number of contacts reported by UI claimants audited by the BAM program in the 45 sample states described in Section 2.4. The dependent variable in columns (3) and (4) is the number of contacts that are not explicitly found to be unacceptable. The dependent variable in columns (5) and (6) is the number of contacts that are explicitly found to be acceptable. Regressions include state fixed effects, year-by-month fixed effects, and controls for industry, occupation, education, UI claim duration, race, ethnicity, and sex. Columns (7) and (8) regress the number of search methods reportedly used in the prior 4 weeks by unemployed job-losers in the monthly CPS, again for the 45 states in question. Columns (9) and (10) report estimates from linear probability models for contacting employers directly using the same CPS sample. Both sets of CPS models include state fixed effects, year-by-month fixed effects, and controls for occupation, industry, race, education, unemployment duration, unemployment reason, and sex.
Table 2: Share of CPS Respondents Reporting Each Search Method

<table>
<thead>
<tr>
<th>Search Method</th>
<th>Share</th>
</tr>
</thead>
<tbody>
<tr>
<td>Contacted employer directly/interview</td>
<td>0.422</td>
</tr>
<tr>
<td>Contacted public employment agency</td>
<td>0.175</td>
</tr>
<tr>
<td>Contacted private employment agency</td>
<td>0.075</td>
</tr>
<tr>
<td>Contacted friends or relatives</td>
<td>0.203</td>
</tr>
<tr>
<td>Contacted school/university employment center</td>
<td>0.022</td>
</tr>
<tr>
<td>Sent out resumes/ filled out applications</td>
<td>0.388</td>
</tr>
<tr>
<td>Checked union/professional registers</td>
<td>0.037</td>
</tr>
<tr>
<td>Placed or answered ads</td>
<td>0.141</td>
</tr>
<tr>
<td>Other active</td>
<td>0.060</td>
</tr>
<tr>
<td>Looked at Ads</td>
<td>0.255</td>
</tr>
<tr>
<td>Attended job training programs/courses</td>
<td>0.011</td>
</tr>
<tr>
<td>Other passive</td>
<td>0.006</td>
</tr>
</tbody>
</table>

N: 175,643

Notes: Table reports the share of unemployed basic monthly CPS respondents reporting each type of job search method over the prior four weeks between 2003 and 2013. Sample includes all CPS respondents ages 25 to 65 who are unemployed and looking for work and who reported losing or being laid off from their previous jobs.

Table 3: Cox Proportional Hazard Models of Policy Effects on Reemployment

<table>
<thead>
<tr>
<th># contacts</th>
<th>Failure=Reemployment</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 contact</td>
<td>0.992 (0.044)</td>
</tr>
<tr>
<td>2 contacts</td>
<td>1.032 (0.042)</td>
</tr>
<tr>
<td>3 contacts</td>
<td>1.005 (0.021)</td>
</tr>
<tr>
<td>4+ contacts</td>
<td>1.052 (0.043)</td>
</tr>
<tr>
<td>Nonspecific</td>
<td>1.027 (0.034)</td>
</tr>
</tbody>
</table>

Observations: 142,825 142,825

χ²-stat, all=0: 1.1 7.1

p-value: 0.574 0.215

Notes: Table reports hazard ratios for measures of search requirement policies using longitudinally-linked CPS data. Included controls allow the hazard to vary proportionally in month, year, state, education, occupation, industry, race, ethnicity, and sex.
### Table 4: Policy Effects on Average UI Weeks Claimed

<table>
<thead>
<tr>
<th># required contacts</th>
<th>Outcome = Running Avg Weeks Claimed</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td># required contacts</td>
</tr>
<tr>
<td></td>
<td>1 contact</td>
</tr>
<tr>
<td></td>
<td>2 contacts</td>
</tr>
<tr>
<td></td>
<td>3 contacts</td>
</tr>
<tr>
<td></td>
<td>4+ contacts</td>
</tr>
<tr>
<td></td>
<td>Nonspecific</td>
</tr>
<tr>
<td></td>
<td>Observations:</td>
</tr>
<tr>
<td></td>
<td>F-stat, all = 0:</td>
</tr>
<tr>
<td></td>
<td>p-value:</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th># required contacts</th>
<th>−0.124</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(0.397)</td>
</tr>
<tr>
<td>1 contact</td>
<td>−0.393</td>
</tr>
<tr>
<td></td>
<td>(1.327)</td>
</tr>
<tr>
<td>2 contacts</td>
<td>−1.004</td>
</tr>
<tr>
<td></td>
<td>(1.394)</td>
</tr>
<tr>
<td>3 contacts</td>
<td>−0.320</td>
</tr>
<tr>
<td></td>
<td>(0.251)</td>
</tr>
<tr>
<td>4+ contacts</td>
<td>−1.803</td>
</tr>
<tr>
<td></td>
<td>(1.434)</td>
</tr>
<tr>
<td>Nonspecific</td>
<td>−0.622</td>
</tr>
<tr>
<td></td>
<td>−1.042</td>
</tr>
<tr>
<td></td>
<td>(1.602)</td>
</tr>
<tr>
<td></td>
<td>(0.644)</td>
</tr>
<tr>
<td>Observations:</td>
<td>2,205</td>
</tr>
<tr>
<td>F-stat, all = 0:</td>
<td>0.08</td>
</tr>
<tr>
<td>p-value:</td>
<td>0.927</td>
</tr>
</tbody>
</table>

Notes: Huber/White/sandwich standard errors clustered at the state level are displayed below estimates in parentheses. Table presents estimates of the effects of search policies on quarterly aggregate average UI claim duration, which is given by (total weeks claimed)/(initial claims), each over the past calendar year. Regressions include state fixed effects and year-by-quarter fixed effects. Estimation sample is 2,205 state-quarter observations from 2001 to 2013.

### Table 5: Relative Policy Effects by Pre-period Labor Market Strength

<table>
<thead>
<tr>
<th>Outcome = Unemployment Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>All</td>
</tr>
<tr>
<td>Low unemployment 2001 to $t^*$</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Low unemployment $t^* - 1$ to $t^*$</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Micro/metro statistical areas:</td>
</tr>
</tbody>
</table>

Notes: Table reports regression estimates from a specification as described by equation (29). Estimates indicate the differential change in unemployment rates for initially-low-unemployment statistical areas when a search requirement increase is implemented at time $t^*$. Low unemployment statistical areas are defined as those that have average unemployment below the state median in the indicated period (2001 to $t^*$ or $t^* - 1$ to $t^*$). Monthly data on statistical area unemployment rates come from the LAUS program.
Table 6: Policy and Shift-Share Interaction Effects

<table>
<thead>
<tr>
<th>Outcome= Unemployment Rate</th>
<th>( \hat{G}_{mt} )</th>
<th>(-0.115^{**} )</th>
<th>(-0.115^{**} )</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.053)</td>
<td></td>
</tr>
<tr>
<td>( \hat{G}_{mt} \times # \text{ contacts} )</td>
<td>-0.030*</td>
<td>(0.018)</td>
<td></td>
</tr>
<tr>
<td>( \hat{G}_{mt} \times 1 \text{ contact} )</td>
<td>0.027</td>
<td>(0.053)</td>
<td></td>
</tr>
<tr>
<td>( \hat{G}_{mt} \times 2 \text{ contacts} )</td>
<td>-0.060</td>
<td>(0.052)</td>
<td></td>
</tr>
<tr>
<td>( \hat{G}_{mt} \times 3 \text{ contacts} )</td>
<td>-0.146***</td>
<td>(0.026)</td>
<td></td>
</tr>
<tr>
<td>( \hat{G}_{mt} \times 4+ \text{ contacts} )</td>
<td>0.016</td>
<td>(0.059)</td>
<td></td>
</tr>
<tr>
<td>( \hat{G}_{mt} \times \text{ Nonspecific} )</td>
<td>-0.140***</td>
<td>-0.142***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.035)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Standard errors are calculated with 1000 repetitions of a clustered bootstrap at the state level. Reported coefficients are estimated analogs of \( \alpha_1 \) and \( \alpha_4 \) from equation (32). \( \hat{G}_{mt} \) is calculated at quarterly frequency using QWI data as described in Section 6. In addition to the reported coefficients, main effects of the policies are included along with statistical area fixed effects and month fixed effects. The outcome measure is the unemployment rate from the LAUS program. Estimation is performed on 70,584 area-month observations from 692 individual statistical areas in 37 states.
Figure 1: Number of States with Each Required Number of Employer Contacts

Notes: Figure indicates the number of states with each specific search requirement from one employer contact per week to five employer contacts per week. Policies are identified using state workforce agency publications.
Notes: Figure indicates share of BAM-audited UI claimants reporting each number of employer contacts by search requirement policy.
Figure 3: Search Policy and Average Contacts Reported by State, states with policy changes

Notes: Thick dashed lines indicate search requirements as found in workforce agency publications. Any time without a dashed line indicates a nonspecific search policy. Thin solid lines show the monthly average number of employer contacts reported by audited claimants in the BAM data.
Figure 4: Initial Economy Equilibrium

Notes: Unrestricted equilibrium for the model described in Section 5.
Figure 5: Optimal Search is an Increasing Function of $\theta$

Notes: Variation in symmetric equilibrium optimal search effort for various levels of label market tightness for the model described in Section 5.
Figure 6: Beveridge Curve Under Search Restriction

Notes: Effects of a search requirement implemented at $s$ for the model described in Section 5.
Figure 7: New Equilibria Under Search Restriction

Notes: New equilibrium under the search requirement for the models described in Section 5 at different levels of labor productivity.
A Appendix Figures

Figure A1: Number of UI Claimants Living Under Each Required Number of Employer Contacts Policy

Notes: Figure indicates the number of UI claimants living under each specific search requirement from one employer contact per week to five employer contacts per week. The thick dashed line indicates movements in national UI claims at one-third scale. Policies are identified using state workforce agency publications. Total claims under each policy are determined by summing continuing claims each month across states.
Figure A2: Share of UI Claimants Living Under Each Required Number of Employer Contacts Policy

Notes: Figure indicates the share of UI claimants living under each specific search requirement from one employer contact per week to five employer contacts per week. Policies are identified using state workforce agency publications. Total claims under each policy are determined by summing continuing claims each month across states. Shares are then recovered by dividing by national claims.
Figure A3: Search Policy and Average Contacts Reported by State, states without policy changes in period of available BAM data, Alaska to Michigan

Notes: Thick dashed lines indicate search requirements as found in workforce agency publications. Any time without a dashed line indicates a nonspecific search policy. Thin solid lines show the monthly average number of employer contacts reported by audited claimants in the BAM data.
Figure A4: Search Policy and Average Contacts Reported by State, states without policy changes in period of available BAM data, Minnesota to Wyoming

Notes: Thick dashed lines indicate search requirements as found in workforce agency publications. Any time without a dashed line indicates a nonspecific search policy. Thin solid lines show the monthly average number of employer contacts reported by audited claimants in the BAM data.
Figure A5: Distribution of Contacts Reported by State, states with policy changes

Notes: Graphs show the share of BAM claimants each month reporting each number of employer contacts. Employer contacts increase from zero to six-plus in increasing darkness.
Figure A6: Distribution of Contacts Reported by State, states without recorded policy changes in period of available BAM data, Alaska to Michigan

Notes: Graphs show the share of BAM claimants each month reporting each number of employer contacts. Employer contacts increase from zero to six-plus in increasing darkness.
Figure A7: Distribution of Contacts Reported by State, states without recorded policy changes in period of available BAM data, Minnesota to Wyoming

Notes: Graphs show the share of BAM claimants each month reporting each number of employer contacts. Employer contacts increase from zero to six-plus in increasing darkness.