

# The General Equilibrium Impacts of Unemployment Insurance: Evidence from a Large Online Job Board

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*Ioana Marinescu, University of Chicago*

## Abstract

Unemployment insurance affects benefit recipients, but also, through general equilibrium effects, non-recipients and vacancies. During the Great Recession, potential benefit duration (PBD) was greatly extended. I estimate the impact of the extensions using an event study and nonlinearities in the relationship between PBD and the unemployment rate. PBD extensions significantly decreased the total number of applications without changing the number of vacancies on CareerBuilder.com. This suggests that PBD extensions increased non-recipients' job finding probability. A calibration based on my estimates suggests that PBD extensions had little effect on aggregate unemployment in the medium run.

## 1. Introduction

During the Great Recession, the duration of unemployment benefits in the US has been extended from 26 to up to 99 weeks. Consistent with a well-established literature (e.g. Katz and Meyer 1990; Meyer 2002; Schmieder, Wachter, and Bender 2012), Rothstein (2011) and Farber and Valletta (2013) find that these benefit extensions increased the unemployment duration of benefit recipients. While the effects were small, the impact of the unemployment benefits extensions on aggregate unemployment may differ from their impact on the unemployment duration of benefit recipients.

Indeed, unemployment benefits create search externalities (Krueger and Meyer 2002). As benefit recipients search less hard, competition for jobs decreases and non-recipients find a job more easily. On the other hand, if unemployment insurance makes it harder for firms to fill vacancies and increases the reservation wage, firms will post fewer vacancies. This makes it harder for all job seekers to find a job. Depending on how these various effects play out, the general equilibrium effect of unemployment insurance may be greater or smaller than the partial equilibrium effect on benefit recipients.

An enlightening theoretical paper by Landais, Michailat, and Saez (2014) formalizes these intuitions. They demonstrate that the difference between the micro and macro effects of unemployment insurance depends on the impact of unemployment insurance on labor market tightness as measured by the ratio of vacancies to aggregate search effort. Specifically, if unemployment insurance increases labor market tightness, the general equilibrium impact of unemployment insurance on unemployment is smaller than the partial equilibrium effect (Landais, Michailat, and Saez 2014).

If one can assume away the impact of unemployment insurance on vacancies, the difference between the macro and the micro impact of unemployment insurance can be recovered by measuring the spillover effects of unemployment insurance on the unemployment duration of non recipients (Landais, Michailat, and Saez 2014). Levine (1993) and Lalive, Landais, and Zweimüller (2013) find that an increase in the generosity of unemployment insurance decreases the unemployment duration of non-recipients. This suggests that the macro effect of unemployment insurance is smaller than the micro effect. However, in order to take into account the impact of unemployment insurance on vacancies and thus identify the full macro impact of unemployment insurance, one needs exogenous variation across comparable labor markets rather than exogenous variation across comparable individuals in a single market as used by Lalive, Landais, and Zweimüller (2013).

This paper uses plausibly exogenous variation in the generosity of unemployment insurance within a labor market over time to identify the impact of unemployment insurance on labor market tightness. I measure aggregate search effort at the state-month level by the number of applications received for jobs on CareerBuilder.com, the largest American employment website. Unemployment benefits recipients allocate the majority of their time to browsing and answering job ads (Krueger and Mueller 2011), implying that applications on a job search website are a good measure of job search effort. To identify the impact of unemployment insurance on vacancies and applications, I use variation in potential unemployment benefits duration (PBD) induced by the Emergency Unemployment Compensation 2008 (EUC) and extended benefits (EB). I start with an event study and show that the number of vacancies stays flat around a benefit extension and as far as seven months after the benefit extension. By contrast, state-level job applications significantly decline in the first month of a benefit extension, and continue to decline for several months thereafter.

A second identification strategy uses the fact that benefit extensions (EUC and EB) depend on state-level unemployment rates reaching specific thresholds. I exploit this source of variation by using a fuzzy regression discontinuity design, as well as a panel specification that relies on nonlinearities in PBD as a function of the state unemployment rate. This second identification strategy confirms that PBD decreases the total number of applications but has no significant impact on the number of vacancies. Across identification strategies, a conservative estimate indicates that a one-week increase in potential benefit duration leads to a 0.4% decline in applications at the state level. Finally, I show that the estimated impact of PBD on applications is robust to changes in job composition by education requirement and industry, and that posted wages are not affected by PBD.

Overall, these results indicate that the unemployment insurance extensions during the Great Recession increased labor market tightness. The positive impact of unemployment insurance on labor market tightness implies that the macro effect of unemployment insurance is smaller than the micro effect and that optimal unemployment insurance is countercyclical (Landais, Michaillat, and Saez 2014). To calibrate the macro impact of unemployment insurance, I use the theoretical results from Landais, Michaillat, and Saez (2014) together with my estimates and Rothstein's (2011) estimates of the micro elasticity of unemployment insurance. A calibration using a conservative estimate of the impact of PBD on applications (-0.4%) implies that the elasticity of aggregate unemployment is as small as 0.07, and the elasticity becomes essentially zero (0.002) if we use a larger estimate of the impact of PBD on applications based on the regression discontinuity estimates. The calibration results imply that, once

general equilibrium effects are taken into account, unemployment insurance extensions during the Great Recession had little impact on aggregate unemployment in the medium run.

My paper is closely related to Hagedorn et al. (2013) but differs in its conclusions. I conclude that the macro impact of the extensions on aggregate unemployment was smaller than the micro impact while Hagedorn et al. (2013) conclude the opposite. The differences in our conclusions can be pinned down to differences in identification strategy and in the definition of labor market tightness. Hagedorn et al. (2013) identify the impact of the extensions by comparing adjacent counties belonging to different states, while I mostly rely on within state variation over time. Hagedorn et al. (2013) measure tightness by the vacancies to unemployment ratio while I measure tightness implicitly by the vacancies to applications ratio. Indeed, as expected from theory and prior empirical evidence (e.g. Card, Chetty, and Weber 2007; Krueger and Mueller 2011), the job search effort of unemployment benefit recipients falls in response to an increase in PBD. This implies that the number of unemployed people overestimates labor supply and the vacancies to unemployment ratio underestimates labor market tightness. Therefore, it is important to take into account the impact of PBD on job search effort when measuring labor market tightness.

The remainder of the paper is organized as follows. Section 2 discusses how unemployment benefit extensions were decided, describes the data and presents the theoretical framework. Section 3 discusses the identification strategy and the results for the event study, the fuzzy regression discontinuity design and the panel regressions using nonlinearities in PBD as a function of the state unemployment rate. Section 4 presents robustness tests and discusses the results. In particular, section 4 presents the calibration results for the impact of PBD extensions on aggregate unemployment. Finally, Section 5 concludes.

## **2. Policy background, data and theoretical framework**

### **Policy background**

PBD in the United States is 26 weeks by default. However, during times of high unemployment, this duration can be extended based on state-level determinants. The extended benefits (EB) program activates in a state under one of two conditions:

- if the state's 13-week average insured unemployment rate (IUR) in the most recent 13 weeks is at least 5.0 percent and at least 120 percent of the average of its 13-week IURs in the last 2 years for the same 13-week calendar period; or
- *at state option*, if its current 13-week average IUR is at least 6.0 percent, and regardless of the experience in previous years. I will say that the **IUR option** is in place for the months when a state chose this option.

States have the option of electing an alternative trigger authorized by the Unemployment Compensation Amendments of 1992 (Public Law 102-318). I will say that the **TUR option** is in place for the months when a state chose this option. This trigger is based on a 3-month average total unemployment rate (TUR) using seasonally adjusted data:

- If this TUR average exceeds 6.5 percent and is at least 110 percent of the same measure in either of the prior 2 years, a State can offer 13 weeks of EB.
- If the average TUR exceeds 8 percent and meets the same 110-percent test, 20 weeks of EB can be offered.

Normally, extended benefits are financed 50% by states and 50% by the federal government. Under the American Recovery and Reinvestment Act of 2009 (ARRA) passed on Feb. 17, 2009, the benefits are financed entirely by the federal government. This provided many states with an incentive to choose the TUR option (the IUR option is mostly irrelevant because few states reach 6% IUR without already having EB under the regular IUR condition). Federal funding of EB was still in place at the end of my sampling frame (July 2011).

The Tax Relief, Unemployment Insurance Reauthorization, and Job Creation Act of 2010 (P.L. 111-312, passed on Dec. 16 2010 and signed on Dec. 17, 2010) temporarily changed the look-back timeframe to three years, as unemployment indicators in most states had been consistently high for the past two years and would have resulted in many states being unable to meet the 120% IUR or 110% TUR conditions. This three-year look-back exception was still in place at the end of my sampling frame (July 2011).

As of January 2011, Arkansas, Iowa, Louisiana, Maryland, Mississippi, Montana, Oklahoma, Utah, and Wyoming could qualify for extended benefits under TUR but chose not to use that option. Legislators in these states were afraid of adopting extended benefits under TUR since 100% federal funding would

eventually expire, and if they kept the TUR trigger on their books the states would have to match federal funding 50/50. This would likely require a tax hike on businesses in order to replenish depleted state unemployment trust funds. Many politicians also believed that existing state benefits were sufficient, and did not want to spend federal tax dollars on extended benefits that would reduce incentives for unemployed workers to find a job<sup>1</sup>.

Another reason why unemployment benefits were extended during the Great Recession is the federal Emergency Unemployment Compensation (EUC) 2008. EUC08 is an emergency federal benefits program that is payable to individuals who have exhausted all rights to regular compensation with respect to a benefit year that ended on or after May 1, 2007. Typically, a jobseeker collects EUC benefits before EB benefits.

- The EUC08 program, signed into law on June 30, 2008, provides up to 13 weeks of 100 percent federally-financed compensation to eligible individuals in all states. Public Law (P.L.) 110-449 expanded the EUC08 program on November 21, 2008 to provide up to 20 weeks of 100 percent federally-funded unemployment compensation to eligible individuals in all states. This constitutes tier 1 or EUC1.
- Tier 2 of EUC (EUC2) was created by Public Law (P.L.) 110-449. It provides 13 weeks of benefits to eligible individuals in states where TUR (defined as for EB) is above 6% or IUR (defined as for EB) is above 4%.
- Public Law No. 111-92, enacted on November 6, 2009, expanded the EUC08 program, in the following ways:
  - It increased the maximum EUC2 entitlement from 13 weeks to 14 weeks of benefits in all states, and this Tier is no longer triggered on by a state reaching a specified rate of unemployment;
  - It created EUC3 providing up to 13 additional weeks of benefits in states with IUR above 4 percent or TUR above 6 percent;
  - It created EUC4 providing up to 6 additional weeks of benefits in states with IUR above 6 percent or TUR above 8.5 percent.

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<sup>1</sup> Another concern was that although the federal government paid for 100% of normal extended benefits claims, state governments would be responsible for paying extended benefits to state and local government employees who had been laid off.

The impact of the EUC and EB extensions on the job search effort of benefit recipients may be somewhat different for three reasons. First, as already mentioned, EB benefits only become available after workers have exhausted their regular and EUC benefits: therefore, due to discounting, the impact of EB on job search is likely to be smaller. Second, EB has somewhat more stringent job search requirements (see Federal-State Extended Unemployment Compensation Act of 1970), which also suggests that EB may have a lower impact on job search effort. Third, the estimated impact of EB and EUC could differ because of econometric identification problems. Indeed, EB is partially determined by the state's decision to adopt a TUR option, while EUC is not. This suggests that some of the variation in weeks of EB may be endogenous: for example, states with high unemployment prospects may choose to provide more EB by adopting the TUR option. For these three reasons, the estimated impact of EUC and EB on aggregate job search effort may be different, and this is something that I will be investigating.

For both EUC and EB, TUR conditions are much more likely to be satisfied than the IUR conditions. For example, EUC2 and EUC3 require that TUR be above 6% or IUR above 4%. In my sample, when TUR is above 6%, IUR is above 4% in 97% of the cases. On the other hand, when IUR is below 4%, TUR is nonetheless above 6% in 54% of the cases.

Since the conditions governing the maximum weeks of UI benefits are fairly complex, Figure 1 summarizes the regulation in a simplified form. The figure ignores some of the finer points of the regulation discussed above to show how PBD depends on TUR during different time frames, and for a state that has the TUR option for EB (i.e. the majority of states). This overview of the policies suggests that one can use sharp changes in benefit duration at 6.5% and 8% TUR (for EB) and 6% and 8.5% TUR (for EUC) to identify the impact of benefit duration on applications. I discuss below how I make use of these in practice.

## Data

The data on applications and job vacancies comes from proprietary data provided to me by CareerBuilder.com. Job vacancies are the total number of job ads posted in a given state during a given month. One can compare job vacancies in CareerBuilder.com with data on job vacancies in the representative JOLTS (Job Openings and Labor Turnover Survey). The number of vacancies on CareerBuilder.com represents 35% of the total number of vacancies in the US in January 2011 as counted in JOLTS. Compared to the distribution of vacancies across industries in JOLTS, some industries are overrepresented in CareerBuilder data, in particular information technology, finance and insurance, and real estate, rental and leasing. The most underrepresented industries are state and local

government, accommodation and food services, other services, and construction. When comparing the distribution of jobs across US regions in JOLTS vs CareerBuilder, I find that CareerBuilder has the same geographic distribution of jobs as JOLTS. While the vacancies on CareerBuilder are not perfectly representative of the ones in the US economy as a whole, they form a substantial fraction of the market (further discussion of the external validity of this study can be found on p.24).

For each state, information is available on the distribution of vacancies by industry, the level of education required, and the posted wage. Only 37% of vacancies specify a required education (Table 1). When no education requirement is specified, it does not necessarily mean that there is no education requirement. Instead, the employer may have skipped the education requirement field and may have mentioned an education requirement in the full text of the job ad. To the degree that education requirements are more important to employers with higher skilled jobs, vacancies without an explicit education requirement are more likely to require low education. Overall, jobs explicitly requiring a four year degree or more represent 18% of jobs on CareerBuilder. As a comparison, in 2010, Current Population Survey data shows that the share of the labor force with a bachelor degree or more is 35%. Only 22% of vacancies post a wage (Table 1). The posted wage is reported in seven bins, and, when a wage range is offered, these bins are based on the upper bound of the offered wage. Among vacancies that post a wage, the median is \$50,000: indeed, 52% of vacancies post a wage of \$50,000 or more. This median wage is somewhat higher than the \$45,230 US average wage in 2011 (BLS Occupational Employment Statistics), but again this is based on the upper bound of the offered wage.

The CareerBuilder data spans September 2007 to July 2011. An individual application is defined as a person clicking on the “Apply Now” button in a job ad. The variable “Applications” is the number of individual applications received by all jobs in a given state and month.

The recorded applications are from unemployment benefit recipients, unemployed workers who do not receive benefits, and employed workers. According to CareerBuilder’s small applicant survey, just under half of the applicants are employed<sup>2</sup>. Therefore, given that applications by non-UI recipients constitute a substantial share of total applications, if we only had data on the applications of UI recipients, we would not be able to adequately quantify the impact of PBD on labor market tightness. Therefore, this data is uniquely suited to study the impact of PBD on labor market tightness.

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<sup>2</sup> This statistic is interesting but cannot be taken at face value since it is based on the selected sample of those applicants who were willing to answer the survey

To determine how many weeks of extended benefits are effectively available for each state and week, I use data from the Department of Labor EB and EUC trigger notices. I refer to these weeks of benefit as PBD, which stands for potential benefit duration. It needs to be emphasized that these numbers are upper bounds on the duration of UI, for two reasons. First, maximal durations are lower for some workers, in particular those who have only worked for a short period of time prior to becoming unemployed. Second, many job-seekers do find a job and do not receive the maximal possible duration of benefits. The trigger notices also contains the TUR, IUR and applicable look-back criteria. This allows me to determine when the conditions for each extension are realized. Since this data is at weekly frequency, I take the monthly average of the PBD, IUR and TUR to merge it with the monthly data on applications and vacancies.

To get additional information on jobs and jobseekers, I use data from the Bureau of Labor Statistics. First, I use data on the total number of unemployed people and the labor force. Second, I use data on vacancies and hires from the Job Openings and Labor Turnover Survey (JOLTS). JOLTS is a representative survey of establishments collecting information on job openings, hires, and separations.

Table 1 shows summary statistics for key variables. Notice in particular the large number of applications per state. The number of Careerbuilder applications is about twice as high as the number of unemployed individuals in the state, and about 30 times as high as Careerbuilder vacancies; on average, each vacancy receives about 30 applications per month. Other statistics on the unemployment rate are familiar. Figure 2 shows the evolution of the unemployment rate, job vacancies (from Job Openings and Labor Turnover Survey) and PBD at the national level. From September 2007 to July 2009, vacancies plummet, unemployment increases and at the same time PBD also increases. Subsequently, vacancies start increasing again, but unemployment barely decreases and PBD continues to increase until December 2009. This graph shows that PBD and unemployment are positively correlated. Given that PBD is increased when unemployment exceeds some thresholds, unemployment and PBD have to be positively correlated. However, this correlation should not be taken to imply that PBD increases unemployment. I will discuss in the section 3 how we can identify the causal impact of PBD on vacancies and job applications using an event study, a fuzzy regression discontinuity design and nonlinearities in PBD as a function of the unemployment rate (TUR).

## **Theoretical framework**

Given prior work, we know that an increase in PBD increases the unemployment duration of UI recipients (Katz and Meyer 1990; J. F. Schmieder, Wachter, and Bender 2012; Rothstein 2011; Farber

and Valletta 2013), and that this increase is likely to happen through a decrease in job search effort rather than an increase in the reservation wage (Card, Chetty, and Weber 2007; Krueger and Mueller 2011; J. Schmieder, von Wachter, and Bender 2012b). Therefore, we expect the number of job applications sent by UI recipients to decrease when PBD increases and, as a result, we expect the unemployment duration of UI recipients to increase: this is the micro or partial equilibrium effect. But what is the impact of a decrease in applications coming from UI recipients on the aggregate unemployment rate? In other terms, what is the macro or general equilibrium effect of unemployment insurance extensions?

If the number of vacancies stays the same, the decrease in applications by UI recipients will facilitate job finding by non-UI recipients (Levine 1993; Lalive, Landais, and Zweimüller 2013). Furthermore, a decrease in applications by UI recipients will also increase the job finding rate of UI recipients *per application*. However, a lower number of applications per vacancy may discourage vacancy creation. Moreover, if PBD increases the reservation wage, this will further discourage vacancy creation. Therefore, the general equilibrium or macro effect of an increase in PBD on the unemployment rate may be smaller or larger than the partial equilibrium or micro effect of PBD on the unemployment duration of UI recipients. The difference between the micro and macro effects depends on whether PBD has a greater impact on applications or vacancies.

Landais, Michailat, and Saez (2014) formalize this reasoning. They show that estimating the impact of unemployment insurance on labor market tightness is key to understanding the impact of UI on aggregate unemployment. The key variables in their model are as follows:

- $e$  is job search effort.
- $o$  are vacancies posted by each firm.
- $l = m(e, o)$  is employment at the representative firm. The matching function  $m$  has constant returns to scale.
- $\theta = o/e$  is labor market tightness.
- $f(\theta) = \frac{m(e, o)}{e} = m(1, \theta)$  is the rate of job finding per unit of search effort. Therefore, a jobseeker search with effort  $e$  finds a job with probability  $e \cdot f(\theta)$ .
- $1 - \eta = \theta \cdot f'(\theta)/f(\theta) > 0$  is the elasticity of  $f$ , the job finding rate by unit of search effort.
- $b$  are unemployment benefits.

- $\epsilon^m$  is the micro elasticity. It is the elasticity of unemployment probability of a worker whose individual benefits change.
- $\epsilon^M$  is the macro elasticity. It is the elasticity of aggregate unemployment when benefits change for all workers.
- $\epsilon_d$  is the discouraged worker elasticity. It measures the percent increase in search effort when the job-finding rate per unit of effort increases by 1%, keeping UI constant.

Landais, Michailat, and Saez (2014) demonstrate that, in a dynamic framework, the macro elasticity of aggregate unemployment with respect to unemployment benefits is given by:

$$\epsilon^M = \epsilon^m - l \cdot (1 + \epsilon^d) \cdot (1 - \eta) \cdot \frac{b}{\theta} \cdot \frac{d\theta}{db}$$

The term  $\frac{b}{\theta} \cdot \frac{d\theta}{db}$  in the formula above is the elasticity of labor market tightness with respect to unemployment benefits<sup>3</sup>. Since  $l \cdot (1 + \epsilon^d) \cdot (1 - \eta)$  is a positive number, it follows that the sign of the elasticity of labor market tightness with respect to unemployment benefits determines whether the elasticity of aggregate unemployment with respect to unemployment benefits is smaller or larger than the elasticity of individual unemployment duration with respect to unemployment benefits. Furthermore, Landais, Michailat, and Saez (2014) demonstrate that optimal unemployment insurance should be countercyclical if the elasticity of labor market tightness with respect to unemployment benefits is positive, and procyclical otherwise.

Now, in the case studied here, the generosity of unemployment benefits is measured by PBD. If PBD increases labor market tightness, then the impact of PBD on aggregate unemployment is lower than the impact of PBD on the unemployment of UI recipients, and the opposite is true if PBD decreases labor market tightness. Empirically, I measure aggregate job search effort by the number of applications on CareerBuilder. I will estimate the impact of PBD on log vacancies, log applications, and log applications given log vacancies. If the impact of PBD on log applications given log vacancies is negative, it implies that PBD increases labor market tightness as defined above ( $\theta = o/e$ , vacancies divided by job search effort), and the opposite is true if the impact of PBD on applications given vacancies is positive. Ultimately, if the impact of PBD on applications given vacancies is negative, then the general equilibrium

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<sup>3</sup> Landais, Michailat, and Saez (2014) write the macro elasticity as a function of the elasticity of labor market tightness with respect to the difference in consumption between employed and unemployed individuals. If we assume that the unemployed consume their unemployment benefit, this is the opposite of  $\frac{b}{\theta} \cdot \frac{d\theta}{db}$ , which is why a minus sign appears after the micro elasticity instead of the plus sign in the original formula.

impact of PBD on aggregate unemployment is lower than the micro impact of PBD on the unemployment of UI recipients.

### 3. Main results

#### Main results using an event study methodology

The event study methodology uses the timing of PBD increases at the state level to identify the impact of PBD on vacancies and applications. If PBD has a negative impact on applications, we expect the number of applications to significantly drop in the first month after a PBD increase. More generally, if PBD has an impact on the outcome of interest, then this outcome should vary over time in the same way as PBD at the state level. The key identification assumption is that there is no other variable that evolves according to the same monthly timing as PBD and determines the outcome of interest.

#### Event study methodology

I identify for each state the largest increase in PBD that is not due to a change in the benefit schedule<sup>4</sup> (i.e. does not occur in the first month where a new benefit schedule is in place, such as December 2008) nor to a temporary lapse in EUC<sup>5</sup>. I focus on the largest increase in PBD for two reasons. First, when there are at least two increases in PBD in a state, some months of observation are both after the first increase and before the second increase, which makes it difficult to uniquely assign these observations to a “before” or “after” period. In other terms, using multiple PBD increases is not straightforward. Second, given that it is more transparent to focus on just one increase in PBD, choosing the largest one should maximize power.

For each state, I only keep observations around the largest increase in PBD that do not involve any further change in PBD: as a result of this restriction, benefit duration is constant during the months prior to the largest PBD increase, and also constant during the months after the largest PBD increase. I then use the following specification to estimate the impact of the largest increase in benefit duration on various outcomes:

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<sup>4</sup> This is to allow for benefit duration increases to occur at different times for different states.

<sup>5</sup> During my sample period, there were two lapses in extended benefits due to legislators struggling to reach an agreement to further extend federal funding of the benefits. These two lapses were in April to July 2010 and in November-December 2010. Ultimately, any benefits lost from these lapses were reinstated once the legislative agreement was reached. I excluded EUC lapses because there seems to be little reaction of job applications to these EUC lapses (results not shown): presumably, jobseekers were expecting these EUC lapses to be temporary indeed.

$$y_{st} = \sum_{i=-8}^7 \beta_i D_{ist} + q_t + \delta_t + \gamma_s + \epsilon_{st}$$

where  $y_{st}$  is the outcome of interest in state  $s$  and month  $t$ .  $D_{ist}$  is a dummy equal to 1 in month  $i$  relative to the largest increase in PBD in a given state, where  $i = 0$  is the first month when the higher PBD is available during all weeks of the month. The dummy for month  $i = -2$  is the omitted category<sup>6</sup>.  $q_t$  is a quarter fixed effect to capture seasonality.  $\delta_t$  is a year fixed effect and  $\gamma_s$  is a state fixed effect. I cluster standard errors at the state level.

In this specification, identification comes from the timing of PBD increases in different states. The key assumption needed to identify the causal effect of PBD on job applications is that there are no other variables that follow the same time course as PBD around the maximum increase in PBD and affect the outcome of interest. This assumption is made more likely by the fact that these PBD increases occur at different times in different states and are mostly driven by states crossing thresholds in the state unemployment rate (TUR or IUR) that qualify them for additional weeks of benefits<sup>7</sup>.

### Results from the event study

I start with showing graphically how vacancies evolved around the largest increase in PBD. There is no significant trend in job vacancies around the largest increase in PBD (Figure 3, Panel A). This implies that, as far as seven months after the largest increase in PBD, employers do not respond by posting fewer vacancies.

I then examine the impact of PBD on applications (Figure 3, Panel B). While there is no significant trend in applications prior to the increase in benefit duration (in 5 out of 7 months, the level of applications is not statistically significantly different from the baseline period), there is a significant drop in applications during the first month (month 0) after the increase in benefit duration. Given that the average increase in benefit duration is 15 weeks, the estimated effect implies that a 1 week increase in PBD leads to a 0.4% decline in applications. This in turn implies that the median state saw a 29% decline in applications due to benefit extensions during the Great Recession (+73 weeks). Applications continue to decline after the first period post benefit increase, and the impact in the third month (month 2) is significantly higher

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<sup>6</sup> Because the data on applications is at the monthly level while PBD is defined weekly, the largest increase in benefit duration defined as above will typically occur over three months, with the first month having the lower potential benefit duration (PBD), the second month having a mix of lower and higher PBD, and the third month having the higher PBD. Therefore, I look for the largest increase in benefit duration relative to two months prior (e.g. May to July). I further exclude any month that contains a mix of weeks with high and low benefit levels.

<sup>7</sup> In a minority of states (20%), the largest increase is due to the state adopting the TUR option.

than the impact in the first month, implying that a 1 week increase in PBD leads to a 1% decline in applications three months after the increase. This decline in applications after the PBD increase is not due to sample selection: indeed, if we restrict the sample to states that are present in month 2 (23 states), the impact in month 2 is still significantly larger than the impact in month 0. It is likely that the decline in applications happens gradually because some jobseekers realize that PBD has increased later than others. This is quite plausible because most states (e.g. California) only notify UI recipients of their eligibility for extended benefits once they exhaust their regular benefits. Therefore, some UI recipients may become informed of a PBD extension quickly by reading the news or going to the state UI website, while others only become aware of the PBD extension once they exhaust their regular benefits. To summarize, I find that a PBD increase coincides with a significant decline in job applications in the very first month of the PBD increase, and that this decline deepens in following months.

Using the event study methodology, I have shown that an increase in PBD has a large, significant and negative impact on job applications. Since PBD does not affect the number of vacancies, I conclude that the increase in PBD led to an increase in labor market tightness: all other things equal, for each application sent, the probability of getting hired increases with PBD. This implies in particular that the job finding rate of non-UI recipients should increase with PBD.

In the next sub-section, I use an alternative identification strategy relying on sharp changes in PBD when a state crosses certain TUR thresholds.

### **Main results using a fuzzy regression discontinuity and extensions of this approach**

The first part of the analysis used an event study to identify the causal impact of PBD on vacancies and applications. However, in such a design, observations around the PBD increase are associated with different levels of the unemployment rate for different states. To address related concerns, I will now use a complementary approach: a fuzzy regression discontinuity design around a fixed state unemployment level (e.g. 6%). In the event study I looked at the evolution of applications around the time of a PBD increase. In the fuzzy regression discontinuity approach, I look at the evolution of applications as a function of state unemployment levels. Given that PBD increases when an unemployment rate threshold is crossed, I expect applications to decline to the right of the threshold. The key identification assumption is that there is no other variable that varies with the unemployment rate in the same way as PBD and affects applications.

To exploit the fact that PBD depends on states crossing TUR thresholds, I will follow two different strategies. First, I will use a fuzzy regression discontinuity design around 6% TUR. While this approach is conceptually appealing, it leaves me with only 44 observations and therefore limited statistical power. Second, I use nonlinearities in PBD as a function of TUR to identify the effect of PBD on applications in a panel regression. In this approach, I use a broader sample that includes all the 1804 observations lying in the zone where there are any discontinuities. This second approach has the highest power but also the highest potential bias due to the inclusion of observations that lie further away from TUR discontinuities.

### **Methodology: fuzzy regression discontinuity**

I now discuss in more detail the fuzzy regression discontinuity design. As discussed above (p. 7), crossing a threshold in the total unemployment rate (TUR) is much more likely to lead to an actual increase in PBD than crossing a threshold in the insured unemployment rate (IUR). I therefore focus on TUR thresholds. Among all the TUR thresholds, the one at 6% TUR for EUC is the best candidate for a regression discontinuity design because it applies to all states. Moreover, around 6% TUR, states would typically not have extended benefits (EB). Indeed, TUR must be above 6.5% in the states with a TUR option for EB to be turned on. By contrast, for 8.5% TUR, which is the other threshold for EUC, some states would have EB (due to the 8% TUR threshold), introducing additional interpretation issues. I will thus first focus on the EUC discontinuity around 6%.

Increases in PBD occur at specific TUR thresholds (Figure 4). The data in the figure is at the week by state level for December 2008 to October 2009, a period during which the PBD schedule as a function of TUR was constant. Prior to December 2008, EUC did not depend on TUR, and after October 2009, PBD increased at more TUR thresholds. Additionally, after October 2009, there were few states with TUR just below 6%, making that latter period unsuitable for a regression discontinuity design at 6%. If we focus on 1 percentage point around 6% TUR, i.e. 5.5% TUR to 6.4% TUR, we observe no clear discontinuity in PBD (Figure 4, blue hollow circles). This is because some states are eligible for EB or EUC under IUR conditions, which means that PBD can be at a higher level even when TUR is below 6%. Additionally, even when TUR is above 6%, some states have the lower PBD level because there is usually a delay between when a state crosses the threshold and when benefits become available. In order to apply a standard fuzzy regression discontinuity design, I drop state-week observations where any EB is available. I also drop state-week observations where TUR is below 6% and the IUR condition for EUC is satisfied. Substantively, what these exclusions do is drop the 15% of state-week observations in the 5.5% to 6.4%

TUR range where IUR is unusually high compared to TUR. This procedure leaves us with observations where TUR is between 5.5% to 6.4% and PBD is either 46 or 59 weeks (red diamonds in Figure 4).

Some states may value higher PBD and might want to manipulate their unemployment rate towards that objective<sup>8</sup>, which would result in bunching of observations at the threshold, potentially compromising the assumptions behind the fuzzy regression discontinuity design. However, I find no evidence for bunching at the 6% TUR threshold (Online Appendix, Figure 8).

The data from CareerBuilder is only available at the monthly level, even though PBD varies at the weekly level. I therefore use the monthly average of PBD, and only include months where TUR lies between 5.5% and 6.4% in *each and every* week. With monthly data on the outcomes of interest, the fuzzy regression discontinuity framework is embodied in the following instrumental variables regression:

$$y_{st} = \beta b_{st} + \alpha' X_{st} + \delta_t + \gamma_s + \epsilon_{st}$$

where  $y_{st}$  is log vacancies or log applications in state  $s$  and month  $t$ ,  $b_{st}$  is the PBD in weeks,  $X_{st}$  is a vector of controls which can include log job vacancies and a quadratic in TUR and IUR,  $\delta_t$  is a year fixed effect,  $\gamma_s$  is a state fixed effect, and  $\epsilon_{st}$  is the error term. Standard errors are clustered at the state level. Given that this is a fuzzy regression discontinuity design,  $b_{st}$  is instrumented with the PBD that should be in place given program rules. The instrument is therefore PBD such that extra weeks of benefit are made available by states without delay. This application of a fuzzy regression discontinuity design is similar to the work by Angrist and Lavy (1999). While it is typical to include the forcing variable (here TUR) as a control in a regression discontinuity design, I report results both with and without this control. Indeed, the TUR is endogenous in a regression with applications on the left-hand side: if jobseekers send in fewer applications, then, all other things equal, unemployment will increase. Finally, when applications are on the left-hand side, I also control for log vacancies since it is likely that the more vacancies there are, the more applications will be sent to these vacancies. Moreover, if PBD affects vacancies, controlling for vacancies yields the impact of PBD on applications for a given number of vacancies, therefore estimating the impact of PBD on (the inverse of) labor market tightness.

Using the EUC 6% TUR threshold is the best one can do with a single threshold in my sample, but this leaves us with only 44 state-month observations. A larger sample would be desirable, though it will not

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<sup>8</sup> Such manipulation would however be exceedingly difficult in practice since states do not control the data collection and calculation of the TUR by the Bureau of Labor statistics.

be possible to use the regression discontinuity framework strictly speaking. Figure 4 illustrates why this is the case: while states crossing TUR thresholds plays an important role in the determination of PBD, the relationship between PBD and TUR does not present very sharp discontinuities.

### **Methodology: nonlinearities in PBD as a function of TUR**

In order to gain statistical power relative to the fuzzy regression discontinuity framework, I can use the whole region that includes TUR thresholds and rely on the nonlinear relationship between TUR and PBD to identify the impact of PBD on TUR.

To visualize this nonlinear relationship graphically, one can plot a smoothed version of PBD and vacancies or applications residuals as a function of TUR. The vacancies or applications residuals are from regressing log vacancies or log applications on state fixed effects. I then apply a kernel-weighted local polynomial smoothing. Additionally, I restrict TUR to be between 4% and 10%, as to include some data prior to any TUR threshold being reached and past the highest TUR threshold, but yet not include data that is far away from the TUR thresholds that are my key source of identification. Second, I run an instrumental variable regression that corresponds to these plots and is similar to the fuzzy regression discontinuity specification, but uses a much larger sample. Specifically, I estimate the following specification, restricting to state-month observations with TUR between 4 and 10%:

$$y_{st} = \beta b_{st} + \alpha' X_{st} + \delta_t + \gamma_s + \epsilon_{st}$$

where  $y_{st}$  is log vacancies or log applications in state  $s$  and month  $t$ ,  $b_{st}$  is the average PBD over the month,  $X_{st}$  is a vector of controls which can include log job vacancies and a quadratic in TUR and IUR,  $\delta_t$  is a year fixed effect,  $\gamma_s$  is a state fixed effect, and  $\epsilon_{st}$  is the error term.  $b_{st}$  is instrumented with the PBD that should be available given rules and assuming that all states have elected the TUR option. Standard errors are clustered at the state level. The instrument for PBD is similar to the one used for the fuzzy regression discontinuity design, except that I take into account some additional factors that become relevant because this specification uses both EUC and EB variation and extends over broader time periods. First, the instrument does not take into account temporary EUC lapses. Second, since states can elect the TUR option and thus provide more generous EB extensions (see Policy background above), such EB benefits are potentially endogenous. For this reason, the PBD as calculated for the instrument assumes that all states have elected the TUR option for EB.

Since, as argued above, the TUR and vacancies controls are endogenous, I use specifications both with and without these controls. Finally, in a version of the regression above, I split  $b_{st}$  into a EUC and an EB component to analyze whether these two programs have the same impact of the outcomes of interest.

To summarize, I will use two approaches to exploit the nonlinear relationship between PBD and TUR. I first use a fuzzy regression discontinuity design at 6% TUR. Second, I use all the observations around the four TUR discontinuities and exploit the nonlinearities in PBD as a function of TUR. The fuzzy regression discontinuity design and its extension complement the event study and allow me to present credible alternative estimates of the impact of PBD on vacancies and job applications.

### **Results from the fuzzy regression discontinuity**

I start with a graphical analysis of the impact of PBD on vacancies using the discontinuity in PBD at 6% TUR. Figure 5, Panel A, plots log vacancies residuals and PBD as a function of TUR. The data is averaged over 0.1 percentage point TUR bins. I also plot a polynomial smooth of the residuals on both sides of the discontinuity. As we can see (triangles), there is a strong increase in average PBD around 6% TUR. This increase in PBD is associated with relatively stable job vacancies residuals (circles in Figure 5, Panel A). If anything, the number of vacancies increases when PBD is higher. On the other hand, the applications residuals (circles in Figure 5, Panel B) are lower to the right of the threshold, suggesting that the increase in PBD reduced the number of applications.

To support the graphical analysis of the impact of PBD on vacancies and applications from Figure 5, regression results for the 6% TUR EUC discontinuity are presented in Table 2 (for vacancies) and Table 3 (for applications). The impact of PBD on vacancies is positive and insignificant both without and with controls (Table 2, cols. 1 and 2).

By contrast, the impact of PBD on applications is negative and significant (Table 3, cols. 1 and 2): a 1-week increase in PBD decreases applications by 1.36%. If we compare this estimate to the results from the event study, a 1.36% decrease in applications corresponds to the impact of PBD on applications 4 months after the largest PBD increase. Indeed, in the event study, the estimated impact in month 3 (see Figure 3) is -0.21, which is for a 15 weeks increase in PBD: this implies that a one week increase in PBD decreases applications by 1.4%. Therefore, while the impact estimated by the fuzzy regression discontinuity design is large, it is within the range of what was found for the event study.

Given that vacancies and unemployment are also important potential determinants of applications, it is important to see how results change when we control for these variables, even though both of them are

endogenous. When controlling for job vacancies (col. 2 in Table 3), the impact of PBD on applications becomes larger and more significant. Finally, when controlling for both job vacancies and a quadratic in TUR, which is the forcing variable, I find an effect that is of the same magnitude than in the absence of controls but statistically insignificant. At the same time, the quadratic in TUR itself does not have a significant impact on applications.

Overall, the results from the fuzzy regression discontinuity approach are consistent with the results from the event study. Indeed, there is no significant impact of PBD on vacancies and a negative impact of PBD on applications. The results from the fuzzy regression discontinuity design therefore agree with the event study to show that increases in PBD increase labor market tightness.

### **Results using nonlinearities in PBD as a function of TUR**

I start with a graphical analysis of the data from the introduction of the TUR thresholds for EUC (Dec 2008 and later) to give an intuitive grasp of the dependency of vacancies residuals, applications residuals, and PBD on TUR (Figure 6). PBD indeed increases steeply around 6 to 6.5% TUR, which are the thresholds for EUC and EB respectively. PBD again increases around 8 to 8.5% TUR, which are the thresholds for EB and EUC respectively (note that the EUC threshold only exists after November 2009). Vacancies residuals tend to increase with TUR but not in a way that is strongly correlated with PBD (Figure 6, Panel A). The positive co-movement of vacancies and TUR may seem surprising because one would expect worse economic conditions to be associated with both higher unemployment and fewer vacancies. However, when one looks back at Figure 2, the positive association between vacancies and TUR makes sense: indeed, early in 2009, the number of vacancies started increasing again from its trough while at the same time the unemployment rate was still increasing. Overall, the graphical analysis suggests that PBD has little impact on job vacancies.

In contrast, application residuals tend to decrease over the same range of TUR values where PBD increases and increase with the unemployment rate (TUR) when PBD remains constant (Figure 6, Panel B). The fact that application residuals tend to increase with the TUR in the absence of a PBD increase should be expected given that a higher TUR means more unemployed jobseekers, who should be collectively sending more applications. This graphical analysis supports the view that, indeed, PBD has a strong negative effect on applications.

Prior to the introduction of EUC, PBD depended on TUR only for states that had the TUR option for EB. Therefore, we should see that PBD goes up and application residuals go down around TUR thresholds in

states with a TUR option for EB, but not so in states without such a TUR option. This is indeed the case (Online Appendix, Figure 9), which suggests that there is no mechanical relationship between application residuals and TUR. A clear negative relationship between applications and TUR only appears when PBD does depend on TUR.

I now use regression analysis to complement the graphical results, and check more systematically whether there is indeed a statistically significant negative impact of PBD on applications and no impact of PBD on vacancies. In Table 2 (cols. 3-6), I regress log vacancies on PBD and a number of controls, using the whole available time period (September 2007 to July 2011), but restricting to TUR between 4 and 10%. PBD is instrumented with PBD as determined by rules, not taking into account EUC lapses and assuming that all states have adopted the TUR option. I first repeat the equivalent of the fuzzy RD specifications (col. 3 and 4). In column 5, I use the same specification as in column 4, but separating weeks of PBD due to EUC from weeks of PBD due to EB. Finally, in column 6, I add months fixed effects (dummies for September 2007, October 2007, December 2007, etc.) to the specification from column 5. In specifications without date fixed effects (cols. 3-5), the impact of PBD on vacancies is negative and significant. However, this result is not robust to the addition of month fixed effects (column 6). This means that, after accounting for common macro factors that vary from month to month, there is no significant impact of PBD on vacancies, consistent with the results from the fuzzy RD design in columns 1 and 2.

Having examined the impact of PBD on vacancies, I now analyze the impact of PBD on applications using the same specifications (Table 3, cols. 4-7). In columns 4 and 5, which use specifications similar to the fuzzy RD but using a broader sample, I find a significant and negative impact of PBD on log applications. A one week increase in PBD yields a 0.62% decline in applications (col. 5). I then estimate separately the impact of EUC and EB on applications (columns 6 and 7). I find that the negative impact of EUC extensions on applications is much larger than the impact of EB extensions. In fact, the impact of EB is not statistically significant. The difference in the impact of EUC vs. EB may be explained by the fact that additional weeks of benefits due to EB only kick in after EUC extension weeks have been used up, and so discounting implies a smaller effect of EB. Furthermore, EB has somewhat stronger job search requirements than EUC, which could dampen its effect on job applications. Finally, I show that the estimated impact of EUC on applications is robust to adding month fixed effects and thus controlling for monthly variation in macro factors (col. 7). Overall, the results that exploit nonlinearities in PBD as a function of TUR imply a strong negative effect of PBD on job applications.

The point estimates in cols. 4-7 from Table 3 are higher than what I found from the event study using the first month post PBD increase (Figure 3), but lower than the point estimates in the fuzzy regression discontinuity regressions from cols 1-3 in Table 3. A one-week increase in PBD yields a 0.4% decline in applications using the first month from the event study, a 1.1% decrease in applications using the fuzzy RD estimate from Table 3 col. 3, and a 0.6% decline in applications in Table 3, col. 5. My preferred estimate, which is also the most conservative, is that a one week increase in PBD yields a 0.4% decline in applications, implying that the median state saw a 29% decline in applications due to PBD extensions during the Great Recession (+73 weeks).

I conclude that the increase in PBD during the Great Recession led to a substantial decline in applications. My preferred estimate indicates that a one-week increase in the duration of benefits yields a 0.4% decline in aggregate job search effort as measured by applications. On the other hand, the number of vacancies was not affected by changes in PBD. My results imply that unemployment insurance extensions during the Great Recession increased labor market tightness. Given the theoretical results from Landais, Michailat, and Saez (2014), a positive impact of unemployment insurance on labor market tightness implies that the general equilibrium impact of unemployment insurance on aggregate unemployment is smaller than its partial equilibrium impact on benefit recipients.

## **4. Robustness, further results and discussion**

### **The impact of PBD on job applications: robustness tests**

I have shown that PBD has a negative impact on applications given vacancies. In this section, I use alternative specifications to check the robustness of this result to alternative specifications. The specification from column 7 in Table 3, with full controls including month fixed effects will serve as the baseline specification for these robustness tests. The baseline specification is reported in the first column of Table 4.

My interpretation of the main results is that the decrease in aggregate job search effort as measured by applications comes from UI recipients decreasing their job search effort in response to an increase in PBD. To support this assumption, I test whether the impact of PBD on applications is greater in states that have a higher share of UI recipients among their unemployed. More specifically, I compute the average share of UI recipients in each state from December 2008 through the end of my sample. I use this time frame because the identifying variation in EUC as a function of TUR only exists when EUC levels change with TUR, which is after December 2008. I find that the impact of EUC PBD on applications is

significantly higher when the share of UI recipients in the state is higher (Table 4, col. 2). Reassuringly, the main impact of EUC PBD on applications, which corresponds to a 0 share of UI recipients among the unemployed, is not statistically significant. I conclude that the impact of PBD on applications is higher in states with a higher share of UI recipients among the unemployed, which is consistent with a negative impact of PBD on the applications of UI recipients.

A potential concern with my analysis is that the impact of PBD on applications may be biased by changes in the composition of posted jobs. To address this issue, I add controls for the share of jobs in each education category and each 2-digit NAICS industry (Table 4, col. 3). The impact of EUC PBD on applications is still significant and negative, and not statistically significantly different from the baseline estimate in column 1. I conclude that the negative impact of PBD on job applications is robust to changes in the composition of jobs by industry and education requirement.

Finally, I add state-specific trends to the specification in col. 1 to control for any systematic decline in applications over time that is unrelated to PBD increases (Table 4, col. 4). This lowers the impact of EUC on applications, but the point estimate is not statistically significantly different from baseline. At the same time, controlling for state specific trends increases the negative impact of EB on applications. The impact of EUC and EB when controlling for state trends is of similar magnitude, though only the impact of EUC is statistically significant. This suggests that PBD due to EB tends to increase just when applications also tend to increase, and so the increasing trend in applications may be masking the negative impact of EB on applications.

I have also performed these robustness tests using the number of vacancies as an outcome (not shown). Neither EUC nor EB have a significant impact on vacancies in these alternative specifications. The absence of an impact of PBD on vacancies is therefore robust.

### **The impact of PBD on job seeker selectivity and posted wages**

I have shown that PBD does not have a significant impact on the number of vacancies. If PBD increased reservation wages, we would expect PBD to also decrease the number of vacancies. In this subsection, I examine the impact of PBD on reservation wages using the same baseline specification as in Table 4.

If PBD increases reservation wages or other demands by UI recipients, this decreases the number of jobs suitable for application. To measure the number of jobs that job seekers consider, I use data on the number of times that a job vacancy snippet was viewed as part of a listing that comes out after a job search query. The number of applications per view is a measure of how selective jobseekers are: if

reservation wages or other demands go up with PBD, the number of applications per view should go down. Empirically, I find that PBD does not have a significant impact on applications per view (Table 5, col. 1). This suggests that PBD does not affect job seeker selectivity.

My finding that PBD does not affect applications per job view is consistent with the lack of evidence for an impact of PBD on the reservation wage found in previous literature (e.g. Card, Chetty, and Weber 2007; Krueger and Mueller 2011). At the same time, this result brings new light to the literature. Indeed, even if the reservation wage does not react to an increase in PBD, jobseekers could become more selective about other job characteristics besides the wage. My results are inconsistent with the idea that PBD increases jobseeker selectivity on non-wage job characteristics. Instead, my results suggest that an increase in PBD chiefly affects job search effort with no significant impact on jobseeker selectivity on either wage *or* non wage job attributes.

While the evidence on applications per view is inconsistent with PBD increasing reservation wages, firms may believe that PBD increases reservation wages. If that is the case, posted wages may increase as an equilibrium response. I first examine the impact of PBD on the log average real posted wage among vacancies with a posted wage. To calculate the average posted wage, I take the mid-point of each bin, and for the highest bin (over \$500,000), I assume that the posted wage is \$600,000; I then use the CPI to convert nominal wages to real wages. I find no effect of the PBD on average real posted wages, and the point estimate is even negative (Table 5, col. 2). I then examine the impact of PBD on the distribution of nominal posted wages: I find no impact of PBD on the share of jobs with a posted wage above \$50,000, the share of jobs with a posted wage below \$30,000 and the share of jobs with a posted wage above \$100,000 (Table 5, cols. 3-5). Finally, given that not all jobs post a wage, there is a concern that PBD may affect the share of jobs that post a wage, and that the absence of an impact of PBD on posted wages may be driven by selection. However, I also find no impact of EUC PBD on the share of jobs with a posted wage (Table 5, col. 6). There is evidence for EB PBD diminishing the share of posted wages, but EB PBD does not have an impact on the number of vacancies (Table 2, col. 5-6), the number of applications (Table 3, col. 6-7) or the level of posted wages, so this result is not particularly enlightening. Overall, I conclude that PBD has no impact on the level or the distribution of posted wages. The absence of an impact of PBD on posted wages is consistent with the absence of an impact of PBD on reservation wages and job seeker selectivity more generally.

In this sub-section, I have shown that PBD does not have an impact on job seeker selectivity nor on posted wages. These results are consistent with the absence of an effect of PBD on the number of vacancies and thus strengthen my main results.

### External validity

One may wonder whether applications on CareerBuilder.com are a good measure of search effort. Unemployment benefit recipients during the Great Recession spent between 24 and 38% of their job search time sending applications and answering job ads (Krueger and Mueller, 2011). Additionally, 27% of job search time is spent browsing job ads. Nowadays, the overwhelming majority of vacancies are posted on the Internet (Barnichon 2010). Furthermore, Internet job search is associated with higher job finding rates than offline job search (Kuhn and Mansour 2011). Therefore, it is plausible to think that more than two thirds of job search time is spent on the Internet and that this is an effective method for job finding.

One can also analyze more directly the relationship between job applications on CareerBuilder and other measures of aggregate job search effort. More specifically, I analyze the relationship between job applications on CareerBuilder and time spent on job search in the American Time Use Survey (ATUS). This survey is a representative survey of time use among Americans, and has been used by one of the few other papers investigating the impact of unemployment insurance on job search effort, that is Krueger and Mueller 2010. However, a big limitation of the ATUS is that it has very few respondents that report any positive amount of time spent on job search. Given this limitation, I aggregate all applications in my CareerBuilder dataset at the state level, to obtain one observation per state. In the ATUS, I calculate total time spent in job search at the state level by adding up any reported time spent job searching within the time frame of my CareerBuilder data, that is September 2007 to July 2011 (I use ATUS weights to obtain representative estimates). Figure 7 shows a very strong positive correlation between the number of applications on CareerBuilder and the time spent in job search across states. This supports the assumption that applications on CareerBuilder are a good proxy for aggregate search effort.

While I have shown that the increase in PBD led to a substantial decrease in applications, one may wonder whether this decline was due to a decrease in applications on the CareerBuilder website only. If so, it is possible that jobseekers applied through other channels and so the overall applications in the state may not have decreased as much as it seems. This scenario is very unlikely, because such changes in jobseekers' use of the CareerBuilder website are unlikely to coincide exactly with the timing of UI

extensions. Still, I provide some additional evidence on this issue. First, one can show that the number of applications on CareerBuilder is significantly and positively related to hires in national monthly data from JOLTS (not shown). If applications on CareerBuilder were substituted by applications through other channels, this relationship would likely not be significant. Additionally, when graphing hires and applications between 2007 and 2011, one does not see that applications fall behind hires in relative terms as time goes by, as would be the case if applications in CareerBuilder had represented a lower and lower share of total applications (not shown).

Second, I present independent evidence showing that jobseekers are unlikely to have moved away from Internet job search during the Great Recession. Indeed, while there is no representative survey about online job search in the US during the Great Recession, a study using quarterly labor force survey data from the United Kingdom (Green et al. 2011) shows that there was an increased use of Internet for job search purposes among jobseekers during 2008-2009. In April to June 2009, over 4 in 5 British jobseekers used the Internet to look for jobs, and this proportion was even higher among the recipients of unemployment benefits (Green et al. 2011). Therefore, there is no reason to assume that jobseekers moved away from online job search during the recession. I conclude that my results cannot be explained by jobseekers moving away from CareerBuilder.com in order to apply elsewhere.

## Discussion and interpretation

### Long-run effects vs. short run effects

My analysis captures the short to medium term effects of PBD on applications and vacancies. Indeed, the event study methodology captures the impact of the largest increase in PBD up to seven months out. When I use a fuzzy regression discontinuity design or nonlinearities in PBD as a function of TUR, the estimated impacts are also only short to medium term. Indeed, we are comparing state-month observations with essentially the same TUR but different levels of PBD: because the unemployment rate is slow-moving and I have a short panel, the observations have to be close together in time in order for TUR to stay similar.

Since PBD does not affect the number of vacancies but decreases the number of applications, I conclude that PBD increases labor market tightness. Given that my results only pertain to the short and medium run, how plausible is it that PBD actually *decreases* labor market tightness in the longer run? The event study (Figure 3, Panel A) shows that the number of vacancies stays constant as far as seven months after the largest increase in PBD. Furthermore, the impact of a PBD increase on applications tends to get

significantly more negative over time (Figure 3, Panel B). Therefore, the event study suggests that the impact of PBD on labor market tightness *increases* with time elapsed since a PBD increase. This suggests that at time horizons of the order of a year, the impact of PBD on labor market tightness is likely to remain positive.

The broader policy question is whether it is desirable to increase the generosity of unemployment insurance during recessions. My findings suggest that, for time horizons of the order of a year, the general equilibrium impact of a PBD increase on aggregate unemployment is smaller than its partial equilibrium impact. Given prior estimates showing that the partial equilibrium effect of PBD on unemployment is small (e.g. Rothstein 2011; J. F. Schmieder, Wachter, and Bender 2012; Farber and Valletta 2013), increasing PBD at the beginning of a recession has relatively small costs in terms of aggregate unemployment. In the next sub-section, I examine more precisely how large the general equilibrium impact of PBD on unemployment may be given my empirical estimates and the theoretical results from Landais, Michaillat, and Saez (2014).

### Implied impact of PBD on aggregate unemployment

As discussed on page 11, Landais, Michaillat, and Saez (2014) demonstrate that the macro elasticity of aggregate unemployment with respect to unemployment benefits is given by:

$$\epsilon^M = \epsilon^m - l \cdot (1 + \epsilon^d) \cdot (1 - \eta) \cdot \frac{b}{\theta} \cdot \frac{d\theta}{db}$$

Some of the parameters in the formula are calibrated by Landais, Michaillat, and Saez (2014) when they discuss empirical evidence about the difference between the macro and the micro elasticity:

- $\eta = 0.7$
- $\epsilon^d = 0$

Furthermore, if we take  $l$  (employed workers in the representative firm) to be one minus the unemployment rate, then, given that the average unemployment rate is 7.38% in my sample, we have:  $(1 - \eta)l = (1 - 0.7) * (1 - 0.0738) = 0.2779$ . Let  $\beta$  be the impact of a one-week increase in PBD on log applications. Remember that labor market tightness is vacancies divided by aggregate job search effort, so the impact of PBD on log applications is the opposite of the impact of PBD on log labor market

tightness<sup>9</sup>. The elasticity of labor market tightness with respect to unemployment insurance is given by  $\beta \Delta PBD / (\Delta PBD / PBD)$ , where the numerator  $\beta \Delta PBD$  is the percent impact ( $\beta$ ) of the PBD increase ( $\Delta PBD$ ) on applications, and the denominator  $\Delta PBD / PBD$  is the percent increase in PBD. Given that, in the median state, PBD went from 26 to 99 weeks, the elasticity of labor market tightness with respect to unemployment insurance is given by  $\beta \Delta PBD / (\Delta PBD / PBD) = 26\beta$ . Given all of these assumptions, the macro elasticity of aggregate unemployment with respect to PBD is:

$$\epsilon^M = \epsilon^m - 0.2779 * 26\beta = \epsilon^m - 7.2254\beta$$

My paper does not estimate the micro elasticity of unemployment with respect to PBD. Instead, I rely on previous estimates: the most relevant here is from Rothstein (2011) and Farber and Valletta (2013), since they use the same extensions. Rothstein (2011) estimates that a one-week increase in benefits increases unemployment duration by 0.0037% (Table 3, column 3-5), which translates into an elasticity of  $\epsilon^m = 0.1$ . This elasticity is the same as the one found by Farber and Valletta (2013). Importantly, this estimate of  $\epsilon^m = 0.1$  is essentially the same as the estimate from the US extended benefits program studied by Card and Levine (2000), and the same as the estimates from the regression discontinuity design in Schmieder, Wachter, and Bender (2012a), which range between 0.12 and 0.13. To get an upper bound of the macro elasticity of unemployment with respect to PBD, I also use the highest elasticity estimate reported by Meyer (2002), which is 0.5 from Katz and Meyer (1990).

For the impact of PBD on labor market tightness,  $\beta$ , my preferred estimate of  $\beta = -0.004$  is from the first month following the PBD increase in the event study; as discussed on page 21, this is a conservative estimate. Therefore, I also use the higher estimate of  $\beta = -0.0136$  from the regression discontinuity design specification in column 1 of Table 3.

If we use the micro elasticity estimate  $\epsilon^m = 0.1$  from Rothstein (2011) and Farber and Valletta (2013), and  $\beta = -0.004$ , the macro elasticity of aggregate unemployment with respect to PBD is  $\epsilon^M = 0.07$  (Table 6, first row). If I use a higher estimate for the impact of PBD on log applications,  $\beta = -0.0136$ , the macro elasticity of aggregate unemployment with respect to PBD becomes essentially nil at  $\epsilon^M = 0.002$  (Table 6, second row). The macro elasticity could be of the order of 0.4 if we take the highest existing estimate of the micro elasticity (Table 6, third and fourth row). However, given that Rothstein (2011) uses essentially the same variation in PBD as I do, the most plausible calibration for the

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<sup>9</sup> Given that PBD does not affect the number of vacancies, the impact of PBD on applications is enough to determine the impact on labor market tightness. In practice,  $\beta$  is rather larger when vacancies are taken into account, so my preferred estimate from the event study is conservative.

micro elasticity comes from his study. Therefore, I conclude that the macro impact of PBD on aggregate unemployment during the Great Recession is very small and could indeed be nil.

Given the most plausible calibration, the medium run general equilibrium impact of PBD on aggregate unemployment is very small. The intuition is the following: after an increase in PBD, UI recipients reduce their number of applications substantially, which has a small positive effect on their unemployment duration (the micro elasticity is small) and a large positive effect on labor market tightness. The large positive impact of PBD on labor market tightness increases the job finding rate per application for both UI recipients and non recipients, and this negative effect of PBD on aggregate unemployment ends up being of the same order of magnitude as the positive impact of PBD on the unemployment duration of UI recipients.

The idea that a PBD-induced decrease in search effort could have no effect on aggregate unemployment may seem counterintuitive. Yet a previous paper studying a job search assistance experiment came to the same conclusion (Crépon et al. 2013). Indeed, in that paper, when a large proportion of job seekers received job search assistance, this had a negative impact on the job finding of those who did not receive job search assistance. Furthermore, the negative impact of the program on the job finding rate of the non recipients was of the same order of magnitude as the positive impact on the job finding rate of the recipients. The general equilibrium effect of job search assistance on job finding is negative, though zero is included in the confidence interval. Therefore, job search assistance had little impact on aggregate unemployment. Since PBD reduces job search effort by UI recipients, it makes sense that PBD had little effect on aggregate unemployment during the Great Recession.

There are at least two caveats to the interpretation that PBD has little impact on aggregate unemployment. First, as mentioned above, my estimates should be seen as medium run effects, so it is possible in theory that the impact of PBD on aggregate unemployment becomes negative in the longer run. This is in fact what happens in the theoretical model by Mitman and Rabinovich (2011), though the mechanisms in that paper are somewhat different from what I described here based on Landais, Michailat, and Saez (2014). Nonetheless, Mitman and Rabinovich (2011) find that optimal PBD should first increase and then decrease during a recession. The initial increase in PBD reduces aggregate unemployment (Mitman and Rabinovich 2011). Therefore, there seems to be agreement over the fact that an increase in PBD early on in a recession has little impact on unemployment, and can even contribute to reducing unemployment. However, how exactly the impact of PBD on aggregate unemployment evolves over the course of a recession remains an open area for empirical investigation.

The second caveat to the interpretation that PBD has little impact on aggregate unemployment is that the decrease in applications may not reduce labor market tightness as much as it seems. Indeed, the theoretical framework used here assumes that every applicant has the same probability of getting hired, so a decrease in the number of applications leads to a proportional increase in the probability of getting hired. However, if PBD reduces applications by candidates that are less hireable from the point of view of the firm, the impact of PBD on labor market tightness is smaller than the impact of PBD on applications.

So how plausible is the idea that PBD reduces applications by less hireable candidates? On the one hand, there is evidence that long-term unemployed job seekers are less likely to get a call back from employers than short-term unemployed and employed job seekers (Kroft, Lange, and Notowidigdo 2013). Therefore, to the extent that the increase in PBD mostly decreased the applications of the long-term unemployed, the increase in PBD may have somewhat increased the average quality of the applicant pool from the point of view of the firm, and therefore the decrease in applications may overestimate the increase in labor market tightness. On the other hand, the evidence that PBD does not affect job seeker selectivity is not consistent with PBD increasing the quality of applications. Therefore, it is not clear that there is PBD reduces applications by less hireable candidates, and therefore it is hard to conclude that the macro elasticity of unemployment with respect to PBD is underestimated based on my calibration.

Given my estimates and calibration results, the macro impact of unemployment insurance extensions is small, and certainly smaller than the micro impact. By contrast, Hagedorn et al. (2013), based on their own estimates and calibration results, conclude that the macro effects of unemployment insurance extensions are large, and larger than the micro effects. The differences in our conclusions can be pinned down to differences in identification strategy, and in the assumed impact of unemployment insurance extensions on search intensity.

Hagedorn et al. (2013) identify the impact of the extensions by comparing unemployment and vacancies in adjacent counties belonging to different states while accounting for permanent differences between county pairs. As explained by Hall (2013), “the central issue of identification is that any random upward shift in state-wide unemployment triggers more generous UI benefits. To the extent that the in-state county in a pair shares in that upward shift while the out-of-state adjacent county does not, Hagedorn et al. (2013)'s estimation strategy will overstate the causal role of UI benefits”. Essentially, Hagedorn et al. (2013) assume that changes over time in the across-county difference in economic outcomes are explained by differential changes in PBD. By contrast, in the event study, I assume that within state

changes in job search outcomes across a couple of months can be attributed to sharp changes in PBD. When I use nonlinearities in PBD as a function of unemployment for identification, I assume that differences in job search outcomes within state when the state experiences a marginal change in the unemployment rate can be attributed to sharp changes in PBD.

The second major difference between this paper and Hagedorn et al. (2013) is that I explicitly account for the impact of UI on job search effort while Hagedorn et al. (2013) do not. Indeed, Hagedorn et al. (2013) measure tightness by the vacancies to unemployment ratio. However, prior empirical evidence shows that increases in the generosity of unemployment insurance do not affect the reservation wage (e.g. Card, Chetty, and Weber 2007; Krueger and Mueller 2011). This prior evidence together with my results showing that PBD decreases job applications but does not affect job seekers' selectivity implies that unemployment insurance decreases job search effort. Since the job search effort of unemployment benefit recipients falls in response to an increase in PBD, the number of unemployed people overestimates aggregate labor supply, and therefore the vacancies to unemployment ratio underestimates labor market tightness. This is the reason why I use applications rather than the number of unemployed people to measure aggregate labor supply and ultimately labor market tightness. All other things equal, the impact of PBD on labor market tightness is smaller if one measures labor supply by the number of unemployed as in Hagedorn et al. (2013) rather than the number of applications as I do here. The difference in the treatment of the impact of PBD on job search effort is therefore one plausible and important reason why Hagedorn et al. (2013) estimates the impact of PBD on labor market tightness to be smaller (more negative) than the impact calibrated in this study.

Overall, we can conclude, first, that the macro impact of PBD on aggregate unemployment is smaller than the micro impact of PBD on the unemployment of UI recipients in the medium run. This first conclusion is based on the negative impact of PBD on applications controlling for vacancies. Second, the calibration performed above suggests that PBD had little impact on aggregate unemployment in the medium run. Overall, I conclude that the general equilibrium effect of PBD is smaller than the partial equilibrium effect on UI recipients, and that PBD has likely had little impact on aggregate unemployment in the medium run.

## **5. Conclusion**

This paper has used state-level variation in the potential unemployment benefit duration during the Great Recession to investigate the general equilibrium impact of unemployment insurance. I measure

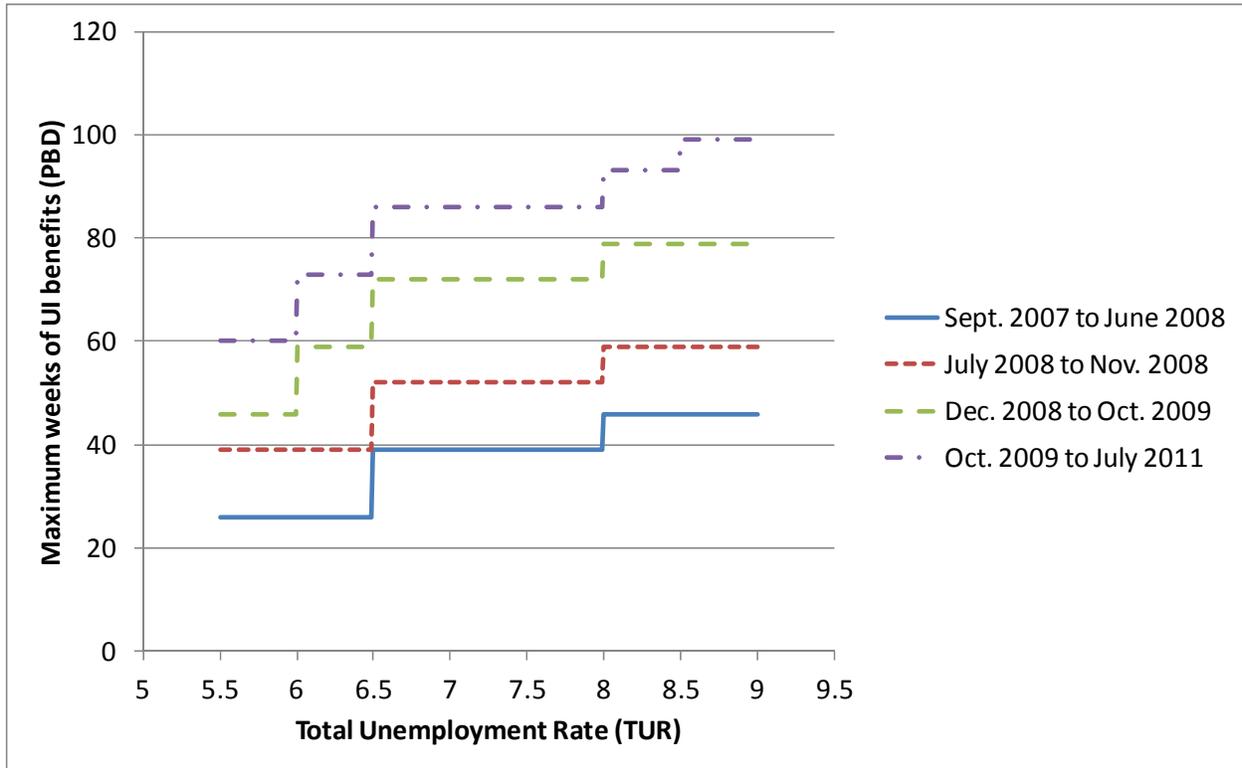
aggregate job search effort by the number of applications received by jobs on CareerBuilder.com, the largest American employment website. I show that, according to a conservative estimate, a one-week increase in the potential duration of unemployment benefits leads to a 0.4% decline in applications. By contrast, PBD does not affect the number of vacancies. My findings imply that the elasticity of labor market tightness with respect to unemployment insurance is positive. Given a calibration based on the framework from Landais, Michailat, and Saez (2014), my findings imply that the general equilibrium effect of unemployment insurance is smaller than the partial equilibrium effect. Furthermore, my estimates taken together with the framework from Landais, Michailat, and Saez (2014) suggest that increasing the generosity of unemployment insurance at the beginning of recessions is welfare improving. However, further empirical investigation is needed to quantify the longer run impacts of unemployment insurance extensions.

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Figure 1: Simplified summary of the regulations governing the total duration of unemployed benefits in states with a TUR option for Extended Benefits (EB)



Note: Both EUC and EB are taken into account to calculate the maximum duration of UI benefits. The total unemployment rate (TUR) is, for each month and state, the average of the weekly three-months seasonally adjusted unemployment rate. This figure assumes that PBD only depends on the current level of TUR, while in fact PBD also depends on the past level of TUR and on the current and past levels of the insured unemployment rate (see text for more details).

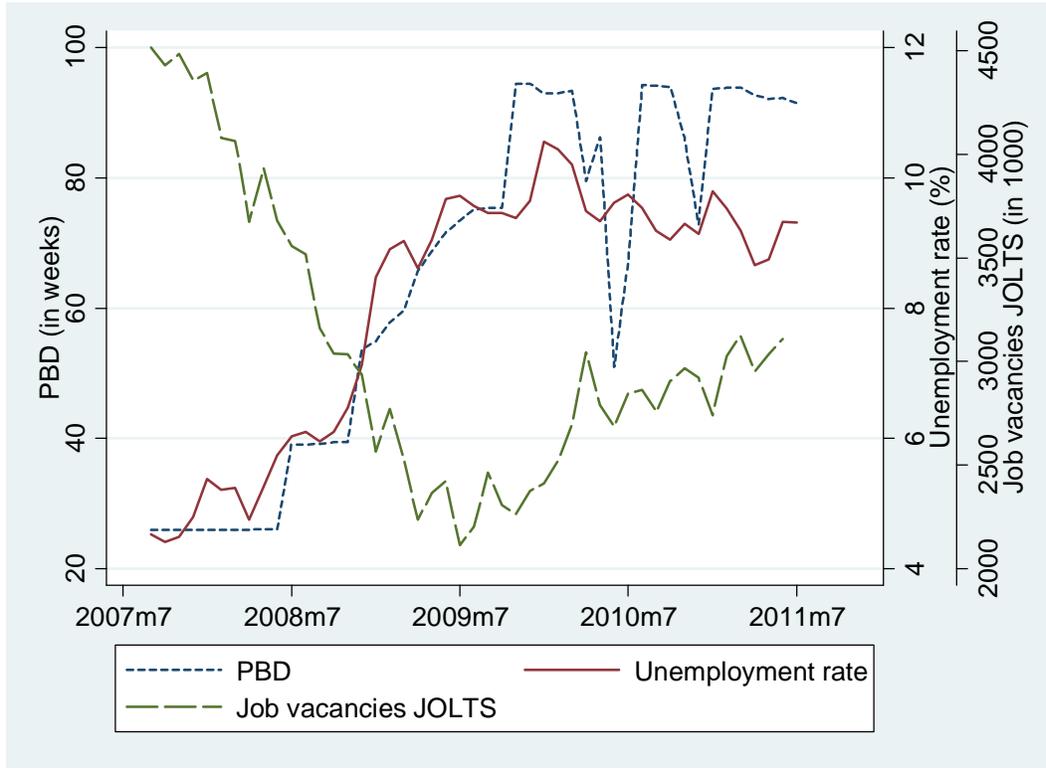
Table 1: Summary statistics

	N	mean	sd	min	max
Applications	2,397	431,725	584,975	2,424	3.179e+06
Vacancies	2,397	13,357	15,455	438	123,090
Job views (snippet)	2,397	7.560e+07	9.336e+07	1.547e+06	6.528e+08
Educ. required: high school	2,397	0.137	0.032	0.047	0.358
Educ. required: 2-year degree	2,397	0.051	0.025	0.007	0.241
Educ. required: 4-year degree	2,397	0.160	0.049	0.043	0.463
Educ. required: graduate degree	2,397	0.020	0.017	0.001	0.593
Educ. required: not specified	2,397	0.633	0.061	0.267	0.857
Share of wages over \$50000 (if posted)	2,397	0.522	0.110	0.134	0.902
Share of wages under \$30000 (if posted)	2,397	0.254	0.096	0.022	0.808
Share of wages over \$100000 (if posted)	2,397	0.161	0.079	0.017	0.784
Share of jobs with a posted wage	2,397	0.216	0.062	0.049	0.433
PBD in weeks	2,397	61.90	26.12	26	99
Total Unemployment Rate (TUR) (%)	2,397	7.138	2.557	2.325	15.20
Insured unemployment rate (IUR) (%)	2,397	3.042	1.326	0.303	7.870

Note: PBD is determined by weeks of UI available in the BLS trigger notices. The total unemployment rate is, for each month and state, the average of the weekly three-months seasonally adjusted unemployment rate. The insured unemployment rate is the 13-weeks insured unemployment rate.

Source: Department of Labor, and CareerBuilder.com for applications and vacancies.

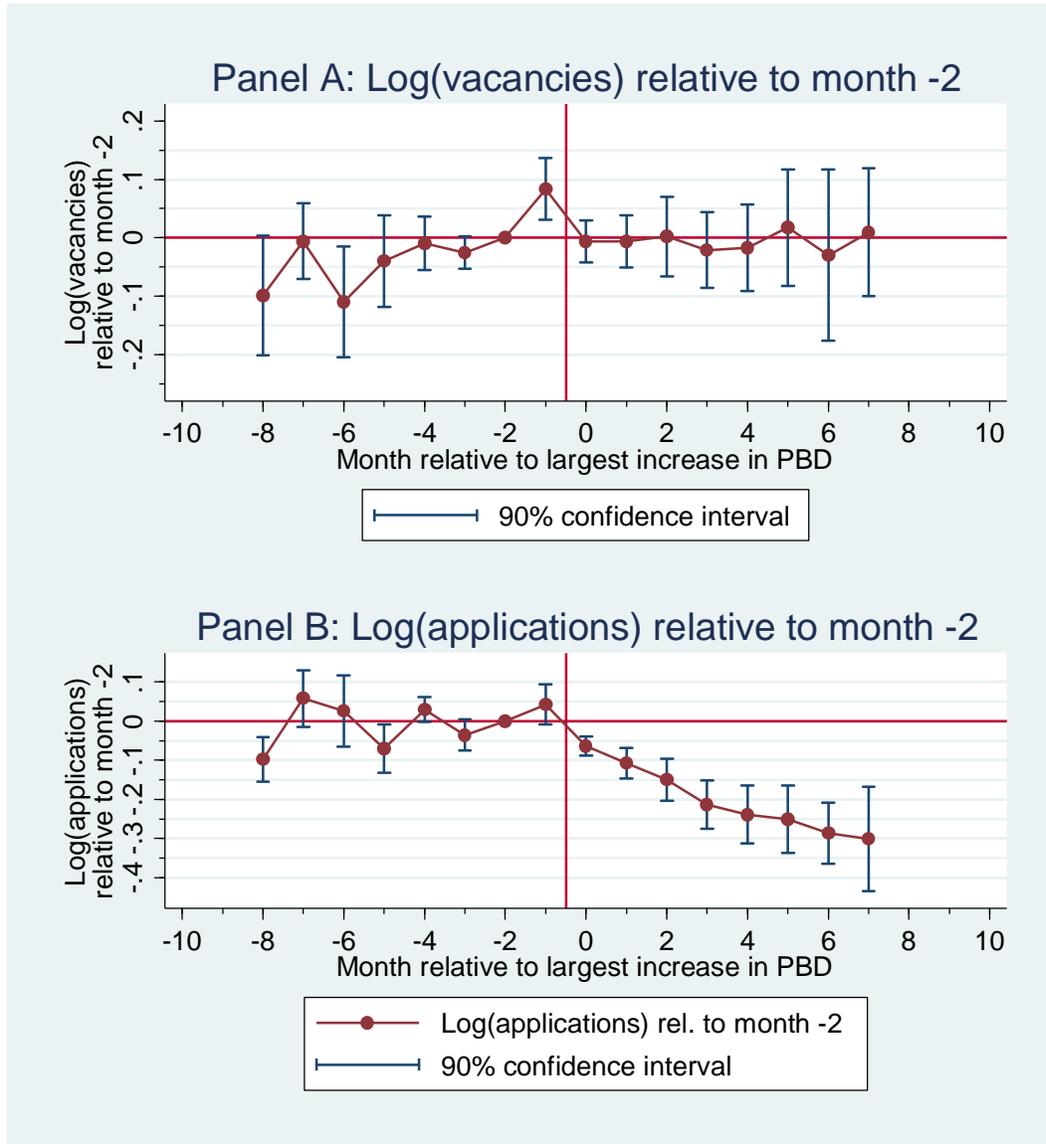
Figure 2: The evolution of the unemployment rate, job vacancies and potential unemployment benefit duration at the national level



Note: Potential unemployment benefit duration (PBD) is calculated as an average over states, where each state is weighted according to the size of its labor force.

Source: Department of Labor.

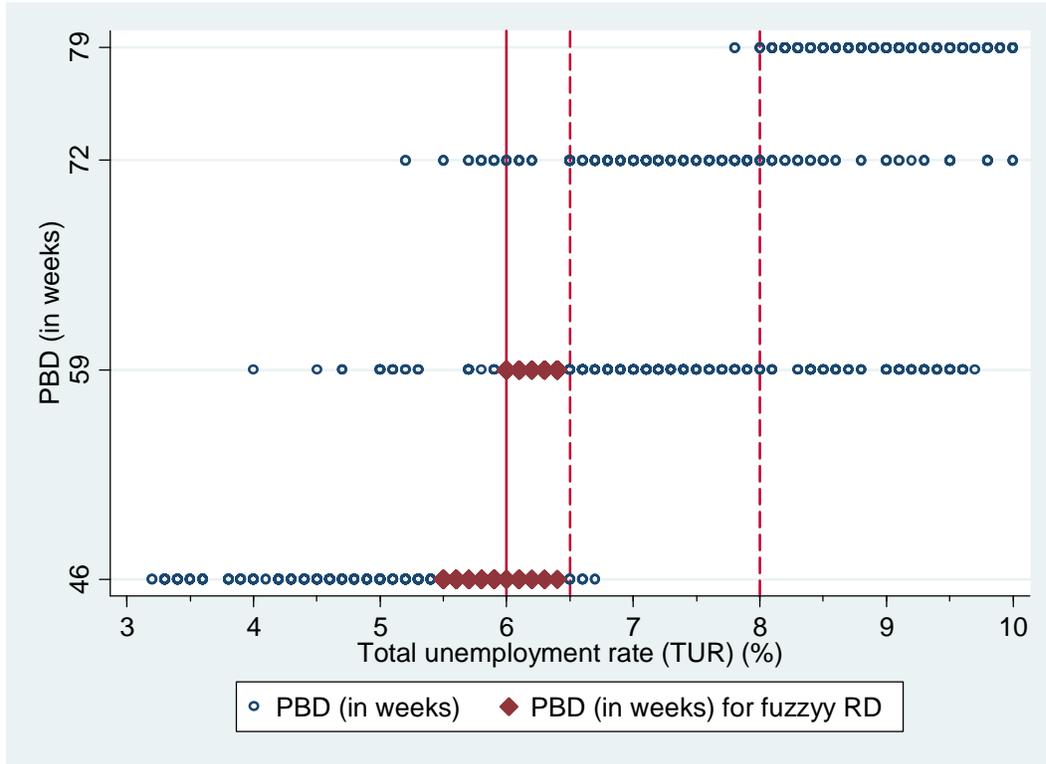
Figure 3: The impact of potential unemployment benefit duration on log applications and log vacancies, event study



Note: The estimates are from a regression of log vacancies (Panel A) or log applications (Panel B) on relative month dummies, state, year and quarter of the year fixed effects. Standard errors are clustered by state. There are 318 observations. The largest increase in PBD is defined, for each state, as the largest increase that does not correspond to a change in the schedule, and is not due to a temporary interruption of EUC.

Source: Department of Labor, and CareerBuilder.com for applications and vacancies.

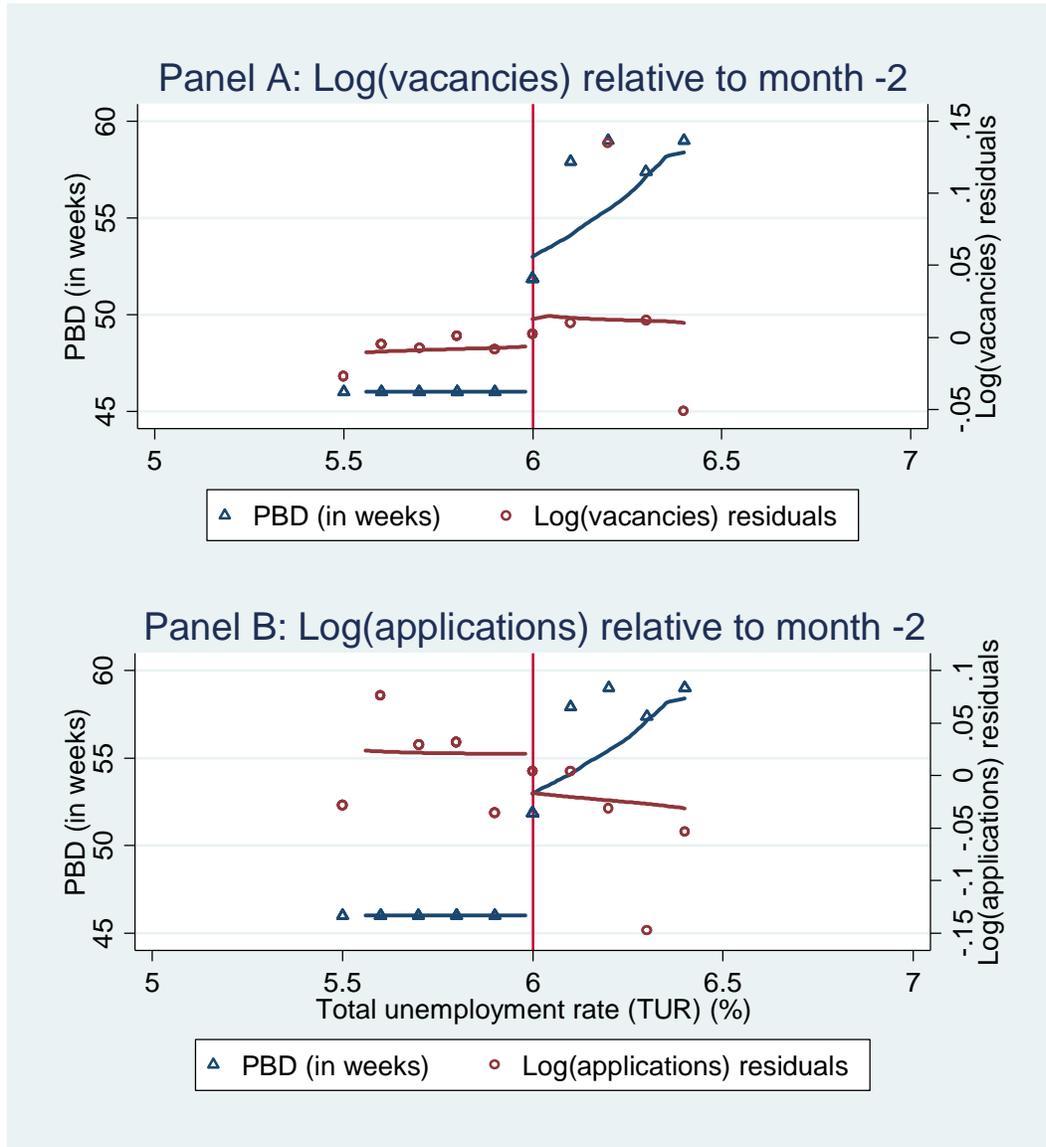
Figure 4: Potential unemployment benefit duration as a function of TUR, weekly level data, December 2008 to October 2009



Note: Each circle corresponds to a state and week. The total unemployment rate is the weekly three-months seasonally adjusted unemployment rate. The diamonds correspond to the states and weeks that are retained for the fuzzy regression discontinuity graphs and regressions. The solid vertical line corresponds to the 6% TUR threshold for EUC. The dashed vertical lines correspond to the 6.5% and 8% thresholds for EB.

Source: Department of Labor.

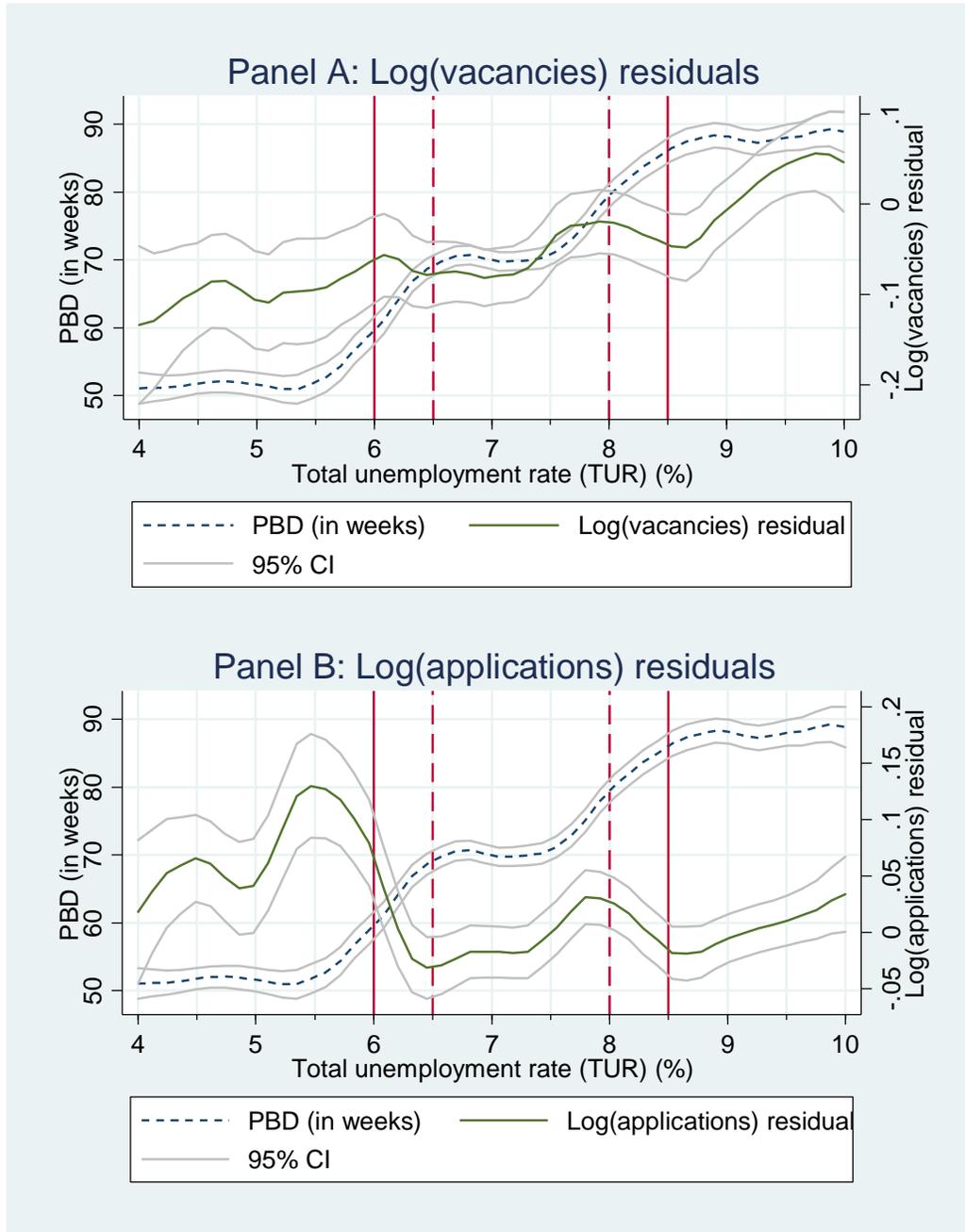
Figure 5: RD graph for the impact of EUC on log vacancies and log applications around 6% unemployment rate (TUR), December 2008 to October 2009, monthly data



Note: The vacancies (Panel A) and applications (Panel B) residuals are obtained after regressing respectively log vacancies and log applications on state and year fixed effects. The sample is restricted to points that are valid to estimate the impact of the discontinuous change in PBD at 6% TUR due to EUC regulations between December 2008 and October 2009 (see text for more details). The total unemployment rate is, for each month and state, the average of the weekly three-months seasonally adjusted unemployment rate. The applications residuals and the PBD is averaged over 0.1 percentage point TUR bins.

Source: Department of Labor, and CareerBuilder.com for applications.

Figure 6: Potential unemployment benefit duration, log vacancies and log applications as a function of the total unemployment rate (Dec. 2008 onwards)



Note: Data is smoothed using kernel-weighted local polynomial smoothing with a 0.2 bandwidth. The applications residual is obtained after regressing log applications on state fixed effects. The total unemployment rate is, for each month and state, the average of the weekly three-months seasonally adjusted unemployment rate. The two solid vertical lines correspond to 6% TUR (threshold for EUC2 when it was conditional, and for EUC3), and 8.5% TUR (threshold for EUC4). The two dashed vertical lines correspond to 6.5% TUR (threshold for 13 weeks extended benefits when the TUR option is active) and 8% TUR (threshold for 20 weeks extended benefits when the TUR option is active).

Source: Department of Labor, and CareerBuilder.com for applications.

**Table 2: The impact of potential unemployment benefit duration on log vacancies**

	6% TUR EUC discontinuity		4%≤TUR≤10%			
	(1)	(2)	(3)	(4)	(5)	(6)
Total PBD in weeks	0.0064 (0.0094)	0.0102 (0.0123)	-0.0050*** (0.0008)	-0.0077*** (0.0010)		
EUC PBD in weeks					-0.0093*** (0.0009)	0.0008 (0.0021)
EB PBD in weeks					0.0021 (0.0031)	0.0013 (0.0028)
Total Unemployment Rate (TUR) (%)		4.2366 (4.0899)		-0.0368 (0.0525)	0.0003 (0.0476)	-0.0433 (0.0449)
TUR <sup>2</sup>		-0.3625 (0.3506)		0.0059* (0.0033)	0.0013 (0.0030)	0.0020 (0.0028)
Insured Unemployment Rate (IUR) (%)				-0.1184*** (0.0345)	-0.0943** (0.0371)	-0.0869** (0.0352)
IUR <sup>2</sup>				0.0068 (0.0043)	0.0024 (0.0046)	0.0014 (0.0043)
Month fixed effects						X
Observations	44	44	1,804	1,804	1,804	1,804
R-squared	0.9962	0.9964	0.9790	0.9793	0.9810	0.9906

Robust standard errors clustered by state in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

All regressions include state and year fixed effects.

Note: Instrumental variables regressions. The instrument is PBD according to statutory rules, not taking into account temporary EUC expirations, and assuming that all states take up the TUR option. In columns 1-2, the sample is restricted to points that are valid to estimate the impact of the discontinuous change in PBD at 6% TUR due to EUC regulations between December 2008 and October 2009 (see text for more details). In columns 3-6, the sample includes any observation for which TUR is between 4% and 10%.

Source: Department of Labor, and CareerBuilder.com for vacancies.

**Table 3: The impact of potential unemployment benefit duration on log applications**

	6% TUR EUC discontinuity			4%≤TUR≤10%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total PBD in weeks	-0.0136*	-0.0197***	-0.0123	-0.0066***	-0.0062***		
	(0.0076)	(0.0054)	(0.0115)	(0.0006)	(0.0014)		
EUC PBD in weeks						-0.0075***	-0.0074***
						(0.0011)	(0.0028)
EB PBD in weeks						0.0004	0.0028
						(0.0065)	(0.0069)
Log(vacancies)		0.9539***	0.8903***		0.2653***	0.2427***	0.2773***
		(0.2594)	(0.2626)		(0.0347)	(0.0294)	(0.0762)
Total Unemployment Rate (TUR) (%)			2.5424		0.0108	0.0353	0.0717
			(3.0270)		(0.0629)	(0.0491)	(0.0545)
TUR <sup>2</sup>			-0.2258		0.0002	-0.0028	-0.0037
			(0.2486)		(0.0038)	(0.0030)	(0.0033)
Insured Unemployment Rate (IUR) (%)					-0.0199	-0.0061	0.0343
					(0.0449)	(0.0465)	(0.0508)
IUR <sup>2</sup>					0.0017	-0.0011	-0.0063
					(0.0060)	(0.0064)	(0.0061)
Month fixed effects							X
Observations	44	44	44	1,804	1,804	1,804	1,804
R-squared	0.9972	0.9985	0.9988	0.9924	0.9933	0.9934	0.9961

Robust standard errors clustered by state in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

All regressions include state and year fixed effects.

Note: Instrumental variables regressions. The instrument is PBD according to statutory rules, not taking into account temporary EUC expirations, and assuming that all states take up the TUR option. In columns 1-3, the sample is restricted to points that are valid to estimate the impact of the discontinuous change in PBD at 6% TUR due to EUC regulations between December 2008 and October 2009 (see text for more details). In columns 4-7, the sample includes any observation for which TUR is between 4% and 10%.

Source: Department of Labor, and CareerBuilder.com for applications and vacancies.

**Table 4: The impact of potential unemployment benefit duration on log applications: robustness**

	Baseline regression (1)	Interaction with share UI recipients (2)	Controlling for industry and required education (3)	State-specific trends (4)
EUC PBD in weeks	-0.0074*** (0.0028)	-0.0029 (0.0037)	-0.0058** (0.0024)	-0.0030** (0.0014)
EB PBD in weeks	0.0028 (0.0069)	-0.0064 (0.0088)	0.0038 (0.0058)	-0.0050 (0.0032)
EUC PBD in weeks*share UI recipients among unemployed		-0.0149* (0.0082)		
EB PBD in weeks*share UI recipients among unemployed		0.0205 (0.0171)		
Log(vacancies)	0.2773*** (0.0762)	0.2851*** (0.0721)	0.2752*** (0.0580)	0.1853*** (0.0449)
Total Unemployment Rate (TUR) (%)	0.0717 (0.0545)	0.0733 (0.0609)	0.0505 (0.0328)	0.0195 (0.0436)
TUR <sup>2</sup>	-0.0037 (0.0033)	-0.0038 (0.0039)	-0.0035 (0.0027)	-0.0009 (0.0029)
Insured Unemployment Rate (IUR) (%)	0.0343 (0.0508)		0.0046 (0.0398)	-0.0394 (0.0283)
IUR <sup>2</sup>	-0.0063 (0.0061)		-0.0019 (0.0053)	0.0055 (0.0033)
Controls for composition of vacancies by required education			X	
Controls for composition of vacancies by 2-digit NAICS industry			X	
State-specific trends				X
Observations	1,804	1,804	1,804	1,804
R-squared	0.9961	0.9960	0.9966	0.9979

Robust standard errors clustered by state in parentheses  
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1  
All regressions include state and month fixed effects.

Note: Instrumental variables regressions. The instrument is PBD according to statutory rules, not taking into account temporary EUC expirations, and assuming that all states take up the TUR option. The sample includes any observation for which TUR is between 4% and 10%. Column 1 is taken from Table 3, column 7. In column 2, the share of UI recipients is, for each state, the average share of UI recipients among all unemployed between December 2008 and the end of the sample; the share of UI recipients (not interacted with PBD) is controlled for in the regression. In column 3, controls are included for the share of vacancies in each education category (see summary statistics Table 1), and for the share of vacancies in each 2-digit NAICS code.

Source: Department of Labor, and CareerBuilder.com for applications and vacancies.

**Table 5: The impact of potential unemployment benefit duration on applications per view and on posted wages**

	Applications per view	Log real posted wage	Posted wage>=50000	Posted wage<=30000	Posted wage>=100000	Share with posted wage
	(1)	(2)	(3)	(4)	(5)	(6)
EUC PBD in weeks	-0.0013 (0.0013)	-0.0005 (0.0030)	-0.0010 (0.0018)	0.0010 (0.0013)	0.0007 (0.0009)	0.0008 (0.0005)
EB PBD in weeks	0.0011 (0.0026)	0.0018 (0.0025)	0.0004 (0.0017)	-0.0011 (0.0014)	0.0004 (0.0010)	-0.0012* (0.0007)
Total Unemployment Rate (TUR) (%)	0.0042 (0.0292)	-0.0078 (0.0400)	0.0128 (0.0258)	0.0070 (0.0165)	-0.0078 (0.0143)	-0.0002 (0.0098)
TUR^2	0.0008 (0.0022)	0.0004 (0.0026)	0.0000 (0.0015)	-0.0004 (0.0010)	0.0003 (0.0010)	0.0002 (0.0007)
Insured Unemployment Rate (IUR) (%)	0.0528*** (0.0182)	0.0840*** (0.0269)	0.0504*** (0.0172)	-0.0491*** (0.0145)	0.0136 (0.0099)	-0.0152 (0.0110)
IUR^2	-0.0058*** (0.0022)	-0.0092*** (0.0031)	-0.0064*** (0.0022)	0.0053*** (0.0017)	-0.0022** (0.0011)	0.0010 (0.0013)
Observations	1,804	1,804	1,804	1,804	1,804	1,804
R-squared	0.9068	0.7859	0.7244	0.7634	0.7316	0.7954

Robust standard errors clustered by state in parentheses

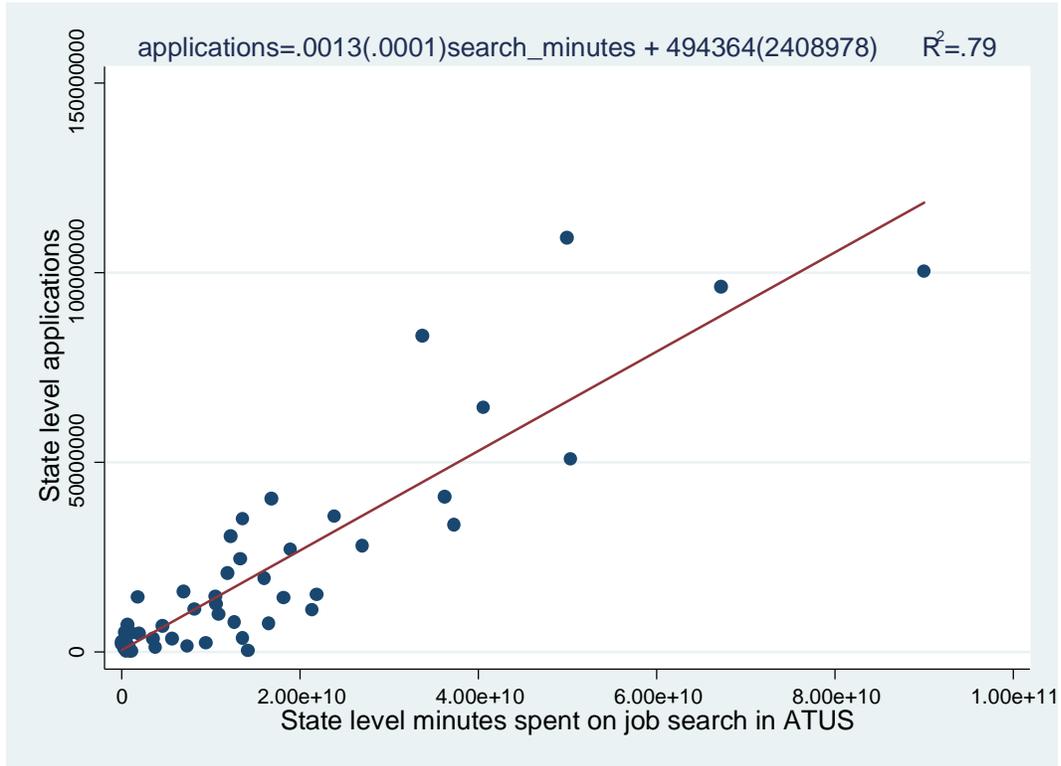
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

All regressions include state and month fixed effects.

Note: Instrumental variables regressions. The instrument is PBD according to statutory rules, not taking into account temporary EUC expirations, and assuming that all states take up the TUR option. The sample includes any observation for which TUR is between 4% and 10%. In columns 3-5, the dependent variable is the share of vacancies that have a posted wage respectively above \$50000, below \$30000 and above \$100000; the denominator for the share is all vacancies with a posted wage.

Source: Department of Labor, and CareerBuilder.com for applications and vacancies.

Figure 7: Applications on CareerBuilder.com and job search durations in the American Time Use Survey (state-level data)



Note: One observation by state. Total minutes spent in job search are calculated using the ATUS between September 2007 and July 2011, by state. ATUS weights are used to get a representative estimate of time spent in job search at the state level.

Source: CareerBuilder.com for applications, American Time Use Survey 2007-2011 for job search minutes.

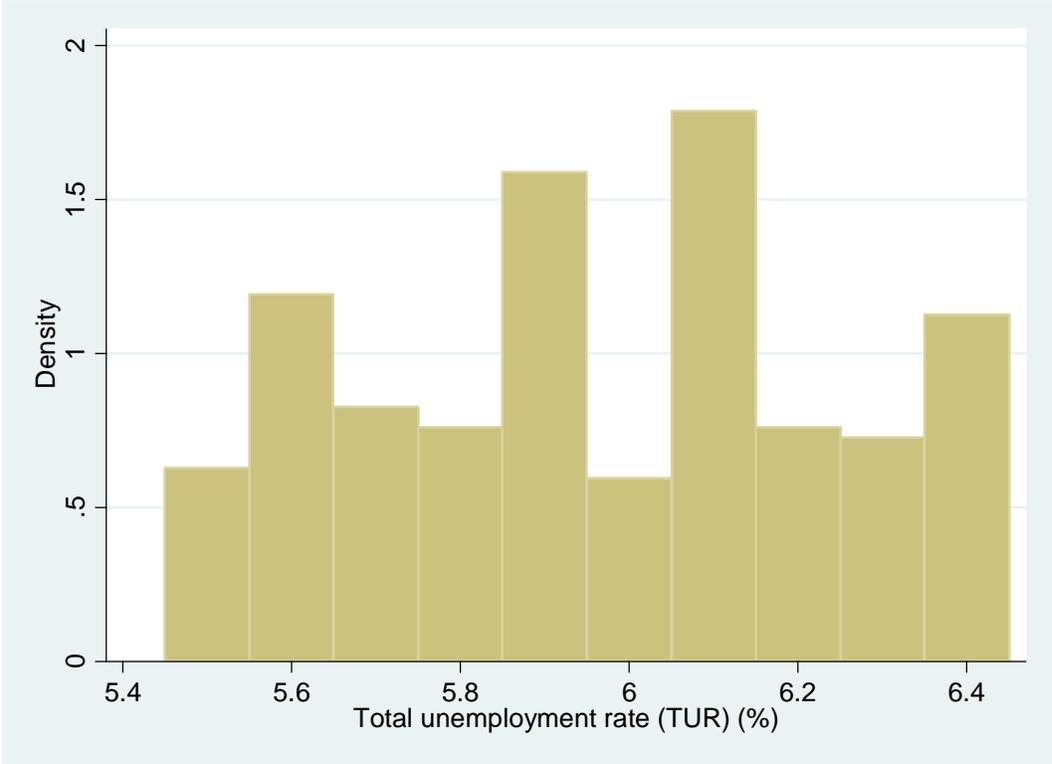
**Table 6: Calibration of the macro elasticity of unemployment with respect to potential benefit duration (PBD)**

<b>Estimate of micro elasticity of unemployment with respect to PBD</b>	<b>Estimate of elasticity of applications with respect to PBD</b>	<b>Calibrated macro elasticity of unemployment with respect to PBD</b>
Rothstein (2011). A one-week increase in benefits increases unemployment duration by 0.0037% (Table 3, column 3-5), which translates into an elasticity of 0.1 for a PBD increase going from 26 to 99 weeks.	Figure 3, event study, first month after PBD increase: estimate is -0.004	0.071
Rothstein (2011). A one-week increase in benefits increases unemployment duration by 0.0037% (Table 3, column 3-5), which translates into an elasticity of 0.1 for a PBD increase going from 26 to 99 weeks.	Table 3, column 1, based on regression discontinuity design: -0.0136	0.002
Meyer (2002). Highest estimate is 0.5 in Table 5, from Meyer (1990) and Katz and Meyer (1990)	Figure 3, event study, first month after PBD increase: estimate is -0.004	0.471
Meyer (2002). Highest estimate is 0.5 in Table 5, from Meyer (1990) and Katz and Meyer (1990)	Table 3, column 1, based on regression discontinuity design: -0.0136	0.402

Note: The calibration is based on the formula on page 27.

**ONLINE APPENDIX: not for publication**

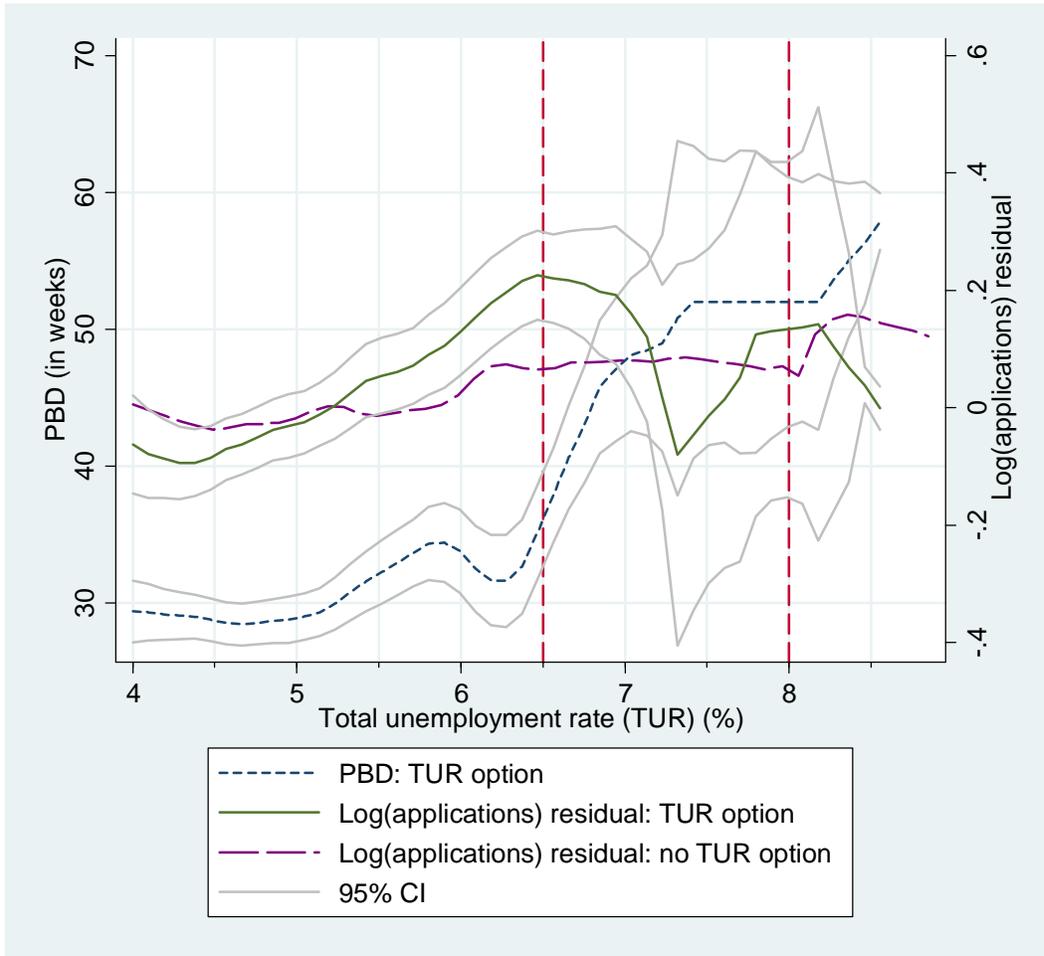
Figure 8: The distribution of the total unemployment rate (TUR) in the fuzzy RD sample, weekly level data



Note: The total unemployment rate is the weekly three-months seasonally adjusted unemployment rate.

Source: Department of Labor.

Figure 9: Potential unemployment benefit duration and applications residuals as a function of the total unemployment rate (before Dec. 2008)



Note: Data is smoothed using kernel-weighted local polynomial smoothing with a 0.2 bandwidth. States that had a TUR option before Dec. 2008 are the following: Alaska, Connecticut, Kansas, New Hampshire, New Jersey, New Mexico, North Carolina, Oregon, Rhode Island, Vermont, Washington. The applications residual is obtained after regressing log applications on state fixed effects. The total unemployment rate is, for each month and state, the average of the weekly three-months seasonally adjusted unemployment rate. The two dashed vertical lines correspond to 6.5% TUR (threshold for 13 weeks extended benefits when the TUR option is active) and 8% TUR (threshold for 20 weeks extended benefits when the TUR option is active).

Source: Department of Labor, and CareerBuilder.com for applications.