Contributions

Krishna Regmi*

Extended Unemployment Insurance and Job Search: Evidence from Time Use Data

Abstract: In this paper, I investigate the effect of extended unemployment insurance (UI) coverage in the United States in recent years on job search. The U.S. government extended UI benefits in several phases in 2008–2009, increasing the duration of the benefits to a maximum of 99 weeks, up from the regular 26 weeks. Using the American Time Use Survey (ATUS) data, I find that women are more sensitive to the extended UI benefits than men. Difference-in-differences estimation shows that the average effect of the UI extensions for women is over a 10 percentage points decline in the probability of job search. However, I do not find any statistically significant effect on men.

Keywords: job search, extended unemployment insurance, difference-in-differences

JEL Classification: J22, J64, J65, J68

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1 Introduction

The U.S. government extended the duration of unemployment insurance (UI) benefits in several phases from mid-2008 to late 2009, raising the duration up to 99 weeks from the regular 26 weeks. The extensions, billed as the most generous in U.S. history, were targeted at financially helping job losers smooth their consumption in a period of severe unemployment resulting from the Great Recession of 2007–2009. However, the government's move to extend benefits stirred a debate over the disincentive effect on recipients' labor supply, which could contribute to a higher unemployment rate. For instance, in an op-ed, Barro (2010) argues that in the absence of extended UI coverage, the U.S. unemployment rate would have been 6.8% in June 2010, instead of the actual

***Corresponding author: Krishna Regmi,** Department of Economics, Lehigh University, Rauch Business Center, 621 Taylor Street, Bethlehem, PA 18015, USA, E-mail: kpr210@lehigh.edu

rate of 9.5%. On the other hand, the administration has argued that UI extensions have had no impact on the persistently high unemployment rate, which is viewed as caused by the severe downturn and sluggish recovery associated with the Great Recession.

Establishing the causal link between the UI benefits extension and the unemployment rate is inherently difficult because of the endogenous nature of the insurance program. In theory, the disincentive effect of an expansion in UI benefits impacts the unemployment rate through two direct channels: discouraging job search behavior of unemployed individuals and increasing their reservation wage (i.e., decreasing their job-acceptance rate). Indirectly, there are at least four additional channels through which extended UI benefits could affect the unemployment rate both in the short run and in the long run. First, the coverage can increase consumption of goods and services, thus edging up overall output and the employment rate in the short run. The Congressional Budget Office (CBO 2012) estimates that one dollar spent on extended UI benefits can create a multiplier effect of $1.10 in the economy. Second, it entails "crowd-out" effect in the short run, which is the effect of the receipt of UI benefits by a family member on the labor supply by other family members. Cullen and Gruber (2000) find that wives' hours of labor supply could go up by about 30% during the period of their husbands' unemployment if the latter did not have access to UI benefits. Likewise, in the long run, unemployment insurance could help better match workers and firms, thus increasing output and, consequently, employment in the economy (Acemoglu and Shimer 2000). With benefits, an unemployed individual could afford to search and wait for a better-suited, higher-paid and higher-productivity job. Finally, the disincentive effect of UI benefits may lengthen the duration of unemployment, deteriorating their skills for future jobs, thus decreasing productivity in the economy and increasing unemployment in the long run.

The direct and indirect disincentive effects require a general equilibrium analysis to quantify the link between the UI program and unemployment rate as well as unemployment spells. Therefore, in an empirical investigation of the unemployment insurance program, evaluating its effect on the key variables such as job search and reservation wage, seems to be more reasonable strategy. Yet there is a paucity of research studying the effects of recent UI extensions on the two key variables. Krueger and Mueller (2010) study the effect of regular UI benefits on job search intensity of unemployed individuals in the United States from 2003 to 2007. One of their findings is that the generosity of unemployment insurance negatively impacts job search activity among the unemployed (estimated elasticities range from −1.6 to −2.2). Econometric methodology and research question in this paper differ somewhat from Krueger and Mueller (2010). Particularly, in this paper I attempt to estimate the causal effect of the expansions in UI coverage during the Great Recession of 2007–2009 and its aftermath, on job search behavior of the unemployed.

In line with the labor supply literature, I focus on the effects on women and men separately. Women's labor supply is considered to be more elastic than men's. For example, Alesina, Ichino, and Karabarbounis (2011) argue for gender-based taxation because men are less responsive to market wages and compensation. They argue that because of historical, social and cultural reasons, men derive more utility from participating in the labor market even while market compensation is low. However, it is socially acceptable for women to fully engage in home production. The intuition of Alesina, Ichino, and Karabarbounis (2011) is equally valid in the UI system. Unemployment benefits could induce women to abandon the labor market in favor of home production.

Furthermore, the maximum amount of unemployment benefits, which is not tied to potential earnings, can replace only a small fraction of lost income or potential earnings for those who possess skills to command a higher salary. (An average beneficiary nationwide received about $300 a week in 2011). Hence, the UI program may be expected to create heterogeneous disincentive effects across groups. For example, educated individuals may find the UI benefits insufficient. In addition, the opportunity cost of not looking for a job and foregoing possible earning opportunities would be high. Thus we attempt to measure the disincentive effects across groups, including those with a college degree and those without it.

This paper's objective is to focus on just one "channel," where UI extension can be treated as plausibly exogenous. In my research design, I assume that eligibility for UI is not affected by search activity. I use the American Time Use Survey (ATUS) data, as it provides information about job search activities of unemployed individuals. This paper's identification strategy uses unemployed persons eligible for UI benefits as a treatment group and those ineligible as a control group (e.g., Farber and Valletta 2013; Rothstein 2011; Krueger and Mueller 2010).

I employ the difference-in-differences (DD) method, since the extensions in 2008–2009 could be treated as a natural experiment or pseudo-experiment. Before proceeding to the actual estimation, I test if the control group could serve as a true counterfactual, so that the proposed DD methodology is a statistically valid approach. The post-treatment sample consists of the period 2010–2011. Even though the UI extension began in June 2008, Congress successively revised and introduced new tiers of extensions with different features and benefits until November 2009 (see Section 2 for details). More importantly, the extensions were still intact through 2010 and 2011 (and continued into 2013, albeit with some changes made in 2012). At the time of writing, the latest year of data available from the ATUS is 2011. I choose the period before June 2008, specifically from April 2004 to May 2008, as the pre-treatment period. Prior to

March 2004, another moderately extended UI coverage – Temporary Extended Unemployment Compensation (TUEC) – was in effect. As UI coverage was extended during recessions in the past, the unemployed might have anticipated an extension in UI coverage, especially after the beginning of recession (December 2007) and might subsequently have reduced job search efforts. To overcome this anticipation effect, I also estimate models using pre-treatment period only until December 2007. It is worth mentioning that even though the potential duration of benefits has been extended, the replacement rate (ratio of the UI benefits relative to previous earnings) has been left unchanged. Therefore, the recipient received the weekly dollar amount of the extended benefits (EBs) equal to the regular benefit amount.

To preview the results, I find women are more responsive to the extended UI benefits (the effect for women is negative and statistically significant) than men. The average effect of the extensions of UI coverage on job search behavior of unemployed women is over a 10 percentage point decline in the probability of job search. I find that the probability of unemployed men searching for a job decreases by one percentage point; however, it is not statistically significant. I apply a placebo test to check whether the model is capturing actual treatment effects, not the pre-existing differential trends between treatment and comparison units.

This paper is related to the literature empirically investigating the disincentive effect of UI benefits. The majority of the literature that is focused on the effect of UI benefits on unemployment durations or exits (e.g., Katz and Meyer 1990; Meyer 1990; Card and Levine 2000; Jurajda and Tannery 2003) shows a wide range of disincentive effects. Furthermore, this paper complements a strand of the literature that looks at the disincentive effect of EBs associated with the Great Recession on unemployment rates. Rothstein (2011), along with examining the effect of UI extensions on unemployment exits and durations, investigates the effect on the unemployment rate, and finds a small effect of about a 0.1 to 0.5 percentage point increase in the unemployment rate. Farber and Valletta (2013) apply a similar approach to Rothstein (2011), and their estimates show a statistically significant and small increase in unemployment duration, and around a 0.4 percentage point rise in the unemployment rate. Fujita (2011) and Mazumder (2011) employ a simulation approach, based on the estimated parameters in the previous research, to measure the disincentive effect of UI on unemployment rates. They find that extended UI benefits contributed to around a 0.8 to 1.2 percentage point increase in the unemployment rate. Using a structural calibrated model, Nakajima (2012) finds that UI extensions of 2008–2009 caused the unemployment rate to increase by 1.4 percentage points, accounting for about 29% of the total unemployment rate increase in the United States between 2005–2007 and 2009–2011. This paper contributes to this literature by looking at the effect of extended UI benefits on job search behavior, a key variable affected by the UI program.

This paper is also related to a growing literature studying the job search effort of the unemployed. Aguiar, Hurst, and Karabarbounis (2013), who study how individuals utilize their lost work hours during the recent recession in the United States, find that around 2 to 6% of the lost market work hours were devoted to job search. Likewise, DeLoach and Kurt (2013), Gomme and Lkhagvasuren (2013), and Mukoyama, Patterson, and Sahin (2014) study the job search behavior over the business cycles using the ATUS data. Though not a primary objective of this paper, its results have implications for understanding the prediction of the standard search and matching model. For instance, Shimer (2005) finds that the textbook search and matching model is unable to account for the observed volatility of the labor market tightness, defined as the vacancy to unemployment ratio. The volatility might be affected by fluctuations in job search behavior over the business cycle due to changes in generosity of unemployment benefits. Note that the U.S. government has historically extended the unemployment insurance during periods of economic downturn.

The remainder of this paper proceeds as follows. Section 2 offers a brief description of the unemployment insurance system in the United States. Section 3 describes data and presents basic estimates. I design a difference-in-differences specification in Section 4. Section 5 presents results, and Section 6 includes placebo tests and further robustness checks. Section 7 presents its conclusions.

2 Background on unemployment insurance extensions

Established under the Social Security Act of 1935 as a joint federal-state program, the unemployment insurance (UI) system provides temporary financial support to workers who lose lobs. The UI aims to ease financial hardships of the unemployed and facilitate their job search behavior. Normally, states set the parameters of the regular UI benefits, including the maximum and minimum amount of the benefits, the potential duration of the benefits, and eligibility criteria. The duration of availability is typically 26 weeks in most states[1] during normal economic periods. The weekly benefit amount varies from state to sate.

1 In Missouri and South Carolina, regular UI benefits are available for 20 weeks, and in Arkansas for 25 weeks (see "Council of Economic Advisors 2011").

In 2011, an average worker received around $300 a week nationwide, replacing almost half of his or her previous earnings. To receive benefits, workers must lose their job through no fault of their own, must have a certain amount of previous earnings, and must actively search for work. Generally, new entrants and re-entrants into the labor force as well as those who voluntarily leave jobs or are fired for a cause like misconduct are not eligible for the benefits. According to Krueger and Meyer (2002), about 40% of all unemployed individuals (both eligible and ineligible) qualify for and receive UI benefits.

One important aspect of the U.S. unemployment insurance system is that the government has historically extended the duration of UI benefits during periods of economic downturn through two channels. Since 1970, EB, a joint federal-state program, has provided an additional 13 and 20 weeks of benefits in states exceeding the unemployment rate of 6.5% and 8%, respectively. On top of that, the federal government has extended the duration of UI coverage on a temporary basis to the unemployed during recessions or periods of severe unemployment. So far Congress has activated such temporary programs eight times – 1958, 1961, 1971, 1974, 1982, 1991, 2002, and 2008 (see Whittaker and Isaacs 2013).

As part of its response to severe unemployment brought about by the Great Recession, Congress enacted the Emergency Unemployment Compensation (EUC) program in June 2008 (see Appendix Table 13). In the beginning, the EUC offered an additional 13 weeks of benefits to the unemployed who exhausted their regular benefits. Congress expanded the duration of EUC benefits in successive phases until November 2009, increasing the duration of the benefits under the EUC up to 53 weeks. There are four tiers of EUC. EUC Tiers I and II offer an additional 20-weeks and 14-weeks of compensation for the unemployed in all states irrespective of unemployment rates. However, the remaining two tiers are based on unemployment rates. EUC Tier III provides benefits for a further 13 weeks in states where the unemployment rate exceeds 6%. EUC Tier IV is implemented in states exceeding 8.5% unemployment and offers an additional 6 weeks of benefits. In total, an unemployed individual in 2010 and 2011 was able to receive UI benefits as long as 99 weeks (26 weeks under the standard UI, 20 weeks under the EB, and 53 weeks under the EUC). These unprecedented extensions have been called the most generous in U.S. history (e.g., Barro 2010).

3 Data and basic estimates

This paper draws its main data from the ATUS as it is the only nationally representative survey that contains information about job search activity. This information enables me to directly investigate the effect of unemployment insurance on job search behavior. The U.S. Census Bureau conducts the ATUS, which is sponsored by the Bureau of Labor Statistics. The ATUS selects respondents from the outgoing sample of the Current Population Survey (CPS). Individuals aged 16 or above are surveyed in the ATUS, 2–5 months after their final CPS interview; about 71% after 3 months. Individuals are asked to report their detailed activities on the basis of their previous day's time use diary. One of the several activities reported in the ATUS is the amount of time spent on job search. Job search includes making contacts to employers, looking at and responding to job advertisements, filling out applications, and traveling to job interviews. A complete list of activities used as job search in this paper is given in Table 14 in Appendix B.

I limit the data to unemployed individuals between ages 25 and 64 years in the main estimations since this group has a strong participation in, and inclination toward, the labor market.[2] This paper's choice of a lower-bound age restriction follows Blau and Kahn (2007) and Cullen and Gruber (2000), in order to avoid complications related to choices of schooling versus market work by young people. According to Mulligans (2012), more than 75% of unemployed youths between ages of 16 and 24 years do not receive UI benefits. Upon reaching 65, an individual's labor market behavior may change due to eligibility for social security and Medicare benefits. I use data from April 2004 to 2011. The ATUS began collecting data in 2003. Its 2003 survey consists of around 20,000 respondents. Since 2004, the number of respondents has decreased to approximately 13,000 each year. The sample (which excludes individuals who are employed, are out of the labor force or aged below 25 and above 64) includes 3,259 unemployed persons. The data are repeated cross-sections.

The definition of an unemployed person in the ATUS is the same as in the CPS. The definition includes those aged 16 years and older who are unemployed, but available for work in the reference week and who have actively been searching for work in the past 4 weeks. Search rule is exempted for those individuals who are expecting to be recalled to a job. (These individuals are still considered unemployed even if they have not looked for work in the past 4 weeks prior to the survey.)

The ATUS does not contain any explicit information about eligibility and ineligibility for UI benefits, nor does it have any information on whether eligible individuals indeed claim the benefits. However, the data contain information about individuals' reasons for unemployment. Using these reasons for unemployment, I follow the classification strategy of Krueger and Mueller (2010) to infer an individuals' eligibility for UI benefits. In recent years, this strategy of classification has become popular in the literature e.g., Farber and Valletta

2 Nonetheless I separately estimate effects of the UI extensions for youths between ages of 16 and 24 years.

(2013), Rothstein (2011). I first divide unemployed persons into four categories: (i) job losers, (ii) those expecting a recall from their previous employer, (iii) voluntary job leavers, and (iv) new entrants and re-entrants into the labor force. Then, I categorize: (i) job losers as well as (ii) those expecting a recall from the previous employer as eligible for UI benefits, and (iii) voluntary job leavers as well as (iv) re- or new entrants into the labor force as the ineligible group. More specifically, each category is defined in the following way:

(i) Job losers: This category includes persons who reported in the CPS interview that they were on layoff, or that they had become unemployed due to the ending of a temporary job, and are still unemployed in the ATUS interview. It also includes those who reported in the CPS that they are employed, but are unemployed in the ATUS interview. It could be recalled that CPS data were collected 2–5 months prior to the ATUS.

(ii) Expecting a recall: This category consists of unemployed persons who indicate in the ATUS interview that they are expecting a recall for work from their previous employer.

(iii) Voluntary job leavers: This category includes those who reported in the CPS that they quit their job and are unemployed in the ATUS. It is worth mentioning that the ATUS questionnaire does not include any information about job quitters, so I turn to the CPS data to identify them.

(iv) Re- and new entrants: This category includes those persons who reported in the CPS that they are out of the labor force, but who are unemployed at the time of the ATUS interview. It also includes those who reported in the CPS interview that they are re-entrants or new entrants in the labor force and are still unemployed at the time of the ATUS.

Presumably, my classification strategy has some flaws since I am unable to observe if eligible persons actually receive UI benefits. One of the flaws in my classification is related to re- and new entrants into the labor force. In my classification strategy, those who are re- and new entrants or those who are out of the labor force at the time of the CPS interview but are unemployed in the ATUS interview are categorized as ineligible. However, it is possible for some of them to have found a job and become unemployed again during the period between the CPS and ATUS interviews. So, it is possible for me to classify a person as ineligible on the basis of the CPS data where he/she in fact became eligible in the months between the CPS and ATUS survey. However, I could not verify this possibility in the ATUS data. This classification error is expected to create a downward bias in the estimation of the parameter of my interest, thus underestimating the true effect of UI extensions. Likewise, I am unable to observe if eligible individuals actually receive benefits. Those unemployed who are not actually collecting unemployment benefits might have a higher participation rate in job search (if the theory that UI benefits discourage job search is to be believed). Excluding them from the eligible group decreases the overall mean of job search of that group, which in turn increases absolute magnitude of UI effect. To sum up, these errors are likely to create a downward bias in my estimates.

3.1 Preliminary Analytics

In this subsection, I attempt to calculate both the probability and intensity of job search across groups before and after the extensions of UI benefits, using the data from April 2004 to May 2008, and 2010 to 2011. Table 1 contains job search participation rates in pre- and post-treatment periods across treatment and control groups. I define the participation rate (i.e., the probability of participation) as the share of unemployed individuals in the sample who spend time in job search on the surveyed day.

It is worth noting that higher probabilities of job search are seen in the treatment group than comparison group in both periods. Put differently, to begin with, individuals in the treatment group have a different level of search effort. However, difference-in-differences (DD) estimator would still be valid, irrespective of initial search effort, as long as the common trend assumption holds.

The calculations in Table 1, which are DD estimates without any control variables, show that UI coverage expansions decreases the job search probability of the unemployed women eligible for UI benefits by around seven percentage points, relative to those ineligible women. Likewise, the average effect of UI extensions for men is around a three percentage points decline in the probability of job search. If the treatment (the extensions of UI coverage) were completely

Table 1: Difference-in-differences (DD) estimates without control variables

	Women			Men		
	Control	Treatment	Difference	Control	Treatment	Difference
Before	0.108	0.205	0.096	0.209	0.275	0.066
	(0.023)	(0.027)	(0.036)	(0.044)	(0.031)	(0.054)
After	0.204	0.227	0.023	0.256	0.291	0.035
	(0.032)	(0.033)	(0.046)	(0.050)	(0.028)	(0.057)
DD			−0.073			−0.031
			(0.058)			(0.079)

Notes: The estimates are the average job search probabilities. Standard errors are reported in parentheses. These are calculated using the ATUS data from April 2004 to May 2008 (before treatment), and from 2010 to 2011 (after treatment), and applying survey weights.

random, this estimate would be the implied causal effect. However, the estimates might be driven by observed or unobserved individual characteristics, instead of being the effect of the extended UI coverage. I then attempt to understand if there is any change in the composition of the control group between pre- and post-treatment periods, to raise the group's job search outcome in a larger proportion than that of the treatment group. For this purpose, I look at the observed characteristics between the pre- and post-treatment period. Table 2 contains observed characteristics of each group before and after the

Table 2: Characteristics of individuals by group

	Control		Treatment	
	Before	**After**	**Before**	**After**
Panel A: Women				
Less than HS	0.216	0.205	0.170	0.093
High school	0.526	0.541	0.522	0.493
Bachelor's degree	0.199	0.195	0.232	0.322
Master's degree/PhD	0.055	0.058	0.074	0.092
Age	39.488	39.537	41.622	44.119
Child dummy	0.645	0.608	0.530	0.465
Married	0.615	0.588	0.608	0.550
White	0.455	0.462	0.657	0.573
Black	0.235	0.220	0.139	0.202
Hispanic	0.220	0.259	0.158	0.164
Others	0.091	0.059	0.045	0.061
Unemployment duration	8.055	15.269	7.07	23.248
Observations	375	293	377	341
Panel B: Men				
Less than HS	0.173	0.219	0.121	0.176
High school	0.553	0.547	0.593	0.553
Bachelor's degree	0.237	0.185	0.201	0.204
Master's degree/PhD	0.0295	0.032	0.071	0.062
Age	43.765	42.022	42.878	43.299
Child dummy	0.405	0.327	0.376	0.414
Married	0.519	0.499	0.597	0.574
White	0.475	0.540	0.577	0.591
Black	0.310	0.292	0.200	0.187
Hispanic	0.126	0.127	0.160	0.177
Others	0.089	0.041	0.063	0.045
Unemployment duration	9.397	11.543	7.107	25.401
Observations	154	143	359	410

Note: The estimates are calculated using the ATUS data from April 2004 to May 2008 (before treatment) and from 2010 to 2011 (after treatment).

treatment (Panel A for women and Panel B for men). The characteristics of each group mostly seem similar between pre- and post-treatment periods. The only noticeable changes are a shift to more educated/experienced women in the treatment group after the recession. But, the big shift in job search is for the control group, not the treatment group. Nonetheless, we cannot deny a possibility that unobserved characteristics might be different between pre- and post-treatment periods, but we cannot verify it. (In this event, the conditional independence assumption could be violated.)

On the other hand, it could be argued that as the job arrival rate is lower during the period of economic slack (the post-treatment period), both groups are required to put extra search efforts to achieve the same level of success as in the normal economic times (pre-treatment period). Hence, those ineligible for unemployment benefits speed up their efforts in the post-treatment period, while those eligible did not speed up due to the generous benefits. This might have led to a slower growth of job search by the treatment group.

Table 3 presents the probabilities of job search over the subgroups of unemployed individuals across gender on a given diary day before and after the UI expansions. On average, around 16% of the total unemployed women engaged in job search (i.e., had greater than zero-minute job search) before the extensions of UI coverage, in comparison to 22% after the extensions. In all subcategories, the participation of women in job search in the post-extension period

Table 3: Probability of job search by group

	Women				Men			
	Before		After		Before		After	
	Prob.	***N***	**Prob.**	***N***	**Prob.**	***N***	**Prob.**	***N***
All unemployed individuals	0.16	752	0.22	634	0.26	513	0.28	553
By types of unemployment								
Job losers	0.20	377	0.23	341	0.28	359	0.29	410
Quitters	0.23	27	0.26	16	0.22	19	0.48	11
New- or re-entrants	0.10	348	0.20	277	0.21	135	0.24	132
Job search by race								
White	0.18	433	0.22	318	0.25	288	0.31	301
Black	0.16	141	0.22	150	0.26	109	0.35	132
Hispanic	0.10	145	0.19	130	0.24	80	0.12	93
Others	0.16	33	0.24	36	0.36	36	0.23	27

Note: Job search probabilities are calculated using the ATUS data from April 2004 to May 2008 (before), and from 2010 to 2011 (after), and applying survey weights.

is higher than pre-extension period. Likewise, the proportion of unemployed men engaging in job search was 28% after the extensions, up from 26% before the extensions. Among the sub-categories of unemployed men, all except Hispanic and other groups have a higher proportion of job-search in the post-extension period than the pre-extension period.

Table 4 reports job search intensity (i.e., the total amount of time an individual spends looking for a job on a given diary day). Except quitters, there has been an increase in job search activity for women by their types of unemployment and race in the post-extension period as compared to the pre-extension period. Among unemployed men, quitters, Hispanic and those in the category of other race has decreased job-search intensity in the post-treatment period.

Table 4: Job search intensity by group

	Women				Men			
	Before		After		Before		After	
	Intens.	*N*	Intens.	*N*	Intens.	*N*	Intens.	*N*
All unemployed individuals	20.90	752	35.37	634	50.82	513	51.68	553
By types of unemployment								
Job losers	31.19	377	36.04	341	54.12	359	56.58	410
Quitters	37.61	27	23.41	16	77.30	19	36.97	11
New- or re-entrants	8.78	348	35.26	277	37.94	135	38.98	132
Job search by race								
White	25.10	433	38.20	318	51.25	288	58.91	301
Black	14.79	141	37.50	150	44.44	109	60.84	132
Hispanic	14.50	145	27.22	130	46.75	80	20.58	93
Others	21.25	33	31.11	36	77.23	36	27.66	27

Notes: Job search intensity is calculated using the ATUS data from April 2004 to May 2008 (before), and from 2010 to 2011 (after), and applying survey weights. The intensity is measured as the number of minutes spent on job search on a given diary day.

Overall, these preliminary analytics show that job search activity has mostly increased across groups, including treatment and control groups, in the post-treatment period as compared to the pre-treatment period. Furthermore, those eligible for UI benefits had higher probability and intensity of job search in the pre-treatment period, in comparison to those ineligible. However, growths in both the probability and intensity of job search between pre- and post-extension periods for the eligible group were considerably slower than that of the ineligible group.

4 Difference-in-differences estimates for job search probability

My goal in this section is to assess the effect of UI extensions on job search behavior during the Great Recession and its aftermath. As the extensions could be seen as a pseudo-experiment, I apply a difference-in-differences (DD) regression, which is very popular in the literature to measure the policy effect or intervention. I use the period from March 2004 to May 2008 as the pre-treatment period and 2010–2011 as the post-treatment period. One of the major issues in DD analysis is whether the control units in a study can truly serve as the counterfactual of the treatment units. If there are systematic differences between the comparison and treatment units, the estimates from the DD approach may reflect results of their differences, not the policy effect (treatment). This is true even if the unconfoundedness assumption still holds in the regression. One indirect way of testing such a systematic difference is to test if there is a sufficient overlap in the covariates between treatment and control units. For this purpose, one may think of an conventional approach which is to use a t-test:

$$t = \frac{\bar{X}_1 - \bar{X}_0}{\sqrt{\frac{S_0^2}{N_0} + \frac{S_1^2}{N_1}}}. \qquad [1]$$

However, the major drawback with this test is that it is very sensitive to sample size, and it tends to over-reject the null hypothesis when the sample size increases. To address this limitation, Imbens and Wooldridge (2009) suggest using a normalized difference (which is also scale-invariant), where the difference is:

$$\Delta X = \frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_0^2 + S_1^2}}. \qquad [2]$$

The authors also suggest that researchers can use the threshold of one-quarter normalized difference as a rule of thumb. Exceeding this specified threshold implies that covariates are different between control and treatment units. In Tables 5 and 6, I report normalized differences for each individual-specific covariates used in the DD model for women and men respectively. The differences are calculated for the control and treatment groups during pre- and post-treatment periods, separately. Almost all of the differences are less than one-quarter. (For women, only one variable – a dummy variable for the white – is slightly higher than one-quarter, and for men, age variables slightly exceed this threshold.) Overall, this suggests that individual characteristics are not

Table 5: Normalized differences: women

Variables	Treat. group		Cont. group		
	Mean	SD	Mean	SD	Difference
Panel A: During the pre-treatment period					
Less than HS	0.170	0.376	0.216	0.412	−0.084
High school	0.522	0.500	0.526	0.500	−0.005
Bachelor's degree	0.232	0.423	0.200	0.400	0.056
Masters degree	0.074	0.262	0.055	0.228	0.056
PhD	0.002	0.043	0.004	0.060	−0.025
Age	41.622	10.119	39.489	9.997	0.150
Age squared	1,834.525	866.403	1,659.023	849.321	0.145
Child dummy	0.530	0.500	0.645	0.479	−0.166
Partner	0.608	0.489	0.615	0.487	−0.010
White	0.657	0.475	0.455	0.499	0.294
Black	0.140	0.347	0.235	0.424	−0.174
Hispanic	0.159	0.366	0.220	0.415	−0.110
Other	0.045	0.207	0.091	0.288	−0.131
N	377		374		
Panel B: During the post-treatment period					
Less than HS	0.093	0.290	0.205	0.404	−0.225
High school	0.493	0.501	0.541	0.499	−0.067
Bachelor's degree	0.322	0.468	0.195	0.397	0.206
Masters degree	0.092	0.290	0.058	0.234	0.092
PhD	0	0	0.001	0.030	−0.030
Age	44.119	10.984	39.537	10.247	0.305
Age squared	2,066.757	965.846	1,667.844	845.857	0.311
Child dummy	0.465	0.500	0.608	0.489	−0.204
Partner	0.550	0.498	0.588	0.493	−0.054
White	0.573	0.495	0.462	0.499	0.158
Black	0.202	0.402	0.220	0.415	−0.032
Hispanic	0.164	0.371	0.259	0.439	−0.165
Other	0.061	0.239	0.059	0.235	0.006
N	341		293		

Notes: The estimates are calculated using the ATUS data from April 2004 to May 2008, and from 2010 to 2011, and applying survey weights.

systematically different by the treatment status. Therefore, the difference-in-differences estimator could be seen as a plausible empirical strategy.

As many respondents in the sample are not participating in job search (i.e., they have zero-search) on a given diary day, I am interested in looking at the impact of the UI benefits on the probability of their decision to participate in the

Table 6: Normalized differences: men

Variables	Treat. group		Cont. group		
	Mean	**SD**	**Mean**	**SD**	**Difference**
Panel A: During the pre-treatment period					
Less than HS	0.121	0.326	0.173	0.379	−0.104
High school	0.593	0.492	0.553	0.499	0.057
Bachelor's degree	0.201	0.401	0.237	0.427	−0.062
Masters degree	0.071	0.258	0.030	0.170	0.135
PhD	0.013	0.115	0.007	0.086	0.042
Age	42.878	10.272	43.765	11.876	−0.057
Age squared	1,943.707	891.635	2,055.520	1,040.115	−0.082
Child dummy	0.376	0.485	0.405	0.493	−0.042
Partner	0.597	0.491	0.519	0.501	0.111
White	0.577	0.495	0.475	0.501	0.144
Black	0.200	0.401	0.310	0.464	−0.179
Hispanic	0.160	0.367	0.126	0.333	0.069
Other	0.063	0.243	0.089	0.286	−0.070
N	359		154		
Panel B: During the post-treatment period					
Less than HS	0.176	0.381	0.219	0.415	−0.077
High school	0.553	0.498	0.547	0.500	0.008
Bachelor's degree	0.204	0.404	0.185	0.390	0.034
Masters degree	0.062	0.242	0.032	0.177	0.101
PhD	0.005	0.072	0.017	0.128	−0.077
Age	43.299	11.568	42.022	11.119	0.080
Age squared	2,008.297	1,004.439	1,888.653	964.058	0.086
Child dummy	0.414	0.493	0.327	0.471	0.127
Partner	0.574	0.495	0.499	0.502	0.107
White	0.591	0.492	0.540	0.500	0.072
Black	0.187	0.390	0.292	0.457	−0.177
Hispanic	0.177	0.383	0.127	0.334	0.100
Other	0.045	0.208	0.041	0.199	0.016
N	410		143		

Notes: The estimates are calculated using the ATUS data from April 2004 to May 2008, and from 2010 to 2011, and applying survey weights.

job search activity. (In an alternative specification and robustness check later, I actually look at the effect on job search intensity using the DD method in the form of eq. [3].) It is important to note that the probability of job search in this paper could be viewed as the probability of sampling an individual who spends a positive amount of time in job search activities. It is true that to be counted as unemployed in the CPS data, an individual should have at least one active job search in the last 4 weeks preceding the surveyed date. Note that respondents in the ATUS data are drawn from the CPS's respondents, and the ATUS follows the CPS's definition of unemployment. In the CPS (and the ATUS), active job search refers to an activity which could land an individual on a job without any additional work. Otherwise, an activity is called passive, which is not enough for an individual to be categorized as an unemployed. For example, looking at an ad on newspaper is a passive activity. However, in the definition of job search in this paper, both active and passive activities are included. I have listed a complete list of job search activities in Appendix A.

One of the shortcomings of time diary data is that we can observe a respondent's activities only on a particular day. Hence, it is possible that the respondent might have spent time looking for a job on other days. Nonetheless, sampling an individual with positive amount of time devoted to job search could be a close proxy to the job search probability of the individual. The intensity with which a job seeker pursues a position should be positively correlated with the probability to have a positive amount of job search on a random day of the survey. For instance, if an individual has only one active job search in the past 4 weeks and is categorized as unemployed, it is very likely that we observe zero job search probability on the surveyed day. On a given surveyed day, around 18.38% unemployed women have greater than zero job search activities (i.e., around 81.62% of them have zero job search). Likewise, around 26.94% unemployed men have greater than zero job search activities. Table 3 contains further details on how many respondents have job search activities greater than zero on a diary day, by the type of unemployment before and after the treatment. Note that the probability of the participation represents the share of unemployed individuals in the sample who have a greater than zero job search on a given diary day.

One advantage of using the binary dependent variable (probability of job search instead of job search intensity) is that it is less prone to measurement error. The reason is that it is easier for individuals to remember whether they looked for a job on a given diary day, rather than recalling the amount of time they spent looking for a job. Specifically, I estimate the logit DD model below on repeated cross-sectional data to measure the causal effects of UI extensions on job search decision.

$$\begin{aligned}\Pr[y_{i(t,s)} = 1|Post_t, Treat_i, x_{i(t,s)}] = \Lambda[&\alpha + \beta_1 Post_t + \beta_2 Treat_i + \delta Post_t * Treat_i \\ &+ \lambda Trend_t + \theta' x_{i(t,s)} + \gamma_3 AWB_s + \gamma_1 un\_rate_s \\ &+ \gamma_2 un\_dur_i + \gamma_5 \ln\widehat{(w)}_{i(t,s)} + \gamma_4 stateresid_s],\end{aligned} \tag{3}$$

where $i = 1, 2, 3, \ldots N$ denotes individuals and s represents state. The outcome variable, $y_{i(t,s)}$, is an indicator variable equal to 1 if an individual participates in job search; otherwise 0. $\Lambda(.)$ is the logistic cumulative distribution function. The variable $Post_t$ is a dichotomous variable which equals 1 if it is the post-extension period. The variable $Treat_i$ is a dummy variable, equal to 1 if an individual is eligible for UI benefits. The variable $Post_t * Treat_i$ is the variable of interest (its coefficient is the treatment effect). The treatment and control groups have the same linear time trend. (In a later robustness check, instead of time trend I use a year fixed-effect model, and also use a quadratic time trend.) $x_{i(t,s)}$ is a vector of individual characteristics, including age, age-squared, level of education, race, sex, presence of child in house, and marital status.[3] The variable AWB_s is average weekly UI benefits (which represents the average dollar amount of benefits received by a recipient per week, calculated by month and state).[4] I also include other control variables described below.

The unemployment rate (un_rate_s): One of the major factors that might influence an individual's job search behavior is a perception about the macroeconomic environment. Unemployed individuals may not look for jobs during a period of severe unemployment if they perceive that jobs may not be available and that the marginal cost of job search will be higher than the expected payoff. Conversely, it could be argued that an individual needs to put extra time and effort to find a job in a period of mass unemployment due to a decline in jobs arrival rate. And, the literature is not conclusive about the cyclical property of job search behavior. DeLoach and Kurt (2013) and Gomme and Lkhagvasuren (2013) find job search behavior to be pro-cyclical (unemployed decrease search efforts during the labor market slack), while Mukoyama, Patterson, and Sahin (2014) show that the aggregate job search effort is countercyclical. To capture the potential change in an individual's job search behavior, I control for the monthly unemployment rate by state.[5] In the ATUS data, I can observe the month of interview of the respondents, so I am able to match the interview month with the monthly unemployment rate.

The unemployment duration (un_dur_i): The canonical model of Mortensen (1977) predicts that unemployed individuals ineligible for UI benefits put constant effort toward job search irrespective of their unemployment duration,

3 The variable partner represents martial status, which includes both the married and individuals cohabiting with a partner as a non-reference group.

4 I collect average weekly unemployment benefits from Department of Labor Employment and Training Administration (http://workforcesecurity.doleta.gov/unemploy/claimssum.asp).

5 I compile monthly state unemployment rates from the Bureau of Labor Statistics. The data can be downloaded at http://www.bls.gov/schedule/archives/laus_nr.htm#2003.

while eligible ones speed up their search effort when their benefits are about to expire. Therefore, controlling for unemployment duration is essential. However, the ATUS data do not include the unemployment duration of individuals. As in Krueger and Mueller (2010), I use the duration reported by unemployed individuals in the CPS interviews. I then add the number of weeks between their CPS and ATUS interview to the duration reported in the CPS. However, for the respondents who are not unemployed in the CPS interview but are in the ATUS, I am not able to observe their duration. Therefore, I impute a proxy duration for them, which is half of the weeks between the CPS and ATUS interviews.

Potential wage ($\widehat{\ln(w)}_{i(t,s)}$): Potential wage is believed to influence an unemployed individual's behavior. For those unemployed persons who expect a higher wage in the labor market, unemployment benefits might have minimal effects as their opportunity cost of staying unemployed would be high. For the low earnings group, the weekly benefits can replace a significant portion of their potential earnings, encouraging them to substitute their labor with leisure. However, the data do not contain information about previous earnings of the unemployed, even if they have work history. To deal with this wage issue, I estimate a predicted wage for each individual. First I use the Current Population Survey's (CPS) Outgoing Rotation Group data and estimate the following state-fixed effect model separately for each year[6] for log-wage:

$$\log(w_{i,s}) = \rho + \upsilon x'_{i,s} + \mathrm{dummy}_s + \varepsilon_{i,s}, \tag{4}$$

where x_{is} is a vector of individual characteristics, which include the level of education, age, age-squared, gender, marital status, and presence of children. I also use state dummies. Then, on the basis of estimated parameters from eq. [4] for a particular year, I predict expected log-wage for each respondent in the ATUS for that year.[7]

Distribution of the potential wage offer (*stateresid*$_s$): Another determinant of job search is the potential wage offer distribution. The rationale behind this is that the more dispersed the distribution is, the more time individuals spend looking for a job as they try to get the best possible wage. As a proxy for the potential wage-offer distribution, I use residual wage dispersion calculated as

6 I use the Central for Economic and Policy Research (CEPR) version of the CPS data. This data adjusts for over-time earnings, tips and commissions. Output results from this estimation are omitted due to space constraints.

7 Because each year I run a separate regression, the number of observations vary by the year. In 2004, the number of observations is 108,752. The number includes 108,228 in 2005, 107,085 in 2006, 106,314 in 2007, 104,910 in 2008, 139,326 in 2010, and 137,828 in 2011.

the standard deviation of the residual ($\varepsilon_{i,s}$) from wage eq. [4] for each state and year (see Krueger and Mueller 2010).

For the non-linear DD model, the interpretation of the policy parameter, δ, is different from the case of a linear regression. The average treatment effect on the treated (ATT) equals the differences in the two cross differences (e.g., Eissa and Liebman 1996; Puhani 2012). Let τ be the effect:

$$\begin{aligned}\tau =&\Lambda[\alpha + \beta_1 Post_t + \beta_2 Treat_i + \delta Post_t * Treat_i + \gamma_1 Trend_t + Z_{i(t,s)}{}'\theta] \\ &- \Lambda[\alpha + \beta_1 Post_t + \beta_2 Treat_i + \gamma_1 Trend_t + Z_{i(t,s)}{}'\theta],\end{aligned} \qquad [5]$$

where $Z_{i(t,s)}$ is a vector of all control variables used in regression [3]. Bertrand, Duflo, and Mullainathan (2004) show that the conventional DD standard errors are severely understated due to serial correlation. As suggested by the authors to correct for possible serial correlation, I calculate the variance–covariance matrix of errors, clustering at the state level, from the regression of eq. [3].[8] Then, I apply the Delta Method to calculate standard errors for estimates.

5 Results

In this section, I present results from a variety of specifications of difference-in-differences (DD) estimation based on eq. [3] to explore the causal effect of the recent UI extensions on job search behavior across gender. The focus of this paper on gender differences in the impact of the extensions in UI benefits is in line with the labor supply literature.

I estimate four-baseline nonlinear DD models for men and women separately; the results are presented in Tables 7 and 8. The first three models include same control group, but slightly different treatment groups. The first model consists of the full sample (all individuals eligible for UI benefits as treatment group). In the second and third models I attempt to exclude those eligible unemployed persons, who are perceived to have exhausted their UI coverage. In the pre-treatment period, UI benefits were usually available for 26 weeks. Therefore, I exclude those eligible individuals having the duration of unemployment more than 26 weeks in the pre-treatment time (from the treatment group). In the second model, I exclude all individuals who are unemployed more than 99 weeks in 2010 and 2011 (from the treatment group), as they were eligible for

8 Indeed, clustered standard errors in this paper are slightly higher than the conventional robust standard errors, supporting Bertrand, Duflo and Mullainathan's (2004) argument.

UI benefits up to 99 weeks. In the third model, I exclude all eligible individuals who are unemployed more than 60 weeks in the post-treatment period (for the reasons explained in the earlier Section).

In model 4, I exclude those unemployed persons who are on temporary layoff and expecting a recall from their previous employer (from both the control and treatment group), and those who voluntarily left the job (from the control group). It is expected that these people behave quite differently from the average labor market participant. The sample then consists of individuals who are on layoff through no fault of their own as eligible (treatment) group, and re- or new entrants in the labor force as ineligible (control) group.

In all these models, I find consistently close results (Table 7) for women. I get positive and significant effect of the eligibility status (i.e., the variable eligible) on job search behavior. One of the potential reasons for having higher job search probability for eligible group could be the requirement of job search to collect UI benefits. And, it is possible that eligible individuals might have kept up looking for a job; however, they might have refused to take up job offers, increasing reservation wage and extending the duration of unemployment (another disincentive effect of UI benefits which I do not explore here). Another possibility for the higher probability of job search by the eligible group than the ineligible group could be the result of the initial differences in job search behavior. It could be noted that the eligible group mostly consists of those who are laid off from work, and the ineligible group consists of re- or new entrants in the labor force. Arguably, a layoff can be considered as a shock to the worker. In light of the shock, the laid-off worker could be in greater need of a job to smooth or maintain consumption than that of a re- or new entrant who can have a relatively longer period to adjust for consumption. Nonetheless, this variable does not represent the effect of UI extensions.

The interaction term between eligibility and post-treatment period (referred as $Treat_i \times Post_t$) is the variable of interest as its coefficient is the implied causal effect of treatment (extensions of UI benefits). In the technical language of program evaluation, the coefficient is termed as average treatment effect on the treated (ATT). The average effect of the UI extensions in 2010 and 2011 is over 10 percentage points decline in the probability that an eligible unemployed woman searches a job. It is statistically significant at the 5% level in the first and third model, and at the 1% level in the second and fourth models. This represents around a 56% of the average predicted probability of job search (i.e., the ratio of the effect to $\bar{p}$). I find around a one percentage point decline in the probability of job search for men (Table 8). However, it is not statistically significant.

Table 7: Results from nonlinear difference-in-differences models: women

Variables	Full sample	Unemp. dur. $\leq$ 99 weeks	Unemp. dur. $\leq$ 60 weeks	W/0 Indivs. expecting a recall and quitters
	(1)	(2)	(3)	(4)
Treat	0.0983***	0.104***	0.106***	0.153***
	(0.0299)	(0.0310)	(0.0312)	(0.0314)
Post	0.0652	0.0613	0.0687	0.0612
	(0.0718)	(0.0754)	(0.0774)	(0.0784)
Treat×Post	−0.107**	−0.117***	−0.113**	−0.132***
	(0.0457)	(0.0437)	(0.0453)	(0.0494)
Time	0.0221	0.0214	0.0196	0.0238
	(0.0163)	(0.0170)	(0.0168)	(0.0186)
High school	0.0577	0.0571	0.0634	0.0583
	(0.0438)	(0.0428)	(0.0450)	(0.0570)
Bachelor's degree	−0.0551	−0.0830	−0.0735	−0.0319
	(0.0894)	(0.0827)	(0.0851)	(0.122)
Master's degree	−0.0209	−0.0514	−0.0563	0.000587
	(0.130)	(0.112)	(0.107)	(0.200)
Age	0.00153	−0.00201	0.0000694	−0.00182
	(0.0125)	(0.0129)	(0.0141)	(0.0152)
Age squared	−0.0000393	−0.00000228	−0.0000294	0.00000957
	(0.000141)	(0.000146)	(0.000163)	(0.000171)
Child dummy	−0.0375	−0.0459	−0.0498	−0.0337
	(0.0363)	(0.0366)	(0.0362)	(0.0452)
Partner	−0.0703**	−0.0804***	−0.0757***	−0.0447
	(0.0283)	(0.0277)	(0.0263)	(0.0337)
Black	−0.00141	−0.000699	0.00259	−0.00790
	(0.0382)	(0.0377)	(0.0360)	(0.0431)
Hispanic	0.0308	0.0421*	0.0475**	0.0161
	(0.0242)	(0.0231)	(0.0240)	(0.0246)
Other	0.00411	0.0233	0.0299	−0.0600
	(0.0680)	(0.0709)	(0.0734)	(0.0586)
Predict wage	0.344**	0.388**	0.374**	0.339*
	(0.148)	(0.151)	(0.150)	(0.190)
State residual	−0.485	−0.473	−0.584	−0.558
	(0.612)	(0.609)	(0.648)	(0.733)
Unemploy rate	−0.00481	−0.00298	−0.00293	0.000502
	(0.00764)	(0.00787)	(0.00823)	(0.00887)
Avg. weekly UIB	−0.000903***	−0.00105***	−0.00103***	−0.00102***
	(0.000324)	(0.000329)	(0.000338)	(0.000386)
Unemploy duration	0.000628	0.000578	0.000588	0.000256
	(0.000635)	(0.000638)	(0.000676)	(0.000713)
N	1,386	1,345	1,308	1,192

Notes: Standard errors are reported in parentheses, which are calculated using the Delta Method. Errors are clustered at the state level. *** denotes the significance at the 1% level, ** at the 5% level, and * at the 10% level.

The regression (1) consists of all unemployed persons. (2) excludes individuals eligible for UI benefits who are unemployed more than 26 weeks in the period between April 2004 and May 2008 and more than 99 weeks in the period between 2010 and 2011. (3) excludes individuals eligible for UI benefits who are unemployed more than 26 weeks in the period between April 2004 and May 2008 and more than 60 weeks in the period between 2010 and 2011. (4) excludes those who voluntarily leave jobs and who are on a temporary layoff expecting a recall from the past employer.

Table 8: Results from baseline nonlinear difference-in-difference models: men

Variables	Full sample	Unemp. dur. $\leq$ 99 weeks	Unemp. dur. $\leq$ 60 weeks	W/0 Indivs. expecting a recall and quitters
	(1)	(2)	(3)	(4)
Treat	0.0501	0.0376	0.0350	0.108*
	(0.0573)	(0.0563)	(0.0564)	(0.0624)
Post	−0.0239	−0.0258	−0.0304	−0.0741
	(0.0827)	(0.0750)	(0.0760)	(0.106)
Treat×Post	−0.0116	0.00677	−0.00575	−0.0148
	(0.0842)	(0.0821)	(0.0840)	(0.0922)
Time	0.0257	0.0253	0.0215	0.0364
	(0.0176)	(0.0170)	(0.0160)	(0.0235)
High school	0.0972	0.105	0.153*	0.106
	(0.102)	(0.104)	(0.0915)	(0.113)
Bachelor's degree	0.270	0.304	0.380*	0.299
	(0.233)	(0.238)	(0.204)	(0.244)
Master's degree	0.448*	0.455*	0.535***	0.469*
	(0.249)	(0.261)	(0.198)	(0.243)
PhD	0.0590	0.138	0.252	0.222
	(0.410)	(0.461)	(0.473)	(0.456)
Age	0.0414***	0.0453***	0.0445***	0.0533***
	(0.0151)	(0.0148)	(0.0138)	(0.0188)
Age squared	−0.000451***	−0.000499***	−0.000480***	−0.000583***
	(0.000168)	(0.000165)	(0.000154)	(0.000204)
Child dummy	0.0635	0.0465	0.0516	0.0718
	(0.0620)	(0.0616)	(0.0583)	(0.0676)
Partner	−0.0457	−0.0326	−0.0288	−0.0474
	(0.0560)	(0.0537)	(0.0520)	(0.0636)
Black	0.0343	0.0360	0.0265	0.00922
	(0.0330)	(0.0335)	(0.0355)	(0.0341)
Hispanic	−0.0875**	−0.0973**	−0.0882**	−0.0800*
	(0.0364)	(0.0406)	(0.0423)	(0.0473)
Other	0.00464	−0.000914	0.00736	0.00710
	(0.0852)	(0.0826)	(0.0837)	(0.0995)
Predict wage	−0.191	−0.230	−0.295	−0.275
	(0.248)	(0.248)	(0.231)	(0.284)
State residual	0.896	0.867	1.069	0.771
	(0.718)	(0.695)	(0.719)	(0.764)
Unemploy rate	−0.0111	−0.0113	−0.00662	−0.0130
	(0.0103)	(0.0101)	(0.0116)	(0.0128)
Avg. Weekly UIB	0.000688	0.000714	0.000558	0.000646
	(0.000471)	(0.000503)	(0.000487)	(0.000523)
Unemploy Duration	−0.000188	−0.000644	−0.00126	−0.000925
	(0.000636)	(0.000818)	(0.000892)	(0.000795)
N	1,066	1,009	968	856

Notes: Standard errors are reported in parentheses, which are calculated using the Delta Method. Errors are clustered at the state level. *** denotes the significance at the 1% level, ** at the 5% level, and * at the 10% level.

The regression (1) consists of all unemployed persons. (2) excludes individuals eligible for UI benefits who are unemployed more than 26 weeks in the period between April 2004 and May 2008 and more than 99 weeks in the period between 2010 and 2011. (3) excludes individuals eligible for UI benefits who are unemployed more than 26 weeks in the period between April 2004 and May 2008 and more than 60 weeks in the period between 2010 and 2011. (4) excludes those who voluntarily leave jobs and who are on a temporary layoff expecting a recall from the past employer.

5.1 The effect of UI extension on youths

In this subsection, I attempt to evaluate the effect of UI extensions on youths between ages of 16 and 24 years by gender. Unemployment benefits are expected to have very minimal impact on this group's labor market decision, since over 75% of unemployed youths do not receive unemployment benefits (see Mulligans 2012). First, most youths either enroll in schools and colleges or are planning to enroll. Second, many might not have sufficient work history to be eligible for UI benefits. Third, as they are in the early phases of their career, finding a job and gaining work experience could be a priority, instead of being dependent on the social safety net. As expected, the effects are not statistically different from zero.[9]

5.2 Anticipation Effects

It is worth considering if the results are clouded by anticipation effects. Historically, unemployment benefits have been extended during recessions. With the advent of the Great Recession in December 2007, the unemployed individuals eligible for UI coverage might have anticipated the extension of UI benefits in the near future. Thus, I attempt to take anticipation effects into account. I exclude 2008-data from the pre-treatment period (include only from March 2004 to 2007), and estimate the two models for women.[10] The first regression includes the full sample, while the second excludes those individuals expecting a recall from their previous employer, quitters, and those eligible individuals who are supposed to have exhausted UI benefits (those having remained unemployed more than 26 weeks in pre-treatment period and 60 weeks in the post-treatment period). Table 9 reports the results. I find consistently close results to the baseline specification. The average effect of the UI extensions is a decline of job search in the range of 10 to 13 percentage points, and statistically significant at the 5% level in the first model, and at the 1% level in the second model.

9 To save some space, I do not report the results and are available upon request.

10 I do not report estimation for men as I do not find statistically significant effects on the baseline specification for them.

Table 9: Anticipation effects: women

Variables	Full sample	W/0 Unemp. Dur. > 60, indivs. expecting a recall and quitters
	(1)	(2)
Treat	0.0951***	0.163***
	(0.0313)	(0.0364)
Post	0.118	0.142
	(0.0942)	(0.102)
Treat × Post	−0.104**	−0.138***
	(0.0481)	(0.0504)
Time	0.0106	0.00444
	(0.0189)	(0.0215)
N	1,312	1,052

Notes: Standard errors are reported in parentheses, which are calculated using the Delta Method. Errors are clustered at the state level. *** denotes the significance at the 1% level, ** at the 5% level, and * at the 10% level.
The regression (1) consists of all unemployed persons. (2) excludes individuals eligible for UI benefits who are unemployed more than 26 weeks in the period between April 2004 and 2007 and more than 60 weeks in the period between 2010 and 2011, and those who voluntarily leave jobs and who are on a temporary layoff expecting a recall from the past employer.

6 Robustness check

6.1 Placebo Test

I apply two placebo tests to further explore if the results are robust, not spurious, and to justify the validity of difference-in-differences (DD) methodology in this paper. One of the most important assumption in DD regression is a common trend (i.e., in the absence of treatment, the treatment group would evolve in the similar fashion as control group). The placebo tests complement the normalized-differences test used in Section 4 to gauge the common trend assumption. If the results in the baseline model were not the true policy effect, rather the reflection of the underlying structural differences between treatment and control units, I should expect statistically significant estimates in the model with only pre-treatment period data.

I carry out two placebo tests using the data from the pre-treatment period. First, I use all pre-treatment data from April 2004 to May 2008, during which no federal emergency unemployment compensation program was introduced. I divide the data into two periods (from April 2004 to 2005 as the pre-treatment period, and from 2006 to May 2008 as the post-treatment period). Second, I extend the placebo test by changing the treatment periods. I use data from April 2004 to 2005 as the pre-treatment period, and 2006 and 2007 as the post-treatment period, excluding 2008. I run the logit difference-in-differences regression with the same specification as in eq. [3]. Unemployed individuals eligible for UI benefits represent the treatment group, and ineligible individuals are the control group, the same definition as in the main baseline DD model. My variable of interest is the interaction between the variables eligibility and post-treatment period (denoted as $Treat_i \times Post_t$), as in the baseline model.

I run two regressions for each placebo test. First regression includes the full sample. Second regression excludes those who are supposed to exhaust UI benefits from the treatment group (those unemployed longer than 26 weeks), those expecting a recall from the past employer, and quitters. Results from the placebo tests are reported in Table 10 for women. (I do not report results for men as I do not find any effect in the main estimations.) The variable of interest ($Treat_i \times Post_t$) is not statistically different from zero in both specifications. To save space, I do not include estimates of controlled variables. To sum up, the placebo tests further validate the findings for the effects of UI extensions on the probability of job search in the baseline DD regression.

Table 10: Placebo test

Variables	**April 2004 to May 2008**		**April 2004 to 2007**	
	Full sample	**Sub-sample**	**Full sample**	**Sub-sample**
	(1)	**(2)**	**(3)**	**(4)**
Treat	0.126**	0.223***	0.121**	0.212***
	(0.0525)	(0.0618)	(0.0493)	(0.0582)
Post	−0.0104	0.0213	0.00828	0.0399
	(0.0597)	(0.0537)	(0.0529)	(0.0598)
Treat×Post	−0.0502	−0.0835	−0.0576	−0.0902
	(0.0632)	(0.0651)	(0.0655)	(0.0690)
Time	0.0348	0.0302	0.0166	0.00618
	(0.0243)	(0.0266)	(0.0337)	(0.0400)
N	752	594	678	530

Notes: Standard errors are reported in parentheses, which are calculated using the Delta Method. Errors are clustered at the state level. *** denotes the significance at the 1% level, ** at the 5% level, and * at the 10% level. Subsample (referring to models 2 and 4) means excluding unemployed individuals expecting a recall from their past employer, quitters, and those having more than 26 weeks of unemployment duration.

6.2 Effect on Job Search Intensity

In this subsection, I attempt to measure the effect of UI extensions in terms of job search intensity. First, I run the ordinary least squares (OLS) model for eq. [3] (linear DD regression) with the amount of job search as the dependent variable. I exclude those who are expecting a recall from their past work, quitters, and those who are supposed to use up their UI benefits (those having remained unemployed more than 26 weeks in the pre-treatment period and 60 weeks in the post-treatment period). Table 11 reports results. The average effect is around 30 minutes decline in job search intensity on a given diary day. The estimate is significant at the 5% level without clustering. With clustering, the estimate is significant at the 10% level.

Table 11: DD estimates of job search intensity

Variables	OLS	Tobit
	(1)	(2)
Treat	28.86	161.2
	(7.459)***	(35.28)***
	[7.756]***	[39.53]***
Post	23.38	84.58
	(20.10)	(73.57)
	[17.38]	[79.45]
Treat×Post	−30.63	−167.4
	(17.37)*	(69.25)**
	[13.83]**	[57.75]***
Time	2.778	17.57
	(3.048)	(15.97)
	[2.929]	[14.30]
N	1,116	1,116

Notes: Standard errors for Tobit model are calculated using the Delta Method. Errors reported in parentheses are clustered at the state level, while errors reported in square brackets are calculated without clustering. *** denotes the significance at the 1% level, ** at the 5% level, and * at the 10% level.

Additionally, I employ a Tobit difference-in-differences model, as job search intensity is highly skewed because most respondents in the survey have zero search (almost 80%). The OLS method does not work in extremely skewed data due to the violation of the normality assumption. Table 11 shows results from the Tobit difference-in-differences model. The model is left-censored (censored below zero). I find a negative and statistically significant (at the 5% level) effect.

6.3 Further Sensitivity Analysis

In this subsection I provide results from additional robustness exercises (see Table 12). First, I estimate a logit difference-in-differences regression using year fixed-effects instead of time trend. I normalize 2004 and the first year of the post-treatment period 2010 to zero to avoid the dummy variable trap. Second, I allow for quadratic time-effect. Third, I exclude unemployed women aged 55 and above from the data and estimate the model, to ensure that results are not driven by matters related to retirement. It is expected that those people who are close to retirement react to the labor market differently. As reported in Table 12, all results from these alternative specifications are close to those from the preferred baseline specification.

Table 12: Further robustness checks

Variables			**Women**
	Year fixed-effects	**Quadratic trend**	**W/O > 55 years**
Treat	0.173***	0.166***	0.157***
	(0.0352)	(0.0335)	(0.0422)
Post	0.158**	0.0403	0.102
	(0.0728)	(0.101)	(0.0993)
Treat×Post	−0.143***	−0.138***	−0.129**
	(0.0475)	(0.0479)	(0.0508)
N	1,116	1,116	984

Notes: Standard errors are reported in parentheses, which are calculated using the Delta Method. Errors are clustered at the state level. *** denotes the significance at the 1% level, ** at the 5% level, and * at the 10% level.

7 Conclusion

In this paper, I investigate the effects of unemployment insurance (UI) extensions on job search, using data from the ATUS. The government made successive decisions about the extensions of UI coverage from June 2008 to November 2009, raising the duration of benefits to as many as 99 weeks. The extensions that are termed as the most generous in history were continued into 2011. In the research design of this paper, I use unemployed persons eligible for UI benefits as the treatment group and the ineligible as the control group.

I apply a difference-in-differences estimator, as the extensions provide a pseudo-experiment. My focus is gender differences in the effect of UI extensions.

I find a negative and statistically significant effect on women, but for men the estimate is not statistically different from zero. The average treatment effect for women is around a 10 percentage point decline in a job search probability.

Some caveats apply to this study. Presumably, there have been some classification errors when it comes to classifying unemployed workers into eligible (for UI benefits) and ineligible groups. On top of that, I use eligible workers as the treatment group. In practice, a moderate portion of the eligible workers do not claim benefits (which makes it inappropriate to put them into treatment group). Both of these limitations will likely cause the results to be underestimated (i.e., downward bias). If the ATUS begins collecting information about UI benefits received by individuals, more precise evaluation of the UI benefits system will be possible.

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Appendix A

Table 13: Maximum weeks of benefits

	Weeks	Eligible states
UI	20–26	All
EUC Tier 1	20	All
EUC Tier 2	14	All
EUC Tier 3	13	States with unemployment rate greater than 6%
EUC Tier 4	6	States with unemployment rate greater than 8.5%
EB Option 1	13	States with unemployment rate greater than 6.5%
EB Option 2	20	States with unemployment rate greater than 8.0%

Notes: UI benefits are available for 20 weeks in Missouri and South Carolina. In Arkansas, the benefits are available for 25 weeks.
Source: Council of Economic Advisors, December 2011. Retrieved February 2013, from http://www.whitehouse.gov/sites/default/files/ui_report_final.pdf

Appendix B

I derive job search activities from the ATUS Activity Coding Lexicon. All activities are coded in a three-tiered classification system. The first-tier category includes major activities, while the second tier includes subcategory of the first tier, and the third tier includes subcategory of the second tier. Table 14 presents detailed codes and search activities in the ATUS 2011 that are used in this paper.

Table 14: Description of the ATUS Lexicon codes and activities

Codes	Job Search Activities
050401	Contacting employer Sending resumes to employers Placing or answering ads Researching details about a job Asking about job openings Researching an employer Submitting applications
050403	Interviewing by phone or in person preparing for interview Scheduling or canceling interview (for self) Preparing for interview
050404	Waiting associated with job search or interview
050405	Security procedures related to job search or interviewing
050499	Job search and interviewing, not elsewhere specified
180504	Travel related to job search and interviewing

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