

Are Reemployment Services Effective?  
Experimental Evidence from the Great Recession

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Abstract

We report the results of a random assignment study of a reemployment program implemented in the United States during the Great Recession. The program expedited participant exit from Unemployment Insurance (UI), produced significant UI savings, and improved participant employment rates and earnings. These effects are associated with: (1) increased participant UI exit prior to services receipt, indicating an effect due to participant efforts to avoid program requirements; and (2) greater exit subsequent to services, implying that the services themselves helped participants conduct an effective job search. Our findings provide compelling evidence that reemployment programs can be effective during recessions.

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## **Introduction**

In the past 25 years, policymakers in the United States and Europe have put much emphasis on programs that require unemployed workers to participate in reemployment services as a condition for collecting unemployment benefits. Funding for these programs grew dramatically during the Great Recession in the United States, when the federal government made substantial investments to enhance the capacity of public employment offices to offer services to jobseekers. Despite this growing interest, there is no recent evidence on the effectiveness of U.S. reemployment programs, with the most recent studies examining programs implemented more than a decade before the start of the Great Recession. In contrast, there are numerous recent studies of reemployment programs in European countries, including Denmark, Germany, Netherlands, Sweden, and the United Kingdom.

Recent studies show that reemployment programs can be effective in increasing the opportunity cost of unemployment, pushing those who are readily employable or noncompliant with work search requirements to discontinue receipt of unemployment benefits. The U.S. studies find that program effects occur exclusively in the period prior to services receipt, leading to the conclusion that reemployment programs are effective mechanisms to encourage program exit but that services themselves provide no direct benefits to jobseekers. This conclusion, however, is premature given that these studies examine programs which provided job search services to very few participants. Although European programs provided extensive services to a large share of participants, recent studies focus on identifying effects occurring because of response to program requirements prior to services receipt but not on identifying the effectiveness of the services themselves. Thus, existing work does not provide convincing evidence that the services offered by reemployment programs help jobseekers to improve their

job search.

Moreover, recent studies examine programs in the context of a relatively strong labour market, and the existing literature provides little guidance on the effectiveness of such programs during recessions. Economic conditions may play a significant role in the potential benefits of reemployment programs, as the structure of job matching and the composition of the unemployed varies greatly across the business cycle, with layoffs affecting a broader population of workers during a recession. Although those unemployed during a recession are likely to have higher skill levels, they may also have less job search experience, and it is possible that services to help make the best use of their skills would be particularly valuable. The lack of evidence on the effectiveness of reemployment programs during downturns is a conspicuous gap, considering that interest in these programs peaks when the labour market is weak.

This paper examines a reemployment program implemented in the state of Nevada that targeted displaced workers who started collecting Unemployment Insurance (UI) benefits in the second half of 2009. During this period, the state unemployment rate averaged 11.7%, the highest in the state in 25 years and among the highest in the U.S. In addition to the fact that the program was implemented during the recession, there are two key features that make this case study compelling. First, Nevada used random assignment to determine which UI recipients would be required to participate in the program (treatment group) and which would not be required to participate (control group). This experimental design yields reliable counterfactual outcomes against which to measure the outcomes of the treated cases. Second, the program required participants to receive a level of services that is more extensive than that of previous U.S programs that have been studied to date. Participants were required to attend a one-on-one meeting with state workforce staff in the first three weeks of their claim in which they: (1)

underwent a UI eligibility review to confirm they were indeed qualified for benefits and were actively searching for a job; and, if determined eligible, (2) were provided comprehensive job-counselling services designed to enhance their job search based on individual needs.

The analyses presented here rely on Nevada administrative UI claims data and wage records for all displaced workers who started collecting UI from July 2009 through December 2009 and were subject to random assignment for participation in the reemployment program. Using these data, we estimate treatment-control differences in outcomes to examine whether the program was successful in reducing UI spells and payments, and in improving participant reemployment rates and earnings. The fact that program services were provided at the beginning of participants' UI claims enables us to disentangle the underlying mechanisms that led to program effects – namely, the extent to which effects were produced because the services requirement pushed some participants out of UI (“moral hazard effect”), or because the services were themselves effective in helping participants to get reemployed (“reemployment services effect”). To do so, we identify the time pattern of treatment-control differences in UI exit rates, assuming that differences in the early stages of the UI claim, when treatment group members were notified of program requirements and were required to receive services, can be attributable to program requirements, whereas subsequent differences occurring after the treatment group received services can be attributable to the effectiveness of services.

## **1. Background**

### ***1.1. U.S. Reemployment Policy***

Since the early 1990s, U.S. policymakers have focused much attention on promoting the exposure of UI recipients to reemployment services. In 1993, Congress enacted the Worker

Profiling and Reemployment Services (WPRS) program, which required state UI agencies to establish a system to identify which new UI recipients were most likely to exhaust benefits and refer them to reemployment services (Wandner, 2010). The recommendation to states was to provide WPRS-referred recipients with the full range of services offered at public employment offices, including: (1) group orientations to learn about available services; (2) registration in automated job banks to connect to available jobs; (3) workshops to improve job search skills such as preparing application materials and improving interview performance; and (4) individual job counselling, including skills assessment, development of a work search plan, and job referrals (Dickinson *et al.*, 1999).

According to the U.S. Department of Labor, from the time that WPRS became fully operational in 1997 through 2007, 11.5 million UI recipients were referred to services, of whom 6.3 million attended the orientation, 3.9 million participated in workshops, and 1.6 million received counselling.<sup>1</sup> Services were funded under the Wagner-Peyser Act, which during that period provided support to states averaging \$766 million annually.<sup>2</sup> At the end of 2007, the U.S. economy entered its worst recession since the Great Depression, with the unemployment rate increasing from 5% in December 2007 to a peak of about 10% by the end of 2009. To facilitate the economic recovery, the American Recovery and Reinvestment Act of 2009 (ARRA) authorized \$400 million – in addition to \$1.4 billion provided under Wagner-Peyser in 2009 and 2010 – to enhance the capacity of states to provide reemployment services to jobseekers. Using these funds, states increased the number of UI recipients referred to services by WPRS from 1.2 million in 2008 to 1.9 million in 2009 and to 2.1 million in 2010.

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<sup>1</sup> Source: Worker Profiling and Reemployment Service Activity (<http://workforcsecurity.doleta.gov/unemploy/profile.asp>).

<sup>2</sup> Wandner (2010), Table 6.1, page 197.

## **1.2. Effectiveness of U.S. Reemployment Programs**

The most recent experimental studies of U.S. reemployment programs examine programs that were implemented in the mid-1990s, more than a decade before the start of the recession and in a period when the average unemployment rate was generally between 5% and 6%. Decker *et al.* (2000) present experimental evidence on the effectiveness of the Florida and Washington, DC Job Search Assistance (JSA) demonstration programs, which required UI recipients to attend a group orientation to obtain information on available services and be referred to employment workshops and job counselling. While 71% of participants attended the orientation, only 5% and 16% participated in workshops and counselling, respectively. Low levels of service receipt are attributable to a number of factors including that participants were referred to services later in their claims and that services participation was not compulsory. The study finds that JSA reduced UI duration by up to 1.1 weeks and UI benefits collected by up to \$182, but it produces mixed evidence on whether the program had positive effects on earnings. Treatment-control group comparisons of UI exit rates show that the program's entire effect occurred around the time participants were notified of JSA requirements, prior to participation in services, leading the authors to conclude that program effects were attributable to the threat of sanctions or the expected cost of meeting the program's service requirement, rather than any benefits of services.

Black *et al.* (2003) present experimental estimates of the effects of the Kentucky WPRS program in the period October 1994 through June 1996. The study finds that the program reduced UI weeks collected by 2.2 weeks and benefit amounts by \$143. The program also had small positive impacts on earnings in Quarters 1-2 after program entry, but no impacts in Quarters 3-6. Analysis of UI exit rates show that the entire program effects occurred in the period when the notification letter was sent. Thus, the study argues that the program's impacts

were attributable to threat effects and not to the effectiveness of services. This conclusion is consistent with the fact that most participants received no services. A government-funded study of WPRS programs (Dickinson *et al.*, 1999) reports that from July 1995 through December 1996, a study period that largely overlaps the Black *et al.* (2003) study period, only 16% and 2% of Kentucky WPRS-referred recipients participated in workshops and counselling, respectively.

Klepinger *et al.* (2002) use data from an experimental program implemented in Maryland to examine the effects of alternative work search requirements on UI recipient outcomes. The study examines three treatments: (1) remind participants that their records of employer contacts might be reviewed for verification, (2) increase the required number of employer contacts to four instead of two each week, and (3) refer participants to an employment workshop. Based on comparisons to a randomly assigned control group, the study finds that these treatments reduced UI spells by 0.6 to 0.9 weeks and benefit amounts by \$75 to \$116, but found no significant effects on employment and earnings. The entire effect on UI was realized in the first two weeks of the UI claim – that is, when participants were notified of the additional requirements and prior to receipt of services – and the authors conclude that effects were entirely due to the added burden imposed by the program.

### ***1.3. Effectiveness of European Reemployment Programs***

Over the past 25 years, many European countries have put into effect programs designed to ensure that unemployed workers who collect UI are actively searching for employment and have access to services offered by public employment services offices. Referred to as activation programs, they typically feature a combination of job search services to help the unemployed connect to suitable jobs, monitoring to confirm that they are conducting an active job search, and

benefit sanctions when program participation and work search requirements are not met. For an overview of programs implemented in European and other developed countries, see OECD (2007, 2013).

The effects of European programs have been examined extensively. An experimental study shows that requiring unemployed workers in Netherlands to receive job-counselling services at the start of their UI claim and to attend work search monitoring meetings every four weeks thereafter led to an 11% increase in job finding rates (Gorter and Kalb, 1996). Also based on experimental evidence, Graversen and van Ours (2008) find that requiring unemployed workers in Denmark to participate in a two-week workshop and attend subsequent monitoring meetings, led to a significant reduction in average unemployment duration of 2.5 weeks. An experimental study of Swedish activation programs (Hägglund, 2011) finds that combined mandatory monitoring and job search assistance increased reemployment rates up to 51%.

Additional experimental studies in Denmark (Pedersen *et al.*, 2012), France (Behaghel *et al.*, 2012), Germany (Krug and Stephan, 2013), and the United Kingdom (Dolton and O'Neill, 2002) and non-experimental studies in Belgium (Cockx and Dejemeppe, 2012), Denmark (Geerdsen, 2006), the Netherlands (Abbring *et al.*, 2005), the United Kingdom (Blundell *et al.*, 2004), and Portugal (Centeno *et al.*, 2009) confirm that programs that combine continued work search monitoring and job search services are successful in increasing unemployment exits and promoting the reemployment of unemployed workers.<sup>3</sup> The consensus from this literature is that imposing additional requirements on the unemployed in the form of services mandates and continued work search monitoring raises the opportunity cost of unemployment and pushes participants to become reemployed (or, in some cases, to drop out of the labour market) earlier than they would in the absence of those requirements.

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<sup>3</sup> These studies do not provide estimates of program effects on earnings.

#### ***1.4 Gaps in the Literature***

Reemployment programs may produce a moral hazard effect because program requirements push participants to exit unemployment before they receive any actual services. Importantly, these programs may produce a services effect because actual services are successful in enhancing participants' job search skills. For instance, directly referring jobseekers to employers or registering them in automated job banks where they can easily access information about available jobs may help reduce search costs and increase job applications (Jacobson and Petta, 2000). Individual job counselling may help participants to recognize their own skills and focus their search on jobs that are suitable to those skills and are likely to offer them acceptable compensation (Jacobson *et al.*, 2004; Wandner, 2008). Employment workshops may improve participants' basic job search skills – including application and interview skills, making them more attractive to potential employers (Balducci *et al.*, 1997; O'Leary, 2006). Some combination of these services may help reduce search costs and enhance search intensity, particularly for those with limited job search experience, reducing the amount of time they remain unemployed.

Existing studies provide little basis for separating the effects of the services themselves from moral hazard effects. A common theme in the U.S. studies is that they examine programs in which very few participants received any actual services. The vast majority of program participants in the Decker *et al.* (2000) and Black *et al.* (2003) studies did not participate in employment workshops or individual job counselling, while participants in the Klepinger *et al.* (2002) study were not required to receive any services whatsoever. Given the low level of services receipt, these studies cannot be used to infer whether such services are of value. Most European programs that have been the focus of recent research required participants to receive

services on a regular basis throughout their claims, so it is difficult to distinguish effects resulting from the burden associated with this requirement and the effects of actual services. In fact, many of those studies focus explicitly on moral hazard effects, examining responses to program requirements prior to initial services receipt, excluding any impact of the services themselves.

Another important gap in the literature is that existing studies examine program efficacy during periods of moderate unemployment; thus, we can only speculate whether their results are generalizable to the Great Recession or other economic downturns. The most recent U.S. studies examine programs implemented from 1994 through 1997, at least a decade before the start of the Great Recession, when the U.S. unemployment rate was in the range of 5% to 6%. Similarly, recent European studies examine programs implemented during relatively strong labour markets.

There are reasons to expect that reemployment services may have been more valuable during the Great Recession. First, the job matching process was particularly slow during the recession, as evidenced by a disproportionately small decline in unemployment associated with each addition job opening and by very high long-term unemployment (Elsby *et al.*, 2010; Barnichon *et al.*, 2012; Hobbijn and Sahin, 2013). This is partly attributable to the fact that employers reduced the intensity with which they recruited workers, as measured by advertising of job vacancies, speed of processing applications, and attractiveness of compensation packages (Davis *et al.*, 2012; Ravenna and Walsch, 2012; Sedlacek, 2014). Jobseekers may therefore have benefited by more intensive job search activities.

Second, the profile of the average jobseeker changed significantly in the Great Recession, when the proportions of male, older, high-education, and white-collar workers reached their highest levels in 20 years (Michaelides and Mueser, 2012). The increase in unemployment and subsequent shortfall of vacancies was more pronounced for certain sectors, including

construction, manufacturing, trade, and leisure and hospitality (Barnichon *et al.*, 2012; Davis *et al.*, 2012). These imply that the recession may have disproportionately affected workers who would not have employability issues under normal circumstances and lacked the skills to conduct an effective job search, inducing an increase in the average unemployment spell.

Third, there is a growing literature arguing that labour market interventions may improve individual decision-making, reducing procrastination and other behavioural biases that cause recipients of public assistance to fail to take necessary actions to improve their circumstances (Thaler and Sunstein, 2009; Babcock *et al.*, 2012; Cockx *et al.*, 2014). During recessions, limited job options and the prospect of long-term unemployment may exacerbate these problems, and thus make displaced workers more responsive to services.

Existing work provides no evidence on the size of the moral hazard effects during a recession. Although moral hazard is of little importance for workers with very poor job prospects, the availability of UI benefits for extended periods increases moral hazard for those whose employment likelihood is sensitive to their search effort. It is possible that the growing prospect of long-term unemployment and attendant difficulties brought on by the recession may have served as a spur to search activity, thus reducing the importance of moral hazard effects. Conversely, it is widely believed that subsidies interact with behavioural problems, lulling workers into inaction; if so, the shock effect of program requirements could be particularly valuable during the recession in pushing workers to find employment.

Our study addresses two important gaps in the literature. First, our analysis of an experimental design program enables us to identify the overall effects of requiring jobseekers to receive comprehensive services, as well as to distinguish the moral hazard effects and direct benefits of actual services. Second, given that our treatment occurred during the most important

recession since the 1930s, we are able to determine whether reemployment programs can be successful during such a period, when the need would appear to be greatest.

## **2. The Nevada Program**

### **2.1. Program Description**

At the beginning of 2009, Nevada replaced WPRS with a new program that required UI recipients to undergo an in-person UI eligibility review and receive staff-assisted reemployment services at the start of their UI claims. This program was created in response to the federal Reemployment and Eligibility Assessment (REA) initiative, which provided states with grants to implement in-person eligibility reviews of UI recipients (Poe-Yamagata *et al.*, 2012). Nevada used REA funding together with Wagner-Peyser and ARRA funding to assure that each participant would attend an in-person meeting with workforce staff at the start of the UI claim in which the participant both underwent the REA-mandated review *and* received staff-assisted reemployment services. In contrast to WPRS, which targeted individuals at risk for exhausting UI benefits, Nevada used random assignment to determine which recipients would be required to participate in the new program.

The Nevada selection process was implemented as follows. Once an unemployed worker filed a UI claim and was deemed eligible for benefits based on the information provided on an application form, Nevada UI agency staff used a set of criteria to determine if the worker was eligible for the program. Recipients on temporary layoff, those attached to a union hiring hall, and those who were active in Workforce Investment Act (WIA) training were excluded from this program. Each week, UI staff placed the remaining pool of new UI recipients in an interface that allowed random assignment to program participation or to the control group.

Once program participants were selected, they were immediately sent letters notifying them

that they had been randomly selected to attend a UI eligibility assessment meeting at a specified public employment office. The letters informed participants that the purpose of the meeting was to assist them in planning their job search and to reduce the amount of time they would remain unemployed. The letter also informed participants of the exact date/time of the meeting, typically scheduled in weeks 2-3 of the UI claim, and explicitly stated that the meeting was mandatory and failure to attend would cause loss of benefits. Participants who failed to attend or reschedule the meeting without providing a reasonable justification (e.g., had found a job, had already participated in services voluntarily, or had enrolled in training services) by week 3 of the UI claim were disqualified from collecting benefits, a requirement that was strictly enforced by the Nevada UI agency.

The meeting between each participant and program staff comprised two components: the UI eligibility review and provision of staff-assisted job-counselling services. In the UI eligibility review portion of the meeting, program staff reviewed agency records of the participant's employment history to confirm that the participant was indeed eligible for benefits.<sup>4</sup> The program staff also questioned the participant to determine if he or she was conducting an active job search while collecting benefits, in accordance with state work search requirements.<sup>5</sup> Participants deemed ineligible for benefits or non-compliant with work search requirements were immediately disqualified from receiving UI payments.

Participants who passed the eligibility review were then offered comprehensive job-counselling services during the same meeting. First, program staff assessed participant occupational skills and work experience and, based on the results, helped the participant to

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<sup>4</sup> In Nevada, unemployed workers qualified for UI if they: (1) had earnings for at least two quarters in the first four of the five quarters prior to the start of the UI claim (the "base period"), (2) earned at least \$400 in one quarter and at least \$600 in the entire base period, and (3) lost their jobs through no fault of their own.

<sup>5</sup> Nevada required UI recipients to be physically able to work, be actively searching for a job, and not reject suitable jobs.

produce a professional resume. Program staff then worked with participants to develop a work search plan designed to focus their search efforts on jobs that matched their skills and experience. As part of this process, participants received information about labour market conditions and were referred to employers with job openings. Finally, participants were encouraged to obtain other services that were available to all jobseekers – including those in the control group – such as registration in automated job banks and participation in employment workshops. Importantly, participants were informed that this meeting was the only program requirement and that they were not required to participate in additional services or follow-up meetings.

To our knowledge, this Nevada program is the first implemented as a random assignment experiment in the U.S. over the past 25 years that required participants to undergo an eligibility review *and* receive comprehensive job-counselling services. These requirements are a departure from U.S. programs examined by previous work which did not require participants to undergo an eligibility review or receive job counselling. But the Nevada requirements are similar to those of many of the European programs cited above, with the distinction that these programs required participants to receive services on a continuing basis. Importantly, more than 75% of Nevada program participants during the study period underwent the UI eligibility review *and* received job-counselling services, a proportion that greatly exceeds service participation rates in U.S. programs studied to date. Finally, according to the Nevada Department of Employment, Training, and Rehabilitation, the state spent \$2,191,905 in 2009 to provide services to 10,905 participants, implying an estimated cost per participant of about \$201. This amount covered all costs associated with program implementation, including the costs of identifying eligible recipients, the referral process, and staff salaries and related centre expenses.

## **2.2. *Potential Effects***

We can identify two ways that the program may have affected participant job search behaviour and outcomes. The first is due to a reduction in moral hazard, reflecting both the exit of ineligible recipients and the expectation of increased costs associated with mandatory service receipt by eligible recipients. The notice of the required meeting may have persuaded some participants who misrepresented their work history in their UI application to stop claiming their weekly UI payments rather than wait for the in-person eligibility review to disqualify them. Moreover, the requirement that participants attend an in-person meeting, in addition to the expectation of possible further requirements, would have increased the expected opportunity cost of collecting UI, causing those who were readily employable or already had jobs under the table to exit UI instead of showing up to receive services. Finally, the UI eligibility review itself would have led to the disqualification of those who attended the meeting but were deemed ineligible, either based on their work history or because they were not compliant with work search requirements. If such moral hazard effects were indeed operating, they most likely would have occurred within weeks 1-3 of the UI claim, the timeframe during which participants were required to attend the meeting.

The second way in which the program may have affected outcomes was through enhanced job search resulting from the comprehensive job-counselling services for participants who passed the eligibility review. Since the program did not require participants to receive additional services after the initial meeting and there was no mechanism to produce further moral hazard effects, we would expect the services effect to overlap only minimally with the moral hazard effect and to occur after week 3, subsequent to the mandatory meeting. Thus, if there was indeed a direct effect of services, we should expect that the program would lead to an increase in UI exit

rates after the initial stages of participants' claims.

### **2.3. *Data Description***

This study uses Nevada UI claims data, which provide information on all unemployed workers who started collecting UI benefits from July 2009 through December 2009 and were eligible for the program. UI claims data provide information on recipient characteristics at program entry, recipient treatment/control status, and the maximum benefit amount and weeks of eligibility the recipient was entitled to collect on the claim. Since Nevada's unemployment rate during the study period (11.7%) exceeded the thresholds for activating the Emergency Unemployment Compensation (EUC) and Extended Benefits (EB) programs, recipients who exhausted regular UI benefits (12-26 weeks) were eligible for up to an additional 53 weeks of EUC and, following exhaustion of EUC, 20 weeks of EB. The UI data used in this study provide information on benefits collected under regular UI and EUC, but not under EB.

Labour market outcomes were obtained from state UI wage records, which provide calendar-quarter earnings for workers within Nevada in each of the four quarters prior to and in each of the six quarters following the start of a UI claim. These data do not report the exact date the recipient started working, number of weeks worked, or hours worked. Thus, wage records can be used to measure whether the recipient had earnings in a given quarter and the total quarterly earnings amounts but not to determine length of employment and hourly wages. Also, the data do not include any earnings from employment in other states.

### **2.4. *Characteristics of Program-Eligible UI Recipients***

During the study period, 31,793 unemployed workers started collecting UI and were deemed

eligible for the program. Of these, 4,673 (15%) were randomly assigned to the treatment group and the remaining 27,120 (85%) to the control group. Table 1 presents means and standard deviations for measures of individual characteristics, as well as UI eligibility, and prior earnings for treatment and control group members. Also, to confirm that random assignment was successfully implemented, the right column of Table 1 presents treatment-control differences in means and t-tests to assess if they are statistically significant. These tests reveal no statistically significant treatment-control differences, confirming that there was no systematic relationship between the probability of being assigned to the treatment group and observed participant characteristics. Thus, any treatment-control differences in post-random assignment outcomes can be confidently attributed to the program.

Unfortunately, the data do not provide individual participant information on service participation and disqualifications resulting from the failure to attend the meeting or satisfy the eligibility review, because the program had not established a systematic way of collecting this information as part of the UI data system. The only source of information on participation and disqualifications are quarterly reports submitted by Nevada to the U.S. Department of Labor. According to those reports, more than 75% of referred participants during the study period underwent the UI eligibility review and received reemployment services. It is also reported that 1.1% of participants were disqualified because they did not attend or reschedule the meeting and 0.7% were disqualified following the eligibility review.

## **2.5. *Recipient Outcomes***

Nevada UI data were used to construct benefit receipt measures, including measures of whether recipients exhausted regular UI benefits, whether they collected EUC after exhausting

regular UI, benefit weeks collected (regular UI, EUC, and total), and benefit amounts collected (regular UI, EUC, and total). As can be seen in Table 2, treatment group members have lower regular UI exhaustion and EUC collection probabilities than control group members. As a result, treatment group members appear to have collected, on average, fewer weeks and lower amounts of regular UI, EUC, and total benefits than have control group members.

Wage records were used to construct two measures of quarterly earnings outcomes in each of the six calendar quarters following the start of the UI claim. First, positive earnings in a calendar quarter provide a measure of whether the sample member was employed at any point during a quarter. Second, we use the data to measure the quarterly earnings amounts earned by each sample member in the six quarters after UI entry. As shown in Table 3, treatment group members appear to have higher employment probabilities and higher earnings amounts than the control group members over the entire six-quarter follow-up period.

### **3. Program Effects**

Comparison of the means for treatment and control groups presented in Tables 2 and 3 provide unbiased estimates of program effects. To improve statistical power, we used linear regression models to estimate program effects, controlling for characteristics and prior earnings, as follows:

$$Y_i = a \cdot T_i + X_i \cdot b + c + u_i$$

The dependent variable ( $Y_i$ ) is the outcome for recipient  $i$ . The treatment indicator ( $T_i$ ), equals 1 if the recipient was in the treatment group and 0 otherwise. Control variables, as indicated in Table 1, include recipient characteristics at program entry, prior earnings, and fixed effects for week of UI entry, represented by the vector  $X_i$ . Also included is a vector of fitted

coefficients for control variables ( $b$ ) and a constant term ( $c$ ), and a zero-mean disturbance term ( $u_i$ ). The parameter of interest is  $a$ , which is the program's average treatment effect.

### ***3.1. Effects on Unemployment Insurance Receipt***

Table 4 presents the estimated treatment effect for each UI receipt outcome, along with the effect as a percentage of the control group mean. Results show that the program led to a 10.4 percentage-point reduction in the probability that participants exhausted regular UI, which means that, compared to the control group mean, the program led to 15% reduction in regular UI exhaustion. The program's effect on regular UI exhaustion translates into a significant reduction in EUC receipt, with the program reducing the likelihood that participants would collect EUC benefits by 9.7 percentage points (16%).

The program also had significant effects on UI duration and benefit received. The program reduced regular UI duration by 1.9 weeks and EUC duration by 1.5 weeks. The program's effect on total UI duration (regular UI plus EUC) was -3.4 weeks, implying that participants collected 11% fewer benefit weeks than non-participants. As a result, the program led to a \$520 reduction in regular UI benefits and a \$357 reduction in EUC benefits, for an \$877 reduction in total benefits collected. Thus, the average UI savings per participant were more than four times the estimated average program cost of \$201.

Which underlying program mechanisms led to these effects? As discussed, two kinds of program effects can be identified: (1) the moral hazard effect, caused by the program requirements pushing participants out of UI; and (2) the reemployment services effect, caused by the effectiveness of services themselves. The moral hazard effect is hypothesized to have occurred in weeks 1-3 of the UI claim, with very limited overlap with the reemployment services

effect, which is hypothesized to have occurred in subsequent weeks. To identify the importance of these effects, we estimated the hazard of exiting UI in each week using the same specification as the regression model used above.

Our approach is consistent with that of Decker *et al.* (2000), Black *et al.* (2003), and Klepinger *et al.* (2002), who inferred that because program effects occurred prior to service receipt, moral hazard effects were dominant. Although random assignment assures that initial hazard estimates or aggregate differences in benefit receipt between treated and control cases are due to program participation, hazard estimates for subsequent weeks may reflect both program effects and selection. This is because the population at risk differs for treated and control groups after the initial period. In particular, in our study, a larger share of treated cases discontinued UI benefit receipt early on, so we expect such selection to reduce subsequent exit probabilities, since those cases most likely to leave would have been driven out in the initial period as a result of moral hazard effects.<sup>6</sup> As a result, the difference in the hazard of leaving UI between the treated and control groups is likely to underestimate the true program effect in later periods.

Figure 1 presents the estimated parameters for weeks 1 through 25, as well as each parameter's 95% confidence interval. The treatment-control differences in the initial weeks are positive and statistically significant and, although there is a generally declining trend, with the exception of weeks 8 and 10, the differences are statistically significant through week 13. Overall, the probability of exiting UI by week 13 was 34.3% for the treatment and 24.3% for the control group, which means that program participation increased UI exit in the first 13 weeks by roughly 41%.

A portion of this effect was realized in weeks 1-3, when the cumulative probability of UI exit

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<sup>6</sup> In theory, it is possible that selection could work in the opposite direction if the program caused the exit of individuals who would be *less* likely to exit at a later point. Such an effect would appear highly implausible.

was 10.2% for the treatment and 6.5% for the control group, a 3.7 percentage-point difference, suggesting that program effects are partly attributable to the moral hazard effect.<sup>7</sup> But the largest portion of the impact occurred *after* the meeting, in weeks 4-13. If we examine the probability of exit by week 13 of those who had not exited by week 3, we find that the UI exit probability in weeks 4-13 was 26.8% for the treatment and 19.1% for the control group. Thus, program participation is associated with a 7.7 percentage-point (41%) increase in UI exit in weeks 4-13, suggesting that services effects were present and were, in fact, larger than moral hazard effects.

One alternative interpretation is that the moral hazard effect persisted after the initial stages of the UI claim because participants felt they might face monitoring under the program. This interpretation, however, is not consistent with previous U.S. studies, which using a similar approach found strong moral hazard effects but no evidence that those effects persisted after the period when participants were notified of and received services. There are no obvious reasons why moral hazard effects would persist for the Nevada program but not for programs examined by previous studies, or why moral hazard effects would be in fact stronger after the period when we would expect them to occur.

### ***3.2. Effects on Employment and Earnings***

Table 5 shows that in Quarters 1 and 2, treatment group recipients were 7.0 and 8.2 percentage points more likely to be employed, respectively. The program's effect on employment gradually declined over time, but remained significantly positive through Quarter 6. Program effects on employment, as with the results on benefit receipt, are to some extent

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<sup>7</sup> The 3.7 percentage-point effect on UI exit in weeks 1-3 is consistent with program quarterly reports over the study period, which report that 1.1% of participants were disqualified because they did not attend or reschedule the meeting and 0.7% were disqualified following the eligibility review. The remaining difference is presumably attributable to participants who do not attend the meeting or who discontinue their UI claim once they are informed of program requirements.

presumably caused by the program's effectiveness in reducing moral hazard. But the time pattern of effects on UI receipt suggests that effects on employment may also be attributable to the fact that services were themselves effective in helping participants find employment after the initial three-week period.

One concern with programs that facilitate UI exit, especially during a high-unemployment period, is that participants may be pushed into jobs that are a poor match for their skills. In this case, positive impacts on employment may be more than compensated for by lower earnings of those with jobs. However, we do not see evidence supporting such concerns, since the program had significant positive effects on earnings, and the program's effect on earnings remained positive and significant through Quarter 6. Over the entire six-quarter period following UI entry, treatment group recipients had \$2,607 (18%) higher earnings than control group recipients.

A related possibility is that the earnings effects are driven solely by the program's positive effects on employment and that, in fact, treatment group recipients were placed in jobs with lower earnings. To illustrate this point, the program's effect on earnings in a given quarter can be expressed as follows:

$$Y_T - Y_C = E_T \cdot W_T - E_C \cdot W_C = (E_T - E_C) \cdot W_T + E_C \cdot (W_T - W_C)$$

where  $Y_T$  and  $Y_C$  are the average earnings for treatment and control group members, respectively;  $E_T$  and  $E_C$  are the employment probabilities; and  $W_T$  and  $W_C$  are the average earnings conditional on employment.<sup>8</sup> The program's effect on earnings can be decomposed into: (1) the *employment component*,  $(E_T - E_C) \cdot W_T$ , due to the greater levels of employment among the treated and (2) the *conditional earnings component*,  $E_C \cdot (W_T - W_C)$ , the treatment-control

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<sup>8</sup> As noted, wage records can be used to measure earnings but not to measure length of employment or hourly wages in a given quarter. Thus, any treatment-control differences in quarterly earnings, conditional on employment, may be attributed both to differences in hourly wages and number of hours worked during the quarter.

difference in earnings, conditional on employment. If the program's effect was primarily due to pressure to accept employment, we might expect earnings for employed treatment group members to be lower than for the employed control group members, with the result that the earnings component would be negative.

Figure 2 presents the contributions of the employment component (dark grey area) and of the conditional earnings component (light grey area) to the program's effects on earnings. As can be seen, both contributed positively to the program's effect on earnings, reflecting the fact that average earnings for those employed are *higher* for the treated group. In the first two quarters, the dominant portion of the effect on earnings is attributed to the employment component (86% and 83%, respectively) while the conditional earnings component accounts for the balance. In Quarters 3-6, 70-85% of the effect is attributable to the employment component and the remaining 15-30% to the conditional earnings component.<sup>9</sup> Thus, although program effects on earnings are largely attributable to employment, a portion of the effects, particularly in Quarters 3-6 results from higher earnings conditional on employment.

These results suggest that the Nevada program was indeed effective in facilitating UI exit and reemployment of recipients, leading to higher participant earnings over the entire six-quarter follow-up period. The fact that participants had higher earnings relative to non-participants conditional on employment would seem to suggest that the program is also associated with getting them placed in jobs that offered higher hourly wages and/or more hours of employment than the jobs they would have obtained in the absence of the program. Alternatively, the program may have facilitated employment for those who would receive higher wages once employed.

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<sup>9</sup> An alternative way of decomposing the earnings effects is to use the conditional earnings of the control group in the employment component, and the probability of employment for the treatment group in the earnings component:  $Y_T - Y_C = (E_T - E_C) \cdot W_C + E_T \cdot (W_T - W_C)$ . This decomposition yields similar results, with the employment component accounting for 83, 79, 65, 69, 83, and 67 percent of the effect in Quarters 1-6, respectively.

In either case, the pattern of results suggests that the program's success in providing participants with services was responsible for an improvement in job matches. Previous studies produced mixed evidence that programs led to positive effects on total earnings and no evidence that earnings contingent on employment were increased. Black *et al.* (2003) found that the program's moral hazard effect led to small effects on earnings in the first two quarters but not in subsequent quarters, while Decker *et al.* (2002) found positive effects in Washington, DC but no effects in Florida. Klepinger *et al.* (2002) did not find any earnings effects, whereas most of the aforementioned studies of European programs did not estimate effects on earnings. The fact that we find positive effects for up to six quarters after entry and higher earnings contingent on employment suggests that the mechanism underlying our results extends beyond the moral hazard effects found in other studies.

Finally, we should note that our results identify program effects for participants as compared to a control group and therefore do not account for the possibility that participants may displace non-participants searching for jobs. It is common to assume that, during a recession, the number of jobs is limited, so that such displacement is particularly likely. In fact, the evidence on this issue is mixed. For example, Blundell *et al.* (2004) find that job search assistance and employment subsidies have a substantial positive impact in the areas where they are provided, suggesting the displacement effects are minor. In contrast, Crepon *et al.* (2013) find large displacement effects, although their focus on long-term unemployed youth may limit the generalizability of their results.

#### **4. Conclusion**

This paper examined the effectiveness of a program implemented in the United States during the Great Recession, which required displaced workers to undergo a UI eligibility review and

receive comprehensive job-counselling services at the early stages of their UI claims. Our analyses show that the program reduced average UI duration by nearly 3.5 weeks and average total benefits collected by \$877, with the average UI savings being more than four times the average program costs. These effects are partly attributable to moral hazard, given that the program led to higher UI exit rates for participants in the first three weeks of their claims, the timeframe in which participants were required to attend a meeting and pass the UI eligibility review to continue collecting benefits. But a larger increase in UI exits occurred in the 10-week period after participants received services. This suggests that the services offered to participants were themselves effective in enhancing participant job search skills, and thus helping them to obtain employment. The plausibility of the view that the value of services was an important contributor to program success is supported by our finding that the program had substantial positive effects on employment throughout the entire six-quarter follow-up period, leading to an increase in earnings of \$2,607 (18%) over that period. Since the program is also associated with increased earnings conditional on employment, it does not appear that employment came at the expense of lower earnings for those employed.

Our findings confirm those of recent U.S. and European studies that reemployment program requirements increase the opportunity cost of collecting UI, pushing participants who are job-ready or noncompliant with work search requirements to exit unemployment before receiving services. Our paper is the first to show that moral hazard effects may plausibly occur during a recession, when jobseekers have fewer job options but are eligible for unemployment benefits for extended periods.

This paper is also the first to separate the moral hazard and reemployment services effects of a program that required participants to receive comprehensive job-counselling services. Our

findings suggest that services may provide direct aid to participant job search efforts, facilitating their UI exit and movement into employment, and helping them to achieve higher earnings. These findings are in sharp contrast to those of recent studies of U.S. programs, which provided participants with a less substantial intervention and were found effective in reducing moral hazard but not in improving participant job search. Our results suggest that programs requiring participation in job-counselling services are successful in part because participants benefit from such services but would not seek them out in the absence of a program requirement. Our results also suggest that European programs that provide a combination of job-counselling services and work search monitoring may influence participants beyond the incentive effects that previous studies have focused on.

In conclusion, the results of this paper lend support to the view that reemployment services programs may be particularly valuable during a recession, when many jobseekers have marketable skills but limited job search experience. If so, increased support for government-sponsored reemployment programs may provide an effective strategy when the labour market is weak, as a way to both reduce the moral hazard of UI and help displaced workers to conduct a more effective job search.

Table 1: Treatment and Control Group Characteristics

	Treatment Group	Control Group	<i>Difference</i>
Female	.422	.433	-.011 [.008]
Hispanic	.211	.202	.010 [.006]
No High School Diploma	.164	.163	.001 [.006]
High School Diploma	.426	.435	-.009 [.008]
Some College	.288	.283	.005 [.007]
College Degree	.122	.119	.003 [.005]
Less than 25 Years	.123	.127	-.003 [.005]
25-34 Years	.257	.249	.008 [.007]
35-44 Years	.221	.229	-.007 [.007]
45-54 Years	.227	.218	.009 [.007]
55-64 Years	.128	.130	-.002 [.005]
65+ Years	.043	.047	-.004 [.003]
U.S. Citizen	.900	.899	.001 [.005]
Disabled	.050	.046	.004 [.003]
White Collar, High Skill	.192	.191	.001 [.006]
White Collar, Low Skill	.319	.320	-.001 [.007]
Blue Collar, High Skill	.233	.224	.009 [.007]
Blue Collar, Low Skill	.256	.265	-.009 [.007]
Maximum Benefit Amount	7,075 (3,046)	7,056 (3,033)	19 [48]
UI Weeks Allowed	22.8 (4.6)	22.9 (4.5)	-.1 [.07]
Prior Earnings, Quarter 1	7,073 (7,186)	7,078 (6,829)	-5 [113]
Prior Earnings, Quarter 2	7,132 (6,573)	7,310 (9,224)	-178 [111]
Prior Earnings, Quarter 3	7,398 (7,362)	7,445 (7,256)	-47 [116]
Prior Earnings, Quarter 4	7,585 (7,008)	7,488 (8,312)	97 [114]

Note: For the treatment and control group, the sample mean is reported for dichotomous outcomes, and the sample mean with standard deviation in parenthesis is reported for non-dichotomous measures. Standard errors in brackets accompany the treatment-control difference in means.

Table 2: Unemployment Insurance Receipt Outcomes

	Treatment Group	Control Group
Exhausted Regular UI Benefits	.604	.710
Collected EUC Benefits	.500	.599
Weeks Collected		
Regular UI	17.1 (8.5)	19.0 (7.9)
EUC	10.9 (12.6)	12.5 (12.6)
Total	28.0 (19.0)	31.5 (17.4)
Benefits Amounts Collected (\$)		
Regular UI	5,352 (3,498)	5,863 (3,416)
EUC	3,238 (4,195)	3,697 (3,902)
Total	8,680 (6,875)	9,560 (6,424)

Note: The sample mean is reported for dichotomous outcomes, and the sample mean with standard deviation in parenthesis is reported for non-dichotomous measures.

Table 3: Employment and Earnings Outcomes

	Treatment Group	Control Group
Employed		
Quarter 1	.476	.406
Quarter 2	.498	.414
Quarter 3	.526	.458
Quarter 4	.551	.487
Quarter 5	.539	.487
Quarter 6	.548	.500
Earnings (\$)		
Quarter 1	1,848 (3,659)	1,529 (3,247)
Quarter 2	2,479 (4,176)	1,977 (3,726)
Quarter 3	3,028 (4,715)	2,475 (4,347)
Quarter 4	3,188 (4,885)	2,674 (4,528)
Quarter 5	3,174 (4,967)	2,811 (5,338)
Quarter 6	3,405 (5,172)	2,987 (4,888)
Total, Quarters 1-6	17,122 (22,570)	14,453 (21,065)

Note: The sample mean is reported for dichotomous outcomes, and the sample mean with standard deviation in parenthesis is reported for non-dichotomous measures.

Table 4: Average Treatment Effects, UI Receipt

	Average Treatment Effect	% of Control Group Mean
Exhausted Regular UI Benefits	-.104 (.007)**	-15%
Collected EUC Benefits	-.097 (.008)**	-16%
Weeks on UI		
Regular	-1.9 (.1)**	-10%
EUC	-1.5 (.2)**	-12%
Total	-3.4 (.3)**	-11%
UI Benefits Collected (\$)		
Regular	-.520 (38)**	-9%
EUC	-.357 (60)**	-10%
Total	-.877 (87)**	-9%

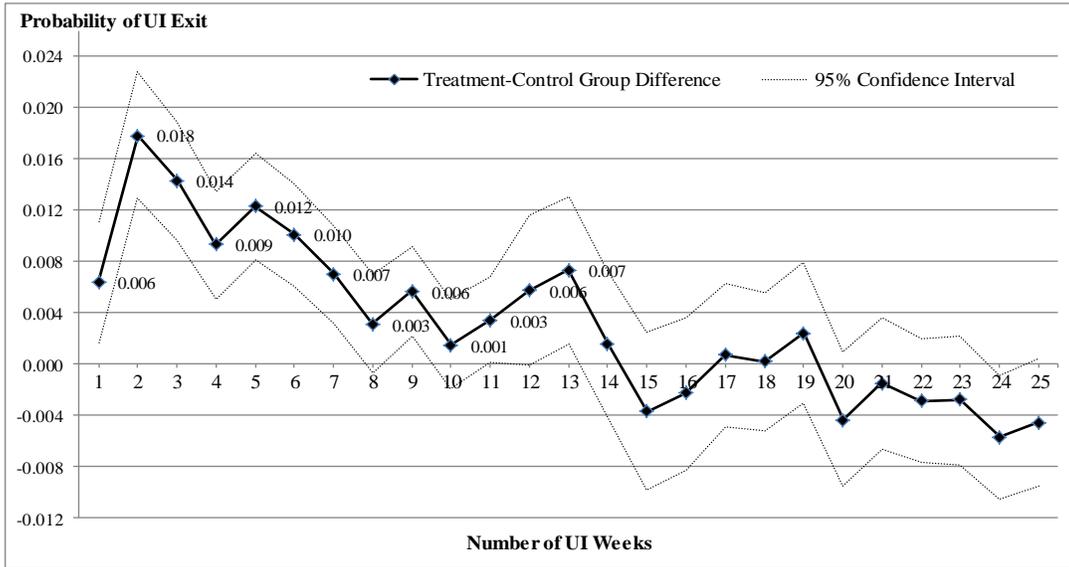
Note: The left column reports the average treatment effect, with standard error in parenthesis; the right column reports the average treatment effect as a percentage of the control group mean. \*\*= statistically significant at the 1% level.

Table 5: Average Treatment Effects, Employment and Earnings

	Average Treatment Effect	% of Control Group Mean
Employed		
Quarter 1	.070 (.008)**	+17%
Quarter 2	.082 (.008)**	+20%
Quarter 3	.066 (.008)**	+14%
Quarter 4	.063 (.008)**	+13%
Quarter 5	.052 (.008)**	+11%
Quarter 6	.046 (.008)**	+9%
Earnings (\$)		
Quarter 1	315 (51)**	+21%
Quarter 2	493 (59)**	+25%
Quarter 3	542 (68)**	+22%
Quarter 4	504 (70)**	+19%
Quarter 5	348 (81)**	+12%
Quarter 6	404 (75)**	+14%
Total, Quarters 1-6	2,607 (322)**	+18%

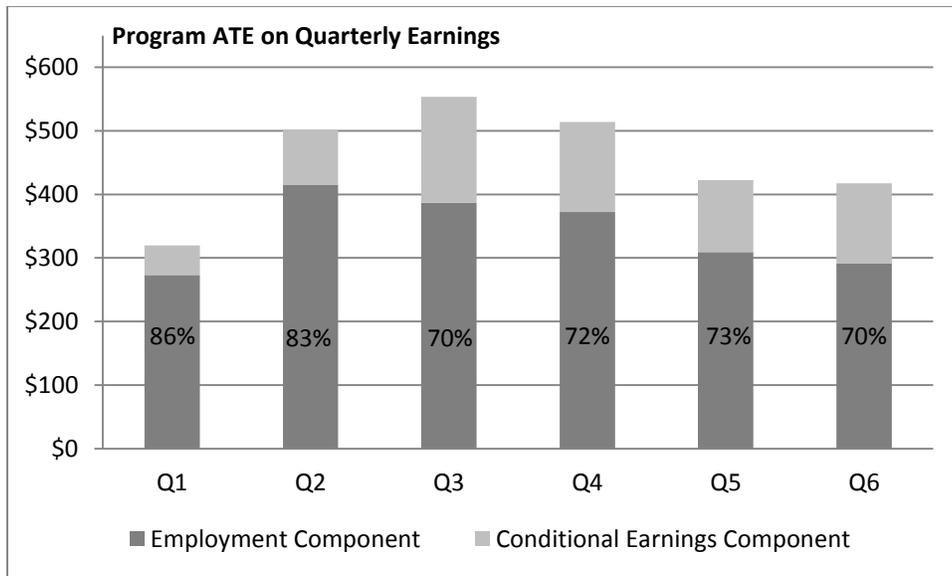
Note: The left column reports the average treatment effect with standard error in parenthesis; the right column reports the average treatment effect as a percentage of the control group mean. \*\*= statistically significant at the 1% level.

Figure 1: Treatment-Control Group Difference in the Probability of UI Exit



Note: Black line presents regression-adjusted treatment-control differences in the probability of UI exit. The grey dotted lines encompass the 95% confidence interval.

Figure 2: Decomposition of Average Treatment Effects on Quarterly Earnings



Note: The full bar height indicates the program's average treatment effect on quarterly earnings. The dark grey area is the portion of the effect attributed to the employment component (actual proportion also reported) and the light grey area is the portion of the effect attributed to the conditional earnings component.

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