Do Temporary Extensions to Unemployment Insurance Benefits Alter Search Behavior? The Effects of the Standby Extended Benefit Program in the United States

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Abstract

During the 2007-2010 recession, the United States temporarily increased the duration of unemployment insurance by 76 weeks, higher than any prior extension. This paper examines the effect of temporary benefit extensions using a Regression Discontinuity approach that addresses the endogeneity of benefit extensions and labor market conditions. Using data from the 1991 recession, the results indicate that the Stand by Extended Benefit (SEB) program has a significant, although somewhat limited, impact on county unemployment rates and the duration of unemployment. The results suggest that the temporary nature of SEB benefit extensions may mitigate their effect on search behavior.

Keywords: unemployment insurance, unemployment, regression discontinuity

JEL Classification Numbers: J65, J68

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1 Introduction

This paper examines the effects of temporary extensions to U.S. unemployment insurance (UI) during recessions on search behavior. During the 2007 - 2010 recession, a combination of programs have temporarily increased the duration the unemployed could receive UI benefits by 73 weeks, to a total of 99 weeks.[1] While benefit extension have been provided in every recession over the past half century, this extension has been exceptionally large. These extensions increase the duration of UI benefits in the United States above that of Sweden and Norway's, countries known for their generous social benefits (Nickell, 1997). Theoretical (Mortensen, 1977) and empirical (Nickell, 1997; Blanchard and Wolfers, 2000) evidence suggests that such an extension lowers job search intensity and increases the wages workers are willing to accept. As a result, the current extensions may be a significant factor in the recent increase in the U.S. unemployment rate to above 10%.

The particular design of UI benefit extension programs in the U.S., however, may mitigate their effect on search behavior. In the U.S., there are two types of programs that extend the duration of UI benefits during recessions. The first is through Congress enacting Emergency Extended Benefit (EEB) programs, which are unique to each recession, some of which tie the generosity of the extension to local unemployment rates. The second program, and the focus of this study, is the Standby Extended Benefit (SEB) program. This program requires no congressional action and automatically provides 13 additional weeks of benefits in states with unemployment rates above certain thresholds. These extensions differ from permanent UI entitlements in two important ways: (1) the availability of the extension is unpredictable given that it is tied to future unemployment rates, and (2) information on whether or not an extension is available may not be known at the start of one's unemployment spell. As a result of these two factors, it is not clear that the effects

[1] The UI duration of 99 weeks during the 2007 - 2010 recession consists of 26 weeks from the regular UI program, a maximum extension available under the 2008 Emergency Unemployment Compensation program of 53 weeks, an extension of 13 weeks under the Stand-by Extended Benefit program available in some states, and 7 weeks provided by the American Reinvestment and Recover ActU.S. Department of Labor (2010).

of a temporary benefit extension would be the same as a permanent change in UI benefit duration.

This paper is the first to focus solely on a temporary extensions, while controlling for the endogeneity between the extension and local labor market conditions using a Regression Discontinuity (RD) approach. What is currently known about the effects of increasing benefit duration comes from two important sets of studies. The first exploits the variation in UI benefit duration that the SEB program, EEB programs, and state variation in regular UI provide, to determine the effects of greater benefits on the length of unemployment spells. Examples include Moffitt and Nicholson (1982), Moffitt (1985), Katz and Meyer (1990), which find that such increases in UI duration increase the amount of time individuals spend unemployed. While increasing our understanding of the effect of temporary extensions, these papers largely set aside an important econometric issue. Poor labor market conditions result in both longer unemployment spells and higher unemployment rates, which trigger benefit extensions under the SEB program and also prompt congressional action to enact EEB programs. As Moffitt (1985) notes "The effects of these benefit extensions are potentially confounded by the effects of the business cycle itself" (86). The second set of studies, which includes Card and Levine (2000), Lalive et al. (2006), Lalive (2007), and Caliendo et al. (2009), confront this issue by using a quasi-experimental design. While they also find a positive effect on the length of unemployment, these studies focus on extensions that are permanent, or have fixed start and end dates.

This paper employs a RD design to estimate the causal effect of the SEB program on county unemployment rates during the recession of the early 1990s. By design, the average unemployment rate and overall labor market conditions in counties within states that do and do not have access to SEB benefits will be very different. However, counties immediately along the border separating states with and without access to this program likely have very similar labor markets. As such, this this border serves as an exogenous decision rule determining which counties are affected by the SEB benefit extension based on their relative geographic position. This strategy is most similar to that of Holmes's (1998) work on right-to-work laws. The RD design identifies the causal effect of the SEB program by estimating the difference in unemployment rates that occurs on each side of this border. The results indicate that the program has a statistically significant positive effect on county unemployment rates.

To compare the results to other papers that focus on the duration of unemployment, I use a simple flow model of the unemployment rate. The equation translates the causal effect of the SEB program on unemployment rates to the effect of the program on the average duration of unemployment spells. My estimates appear to be at the lower end of other studies that also use a quasi-experimental approach. This may be because they focus on permanent benefit extensions that do not have the unpredictability that is a part of the SEB program. The causal effect is also at the low end of estimates from studies that use samples that cover SEB and EEB programs. This may be due to the RD approach this paper employs which accounts for the endogeneity between benefit extensions and labor market conditions. While caution should be taken in generalizing, the results suggest that the temporary nature of the extensions provided in the 2007 - 2010 downturn may mitigate its effects on the unemployment rate and average duration of unemployment.

The remainder of the paper is organized as follows: Section 2 provides a background on extension programs in the United States, the theoretical framework, and a discussion of prior estimates of the effects of UI generosity. Section 3 describes the empirical and estimation approaches. Section 4 discusses the sample and Section 5 provides support the use of a quasi-experimental approach. Section 6 presents both graphical and econometric evidence of the effects of the SEB program on county unemployment rates and Section 7 develops the relationship between the SEB program's effect on the unemployment rate and the effect on average duration of unemployment. Section 8 concludes.

2 Background on Benefit Extension Programs and Research

There has been a long tradition in the United States of extending unemployment insurance benefits during economic downturns. These benefit changes, and others abroad, have provided researchers ample opportunities to study the effects of the maximum potential benefit duration. This section discusses these programs and the theoretical and empirical evidence of their impact.

2.1 U.S. Unemployment Insurance Benefit Extensions

The U.S. unemployment system is characterized by three tiers: (1) the permanent regular unemployment insurance system, (2) Emergency Extended Benefit programs enacted in each recession and (3) the automatic Stand-by Extended Benefit program which is the focus of this study. Each tier differs in their generosity, when the benefit is available, and when state UI agencies communicate the entitlement to recipients.

During periods of stable and low unemployment, only benefits under the regular unemployment insurance system are available. States determine the maximum allowable potential duration of benefits, the method used to determine potential duration on an individual basis[2] and the benefit calculation within certain federal guidelines.[3] Upon filing a claim, a state workforce agency will communicate to the worker their benefit amount and the duration, for which they will draw benefits, and are not subject to change during the course of the claimant's unemployment spell. While variation in benefits exists across and within states, the benefit entitlement typically includes a payment approximately equal to half of the worker's wages to be paid for 26 weeks.

During each recession since 1958, the U.S. Congress has passed separate pieces of legislation to extend unemployment insurance (Vroman et al., 2003). The legislation for each program specifies the magnitude of the extension and benefit amount, as well as when the program will start and expire. In addition, it is common for these programs to link the magnitude of the extension available to state unemployment rates. The public at large typically learns of EEB programs from the press and recipients learn of their individual eligibility when they exhaust their regular UI benefits. Since there are finite start and end dates, the availability of benefits under an EEB program are fairly predictable. However, because the initial passage of an EEB program is unpredictable and Congress usually acts to extend the expiration date of EEB programs, it is possible that the total duration of benefits will change during one's unemployment spell during periods

[2] In a small number of states, the same duration is provided to all individuals. However, for a majority of states, the maximum number of weeks available is a function of a person's earnings or employment history. (Woodbury, 1995).

[3] The benefit calculation is a percentage of a defined period's wage, given that the resulting benefit amount falls within state minimum and maximums (Woodbury, 1995).

when EEB programs are in effect.

Under the SEB program, benefit entitlement can also change unpredictably and information about whether the benefit is available is often not provided to the recipient until they exhaust their regular UI benefits. The SEB program provides a benefit amount equal to a recipient's regular UI benefits for a duration equal to fifty percent of their UI benefit entitlement up to 13 weeks. The unpredictability arises from a complex "trigger" system. A state triggers on SEB benefits if the insured unemployment rate (IUR) [4] is greater than 5 percent and has increased 20 percent over the prior two years.[5] States could also employ a trigger that turns on when the IUR is in excess of 6 percent with no relative increase (Vroman and Woodbury, 2004).[6] The trigger remains on in a state for at least 13 weeks. After 13 weeks, the trigger turns off three weeks after the unemployment rate no longer meets any of the threshold levels.

SEB payments can not be made during periods when a state's trigger is off, which has the practical effect of making the total duration of benefits available to an UI claimant unpredictable. For instance, if an individual initially becomes unemployed during a period where their state's trigger is on, they would be eligible for 39 weeks of UI benefits only if the state's trigger status remains constant. If the state's SEB triggers turns off due to the unemployment rate no longer meeting the unemployment rate threshold levels, claimants would not receive further SEB payments even if they had not exhausted, or even begun to receive, their 13 week benefit extension. In addition, a worker who exhausts their regular UI entitlement in a period where a state trigger is off can suddenly begin to receive UI benefits again if the unemployment rate rises above the threshold level.

Information about the SEB extension may also not be as well known as other tiers of the UI system. Similar to EEB programs, one often first learns about additional benefits available through the press. Work-

[4] The insured unemployment rate for the purpose of triggering on extended benefits is the 13 week average of the number of UI recipients divided by covered unemployment in the first four of the last six completed quarters (Vroman and Woodbury, 2004).

[5] States trigger on two weeks after they first reach the trigger threshold (Wenger and Walters, 2006).

[6] Prior to 1981, an insured unemployment rate greater than 4.0% at the state level and 20.0% higher than the prior two years was required to turn on the trigger. Alternatively, at the national level, a 13 week average IUR greater than 4.5% would also trigger the SEB program. In 1992, a total unemployment rate trigger greater than 6.5%, and exceeding the same rate in the previous two years by 10.0%, was added (Vroman and Woodbury, 2004).

force agencies of states that triggered on benefits in 2009 anecdotally reported that their press releases when they triggered on SEB benefits receive a good deal of coverage from the local media. EEB programs, in contrast, typically receive much greater coverage and often at the national level. For instance, the debate in Congress and on the presidential campaign trail regarding the EEB program that passed on June 30, 2008 was given fairly broad coverage at the national level for at least six months before it was signed into law.

Beyond media attention, individuals do not receive information about their specific eligibility for SEB extensions until they exhaust their regular UI benefits. As noted by several state workforce agencies, individuals are notified that they are eligible for an extension only as they near exhaustion of their existing entitlement.[7] In addition, at least one workforce agency noted a reluctance to specify eligibility for additional benefits prior to exhausting benefits under other UI programs since the state could trigger off benefits. Similarly, workforce agencies seem hesitant to provide information to individuals currently collecting benefits under the SEB program about the likelihood that the program would end before they could collect their full 13 week extension. As a result, individuals may not have full information about whether they may receive a benefit extension under the SEB program.

Another feature of the SEB program is that available benefits differ depending on the type of claim. Assuming a constant trigger status, an intra-state claim, which a claimant files when they live and work in the same state, always receives the full SEB extension whenever the claimant's state of residency has its trigger on. A claimant will file an inter-state claim, when their prior employer is not located in their current state of residence and they are no longer seeking work in the state where they had previously worked. The inter-state claimant only receives the full extension when both the employer's state and state of residency have their trigger on. When only their former employer's state has its trigger on, then the claimant collects just two weeks of benefits. When the claimant's state of residency's trigger is on, but not their former employer's state, no SEB benefits are available. Finally, individuals file a commuter claims when they had

[7]In 2009, several workforce agencies said that they notified individuals on their last benefit check, when filing their last claim and when claimants respond to reporting requirements over the phone. Similar methods, subject to the technology available at the time, were likely employed in prior years.

commuted to work in an adjacent state and intend to search for work in that state. The SEB program treats commuters as if they were residents of the state they commuted to and benefit eligibility is entirely based on the trigger status of the former employer's state.

In many ways the procedures of inter-state and commuter claims is critical for the RD strategy. It makes it nearly impossible for an individual to move to collect additional SEB benefits, since this would require finding a job in a state with its trigger on, subsequently being laid off, and filing a claim while the trigger in that state is still on. Thus manipulating your geographic position to take advantage of SEB benefits is quite difficult.

The various claims do make it possible, however, for workers within states that have triggered on benefits to have different benefit entitlements under the SEB program based on the claims filed. While it is not possible to adjust county data for these types of claims, fortunately their numbers are quite small. Vroman (2001) reports that just four percent of claims filed within a state are inter-state claims. Vroman (2001) also examines commuter claims for New Hampshire, Massachusetts, and Rhode Island, states where almost all counties border another state, and where there is heavy commuting to the Boston metro area. From Vroman's (2001) analysis, it appears that commuter claims are less than 6% of total state claims in these states. In addition, from Vroman's (2001) discussions with workforce agency's he states that "In most states commuter claims were viewed as small and quantitatively unimportant in agency operations" (118). Given that the overwhelming number of claims, even along state borders, are intra-state claims and that commuter claims are "quantitatively unimportant," average benefit entitlement in counties within states that have triggered on is likely very close to thirteen weeks higher than in counties within states that have not triggered on benefits, even very close to state borders.

2.2 Theoretical Framework

It has long been understood that the parameters of the UI system influence job search behavior. Mortensen (1977) theorizes that there are two opposing mechanisms that influence search. The first is UI's disincentive effects on search behavior. Providing more generous UI compensation, in terms of a longer benefit duration, or benefit amount, increases the value of being unemployed and decreases the gain from moving to employment. This decreases job search intensity and increases the selectivity of positions the unemployed will accept. The result is a lower escape rate from unemployment, longer unemployment spells and a higher overall unemployment rate. The second mechanism Atkinson and Micklewright (1991) refer to as the "entitlement effect". Being entitled to unemployment insurance increases the value of employment, since there is a positive probability of being laid off and benefiting from the UI . Mortensen (1977) finds that this causes the unemployed near the end of their spell to increase search effort and decrease selectivity when UI generosity increases, resulting in a higher escape rate. These two opposing mechanisms make the effect of increasing the generosity of UI theoretically ambiguous. Katz and Meyer (1990), note however that, "[s]ince the entitlement effect is likely to be small relative to the standard search subsidy effect, the average duration of unemployment is likely to rise with increases in both the level and potential duration of benefits" (50). Several modelers, such as Hopenhayn and Nicolini (1997), have chosen to exclude the entitlement effect from their analysis.

To understand the implication of an uncertain benefit duration, consider the following simple hybrid of Mortensen's (1977) and Hopenhayn and Nicolini's (1997) models. Suppose utility is intertemporaly separable and workers consume their entire income each period. Also, assume that workers enter the world unemployed and, as in Hopenhayn and Nicolini (1997), all positions are permanent.[8] As a consequence of holding a job in perpetuity, adjustments to the UI system are immaterial to the value of employment, so there is no entitlement effect. The value of a position with an infinite tenure is given by $W = \frac{hu(w)}{r}$, where $hu(w)$ is the utility flow for a small period of time, h, and r is the subjective time preference.

The unemployed choose their search intensity, s, and their minimum acceptable wage offer, known as the reservation wage, $w*$. Greater search effort increases the arrival rate of job offers, given by $s\alpha h$. The utility flow for the unemployed is $h(u(c) - \gamma(s))$, where c is consumption and $\gamma(s)$ is a strictly convex

[8] The unemployed in this case can be interpreted as new entrants into the labor force.

function capturing the disutility of search.[9] Job offers are drawn from a fixed distribution, $F(x)$, with support 0 and $\bar{w}$, and workers accept all job offers greater than $w*$. The escape rate from unemployment is $hs\alpha(1 - F(w*))$.

Two parameters describe the benefit system, (1) the benefit amount, b, and (2) the maximum duration of benefits, T. For an individual with a spell length, t, the benefit system provides consumption as follows:

$$\left.\begin{array}{l} c = b \text{ if } t \leq T \\ c = 0 \text{ if } t > T \end{array}\right\}$$

Once the spell length is greater than the maximum potential, duration benefits are exhausted and consumption falls to zero. The government may also increase or decrease T by τ, based on labor market conditions summarized by θ. The unemployed formulate the probability that such a change will occur using the function $p(\theta)$. Rogers (1998) provides empirical evidence that individuals do recognize, at least imperfectly, that their benefit entitlement may increase or decrease during the course of their unemployment spell, and adjust their search behavior accordingly.

The present value of discounted utility of the unemployed is as follows:

$$\begin{aligned} U(t,T) &= \frac{1}{1+rh} \max_{s\in[0,1],w\in[0,)} h(u(c) - \gamma(s)) \qquad (1) \\ &+ hs\alpha \int_{w*}^{\bar{w}} [\frac{w}{r} - ((1-p(\theta))U(t+h,T) + p(\theta)U(t+h,T+\tau))]dF(w) \\ &+ (1-p(\theta))U(t+h,T) + p(\theta)U(t+h,T+\tau) \end{aligned}$$

In (1) workers choose their search intensity and reservation wage in order to maximize their discounted utility. If the worker draws a wage that is above their reservation value, they gain the discounted difference between earning the wage in perpetuity and the expected future value of unemployment.

The reservation wage and search intensity is chosen according to:

[9] $u(c)$ conforms to standard utility assumptions, with $u(0) = 0$.

$$\frac{w*}{r} = U(t+h,T) + p(\theta)(U(t+h,T+\tau) - U(t+h,T)) \quad (2)$$

$$\gamma(s)' = \alpha h \int_{w*}^{\bar{w}} [\frac{w}{r} - U(t+h,T) - p(\theta)(U(t+h,T+\tau) - U(t+h,T))]dF(w) \quad (3)$$

Equations (2) and (3) state that the search intensity and the reservation wage are chosen such that the value of employment is equal to the expected value of unemployment and that the marginal costs of search balance the marginal gain from increasing the arrival rate of job offers.

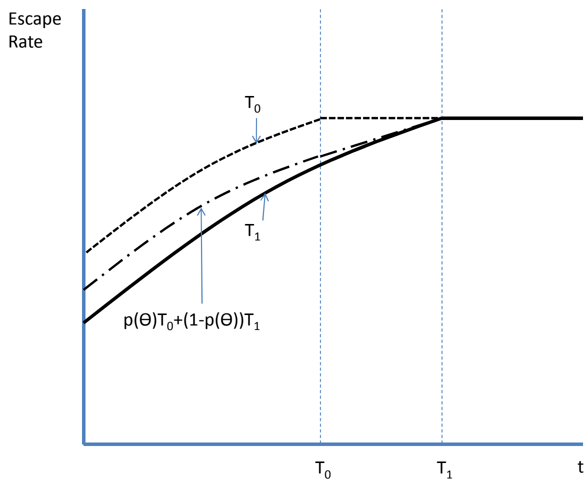
Both equations reduce, to first order conditions similar to that of Mortensen (1977), when $p(\theta) = 0$. In this case Mortensen (1977) shows that, since the value of unemployment falls as one gets closer to exhausting their benefits, the escape rate increases (an increase in search intensity and a decrease in selectivity) with t if $t < T$. After exhausting benefits, the escape rate remains constant. In addition, Mortensen's (1977) model also indicates that $U(t,T)$ is increasing in T. As a result, given there is no entitlement effect, the escape rate is decreasing in T. The escape rate for two benefit durations $T_1 > T_0$ are plotted in Figure 1.

Figure 1 also plots the escape rate when there is a possibility that the benefit entitlement may change. Suppose that current UI duration is T_1, but there is a possibility that it may fall to T_0. In this case, $p(\theta) > 0$ and $\tau < 0$. As a result $(U(t+h,T+\tau) - U(t+h,T)) < 0$ and the reservation wage will be lower than the $p(\theta) = 0$ and $T = T_1$ case. In addition, the gain from moving to employment is greater than when T_1 is permanent and search intensity will be higher. Figure 1 plots the escape rate for the scenario where it is possible for T_1 to be reduced to T_0, but such a reduction never occurs. In this case the escape rate is higher than when T is certain to be T_1. This simple adaption of Mortensen (1977) indicates that some uncertainty in extending benefits may mitigate its effects relative to a permanent increase in UI duration.

2.3 Prior Estimates of the Effects of Benefit Duration

It is important to put the estimates in this paper in context of the literature on the effects of UI on unemployment duration, as well as the limited literature on its effects on local unemployment rates. Several

Figure 1: Escape rates for permanent durations of T_0, T_1, and a positive probability of benefit duration falling from T_1 to T_0



Note: T_0 and T_1 indicate escape rates with a certain benefit duration where $T_0 < T_1$.
$p(\theta)T_0 + (1 - p(\theta))T_1$ indicates the escape rates where current duration is T_1 throughout the spell, but a positive probability exists of benefit duration decreasing to T_0.

papers examine the disincentive effects of temporary benefit extensions, but largely use econometric techniques that do not address the link between the policy and labor market conditions. For instance, Moffitt and Nicholson (1982) and Moffitt (1985) examine the effect of maximum duration on the length of unemployment spells with a sample that includes individuals eligible for a EEB program and SEB program benefits. They find that an additional one week of benefits leads to an increase of 0.10 and 0.15 weeks of unemployment. Katz and Meyer (1990) find a slightly larger effect of 0.16 to 0.20 weeks of unemployment for every additional week of benefits. Outside the U.S., Ham and Rea (1987) estimate a hazard model to analyze the effect of Canada's UI system during the late 1970s, which determines benefit duration by the amount of time the claimant works and the economic region's unemployment rate. The authors estimate that an increase in benefit duration of one week increases the length of unemployment by 0.33 weeks. The magnitude of these estimates may be influenced by the endogeneity between benefit duration and current labor market conditions.

Several papers address the endogeneity problem with a quasi-experimental approach. Hunt (1995) examines a change in UI policy based on age in Germany and finds similar effects to Moffitt (1985). Lalive et al. (2006) and Lalive (2007) study changes in the Austrian UI system. In Lalive et al. (2006), the authors examine a 1989 policy where one group experience a change in the level and length of their UI benefits, another group only a change in the level, another only a change in the length and still another group that did not have a benefit change. This allows for a difference-in-differences approach that yields an estimate of 0.05-0.10 weeks of additional unemployment per week of benefits. Lalive (2007) studies the effect of a large increase in potential duration for those above a certain age and that reside within a given region. The author uses a Sharp RD approach, similar to that of this paper, and finds that each additional week of benefits adds 0.32 weeks of additional unemployment for women and 0.09 for men. In the U.S., Card and Levine (2000) analyze a 1996 extension to benefits in New Jersey that arose from a legislative compromise rather than worsening labor market conditions. The authors find that for an individual who starts their spell with the extension in place would be unemployed one additional week as a result of the 13 week extension. Using Slovenian data, van Ours and Vodopivec (2004) show that a large decrease in UI entitlement significantly decreases UI duration by 0.86 weeks for one week of lower UI benefits. Caliendo et al. (2009) also apply a RD approach to German data exploiting a non-linear change in UI benefits at age 45 and find that greater benefits significantly decreases the job finding rate. These quasi-experimental designs attempt to account for the endogeneity between greater benefits and worsening labor market conditions. However, these studies deal with more permanent or predictable changes in UI and may not generalize to the U.S. SEB program.

Evidence of the effect of social programs on local unemployment rates is far more limited. Wunnava and Mehdi (1994) study state unemployment rates and the average duration of unemployment. The authors find that a 10 percent increase in the UI replacement rate increases the insured unemployment rate by 2.4 percent. Both Partridge and Rickman (1995) and Hyclak (1996) report a positive effect of unemployment insurance generosity on local unemployment, although in most of Partridge and Rickman's (1995) specifications the effect is insignificant. In Vedder and Gallaway (1996), the authors find that the percentage of the population collecting welfare payments has a strong effect on the permanent amount of state unemployment. Moomaw (1998) shows that the size of unemployment insurance benefits has a strong positive effect on county unemployment rates. This paper makes an important contribution to the research by adding to the limited literature on the effect of unemployment benefit duration on county-level unemployment rates.

3 Empirical Approach

3.1 Identification

To understand why standard standard econometric techniques are unable to identify the causal effects of the SEB program consider the following regression:

$$u_{ij} = \alpha x_{ij} + \tau d_{ij} + \beta T_j + \epsilon_{ij} \tag{4}$$

In equation (4), the unemployment rate in county i, within state j, is a function of a vector of demographic and economic characteristics, x_{ij}, its geographic location, summarized by d_{ij}, the SEB trigger status of state j, T_j, and unobservable characteristics ϵ_{ij}. The treatment, T_j, is given by the following rule:

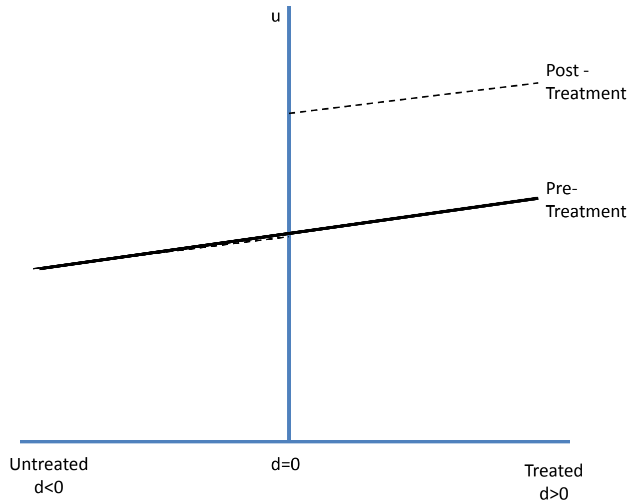
$$\begin{aligned} T_j &= 1 \text{ if } \sum_i w_{ij} u_{ij} > \gamma_j \text{ for } j = 1, ..., 50 \\ T_j &= 0 \text{ otherwise} \end{aligned} \tag{5}$$

Treatment is determined by comparing a weighted average (with weights w_{ij}) of the unemployment rate for counties within a state to a threshold level for the state, γ_j, which is determined by the trigger mechanism the prior section discusses. Equations (4) and (5) reveal the difficulty in identifying the casual effect based on β. Changes in the residual ϵ_{ij} influence the unemployment rate, which in turn influences the probability of treatment. As a result, OLS estimates of β will be biased.

This paper exploits the SEB programs non-linear process of increasing the average benefit entitlement in

Figure 2: [

Hypothesized Unemployment Rates as a Function of Distance



Note: The policy border is at $d = 0$.

certain counties. This change occurs at the border separating states with and without access to SEB benefits, which this paper refers to as the "policy border", a term borrowed from Holmes (1998). Figure 3.1 shows how this sharp jump in the assignment rule identifies the effect of the program. Consider counties arranged along a linear space depending on their distance to the policy border, d, where the policy border is at $d = 0$. Counties with access to SEB benefit extensions are on the positive portion of this linear space and those without access to extension are on the negative portion.

First, consider a counter-factual world where for the same period, UI benefits duration on both sides of the policy border are the standard 26 weeks. As Figure 3.1 indicates the unemployment rate (u), with no other non-linear changes at the policy border, would be a smooth function of distance, where the unemployment rates are identical immediately on either side of the policy border. The function may slope upwards since states that are arranged along the positive side of the policy border have unemployment levels above the trigger thresholds (although we are assuming the counter-factual that this does not result in a change in unemployment benefits). As distance increases, on average, you move to an area with poorer labor market conditions. With the SEB program in effect, and the positive side of the border having access to 13 weeks of additional benefits, theory suggests that unemployment escape rates will decrease and the unemployment rate will increase, as Figure 3.1 indicates. The abrupt change at the policy border identifies the treatment effect of the SEB program.

Identification by use of an exogenous decision rule, with a clear cut-off point, is known as a Sharp Regression Discontinuity (RD) design. The approach, pioneered by Campbell (1969) and Thistlethwaite and Campbell (1960) has been applied in numerous contexts, including the impact of financial aid on college acceptance (van der Klaauw, 2002), incumbents' ability to win elections (Lee, 2001), and the effect of a large increase in UI entitlement in Austria (Lalive, 2007), among many others. The approach in this paper estimates the discontinuity in the outcome variable, u, at the policy border, $d = 0$, where treatment, T, follows the rule that $T = 1$ if $d > 0$. In the literature, d is known as the forcing variable and $d = 0$ the cut-off point. The causal effect of the SEB program can be identified by the following:

$$\lim_{d \downarrow c} E[u_i | d_i = d] - \lim_{d \uparrow c} E[u_i | d_i = d] \tag{6}$$

As Lee (2001) and Lemieux and Lee (2009) discuss, the essential assumption allowing for identification in a RD design is that observations can be considered randomized around the cutoff point. The following must hold for a valid RD design:

$$E[\epsilon_i | -\delta < d < c] = E[\epsilon_i | c < d < \delta] \tag{7}$$

As Lemieux and Lee (2009) point out, a biased estimate of the causal effect will result if observations can manipulate their geographic position with respect to the policy border. While counties certainly cannot manipulate their location, individuals may move across the policy border, creating a selection bias. As the previous section discusses. This is highly unlikely because it involves establishing work history in a new state. Still to ensure the identification strategy is valid, Section 5 provides a series of diagnostics which support evaluating the SEB program using a RD design.

An additional concern is that other policy differences may create a discontinuity in unemployment rates at the policy border. The RD design requires:

$$E[u_0|d_i = d] \text{ and } E[u_1|d_i = d] \text{ are continuous in } d \text{ at } c \tag{8}$$

where u_1 is the unemployment rate under treatment and u_0 is the rate under the control (no SEB benefits). Equation (8) states that if we were to observe u_1 or u_0 for all observations, they should be continuous functions of distance.

The uncertainty and randomness of when SEB benefits are available make it unlikely that there are other changes in policy that occur simultaneously to SEB benefits triggering on, but there may be existing differences in state policy that lead to a discontinuity absent variations in benefit duration. For instance, there are large differences in regular UI benefits or tax structures across states. Individuals with high incidences of unemployment may sort themselves geographically based on their preferences for generous UI benefits and high income taxes to support more larger social programs. This may result in a discontinuity in county unemployment rates at the policy border that is unrelated to the SEB program. To ensure that (8) holds, this paper examines a period prior to the SEB program triggering on in any state. This validates the interpretation of the discontinuity as the casual effect of the SEB program by ensuring that when extensions are not available, there is no discontinuity.

3.2 Estimation

This paper estimates the effect of the program on county unemployment rates using both a graphical and econometric approach. One of the strengths of the RD design is that the treatment effect can be seen graphically. To accomplish this, it is standard to divide the observations into separate bins. Each bin includes observations within a given continuous range of the forcing variable. Then bin averages of the outcome variable are taken based upon:

$$\bar{Y_k} = \frac{\sum_{i=1}^{N} Y_i 1[b_k < d_i \leq b_{k+1}]}{\sum_{i=1}^{N} 1[b_k < d_i \leq b_{k+1}]} \tag{9}$$

where b_k are successive cutoff points for each bin. $\bar{Y_k}$ is then graphed along with a fitted polynomial that takes into account the possible discontinuity at the policy border.

Implementing the graphical approach requires a choice of bin size, the number of the bins to include and the order of the fitted polynomial. I choose a bin size of 30 miles, which is approximately the average width of the counties within the sample used in this paper.[10] I further validate this choice by employing a test Lemieux and Lee (2009) propose to ensure that the bin means are representative. If observations within a bin contain a trend the mean may not represent the bin's extremes, if it is too wide. The test involves running a regression of the outcome variable on dummy variables for each bin and each bin dummy interacted with d_i, along with the available covariates. If the bin is representative, then the interaction terms should not add explanatory power to the model, which is revealed through an F-test. I find that a 30 mile bin is representative of the data within each bin (F-statistic of 0.75).[11] In addition, in the estimations I include all seven bins on each side of the policy border, which covers the entire sample on the treated side of the border and a symmetric number of bins on the untreated side. I choose the fitted polynomial order by determining the polynomial with the minimum Akaike Information Criterion (AIC), as suggested by Lemieux and Lee (2009).[12]

An econometric approach to estimating the treatment effect uses the following parametric regression:

[10]I determine the average "width" of a county by taking the square root of the average area, which is 23 miles. I round up to 30 miles to ensure that all counties adjacent to the policy border are included in the first bin.

[11]This test is conducted with five bins on either side of the border. There are only nine treated observations with a distance greater than 150 miles making estimates for these bins highly imprecise.

[12]The AIC is given by $AIC = N \ln(\hat{\sigma}^2) + 2p$ where N is the sample size, σ is the mean squared error and p is the number of parameters. The AIC reflects the trade off between a more precise model and a more parsimonious one.

$$\min_{\alpha,\beta,\tau,\delta} \sum_{i=1}^{N} 1[-h \leq d_i \leq h](Y_i - \alpha_0 - \alpha_1 T_i - \sum_{p=1} [\beta_0^p d_i^p + \beta_1^p d_i^p T_i] - \delta' X_i)^2 K_h(|d_i|) \tag{10}$$

In (10), α, β^p, and δ represent vectors of parameters (i.e. $\alpha = \begin{bmatrix} \alpha_0 & \alpha_1 \end{bmatrix}$) to be estimated, the bandwidth, h, defines a set of observations to be included in the analysis that lie within h miles of the policy border, the treatment, $T_i = 1[d_i > 0]$, indicates that the observation is in a state with access to SEB benefits, X_i is a set of covariates and $K_h(d_i)$ is weighting that is based on distance to the policy border. The parameter α_1 provides a parametric estimate of (6).

Estimation of (10) requires a choice of the bandwidth, the polynomial order, and a weighting method. Determining the most appropriate bandwidth involves balancing the benefits of having more information in the sample (a larger window around the discontinuity) against the costs of including observations that are farther away from the cut-off point and may be less relevant to the process causing the discontinuity. I deal with this by determining the bandwidth in a systematic way using the cross-validation procedure of Ludwig and Miller (2007). This procedure determines the bandwidth that is most accurate in predicting the outcomes for a set of observations close to the discontinuity (see Appendix C for more detail about this procedure). This procedure selects a bandwidth of 70 miles.

As is standard in the literature, when restricting the sample I use local linear regression. As in Lalive (2007) I also estimate the treatment effect with the entire sample as well.[13] In these cases I again use the AIC to select the preferred functional form. To ensure robustness, I vary the bandwidth and order of the fitted polynomial. The results section provides estimates unweighted and weighted by the county's distance to the policy border using an Epanechnikov kernel.[14]

[13] I consider only symmetric samples where the sample is always restricted to the lesser of the maximum distance of a county in the treated or the untreated group.

[14] Epanechnikov kernel is as follows $K = .75(1 - (D_i/h)^2)$.

While Section 6.1 presents evidence that (8) holds and that, in the absence of the SEB program, there is no discontinuity at the policy border, to further ensure the results are not biased by an existing discontinuity in the data, I also estimate a specification similar to that of Lalive (2007), who combines the RD design with a difference-in-differences estimate using:

$$\min_{\alpha,\beta,\tau,\delta} \sum_{i=1}^{N} 1[-h \leq d_i \leq h](Y_i - \alpha_0 - \alpha_1 T_i - \alpha_2 S_i - \alpha_3 S_i T_i - \sum_{p=1} [\beta_0^p d_i^p + \beta_1^p d_i^p T_i + \beta_2^p d_i^p S_i + \beta_3^p d_i^p T_i S_i] - \delta' X_i)^2 K_h(|d_i|) \tag{11}$$

In (11), the sample is expanded to include data for each county from January 1991, one month before any state triggers on SEB benefits.[15] For January 1991, S_i is set to zero and is one for June 1991 when several states triggered on benefits. T_i is one if the observation is within a state that has a trigger on during June 1991 and zero otherwise. This allows for the treatment effect, which controls for any discontinuity that may occur during periods when the SEB program is not in effect, to be estimated as α_3.

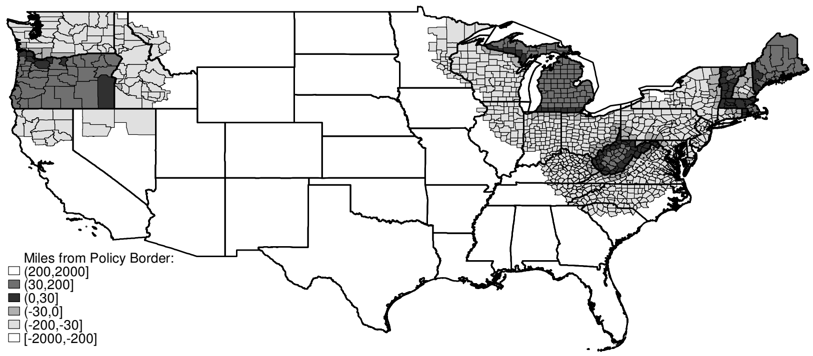
4 Data

This paper focuses on the recession of the early 1990s. During 1991, a total of eight states triggered on SEB benefits during a period prior to the Emergency Unemployment Compensation (EUC) Act of 1991 (an EEB program) taking effect. The eight states, seven excluding Alaska, on average triggered on benefits for slightly more than four months.[16] The analysis focuses on June 1991, when all states had access to SEB benefits for at least two months, and well before the EUC program became effective. The first step in constructing the sample is determining the distance of each county to the policy border and their treatment status. This paper uses a simple measure, the straight line distance between the centroid of the county and

[15]Two states, Rhode Island and Maine, triggered on benefits during February, Vermont and Michigan in March, and Massachusetts, Oregon, and West Virginia triggered on in April.

[16]Rhode Island triggered on for nine months, Maine for eight, Vermont for five, and Massachusetts, Michigan, Oregon and West Virginia for four.

Figure 3: Counties Along the Policy Border



Notes: Positive distances indicates the county is in a state with an SEB trigger on and a negative distance indicates a county within a state that has triggered off.

the nearest state border with an opposing trigger status as the state the county is in. More detail can be found in Appendix A. Distances within a state that has triggered on SEB benefits are denoted as a positive, and distances within a state that has not triggered on are negative, with zero indicating the policy border. Figure 3 presents the counties that surround the policy border. The map indicates that the policy variation is dispersed between Oregon in the West, Michigan in the Mid-West, West Virginia in the Mid-Atlantic and several New England states.

Two issues result from the geographic nature of the data. The first is the variation in the size of the counties across the country. It is clear from Figure 3 that the counties surrounding Oregon in the West are much larger than the more uniform counties of the east. This makes pooling the West and East difficult. As a result, I follow Holmes (1998) and focus on the eastern portion of the country by excluding Oregon and its surrounding counties. The second issue arises from the policy variation being dispersed across the country. As long as the policy border serves as an exogenous decision rule to assign benefits, and the assumptions laid out in the prior section hold, this is not an issue for identification. However, taking into account the geographic position of the counties may provide more efficient estimates, so I estimate specifications that include dummy variables for the Mid-West counties (counties within Michigan and counties that are closest to policy border with Michigan) and the Mid-Atlantic (counties within West Virginia and counties that are closest to the policy border with West Virginia). I also estimate a specification that includes dummy variables for MSAs with more than four counties.

The outcome variable in this paper is the log of the county unemployment rate from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) database. Similar to the inclusion of regional dummy variables, the inclusion of other covariates can improve the efficiency of estimates, so most specifications include data from the Census' USA County Data Files. To represent the economy of the county, I include the percentage of employment in construction, manufacturing, and government, as well as the log difference between per-capita income over the prior decade. To describe the demographics of the population, I include the log of the 1991 population, the median age and the percentage of the population that is Black, Hispanic and foreign born. To measure the education of the workforce, I include the percentage of the workforce that has a high-school diploma and the percentage that has a college diploma. As an indication worker's mobility, I include the percentage of the workforce that works outside of the county, the average commute time, and the percentage of home ownership.

I also estimate a specification that includes state UI variables. Unfortunately, these variables are only available at the state level, but their inclusion is another helpful test to ensure that the results are not driven by state differences in the regular UI system. From the July 1991 Department of Labor's Significant Provisions of State Unemployment Insurance Laws, I include the number of weeks that individuals must wait before collecting UI, minimum and maximum benefit amounts, and minimum and maximum benefit durations. From the June 1991 Department of Labor Unemployment Insurance Chartbook, I include the proportion of wages replaced by UI benefits.

Table 1 provides sample means for counties within a narrow band of 30 miles surrounding the policy border, and a broader band of 200 miles, separated into counties that are treated and untreated. Columns (3)

Table 1: Sample Descriptive Statistics

	30 Mile Bandwidth			200 Mile Bandwidth		
	$d > 0$	$d < 0$	p-value	$d > 0$	$d < 0$	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Log of Unemployment	2.24	2.07	0.00	2.26	1.93	0.00
Demographics						
Log of Population	10.59	10.74	0.39	10.64	10.82	0.06
Median Age	34.96	34.31	0.16	34.72	34.19	0.02
% Population Black	1.91	2.79	0.07	2.33	8.01	0.00
% Population Hispanic	1.06	0.98	0.72	1.19	1.43	0.28
% Population Foreign	2.14	1.72	0.20	2.22	2.03	0.48
% Population High School Graduates	70.27	70.12	0.91	72.42	69.75	0.00
% Population Bachelors Degree	14.29	13.69	0.59	14.49	14.21	0.64
Economy						
Income Growth	62.85	60.99	0.25	62.72	64.54	0.02
% Employment in Construction Sector	4.99	5.20	0.68	5.41	5.45	0.90
% Employment in MFG Sector	26.10	26.44	0.89	24.21	29.67	0.00
% Employment in Government Sector	33.78	27.69	0.06	32.68	30.70	0.46
Labor Mobility						
% Work Outside County	13.36	13.58	0.84	11.47	15.32	0.00
% of Homes Owner Occupied	74.39	73.08	0.24	74.74	72.80	0.00
Commute Time	21.23	21.16	0.92	20.24	21.50	0.00

Notes: 30 mile bandwidth includes approximately one row of counties surrounding the policy border.
200 mile bandwidth is just sufficient to include every county that is within a state that has triggered on benefits.
$d > 0$ corresponds to treated counties and $d < 0$ to untreated counties.
P-values are tests for the equivalence between the treated and untreated means within each bandwidth.

and (6) indicate the p-values for the null hypothesis that means on either side of the border are equivalent. A simple differencing of the treated and untreated means for the log of the unemployment rate around the narrow band suggests a treatment effect of 17%. The wider band indicates an effect of 33%. The counties within the narrow band appear to be very similar. None of the covariates' means are significantly different at the five percent level, while for the broader band there are eight instances where the means are signficiantly different. It is also important to note that the percentage of individuals in the sample that commute out of the county to work (out of state statistics are not available) are a small minority, further evidence that commuter claims do not drive this paper's results.

The descriptive statistics alone provide some evidence that estimating the treatment effect based on observations close to the policy border appears to be important, not only for the size of the treatment effect, but also in ensuring that the covariates are balanced, and that the sample approximates a random experiment. The next section provides further diagnostics that the supports the use of a RD design.

5 Validity of the SEB Program as a Quasi-Experiment

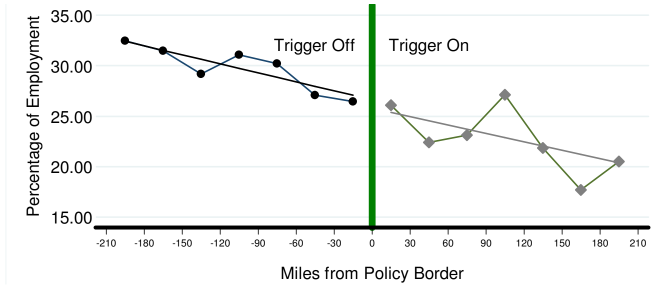
This section presents several diagnostics that support the validity of the RD strategy. The first diagnostic examines whether county observables are balanced across the policy border. Figure (4) plots bin averages for two select covariates along with a fitted polynomial that allows for a discontinuity at the policy border. The order of the polynomial is based on the AIC and is constructed by running a regression that weights each bin by the observations within it. In panels (a) and (b), the percentage of employment in manufacturing, along with a linear fit, and the percentage of the population that is black, along with a cubic fitted polynomial, are shown. I examine these covariates since manufacturing workers and minorities may have higher rates of unemployment in recessions. If there was an increase in these two groups at the policy border, it may bias the results towards finding a treatment effect. In both cases, the covariates appear to be smooth functions across the border suggesting that these covariates are balanced across the policy border.

In addition to examining individual covariates, Lemieux and Lee (2009) suggest a joint test of a discontinuity in all covariates at the cut-off point. I perform the test using all the covariates in Table 1 and a local linear regression. Further details of this test can be found in Appendix B. The joint test of no discontinuities has a F-statistic of 1.07, which is not significant at standard levels. This suggests that the covariates are balanced across the policy border supporting a quasi-experimental approach.

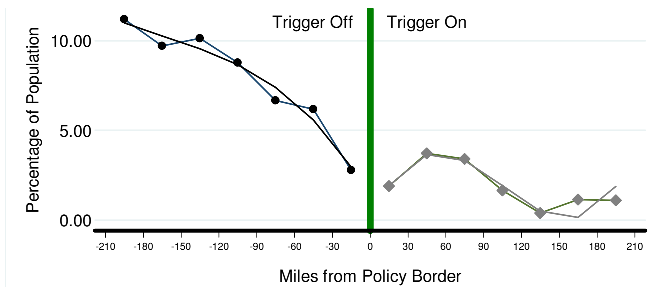
To ensure that individuals do not manipulate their position relative to the policy border, I perform a test similar to McCrary's (2008) density test. The author suggests examining the density of observations for a discontinuity at the cut-off point. If one exists, then individuals may be moving to states with their trigger on to take advantage of the SEB program. As previously noted, this is likely difficult to do given that it requires a previous work history in a trigger on state. Figure 5 shows the total number of unemployed at 30 mile bins along with a cubic polynomial. There appears to be no discontinuity at the policy border.[17] Given

[17] It is also possible that individuals may try to establish a commuter claim by moving closer to a state that has its trigger on. While the data does not allow one to test this explicitly, as Section 3.1 mentions these types of claims are a very small portion of total claims. If this were a major issue, the density in Figure 5 may also rise immediately on the negative side of the border. However, this does not appear in the figure, suggesting that individuals do not move close to a state that has triggered on benefits in order to establish a commuter claim.

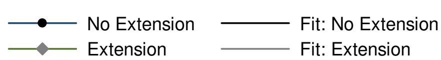
Figure 4: Discontinuity in Selected Covariates at the Policy Border



(a) Percentage of Employment in Manufacturing



(b) Blacks as a Percentage of Population



Notes: Positive distances indicate states with triggers on and negative distances with triggers off.
Fitted lines are based on OLS regression with observations weighted by the count of counties within each bin.
Order of polynomial is chosen by the AIC.
In panel (a), the discontinuity is -0.8070 with a robust standard error of 1.3416.
In panel (b), the discontinuity is -1.3740 with a robust standard error of 0.9260.

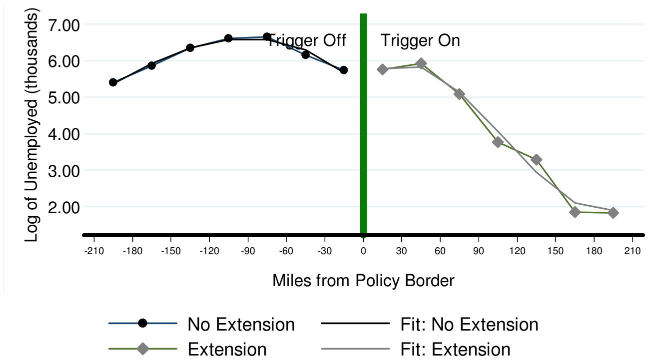
that the covariates are balanced and there is no evidence of manipulation of the forcing variable, evaluating the SEB program as a quasi-experiment is valid.

6 Results

6.1 Graphical Analysis

Figure 6 presents graphical evidence of the effect of the SEB program. Panel (a) plots the bin averages for the log unemployment rate for January 1991, a month prior to the SEB trigger turning on any state. In order for the RD estimates to be interpreted as a casual effect, in the absence of treatment there must not

Figure 5: Test for Manipulation of the Forcing Variable



Notes: Positive distances indicate observations within states that have triggered on and negative distances indicate observations within states that have their trigger off.
Fitted lines are based on OLS regression with observations weighted by the count of counties within each bin.
The estimate of the discontinuity is 0.1586 with a robust standard error of 0.2451

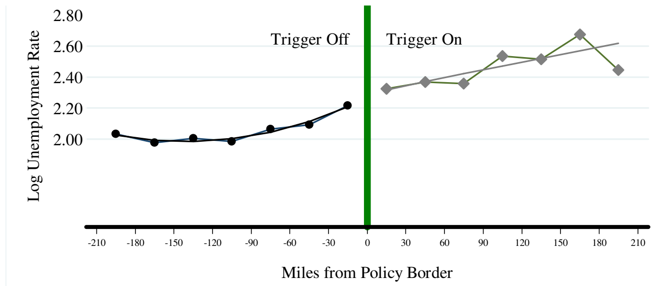
be discontinuity at the policy border. The AIC selects a quadratic function in Panel (a). While the function allows for a discontinuity at the policy border it appears that the best fit is a smooth function across the policy border. Policies, such as income taxes, government expenditures and variations in regular UI policy that differ between states, do not seem to have a strong impact on unemployment rates at the border.

Panel (b) presents the results for June 1991, where states on the positive side of the policy border had triggered on 13 weeks of additional benefits for at least two months. The AIC selects a quadratic function as the fitted polynomial. Similar to January 1991, the function slopes upward reflecting that a county is more likely to be in a high unemployment state, and farther from a low unemployment state, as distance increases. In June 1991, however, there is a discontinuity at the policy border of 0.14. Given the results in panel (a), this discontinuity can be viewed as the casual effect of the SEB program.

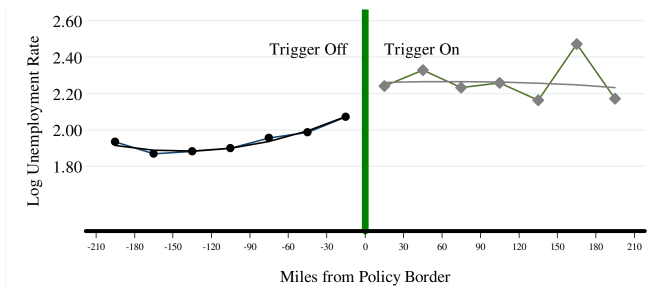
6.2 Econometric Results

This section presents results based on individual counties rather than bin averages. The estimates in Table 2 use several bandwidths and vary the order of the polynomial. Each case includes covariates from the Census Bureau and counties are not weighted. Column (1) presents the estimate with the entire sample (200 miles) where the AIC selects a quadratic polynomial. The estimate of the causal effect is about 14%, similar to discontniuty in the graphical estimates and significant at the 1% level. Column (2) uses the bandwidth of

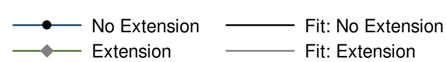
Figure 6: Local Unemployment Rates Before and After the States Trigger on the SEB Program



(a) January 1991 Log of County Unemployment Rates



(b) June 1991 Log of County Unemployment Rates



Notes: Positive distances indicate states with triggers on and negative distances with triggers off.
The AIC selects a quadratic function for both January 1991 and June 1991.
Fitted lines are based on OLS regression with observations weighted by the count of counties within each bin.
In panel (a), the treatment effect is 0.0273 with a robust standard error of 0.0370.
In panel (b), the treatment effect is 0.1354 with a robust standard error of 0.0379.

70 miles that the cross-validation procedures selects. The specification uses a local linear regression, which is standard when limiting the sample.[18] The treatment effect is slightly lower, at 12%. Columns (3) and (4) again use a local linear specification that decreases and increases the bandwidth by 30 miles.[19] The treatment effect in these specifications is slightly larger at 15% and 16% and are significant at the five percent level. The final two columns use the entire sample with a linear specification and the 70 mile bandwidth with a quadratic. In both cases the treatment effects are larger than the previous columns, suggesting that using a misspecified polynomial can greatly increase the estimate of the treatment effect.

[18]The AIC also selects a linear specification over higher order polynomials.
[19]The AIC selects a linear specificaiton for both of these specifications.

Table 2: SEB Program Treatment Effect: Alternate Bandwidths and Functions of Distance

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect	0.1378***	0.1195**	0.1523**	0.1563***	0.2030**	0.2496***
T-Statistic	(2.9730)	(2.4765)	(2.0172)	(3.7680)	(2.2219)	(8.0640)
Polynomial Order	2	1	1	1	2	1
Bandwidth (miles)	200	70	40	100	70	200
Covariates	Census	Census	Census	Census	Census	Census
Weighted	No	No	No	No	No	No
Observations	984	364	218	522	364	984
R^2	0.5522	0.6544	0.6550	0.6190	0.6561	0.5482

Notes: *Indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.
Census indicates that specification includes the covariates in Table 1.

Table 3: SEB Program Treatment Effect: Alternate Covariates

	(1)	(2)	(3)	(4)	(5)
Treatment Effect	0.1378***	0.1142**	0.1241***	0.1478***	0.1478**
T-Statistic	(2.9730)	(2.3497)	(2.6504)	(3.0596)	(2.3747)
Polynomial Order	2	2	2	2	2
Bandwidth (miles)	200	200	200	200	200
Covariates	Census	Census & UI	Census & Region	Census & MSA	None
Weighted	No	No	No	No	No
Observations	984	984	984	984	1015
R^2	0.5522	0.5798	0.5617	0.5897	0.1412

Notes: *Indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.
Census covariates indicates that specification includes the covariates in Table 1. Census & UI include the Census covariates along with the state UI covariates. Census & Region include the Census covariates along with regional dummy variables for the Mid-West and Mid-Atlantic. Census & MSA includes the Census covariates along with covariates for each MSA with more than four counties.

Table 4: SEB Program Treatment Effect: Weighted Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect	0.1107**	0.1318**	0.1684**	0.1378***	0.2275**	0.2198***
T-Statistic	(2.3741)	(2.4792)	(2.0162)	(3.3256)	(2.3555)	(6.8934)
Polynomial Order	2	1	1	1	2	1
Bandwidth (miles)	200	70	40	100	70	200
Covariates	Census	Census	Census	Census	Census	Census
Weighted	Yes	Yes	Yes	Yes	Yes	Yes
Observations	984	364	218	522	364	984
R^2	0.5836	0.6480	0.5852	0.6377	0.6501	0.5790

Notes: *Indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.
Census covariates indicates that specification includes the covariates in Table 1.

Table 3 presents specifications with different sets of covariates. In each case, the 200-mile bandwidth is used and the observations again are unweighted. Column (1) reproduces Column (1) from Table 2 for comparison purposes. Column (2) adds the various UI state variables to the Census covariates, which only slightly decreases the treatment effect. Columns (3) and (4) account for any regional differences by including regional dummy variables and MSA dummies. The estimates are robust to the inclusion of these dummy variables with estimates of 12% and 15%. The RD design does not require covariates for an unbiased estimate of the treatment effect, and the estimate should be robust when they are removed. Column (5) test this by removing all covariates. The treatment effect is only slightly higher in this specification than Column (1) and nearly identical to Column (4).

Table 4 provides the same specifications as Table 2, while using Epanichov Kernels to put more weight on observations close to the policy border. The results seem to be robust to this weighting. In each case, the weighted results are within 0.03 of the unweighted results.

The final set of results, in Table 5, present three differenced RD specifications in Columns (1) - (3), along with three specifications that include Oregon and the surrounding counties, in Columns (4) - (6). Each specification uses the 200-mile bandwidth and selects the polynomial order with the AIC. For the differenced RD columns and the inclusion of the western counties, specifications with and without the Census covariates are estimated, along with a specification that weights the observations using the Epanichov Kernel. For Columns (1) - (3), the results are only slightly lower than the standard RD results in the previous tables.

Table 5: SEB Program Treatment Effect: Weighted Specifications

	RD-Differenced			West Counties Included		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect	0.1232**	0.1034**	0.1045*	0.1207***	0.1670***	0.1103**
T-Statistic	(2.3535)	(2.0000)	(1.9437)	(1.9492)	(3.8305)	(2.3603)
Polynomial Order	2	2	2	2	2	2
Bandwidth (miles)	200	200	200	200	200	200
Covariates	Census	None	Census	Census	None	Census
Weighted	No	No	Yes	No	No	Yes
Observations	1968	2030	1968	1106	1137	1106
R2	0.5381	0.1561	0.5554	0.5006	0.1026	0.5316

Notes: *Indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.
Census covariates indicates that specification includes the covariates in Table 1.
When indicated, covariates are weighted using an Epanichov Kernel.

Columns (4) - (6) show that the results are not sensitive to the exclusion of the western counties, with the estimates ranging between 11% and 13%.

Across all estimates the median is 0.1318 with a maximum effect of 0.2496 and a minimum of 0.1034. While this is a large range, 77% of the esitmates fall within 0.03 of the median, making the results quite robust.

6.3 Falsification Tests

A threat to the credibility of the estimates in this section is that discontinuities may exist at cutoff points other than the policy border. This would suggest that the discontinuity at the policy border is a result of the volatility in the data, rather than the effects of the SEB program. In order to test if this is the case, I follow the method proposed by Imbens and Lemieux (2008). First, I divide the entire sample within the 200-mile bandwidth into two at the policy border. Then, on either side of the border, I test for a discontinuity at the 10th, 20th and 30th percentile of the absolute value of d using Equation (10). In all cases I include the Census covariates and leave the observations unweighted. I use a quadratic fit to be consistent with the results for the discontinuity at the policy border. The discontinuities at these cut-off points, along with the t-statistics in parentheses, are reported in Table 6. In only one case is there a t-statistic above one, -1.1793, indicating that the discontinuity at the policy border is not a result of volatility in the data.

Table 6: Falsification Tests: Discontinuities at Alternative Cut-Off Points

	Trigger Off	Trigger On
10th Percentile	0.04512	-0.1259
	(0.6405)	(-1.1793)
20th Percentile	-0.0585	0.01492
	(-0.8705)	(0.13662)
30th Percentile	0.02993	0.03396
	(0.5377)	(0.4958)

Note: T-statistics are reported in parentheses.

7 Effect on Average Duration of Unemployment

To compare the county-level treatment effect on local unemployment rates to other estimates in the literature that analyze the effect of UI on average duration, I perform the following simple calculation. First, I assume changes in the unemployment rate can be modeled using the equation:

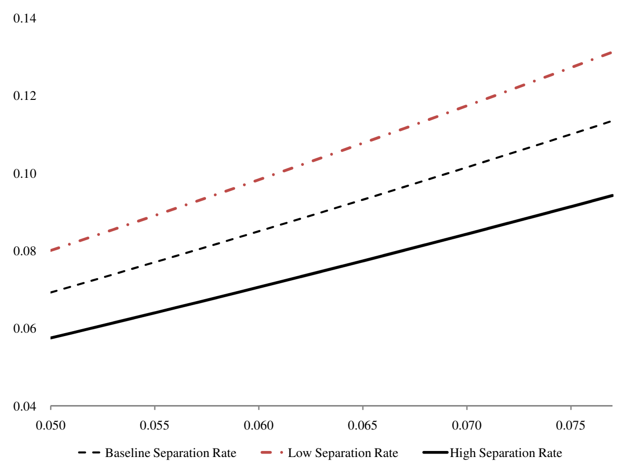
$$\Delta u_t = s(1 - u_{t-1}) - pu_{t-1} \tag{12}$$

where u_t is the unemployment rate, Δu_t is the change in the unemployment rate, s is the monthly probability of separating from employment and p is the monthly probability of finding a job, or the hazard rate. I focus on a steady state analysis where Δu_t is set to zero and the average duration of unemployment can be determined by the inverse of p. Elsby et al. (2009) state that "...the evolution of the actual unemployment rate, ..., is closely approximated by the steady state unemployment rate" (7), suggesting that little is lost by setting $\Delta u_t = 0$.

I calculate the effect of the SEB program on average duration by using the following method. First, given a monthly separation rate, s, and u_{t-1}, I calculate p and the average duration. Then, using the same separation rate, I increase u_{t-1} by the estimated treatment effect of the SEB program, and recalculate p and the steady state treated average duration. The difference between the untreated and treated duration provides the treatment effect of the SEB program on the average duration of employment.

Figure 7 provides the results for various parameter estimates using the treatment effect from Table 2 Column (1). The results are in terms of the increase in average duration per one week increase in UI benefits (the effect on total average duration is divided by 13). The graph presents results for untreated

Figure 7: SEB Program's Effect on Average Duration



Notes: Based on a treatment effect of 0.1378.
The baseline separation rate is 0.037, taken from Shimer (2005), the low separation rate is 0.032 and the high separation rate is 0.042.

unemployment rates between 5.0% and 7.7%, which is approximately the range of unemployment rates during the recession of the early 1990s. I set the separation rate to 0.037 as a baseline, based on Shimer's (2005) calculations from the first half of 1991, and also graph alternatives for the monthly separation rate of 0.32 (low separation rate) and 0.042 (high separation rate).

The estimates of the effect on average duration in Figure 7 range from 0.06 to 0.13 weeks of additional unemployment for each additional week of UI benefits. These estimates are at the low end of other papers that use standard econometric techniques, but study periods where temporary extensions are in effect. Only Moffitt and Nicholson's (1982) estimate of 0.10 weeks is below a portion of the results in Figure 7. This may be due to the quasi-experimental approach, which controls for the endogeneity of the policy. In addition, the average estimate on the effect on unemployment spell length of 0.08 is at the lower end of papers that use a quasi-experimental approach. Of the papers cited previously only some of Lalive et al.'s (2006) estimates and Lalive's (2007) estimates for men are lower. This may be a result of the fact that these papers analyzing more permanent extensions than the SEB program, which has built in uncertainty that may mitigate the effects of more generous UI.[20]

8 Conclusion

The Standby Extended Benefit program in the United States provides an opportunity to study the impact of temporary changes in UI benefit duration. Since access to extensions under the SEB program are tied to state unemployment rates, they are more uncertain than other policy changes studied and information about the availability of these extensions may be limited. In addition, by using a quasi-experimental design, this paper disentangles the effects of SEB extensions from deteriorating labor market conditions which triggers the benefit extension. Various diagnostics show that the regression discontinuity design is appropriate for analyzing the SEB program.

The results indicate a significant casual effect on the order of a 13% increase in county-level unemployment rates as a result of the SEB extension. This effect appears to be fairly robust to alternative specifications, with just a few estimates falling outside of the 11% to 17% range. A simple flow equation allows me to convert the effects of the SEB program on county unemployment rates to an effect on the average duration of unemployment. I find that the 14% effect on county unemployment rates implies an increase of 0.08 weeks of unemployment for each week of additional benefits using reasonable values of the separation and unemployment rates. There are two likely reasons why the estimates are low relative to other papers. First, the RD approach accounts for the endogeneity between the increase in benefit duration and labor market conditions, while other papers often ignored this issue. Secondly, this paper focuses exclusively on the SEB program where benefits are very uncertain and information may be imperfect, which may mitigage the disincentive effects of the benefit extensions.

One limitation of this study was the inability to adjust the data for commuter and inter-state claims. As a result it may be possible that some UI recipients residing in states that have triggered on benefits are not eligible for an extension and some recipients residing in a state that has their trigger off may be eligible for a SEB extension. This results in the average benefit entitlement on each side of the policy border being more similar than if the SEB program was purely based on residency and may bias the results downward. However, these claims are very limited and I do not believe they are driving this paper's results.

[20]While not presented, the estimated effect on average duration increases by 0.01 for each 0.02 increase in the treatment effect on the unemployment rate. Given the majority of the estimates in the prior section are within the 0.11 to 0.15 range, the range of estimates for the effect on average duration would be between 0.05 and 0.14, which would still be at the lower end of the treatment effects cited in this paper.

During the current 2007 - 2010 recession, UI benefits have been extended by 76 weeks as a result of both the SEB program and the Emergency Unemployment Compensation Program of 2008 (an EEB program). As Congress debates these extensions, one of the major concerns is their effect on search behavior. While one should always use caution in generalizing the results from a quasi-experiment, the 1991 sample this paper analyzes indicates that, while significant, the impact on search behavior from these extensions may be small, at least for the portion that is due to the SEB program. If Congress is interested in mitigating the effects of extending UI benefits in future recessions, policymakers may want to consider relying solely on a SEB type program, where future extensions are contingent on future and uncertain unemployment rates.

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A Determining the Minimum Distance to the Border

To determine the minimum distance between a county and the policy border, the following algorithm is used: First, start at a given county. Then eliminate all other observations that fall in states with the same trigger status. For the remaining counties, calculate the direct line distance between these observations and the current observation being examined using the formula:

$$\begin{aligned} d &= 3963 \arccos[\sin(Lattitude_a/57.2958) \sin(Lattitude_b/57.2958) \\ &\quad + \cos(Lattitude_a/57.2958) \cos(Lattitude_a/57.2958) \\ &\quad \times \cos(Longitude_b/57.2958 - Longitude_a/57.2958)] \end{aligned}$$

where d is the distance between counties a and b and $Latitude$ and $Longitude$ are the associated coordinates for the two observations. With all of the distances calculated for the current observation, the minimum is selected as the distance to the policy border. Then, the next observation is analyzed until distances are calculated for all observations. To normalize the distances, the minimum number of miles that a county's centroid is away from the policy border was subtracted from each county's distance, d. The results are not sensitive to this normalization.

B Joint Test of Discontinuities in the Covariates

Lemieux and Lee (2009) suggest, that in order to ensure that a RD design is appropriate that all available covariates be tested jointly to ensure that they are balanced on either side of the cut-off point. The test is constructed by stacking the following regressions:

$$\begin{aligned} x_1 &= \alpha_1 + \beta_1 T + \gamma_1 d + \tau_1 dT \\ \vdots & \quad \vdots \\ x_K &= \alpha_K + \beta_K T + \gamma_K d + \tau_K dT \end{aligned} \tag{13}$$

where z_1 to z_K are the covariates included in the analysis. Regressions are stacked by pooling all covariates as dependent variables and interacting d and T fully with dummy variables representing each covariate. Robust standard errors are then clustered by individual observations, which repeats K times for each covariate.

One issue that arises when stacking the equations in (13) is again the appropriate function to use with respect to d_i, particularly if the AIC would select different polynomials for different covariates. One solution, cited by Lemieux and Lee (2009), is to use a narrower range around the c where a linear specification is more likely to be appropriate. For this test, I use a bandwidth half the size (35 miles) of that chosen by the cross-validation procedure. The test is a joint F-test that the β's are zero and the resulting test statistic is 1.07.

C Cross-Validation Procedure

To derive the most appropriate bandwidth, I use the cross-validation procedure Ludwig and Miller (2007) utilizes. The first step is to define a set of observations that are close to the border, which I refer to as the cross-validation set. I define this set as those observations within 18.2 miles, which is the 25th percentile of observations in the treated group.

Next, a given bandwidth, h, is chosen. Then, for observation j in the cross-validation set, the following local linear regression is run for observations in the untreated group:

$$\min_{\alpha,\beta,\tau,\gamma,\delta} \sum_{i=1}^{N} 1[-h \leq d_i < d_j](u_i - \alpha - \beta d_i - \delta' X_i)^2 \tag{14}$$

where d_j is the distance to the policy border for observation j. In the case of an observation in the treated group, a comparable regression is run:

$$\min_{\alpha,\beta,\tau,\gamma,\delta} \sum_{i=1}^{N} 1[d_j > d_i \geq h](u_i - \alpha - \beta d_i - \delta' X_i)^2 \tag{15}$$

I use the parameters from either (14) or (15) to make a prediction for the outcome variable for observation j, $\widehat{u(d_j)}$. These steps then repeat for all observations in the cross-validation set and I construct the following statistic for a specific bandwidth:

$$CV_u(h) = \frac{1}{N} \sum_{i=1}^{N} (u_i - \widehat{u(d_j)})^2 \tag{16}$$

The bandwidth choice is determined by:

$$h_{CV} = arg\min_{h} CV_y(h)$$