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Are Reemployment Services Effective? Experimental Evidence from the Great Recession

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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 which required Unemployment Insurance (UI) recipients to undergo an eligibility review and receive comprehensive job-counseling services. The program expedited participant exit from UI, produced substantial UI savings, and improved participant employment rates and earnings. These effects are associated with: (1) increased participant UI exit up to the time of services receipt, indicating an effect due to participant efforts to avoid program activities or failure to meet UI eligibility requirements; and (2) greater exit subsequent to services receipt, 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.

JEL Classifications: J6, H4.

Keywords: Great Recession, job search services, unemployment, Unemployment Insurance, program evaluation.

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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 receive reemployment services as a condition for collecting unemployment benefits (Wandner, 2010; OECD, 2013). Funding for these programs grew dramatically during the Great Recession in the U.S., when the government made substantial investments to enhance the capacity of public employment offices to offer services to jobseekers (Wandner and Eberts, 2014). However, there is no evidence on the effectiveness of U.S. reemployment programs during the Great Recession, with the most recent studies examining programs implemented more than a decade before the start of the recession (Decker *et al.*, 2000; Klepinger *et al.*, 2002; Black *et al.*, 2003). Although there are many studies of such programs in Europe, evaluations are generally in the context of relatively strong labor markets (e.g., Blundell *et al.*, 2004; Abbring *et al.*, 2005; Graversen and van Ours, 2008; Hägglund, 2011).

This paper examines a reemployment program implemented in the state of Nevada that targeted workers who started collecting Unemployment Insurance (UI) benefits in the second half of 2009. During this period, the Nevada unemployment rate averaged over 12 percent, reaching a peak near 14 percent in the subsequent 15 months, 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 a recession, this case study is compelling because Nevada used random assignment at the start of the UI spell to determine which eligible UI recipients would be required to participate in the program (treatment group) and which would not be required to participate (control group). Treatment cases were required to attend a one-on-one meeting with program staff in the early stages of their UI spell in which they: (1) underwent an eligibility review to confirm they were qualified for benefits and were actively searching for a job; and, if determined eligible, (2) were provided services designed to enhance their job search based on individual needs, including direct referrals to job openings.

The analyses presented here rely on administrative UI claims data, UI wage records, and employment services data for all workers who started collecting UI in Nevada from July 2009 through December 2009 and were eligible for participation in the reemployment program and thus subject to random assignment. The UI claims data report: (1) the total number of regular UI benefit weeks and dollar amounts each recipient collected under the UI claim; and (2) the total benefit weeks and amounts collected under the Emergency Unemployment Compensation (EUC) program, available to recipients who exhausted their regular UI entitlement. UI wage records report quarterly earnings in each of the six calendar quarters after the start of the UI claim when random assignment was done. The employment services data report when meetings were scheduled, whether participants attended, and whether they were disqualified for findings of ineligibility during the meeting or for failure to attend the meeting. The data also report the specific job-search services received by both treatment and control cases.

We first examine the characteristics of UI recipients, including tests that confirm that random assignment was successful in assuring that treatment and control cases were comparable. Analyses of the program's effects show that the program reduced UI spells and payments, and increased quarterly employment and earnings in the six-quarter period after program entry. The fact that program requirements were scheduled at the beginning of participants' UI spells enables us to provide some evidence on the underlying mechanisms that led to program effects. In particular, we attempt to identify the relative importance of: (1) *moral hazard effects*, reflecting voluntary exit of participants to avoid program requirements and disqualifications of participants who were found ineligible during the review or failed to undergo the review, and (2) *services effects*, reflecting the value of services in helping participants to conduct a more effective job search.

Analyses of program effects on the timing of UI exit show that effects were partly realized in the initial stages of the UI spell, suggesting that moral hazard effects were important. But a substantial portion of program effects on UI exit occurred in the period after most participants had received services and their interactions with the program had ended, suggesting that the services provided by the program may have led to participants conducting a more effective job search. Although exit at later points in the UI spell could be influenced by selection, we show that selection cannot explain estimated effects at later points in the spells. Overall, these findings provide evidence that programs requiring UI recipients to undergo an eligibility review and receive job-search services can be effective during a recession, and suggest that both moral hazard and services effects may play an important role in program effects.

BACKGROUND

Since the early 1990s, U.S. policymakers have focused substantial attention on promoting the exposure of UI recipients to reemployment services. Although not all unemployed workers receive UI benefits, among displaced workers, most earnings losses occur for this group (Couch and Placzek, 2010). 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. According to the U.S. Department of Labor, from 1997 to 2007, 11.5 million UI recipients were referred to services – 6.3 million attending an orientation, 3.9 million participating in workshops, and 1.6 million receiving counseling.[1] Services were funded under the Wagner-Peyser Act, which established the national employment

[1] See Worker Profiling and Reemployment Service Activity (http://workforcesecurity.doleta.gov/unemploy/profile.asp).

system and is the funding authority for reemployment services, providing support to the states averaging $766 million annually (Wandner, 2010, Table 6.1, page 197).

At the end of 2007, the U.S. economy entered its worst recession since the Great Depression, with the unemployment rate increasing from 5 percent in December 2007 to a peak of around 10 percent 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. 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.

A number of experimental studies examine U.S. reemployment programs implemented from the late 1970s through the mid-1990s. Meyer (1995) reviews experimental evaluations of five programs implemented in the late 1970s and 1980s. In each program, randomly-assigned UI recipients were required to participate in activities to support their job search. Activities varied by program and included interviews with counsellors, attendance at employment workshops, and participation in other job-search services. In addition, some of the programs included eligibility reviews designed to assure that participants were actively searching for work. Point estimates implied that four of the five programs reduced the number of weeks of UI receipt by no more than about a week, and one program reduced receipt by nearly four weeks. Generally, estimated declines in payments of benefits exceeded the costs of the programs. For the three programs where effects on participants' earnings were estimated, effects were generally positive but statistical power was very limited. Meyer observed that programs involving more intensive interventions and higher services participation among selected individuals tended to have greater effects, but he also commented that none of the studies allowed one to infer the extent to which program effects result from moral hazard or service effects: “To date, the experiments have not convincingly separated the effects of requirements and assistance.” (p. 127)

In contrast, three more recent studies appear to provide relevant evidence. Klepinger *et al.* (2002) use data from a 1994 experimental program implemented in Maryland to examine the effects of imposing alternative work search requirements on UI recipients. The study examines three treatments: (1) remind participants that their records of employer contacts may be reviewed for verification; (2) increase the required weekly number of employer contacts to four instead of two; and (3) refer participants to employment workshops. Based on comparisons to the control condition, 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 effects on employment and earnings. The entire effect on UI was realized in the first two weeks of the UI spell, when treatment cases were notified of program requirements and prior to engaging in program activities.

Decker *et al.* (2000) present experimental evidence on job search assistance demonstration programs implemented in the mid-1990s in Florida and the District of Columbia. These programs required randomly assigned participants to attend a group orientation providing information on services and referrals to employment workshops and job counseling. The study finds that the program reduced UI duration by up to 1.1 weeks and UI benefits collected by up to $182, but produces mixed evidence of program effects on earnings. Treatment-control comparisons of UI exit rates show that the program’s entire effect occurred around the time participants were notified of program requirements, or at the time of participation in services.

Taking advantage of a procedure that randomly assigned eligible individuals with the same priority score to participate in Kentucky’s WPRS program, Black *et al.* (2003) present experimental estimates of the program in the period October 1994 through June 1996. The study finds that the program reduced UI duration by 2.2 weeks and benefit amounts collected by $143. The program had small positive effects on earnings in quarters 1-2 after program entry, but no impacts in quarters 3-6. Analysis of UI exit rates show that the only statistically significant effects occurred in weeks 1-2, the period when the notification letter was sent and prior to services receipt. Thus, the study argues that program effects were attributable largely to moral hazard and not to the effectiveness of services.

While not definitive, there is some evidence among U.S. studies that job-counseling and related services may be valuable for certain groups of disadvantaged workers. A random assignment study of a job-training program in the early 2000s finds that structured job counseling led to positive long-run effects on reemployment rates for the most disadvantaged workers but not for others (Perez-Johnson *et al.*, 2011). Similarly, a study of Social Security disability insurance beneficiaries in 2007-2009 finds that those randomly assigned to receive medical care management and job-counseling services had greater labor market success than those assigned to a control group that did not have such services (Weathers and Bailey, 2014).

Over the past 25 years, many European countries have put into effect programs designed to ensure that workers who collect unemployment benefits are actively searching for jobs and have access to services offered by public employment 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 in European and other developed countries, see OECD (2007, 2013).[2]

[2] Services to job seekers in many European countries are integrated with extensive job training programs, which are often required for some benefit recipients. We limit our focus here to programs focusing on job search requirements and related services. Such government-supported active labor market programs are generally more extensive in Europe than in the U.S. (see Kahn, 2012).

The effects of European programs have been examined extensively. An experimental study shows that requiring unemployed workers in the Netherlands to receive job-counseling services at the start of their UI spell and to attend work search monitoring meetings every four weeks thereafter led to an 11 percent 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 reduced average unemployment duration by 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 percent.

Additional experimental studies in 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.[3] Pedersen *et al.* (2012) report on a random assignment experiment in Denmark showing that intensive individual counseling (sessions every other week for 14 weeks) had a substantial impact on job finding for unemployed workers. Perhaps most notable, the study reports that rates of job finding for participants continued to exceed those of the control group for many weeks after the intervention was completed. Rosholm (2014) provides a summary of studies suggesting that job counseling is of substantial value in helping unemployed workers obtain jobs.

An important gap in the literature is that most of the above studies examine program efficacy

[3] These studies do not provide estimates of program effects on earnings.

during periods of moderate unemployment and there are no studies of U.S. programs operating during the Great Recession. Although two of the studies reviewed by Meyer (1995) focus on high unemployment periods in the late 1970s, the most recent U.S. studies examine programs implemented in the mid-1990s, when the U.S. unemployment rate was between 5 and 6 percent. Similarly, with the exception of Martins and Pessoa e Costa (2014),[4] recent European studies examine programs implemented during relatively strong labor markets. Hence, we have little knowledge about the value of job-search assistance programs during those times when the need would appear to be greatest. Our study addresses this gap by examining a job-search program implemented during one of the most important recessions since the 1930s.

THE NEVADA PROGRAM

In 2009, Nevada implemented a new program that required UI recipients to undergo an in-person eligibility review and receive staff-assisted reemployment services near the start of their UI spells. 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 replaced its existing WPRS program with the new program in the workforce regions covering the two large metropolitan areas in the state (Las Vegas-Henderson-Paradise and Reno).[5] REA funding was combined with Wagner-Peyser and ARRA funds to support the program, in which each participant was required to attend an in-person meeting with program staff in which the participant underwent the REA-mandated

[4] Using a fuzzy regression discontinuity design, they find that a job search support program implemented in Portugal in 2010 during a period of high unemployment doubled participants' likelihood of finding employment.

[5] Authors' tabulations of the American Community Survey show that these workforce areas covered 87 percent of the unemployed population in Nevada in 2009. Workforce regions in the rest of the state, serving mostly rural counties, were not affected by the new program.

review *and* received staff-assisted services. Nevada used random assignment to determine which recipients would be required to participate in the new program.

The Nevada selection process was as follows. Once an unemployed worker filed a UI claim and was deemed eligible for UI benefits,[6] the individual was scheduled to start collecting weekly UI payments after a one-week waiting period. During that waiting period, Nevada UI agency staff determined if the worker was eligible for the program. Similar to U.S. programs evaluated in previous studies, and in compliance with WPRS and REA directives, the Nevada program included all new UI recipients, except those on temporary layoff, those attached to a union hiring hall, and those who were active in training programs. Each week, the pool of program-eligible UI recipients was placed in an interface that allowed random assignment to the treatment group (subject to program requirements) or to the control group (no program requirements).

Once treatment cases were identified, they were sent letters notifying them that they had been randomly selected to attend a UI eligibility assessment meeting at a specified public employment office. Letters were sent around the time treatment cases received their first weekly UI payment (week 1 of their UI spell), informing them that the purpose of the meeting was to assist them in planning their job search and reduce the amount of time they would remain unemployed. The letter also indicated the exact date/time of the meeting, typically scheduled in weeks 2-4 of the UI spell,[7] and explicitly stated that the meeting was mandatory and that failure to attend would cause loss of benefits. Treatment cases that failed to attend or reschedule the meeting and did not meet the above conditions, were disqualified from collecting additional UI benefits. Although not stated

[6] UI claimants had to meet the following criteria to qualify for benefits: (1) lost their jobs through no fault of their own; (2) were willing and able to start a new job; and (3) earned at least $600 during the base period (the first four of the five calendar quarters prior to the UI claim date) and at least $400 during the quarter in the base period with the highest earnings. For details, see http://www.ows.doleta.gov/unemploy/uilawcompar/2009/comparison2009.asp.
[7] As we will see later, some treatment cases rescheduled their meetings. Nonetheless, the final schedule that included postponements placed 95 percent of all meetings in weeks 2-6.

in the letter, according to program rules, treatment cases that, by the time of the meeting, had participated in job-search services, found a job, or enrolled in training were not subject to disqualification because of failure to attend the meeting. Control cases had no requirements under the reemployment program. They received no program letter and had no requirement to meet with program staff, undergo an eligibility review, or receive services, but were subject to the usual UI rules.[8]

The meeting between each participant and program staff comprised two components: the eligibility review and provision of staff-assisted job-counseling services. In the 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. 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 law. Participants deemed ineligible for benefits or non-compliant with work-search requirements were disqualified from receiving UI payments.

Participants who passed the review were offered job-counseling services during the same meeting. Program staff assessed participant occupational skills and work experience and, based on the results, helped the participant to produce a professional resume if appropriate. Program staff also worked with participants to develop a work search plan designed to focus their search efforts on jobs that matched their skills and experience. A key part of this process was that participants were directly referred to employers with job openings that suited their skills. Notably, participants were offered services based on their needs, and they did not necessarily receive the entire range of available services. Although there is no data on the length of these meetings, the letter sent to treatment cases indicated that the meeting would take about one hour, which was

[8] The law specifies that all UI recipients in Nevada are required to be available for work, be actively searching for a job, and not reject suitable employment.

consistent with the estimate of program administrators we interviewed. Importantly, participants were informed that this meeting was the only requirement under the reemployment program and that they were not required to participate in additional services or meetings. Nonetheless, it is likely that participants were reminded that they needed to comply with the usual UI work search requirements, and they may have been encouraged to receive additional services.

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 in the program, 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, staff salaries, and related office expenses. It did not, however, cover costs of providing services not directly associated with the program. As noted below, some program participants obtained services following the week of their required meeting, and insofar as the costs of these services exceed those for the control group, this measure underestimates program expenses.

DATA

UI Claims Data

We use Nevada UI claims data, which provide information on all unemployed workers in the metropolitan areas of Las Vegas-Henderson-Paradise and Reno who started collecting UI benefits from July 2009 through December 2009 and were eligible for random assignment and participation in the reemployment program. Our sample includes all new UI recipients who were assigned to the treatment and all new UI recipients who were assigned to the control group. UI claims data provide individual characteristics at program entry (i.e., when they applied for UI, about a week prior to random assignment) and treatment/control status for the reemployment program. To determine UI eligibility, the Nevada UI agency used information on prior employment and earnings to determine the number of regular UI weeks, weekly benefit amount (WBA), and cumulative benefit entitlement (the maximum total payment the recipient was eligible to collect during the claim's benefit year).[9] Nevada's unemployment rate during the study period exceeded the thresholds for activating the federal Emergency Unemployment Compensation (EUC) and Extended Benefits (EB) programs. Thus, treatment and control cases in our sample that exhausted their regular UI benefits (12-26 weeks) could apply to receive up to an additional 53 weeks of EUC benefits.[10] Moreover, those who exhausted EUC could apply to collect up to an additional 20 weeks of EB.[11] The WBA for EUC and EB was identical to that under regular UI.

The UI data used in this study report the number of regular UI benefit weeks and cumulative benefit amount each recipient was entitled to collect during the claim's benefit year for all treatment and control cases. The data also provide information that we used to calculate each recipient's EUC and EB entitlements, which measure the additional benefit weeks and amounts each recipient would be eligible to collect after exhausting regular UI benefits. The data also provide the total number of regular UI and EUC benefit weeks and amounts actually collected by each recipient. The data do not provide information on benefits collected under EB, an omission reflecting an early data management decision. The omission of EB data has two implications: (1) we cannot calculate the full UI spells for individuals who exhausted both regular UI and EUC

[9] The WBA was equal to 1/25 of earnings in the quarter with the highest earnings during the base period, subject to a $16 minimum and a $393 maximum. Weeks of eligibility were equal to one third of the benefit year earnings divided by the WBA, with a 12-week minimum and a 26-week maximum. The cumulative entitlement is equal to the WBA times weeks of eligibility. The benefit year lasted 365 days from the date the claim was filed.

[10] Recipients who exhausted regular UI, applied for EUC, and had at least 20 weeks of prior employment were eligible for: (1) up to 20 weeks of EUC tier 1 benefits (lesser of 80 percent of regular UI and 20 weeks); (2) up to 14 weeks of EUC tier 2 benefits (lesser of 50 percent of regular UI and 13 weeks, plus one week); (3) up to 13 weeks of EUC tier 3 benefits (lesser of 50 percent of regular UI and 13 weeks); and (4) up to 6 weeks of EUC tier 4 benefits (lesser of 25 percent of regular UI and 6 weeks).

[11] Recipients who exhausted EUC were eligible for up to 20 weeks of EB (lesser of 80 percent of regular UI or 20 weeks).

benefits;[12] and (2) to the extent that the program was effective, we underestimate program effects on total UI spells and benefit amounts collected.

Using UI claims data, we find that, during the study period, 31,793 unemployed workers started collecting UI in the Las Vegas-Henderson-Paradise and Reno metropolitan areas and were deemed eligible for the program.[13] Of these, 4,673 (15 percent) were randomly assigned to the treatment group and the remaining 27,120 (85 percent) to the control group. Table 1 presents sample proportions for measures of individual characteristics derived from UI claims data and means and standard deviations of prior earnings from UI wage records. Also, to check if random assignment produced a balance in the characteristics of treatment and control group members, the right column of Table 1 presents treatment-control differences in means and standard errors to assess their statistical significance.

The table provides no surprises. For both treatment and control cases, just over two-fifths were women and a fifth were Hispanic, with relatively low proportions under age 25 and over age 55. The occupational distribution, based on the occupation associated with the most recent job held, shows that the low-skill white-collar group was the largest, reflecting the dominance of Nevada's service industry. Information on industry of prior employment was not available in the data. The bottom panel of Table 1 reports average quarterly earnings in the four-quarters prior to UI entry based on UI wage records.[14] As expected, UI recipients experienced a slight decline in average earnings over the year prior to the time they filed their UI claims. T-tests reveal no statistically

[12] During the study period, 16.2 percent of the treatment and 19.1 percent of the control group exhausted regular UI and EUC, and thus were eligible for EB.

[13] There were about 152,000 unemployed workers in Nevada each month during the study period (Bureau of Labor Statistics, https://www.bls.gov/lau/staadata.txt). According to the U.S. Department of Labor's UI Data Summary (https://workforcesecurity.doleta.gov/unemploy/content/data.asp), 80,984 unemployed workers started collecting UI in Nevada during the second half of 2009. The sample used here excludes UI claims filed outside the two metropolitan areas in which the program was operating and UI claimants who were not eligible for the program.

[14] Individuals with no reported earnings in a quarter are included with a value of zero.

significant treatment-control differences in characteristics and prior earnings.

Table 2 presents means and standard deviations of regular UI eligibility measures. Individuals in the sample were eligible for about 23 weeks of regular UI, with just over $7,000 in cumulative benefit entitlements. Regular UI eligibility ranged from 12 to 26 weeks, with nearly 60 percent of recipients eligible for the maximum 26 weeks. Table 2 shows that treatment and control cases that exhausted regular UI were entitled to about 46-47 weeks of EUC benefits, and an additional 18 weeks of EB after they exhausted EUC. EUC eligibility ranged from 25 to 53 weeks, with 58 percent of recipients eligible for the full 53 weeks; EB eligibility ranged from 9 to 20 weeks, with 61 percent of recipients eligible for the full 20 weeks. Overall, treatment and control cases in our sample were eligible for an average 87 weeks in total benefits (the sum of regular UI, EUC and EB), with an average total cumulative entitlement of nearly $27,000. Total benefit eligibility ranged from 46 to 99 weeks, with nearly 58 percent of recipients entitled to the full 99 weeks. T-tests in Table 2 show no significant treatment-control differences in entitlements, confirming that random assignment was successful in balancing the samples to within statistical expectation.

We also used Nevada UI claims data to construct benefit receipt measures, including whether recipients exhausted regular UI benefits, whether they collected EUC after exhausting regular UI, benefit weeks collected (regular UI and EUC), and benefit amounts collected (regular UI and EUC). Total UI weeks collected are measured using the sum of regular UI weeks plus EUC weeks collected, and thus our measure of total benefits collected equals the sum of regular UI and EUC amounts collected. Table 3 shows that treatment cases had lower regular UI exhaustion and EUC collection probabilities than control cases. As a result, treatment cases collected fewer weeks and lower amounts of regular UI, EUC, and total benefits (regular UI plus EUC) than control cases.[15]

[15] Note that the proportion exhausting regular UI benefits is about 10 percentage points greater than the proportion collecting EUC benefits. This difference is likely attributable to the fact that some recipients did not have the required

Treatment cases were less likely to exhaust EUC benefits, suggesting that they were less likely to collect EB (information on EB collection was not reported in our data). Thus, if the program reduced UI duration, the average treatment effects reported below underestimate the program's effect on total UI duration and benefits collected (including regular UI, EUC, and EB).

UI Wage Records

Our second data source derives from Nevada UI wage records, which provide calendar-quarter earnings within Nevada for all treatment and control cases in our sample in each of the four quarters prior to and in each of the six quarters following the start of the UI claim. We use these data to identify employment and earnings in each quarter. The data do not report dates of employment or hours of work within the quarter, and thus they cannot be used to determine length of employment or hourly wages. They also omit federal jobs, self-employment, informal employment, or the small number of jobs not subject to UI reporting requirements. Also, the data do not include earnings from employment in other states, an omission that could potentially affect our results. If more treatment cases were able to find jobs in other states relative to control cases, then treatment-control differences in employment and earnings would understate the true program effects. In contrast, if control cases were more likely to migrate because of lack of available jobs in their state, our estimates would overstate the program's effects. Notwithstanding these omissions, it has been suggested that program effects on employment and earnings based on wage records are generally comparable to those obtained in surveys, at least in the context of training programs (Kornfeld and Bloom, 1999) and welfare programs (Wallace and Haveman, 2007). In fact, wage records have been used extensively in studies that assess the effects of reemployment

20 weeks of employment in the claim's base period, and others who did have the required prior employment did not apply for EUC after exhausting regular UI.

programs in lieu of survey data, including the aforementioned U.S. studies (Decker *et al.*, 2000; Klepinger *et al.*, 2002; Black *et al.*, 2003).

These data are used to construct two measures of employment outcomes in each of the six calendar quarters following the start of the UI claim. First, positive earnings in a calendar quarter provide our measure of employment, indicating whether the individual was employed at any point during a quarter. Second, we measure total earnings in a quarter for each individual; those with no Nevada earnings were included with values of zero. As seen in Table 4, treatment cases had higher employment and earnings than control cases over the entire six-quarter follow-up period.

Employment Service Data

The third data source used here derives from employment service records, which provide the exact scheduled date of the required program meetings for the 4,673 treatment cases, indicate whether they attended the meeting, and report whether they were disqualified because of findings of ineligibility or failure to attend the meeting. Table 5 presents the final REA meeting schedule, which reflects any postponements of an originally scheduled meeting; we do not have information on the initial assigned meeting dates. As shown in Table 5, the vast majority of treatment cases (95 percent) were scheduled to have their meeting in weeks 2-6, and that 3,717 (80 percent) of the 4,673 treatment cases attended the meeting.

Using employment service data, Table 6 provides information on the services received during the UI claim's benefit year.[16] We see that 68.4 percent of treatment cases and only 9.7 percent of control cases received at least one of four job-counseling services offered during the meeting (work

[16] As noted below, we do not have information on the date of the service for most of the listed services. The services tabulated in Table 6 are those that are associated with the indicated claim. For services where we do have dates, 99 percent were received within the claim's benefit year (i.e., within 365 days of the claim date).

search plan, resume assistance, individual needs assessment, and job referrals). The most common service was aid in developing a work search plan, with more than 55 percent of treatment cases served, as compared with only about 6 percent of control cases. Moreover, treatment cases were more likely to participate in group orientations and employment workshops offered at local employment offices.

Combining the information on the meeting and services received, we find that of the 4,673 treatment cases: (1) 3,717 (80 percent) attended the meeting; (2) 491 (11 percent) did not attend the meeting but received at least one service; and (3) 465 (10 percent) did not attend the meeting and did not receive any services. Under program rules, those who attended the meeting and passed the eligibility review were not subject to any further requirements. As noted above, those who did not attend the meeting but received at least one service were not subject to disqualification.[17] Treatment cases that did not attend the meeting and did not receive services were subject to disqualification, but program rules specify that if participants reported that they had found a job (presumably implying they would soon exit UI) or were participating in training, they were excused from attending the meeting. Overall, 0.7 percent of treatment cases were disqualified because of eligibility issues and 1.1 percent because of failure to undergo the review.[18]

We also have dates when services were received for about 40 percent of cases, although even when a date for the service is provided, except for job referrals, only the most recent date of that service is reported. Appendix A provides tabulations based on available information. Of the 1,296

[17] None of the participants who missed the meeting but received services were identified in our data as terminated.

[18] Of the 3,717 cases that underwent the review, 34 were disqualified due to eligibility issues identified in the review. Of the 465 cases that did not attend the meeting and did not receive any services, 52 were disqualified for failure to attend the meeting. The timing of disqualifications is summarized in Appendix B, which shows that exits from UI due to disqualifications occurred only in the first eight weeks of the of the UI spell. Separate tabulations show that all disqualifications produced exits that occurred within two weeks of the meeting. Our data do not allow us to identify the particular exemptions allowing some program participants who missed meetings and did not receive services to avoid disqualification.

treatment cases that received services and for which a date is available, at least 80 percent received a counseling service in the week of the meeting.[19] This is consistent with the program's reported structure, in which such services were provided during the meeting. In addition, at least 27 percent received a service before the week of the meeting and 24 percent received a service following the week of the meeting. Hence, it appears that the program induced some participants to undertake activities beyond those that were strictly required. This may be because the meeting motivated participants to seek out additional services to improve their job search, or that participants believed they were expected to participate in additional services. Also, among those participants who had a job referral for which the date was available, nearly half received a referral following the week of the meeting. This suggests that job counselors may have contacted participants to refer them to vacancies after the meeting. It is also possible that subsequent job referrals occurred because participants who remained unemployed later in their UI claims contacted job counselors on their own initiative to seek assistance.

METHODS

Effects on UI Receipt, Employment, and Earnings

Comparisons of means for measures of UI receipt, employment, and earnings between treatment and control cases, presented in Table 3, provide estimates of the program's average treatment effects. To improve statistical power, we used linear regression models to estimate program effects, controlling for characteristics and prior earnings, as follows:

$$[1] \qquad Y_i = a + b \cdot T_i + X_i \cdot c + u_i$$

[19] The 80 percent figure is a lower bound. Since only the date of the most recent service is listed for each category of service, services provided during the week of the meeting will not be counted if a service in the same category occurs later.

The dependent variable (Y_i) is the outcome for individual i. The treatment indicator (T_i) equals 1 if the individual was in the treatment group and 0 otherwise. The vector of control variables (X_i) includes individual characteristics at program entry and prior earnings (as seen in Table 1), fixed effects capturing the number of regular UI weeks for which the individual is eligible, the logarithm of the regular UI benefit entitlement, and fixed effects for week of UI entry. Estimated parameters include a constant term (a), a vector of coefficients for control variables (c), and a zero-mean disturbance term (u_i). The estimation sample includes all treatment and control cases during the study period; thus, the fitted parameter b estimates the program's average treatment effect.

To examine program effects on UI use, we estimate equation [1] taking as dependent variables measures of UI exhaustion, number of weeks of receipt, and the value of benefits received (see Table 3). To examine the labor market effects of the program we estimate the same equation taking employment and earnings as dependent variables. Although employment and earnings measures are available for all treatment and control cases, they derive from wage records, so they cover a slightly different period than UI claims data.

Effects on UI Exit Likelihood

We expect moral hazard effects to occur between receipt of the notification letter by treatment cases and the week they actually met with counselors. In contrast, services effects are likely to occur after the meeting, when the benefits of services in aiding job search would be realized. To provide an indicator of the relatively importance of moral hazard effects as compared to services effects in each week, we divided the sample of treatment cases that are in the risk set for exiting UI in each week into three groups. Group A for a particular week t consists of treatment cases that: (1) were scheduled for the meeting in week t or later; (2) did not show up for their scheduled meeting prior to week *t* and were not coded as receiving any job-search services; or (3) attended the meeting prior to week *t* and were disqualified because of findings of ineligibility. We assume that these treatment cases are subject to moral hazard effects, stemming from voluntary UI exit to avoid program requirements or from disqualification for failure to meet requirements. It is unlikely that services effects are relevant for these cases.

Group B consists of treatment cases that missed their scheduled meeting prior to week *t*, but received job-search services. According to program rules, these individuals were not subject to disqualification because of failure to show up for the meeting, because their services receipt was taken as indication that they were searching for a job. We assume that, for many in this group, both moral hazard and services effects are likely to be relevant. Moral hazard effects may occur because these participants are unaware that they are exempt from the meeting, so some may drop out of UI to avoid program requirements. Services effects may occur if they received services that provided them with direct aid to their job search, or because those services motivated them to exert a more intensive job-search effort. Finally, Group C comprises those who attended the meeting and passed the eligibility review, and thus fulfilled program requirements, prior to week *t*. Given that these individuals were informed that they had satisfied program requirements, services effects are likely to be of greater importance.

Figure 1 identifies the relative sizes of these groups as a function of the time since the start of the UI spell. In weeks 1-2, all individuals are in Group A, since no meetings occurred prior to that point. With many treatment cases attending scheduled meetings in weeks 2–5, the proportion in Group A declines to less than half by week 4 and to only about 18 percent by week 6. Starting in week 5, the majority of the at-risk participants consists of those in Group C. This grows to 73 percent in week 6, and approaches a maximum of over 85 percent in week 11. Although Group B increases through week 6, it levels out at that point, remaining under 9 percent in later weeks.

These figures suggest that the timing of program effects on the likelihood of exiting UI, conditional on having not exited in a prior week,[20] can be used to assess the relative importance of moral hazard and services effects. Any program effects on UI exit in weeks 1-3 would be primarily attributable to moral hazard, while any effects in weeks 4-5 are likely attributable to both moral hazard and services. Effects on UI exit in week 6 or later, after the interactions of most participants with the program had ended, would be most likely to reflect services effects.

To identify the importance of moral hazard and services, we estimate treatment-control differences in the likelihood of exiting UI in each week, conditional on having not exited in prior weeks. We adopt a very general structure, taking the UI exit likelihood in week t for individual i (H_{ti}) as a function of a time-varying constant term (a_t), program assignment (T_i), and measured characteristics (X_i):

$$[2] \qquad H_{ti} = a_t + b_t \cdot T_i + X_i \cdot c_t$$

Central to this specification is that we allow the program effect (b_t) to change over time, which is consistent with the view that program effects are likely to vary between weeks early in the UI spell, when moral hazard effects would predominate, and in subsequent weeks, when services effects would be operable. Our model also allows for the possibility of selection on measured characteristics and that measured characteristics have differing impacts over time, as reflected in the vector of parameters c_t. This accounts for potential heterogeneity of effects in measured characteristics without making strong assumptions about the structure of the relationship between observables and the UI exit likelihood. In our analysis, the time period will be a week, and the

[20] In the discussion that follows, our reference to the likelihood of exit, or simply UI exit, will refer to the probability of exiting in a given week, conditional on not having exited in a prior week. This probability is a good approximation to the continuous-time hazard for small probabilities.

dependent variable will be a dichotomous measure indicating exit from UI during that week, with the sample limited to those who had not exited in prior weeks.[21] Our approach is more flexible than previous studies that estimate UI exit assuming program effects and/or the effects of observed characteristics are constant over time and affect the exit likelihood proportionally (e.g., Gorter and Kalb, 1996; Abbring *et al.*, 2005; Hägglund, 2011).

Our specification does not account for unobserved heterogeneity across individuals, which could cause bias if selection occurred early in the spell on unmeasured factors that influence UI exit. Studies that model unobserved heterogeneity in estimating reemployment program effects assume that dynamic selection downwardly biases program effect estimates later in a spell (Graverson and van Ours, 2008; Geerdsen, 2006). Although we know of no estimated models that allow for unmeasured heterogeneity where dynamic selection causes program effect estimates to be upwardly biased later in a spell, this is a theoretical possibility. For example, Black *et al.* (2003) observe "that persons with low hazard rates in the treatment group [could] exit UI in the first few weeks at higher rates than similar persons in the control group," which would cause the treatment-control difference in the observed exit likelihoods to overestimate the treatment's causal impact (p. 1322). Following our main results below, we report results based on a formal model that examines the potential effects of such selection.

RESULTS

Effects on UI Receipt, Employment, and Earnings

Table 7 presents the average treatment effect for UI receipt outcomes based on equation [1], along with the effect as a percentage of the control group mean. The program led to a 10.4

[21] This model was estimated using a linear probability model; we also estimated this model using probit, but results were essentially identical.

percentage-point reduction in the likelihood of exhausting regular UI, a 15 percent reduction relative to the control group. This effect translates into a 16 percent reduction in the likelihood of EUC receipt. The program also had substantial effects on UI duration, reducing regular UI spells by 1.9 weeks and EUC spells by 2.5 weeks.[22] As a result, the program led to a $520 reduction in regular UI and a $625 reduction in EUC, for a $1,145 reduction in our measure of total benefits collected. Thus, the average UI savings per treatment case were nearly six times the estimated average program cost of $201.

Finally, the program reduced the likelihood of EUC exhaustion by 2.8 percentage points, a 15-percent reduction over the control group mean. This result suggests that the reported program effects on total UI duration and benefits collected under regular UI and EUC likely underestimate effects on UI receipt outcomes that also include EB. If we assume that, for those who exhausted EUC, the program had no effect on the length of time collecting EB, and that all individuals used their entire EB entitlement, we underestimate program effects on total UI spells by .5 weeks and on total benefit collected by $151. Although other assumptions would produce somewhat different estimates, it is nonetheless clear that omission of EB does not substantively change our findings.

Table 8 presents estimated program effects on the level of employment and earnings. Results show that the program increased employment by 7.0 and 8.2 percentage points in quarters 1 and 2, respectively. The program's effect on employment gradually declined over time, but remained positive and statistically significant through quarter 6, the last quarter for which we have employment data. The program had positive effects on earnings in each of the six quarters following UI entry; over the entire six-quarter period following UI entry, the program increased

[22] Since total UI weeks collected is equal to the sum of regular UI weeks plus EUC weeks collected, the treatment-control difference in total UI weeks collected are equal to the sum of the treatment-control difference in regular UI weeks *plus* the treatment-control difference in EUC weeks collected.

total earnings by $2,607 (18 percent).

Effects on UI Exit Likelihood

Figure 2 presents estimated program effects on the UI exit likelihood based on equation [2] for weeks 1 through 25, as well as each parameter's .05 confidence interval. For example, the figure shows that, in week 2, for those who had not exited in week 1, treatment cases were 1.8 percentage points more likely to exit than control cases. Program effects in the initial weeks are positive and statistically significant and, although there is a generally declining trend, with the exception of week 10, the estimates are statistically significant through week 15. Overall, the UI exit likelihood by week 15 is estimated to be 31.5 percent for control cases and 40.6 percent for treatment cases, which means that program participation increased UI exit in the first 15 weeks by 9.1 percentage points (29 percent).[23]

A portion of this effect was realized in weeks 1-5, when the cumulative UI exit likelihood was 15.6 percent for the treatment and 9.8 percent for the control group, a 5.8 percentage-point difference. In week 1, the program increased UI exit by 0.7 percentage points, an effect that is entirely due to moral hazard because none of the at-risk population was scheduled to undergo the review prior to week 1. In each subsequent week, the proportion of the at-risk sample subject to moral hazard effects declined, as more treatment cases attended the meeting. Still, the observed effects in weeks 2-5 – ranging from 1.1 to 1.8 percentage points – could be primarily attributable to moral hazard. These effects include disqualifications for failure to attend or reschedule the meeting or because of findings of ineligibility during the review (see Appendix B).

A substantial portion of program effects occurred in week 6 or later, when moral hazard effects

[23] Throughout this section, the exit rates for program participants are based on the regression adjustment obtained from equation [2]. Appendix C provides a comparison of results obtained with and without the regression adjustment.

due to voluntary exit or disqualifications are less likely and services effects may be more important. As noted above, in week 6, the proportion of the at-risk population subject primarily to services effects (Group C) was about 73 percent, increasing over time and approaching 85 percent by week 10. Among those who had not exited by week 5, we find that the UI exit likelihood in weeks 6-15 was 29.6 percent for treatment and 24.1 percent for control cases. Thus, program participation is associated with a 5.5 percentage-point (23 percent) increase in UI exit in weeks 6-15.[24]

Discussion

The above analyses show that the Nevada REA program helped participants reduce the amount of time they spent collecting UI benefits and produced average UI savings that exceeded program costs by more than four times. Our analyses show that a portion of these effects were realized in the initial stages of the UI claim, reflecting voluntary exit to avoid program requirements or disqualifications. But the program also induced UI exits in a period when most participants had completed program requirements, suggesting that the services offered by the program may have helped participants to improve their job-search outcomes. These findings are consistent with the fact that, in the six-quarter period after program entry, program participants had significantly higher employment rates and earnings than control cases.

Most of the programs evaluated in prior experimental studies reduced average UI durations by no more than about one week (Meyer, 1995; Decker *et al.,* 2000; Klepinger *et al.,* 2002), although Black *et al.* (2003) found effects of 2.2 weeks. The 1977-1978 Nevada Claimant Placement Program was found to have the largest impact, reducing UI spells by 3.9 weeks (Meyer, 1995). This program had two key similarities with the Nevada program examined here – first, it included

[24] Only 0.6 percentage points (13 percent) of this effect is attributable to disqualifications occurring in weeks 6-8.

formal eligibility reviews and mandatory job-search services, and, second, it operated in a period of high unemployment. Our analyses estimate that the Nevada program operating during the Great Recession reduced UI benefit spells by an average of 4.4 weeks, more than any of these programs. However, more than half of that effect was due to a reduction in the receipt of EUC, largely reflecting the increased likelihood that a participant would have discontinued UI receipt prior to exhausting regular benefits. In the absence of EUC legislation, the estimated effect would have been 1.9 weeks, greater than most but not all prior studies.

Our results on the timing of program effects differ from those in the most recent studies which attempted to separate moral hazard from services effects. Black *et al.* (2003) report that three-quarters of program effects occurred in weeks 1 and 2 of the 12-week period in which positive effects were observed, while estimated effects in weeks 3-12 were not statistically significant. Decker *et al.* (2000) found that the job-search assistance programs in Florida and Washington, DC increased exit for participants in the initial period of the UI claims, when most participants had not yet fulfilled program requirements. Similarly, Klepinger *et al.* (2002) found that the Maryland job-search assistance program increased exits from UI only in the first few weeks of the UI claim.

None of the previous studies have yielded the large effects on employment and earnings found in our study. The five programs examined by Meyer (1995) had small effects on employment and earnings, which in many cases lacked statistical significance. Black *et al.* (2003) found that the Kentucky WPRS program had small effects on employment and earnings in the first two quarters after entry but not in subsequent quarters, while Decker *et al.* (2000) found small effects on employment and earnings in Washington, DC and no effects in Florida. Similarly, the Maryland program studied by Klepinger *et al.* (2002) yielded small or zero effects on employment and earnings.

The fact that the Nevada program examined here had larger and more persistent effects on UI receipt and on employment and earnings suggests that the mechanisms underlying our results may differ from those of the other programs. However, based on our analyses, we are not able to identify the reason. One potential explanation is that the Nevada program provided participants with a different set of services than the programs examined in previous studies. In addition to the required eligibility review, more than 68 percent of Nevada participants received job-counseling services, including assistance in developing a work search plan (56 percent), resume development assistance (26 percent), and direct job referrals (21 percent). Service participation requirements in the Black *et al.* study were not strictly enforced, with the result that fewer than 50 percent of participants actually obtained at least one service, while in the Klepinger *et al.* study, only 30 percent of those assigned to participate in the job-search workshop, actually attended. Participation in services for the programs reviewed by Decker *et al.* were appreciably higher, with the authors reporting that in one of the program they studied 79 percent of participants underwent testing and 49 percent received counseling.

A second possibility is that the value of job-search services is higher during a recession. As noted above, the availability of additional weeks of benefits under EUC legislation increased the program effect on total benefit receipt in a mechanical way. However, even in the absence of such legislation, a program that increases job finding early in an unemployment spell will have larger effects on cumulative employment if UI recipients experience longer unemployment spells, since those who find jobs due to the program would have remained unemployed longer during a weak labor market.

Although one might assume that the lack of good job options during a recession would reduce the benefits of job-search in general, there may be compensating factors as well. The profile of the average jobseeker changed substantially in the Great Recession, with the proportions of unemployed male, college-educated, experienced, and white-collar workers reaching their highest levels in 20 years (Michaelides and Mueser, 2012). Insofar as the recession disproportionately affected workers who had no employability issues prior to the recession and thus had limited job-search experience, services may have been particularly valuable (Jacobson and Petta, 2000; Jacobson *et al.*, 2004; Barnichon *et al.*, 2012). There is also a growing literature arguing that strategic provision of information may improve individual decision making, reducing procrastination and other nonfunctional behaviors that limit the ability of individuals to take effective actions to improve their circumstances (Thaler and Sunstein, 2009; Babcock *et al.*, 2012; Cockx *et al.*, 2014). Unemployed workers may be more responsive to services if these problems become more severe when they face the prospect of long-term unemployment.

As noted above, since the program induces some participants to exit near the beginning of the UI spell, selection may alter the composition of treatment and control groups, inducing bias in estimated program effects on UI exit in later weeks. A simple formal model – presented in Appendix D – allows us to characterize the potential bias for the estimated effect in any given week. Our primary concern focuses on the possibility that estimated program effects after weeks 5 or 6 are largely spurious. The bias in the estimate of the program effect on the exit rate in a given week is a function of the proportion of cases that are omitted due to the program prior to the week in question and the likelihood that these cases would have exited in that week if they had not exited previously. The former measure is available from our program estimates, but there is no way to determine the latter. If those induced to exit by the program would have been very unlikely to exit later, this could cause subsequent program effects to be overestimated. In fact, we find that if we make the most extreme assumption – that such individuals would *never* have exited at a later point

– less than a third of estimated program effects in any week up to week 13, the period during which most of the statistically significant effects are observed, could plausibly be due to selection.

The conclusion that services are important in explaining program effects assume that, after completing the scheduled meeting, participants no longer felt pressure to exit UI. If this assumption is false, moral hazard effects could be responsible for observed effects even in later weeks. We cannot rule out the possibility that, by providing job-counseling services, counselors imposed implicit pressure on participants to undertake more intense job search. If this was the case, the program would have been more successful than prior programs partly because it activated increased search efforts, and not necessarily because the services themselves were effective.

Note also that the size of the moral hazard effect depends on the perceived burden of the program, prior to participants engaging in program activities. It is possible that if the program were in place for an extended period, participants would realize that the program's requirements were less onerous than participants in the study believed them to be. In this case, if the program was implemented over the long-term, the moral hazard effect would be reduced. In contrast, the effects of job-search services might be expected to survive even in the long-term if they reflected direct service benefits.

Finally, in common with other experimental studies of reemployment programs, our results identify program effects for participants as compared to a control group, which may not be the same as effects of the program if it were implemented for the full population. As Graversen and van Ours (2008) observe, where the treatment group is small relative to the control, as is the case in our study, it is unlikely that results will be biased by spillover effects altering control outcomes. However, it is still possible that participants may displace non-participants searching for jobs, so that estimated effects would be larger than the general equilibrium effects that would occur if the program was widely implemented. During a recession, with the number of jobs limited, one might assume that such displacement would be particularly likely. In fact, the evidence on this issue is mixed. Some studies find substantial evidence of displacement effects, particularly among the long-term unemployed youth (Feracci *et al.*, 2010; Gautier *et al.*, 2012; Crepon *et al.*, 2013). Toohey (2015) finds that increased job-search efforts due to state rules have only modest effects on unemployment spells, especially during downturns, and he suggests that this is due to displacement (see also Lise *et al.*, 2004). In contrast, Martins and Possoa e Costa (2014) conclude that displacement is not important and that targeted groups which benefited from the reemployment program they examined did not do so at the expense of other groups. Other studies find that job-search assistance and employment subsidies have substantial positive impacts even when they are provided to a large share of unemployed workers in particular geographic areas, suggesting the displacement effects are minor (Blundell *et al.*, 2004; De Giorgi, 2005).

CONCLUSION

This study examines the effectiveness of a program implemented in the United States during the Great Recession that required new UI recipients to undergo an eligibility review and receive job-search services at the early stages of their UI spells. Analyses show that the program reduced average UI duration by nearly 4.4 weeks and average total benefits collected by $1,145, with UI savings exceeding average program costs by nearly six times. These effects are partly attributable to moral hazard, given that the program pushed some participants to exit UI during the timeframe in which participants received notification of their assignment to the program and were required to attend a meeting and pass an eligibility review to continue collecting benefits. But a substantial increase in UI exits occurred in the 10-week period after the interactions of most participants with the program had ended, when services effects were likely to have been important. We also find 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 percent) over that period.

The findings of this paper confirm those of recent U.S. and European studies that reemployment program requirements increase the cost of collecting UI, pushing participants who are job-ready or noncompliant with work-search requirements to exit unemployment. Our findings suggest that such moral hazard effects may occur during a recession when jobseekers have fewer job options and are eligible for unemployment benefits for extended periods. They also show that, in addition to moral hazard effects, programs requiring participation in job-counseling services may facilitate participants' UI exit and movement into employment. This suggests that programs requiring job-counseling services may be successful in part because participants would not seek them out in the absence of a requirement. Another possible interpretation is that provision of services may encourage of participants to conduct more intensive job search after program requirements are satisfied. In conclusion, our results lend support to the view that government-sponsored reemployment services programs may be valuable during a recession, when the need is greatest. Programs that combine mandatory monitoring activities and job-search services participation may provide an effective strategy to both reduce the moral hazard of UI and help unemployed workers to conduct more effective job search.

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Table 1: Treatment and Control Group Characteristics

	Treatment	Control	*Difference*
Sample Size	4,673	27,120	
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]*
Prior Quarter 1 Earnings	7,073 (7,186)	7,078 (6,829)	*-5 [113]*
Prior Quarter 2 Earnings	7,132 (6,573)	7,310 (9,224)	*-178 [111]*
Prior Quarter 3 Earnings	7,398 (7,362)	7,445 (7,256)	*-47 [116]*
Prior Quarter 4 Earnings	7,585 (7,008)	7,488 (8,312)	*97 [114]*

Note: The treatment and control columns report the sample means for dichotomous measures and the sample means with standard deviations in parentheses for non-dichotomous measures. The difference column reports the treatment-control differences in means with standard errors in brackets.

Source: Nevada UI claims data (individual characteristics); Nevada UI wage records (prior earnings).

Table 2: Treatment and Control Group Benefit Entitlements

	Treatment	Control	*Difference*
Regular UI Weeks Eligibility	22.8 (4.6)	22.9 (4.5)	*-.1 [.07]*
Regular UI Cumulative Entitlement ($)	7,075 (3,046)	7,056 (3,033)	*19 [48]*
EUC Weeks Eligibility	46.3 (9.4)	46.6 (9.2)	*-.3 [.2]*
EUC Cumulative Entitlement ($)	14,271 (6,084)	14,238 (6,057)	*33 [96]*
EB Weeks Eligibility	17.6 (3.5)	17.7 (3.5)	*-.1 [.1]*
EB Cumulative Entitlement ($)	5,410 (2,296)	5,396 (2,284)	*13 [36]*
Total Weeks Eligibility	86.7 (17.6)	87.2 (17.3)	*-.5 [.3]*
Total Cumulative Entitlement ($)	26,756 (11,603)	26,690 (11,556)	*66 [183]*

Note: The treatment group and control group columns report the sample means with standard deviations in parentheses. The difference column reports the treatment-control differences in means with standard errors in brackets.

Source: Nevada UI claims data.

Table 3: Unemployment Insurance Receipt Outcomes

	Treatment	Control
Exhausted Regular UI Benefits	.604	.710
Collected EUC Benefits	.500	.599
Exhausted EUC Benefits	.162	.191
Weeks Collected		
Regular UI	17.1 (8.5)	19.0 (7.9)
EUC	14.9 (18.9)	17.5 (19.2)
Total	32.0 (24.4)	36.5 (23.6)
Benefits Amounts Collected ($)		
Regular UI	5,352 (3,498)	5,863 (3,416)
EUC	4.621 (5,258)	5,258 (6,274)
Total	9,973 (8.749)	11,119 (8,535)

Note: The treatment and control columns report sample means for dichotomous outcomes and the sample means with standard deviations in parentheses for non-dichotomous measures.

Source: Nevada UI claims data.

Table 4: Employment and Earnings Outcomes

	Treatment	Control
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 Earnings, Quarters 1-6	17,122 (22,570)	14,453 (21,065)

Note: The treatment and control columns report the sample means for dichotomous outcomes and the sample means with standard deviations in parentheses for non-dichotomous measures.

Source: Nevada UI claims data.

Table 5: Meeting Scheduling and Completion Rates

	Meetings Scheduled [proportion of treatment cases]	Completers (proportion of meetings scheduled)
Week	4,673 [1.000]	3,717 (80%)
1	--	--
2	993 [.212]	796 (80%)
3	1,563 [.334]	1,227 (79%)
4	997 [.213]	773 (78%)
5	602 [.129]	482 (80%)
6	291 [.062]	242 (83%)
7	107 [.023]	95 (89%)
8+	120 [.026]	102 (85%)

Note: The left column reports the distribution of meetings scheduled by week and the right column reports the number and proportion of treatment cases that completed the meeting, as scheduled, in the specified week.

Source: Nevada employment service data.

Table 6: Service Take-Up Rates, Treatment vs. Control Group

	Treatment	Control	*Difference*
Any Job-Counseling Service	.684	.097	*.587 [.005]***
Work Search Plan	.555	.061	*.494 [.005]***
Resume Assistance	.257	.025	*.231 [.004]***
Individual Needs Assessment	.326	.039	*.286 [.040]***
Job Referral	.214	.042	*.172 [.004]***
Number of Job Referrals	.31 (.87)	.11 (.68)	*.21 [.01]***
Group Orientation	.315	.035	*.279 [.004]***
Employment Workshop	.138	.015	*.122 [.003]***

Note: Job-counseling services include: work search plan, resume assistance, individual needs assessment, and job referrals. They do not include group orientations, employment workshops, or the eligibility review.

** = treatment-control difference is statistically significant at the 1 percent level.

Source: Nevada employment service data.

Table 7: Average Treatment Effects, UI Receipt

	Average Treatment Effect	Percentage of Control Group Mean
Exhausted Regular UI Benefits	-.104 (.007)**	-15
Collected EUC Benefits	-.097 (.008)**	-16
Exhausted EUC Benefits	-.028 (.006)**	-15
Weeks on UI		
Regular	-1.9 (.1)**	-10
EUC	-2.5 (.3)**	-14
Regular + EUC	-4.4 (.4)**	-12
UI Benefits Collected ($)		
Regular	-520 (38)**	-9
EUC	-625 (96)**	-12
Regular + EUC	-1,145 (120)**	-10

Note: The left column reports average treatment effects, with standard errors in parentheses; the right column reports each average treatment effect as a percentage of the control group mean.

**= statistically significant at the 1 percent level.

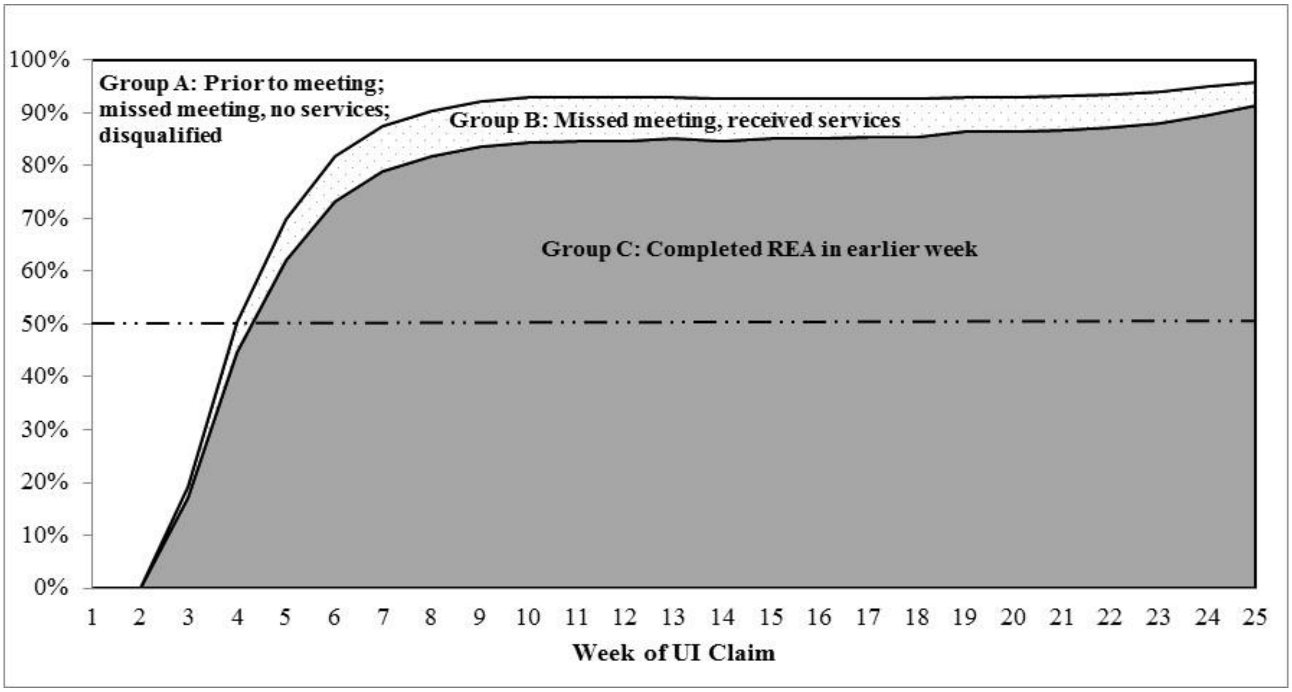
Table 8: Average Treatment Effects, Employment and Earnings

	Average Treatment Effect	Percentage 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 Earnings, Quarters 1-6	2,607 (322)**	18

Note: The left column reports the average treatment effects with standard errors in parentheses; the right column reports each average treatment effect as percentage of the control group mean.

**= statistically significant at the 1 percent level.

Figure 1: Treatment Cases, Classified by Meeting and Services Receipt



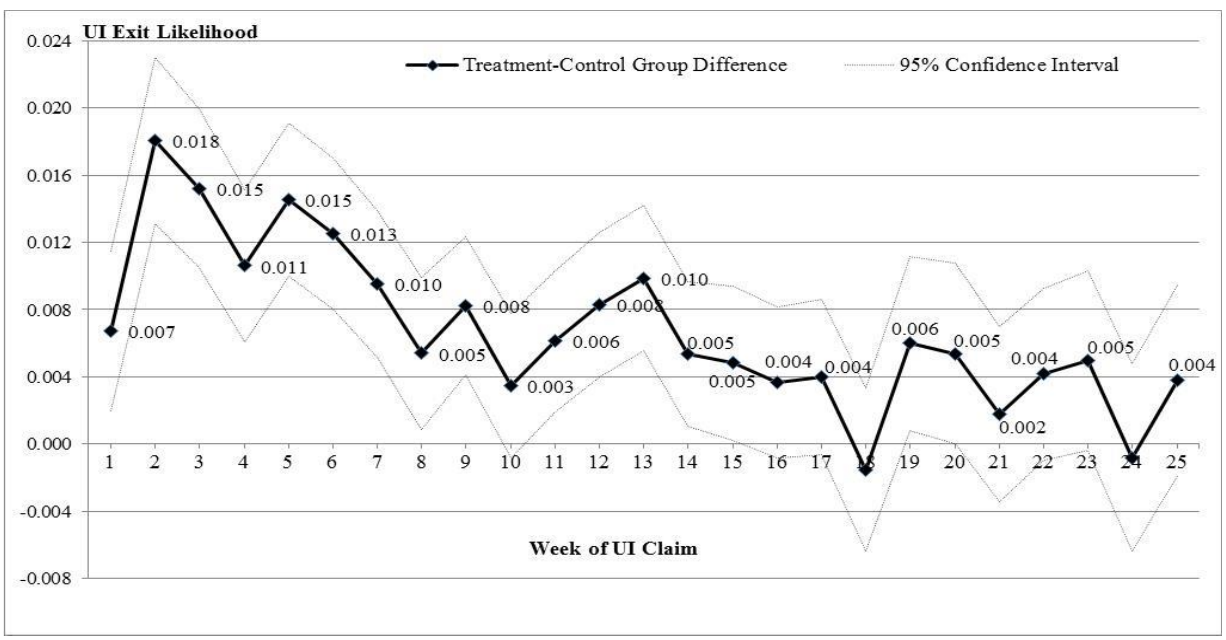
Note: Proportion of at-risk treatment cases in each week for Groups A, B and C.

Group A = treatment cases scheduled for the meeting in current or subsequent week; missed their meeting and did not receive services; or were disqualified for failure to attend meeting or for findings of ineligibility.

Group B = treatment cases that missed scheduled meeting but received services.

Group C = treatment cases that attended the scheduled meeting and passed the eligibility review.

Figure 2: Regression-Adjusted Treatment-Control Group Difference in UI Exit Likelihood



Note: Black line presents regression-adjusted treatment-control differences in UI exit likelihood. The grey dotted lines encompass the 95 percent confidence interval. Estimates based on equation [3].

APPENDICES

Appendix A: Timing of Services, Treatment Group

	Received Service	Individuals with Service Dates	Date of Service Available		
			Prior to Week of Meeting	Week of Meeting	After Week of Meeting
Any Job-Counseling Service*	3,195	1,296	346 (27%)	1,037 (80%)	307 (24%)
Work Search Plan**	2,593	1,085	189 (17%)	824 (76%)	72 (7%)
Resume Assistance**	1,200	504	86 (17%)	374 (74%)	44 (9%)
Individual Needs Assessment**	1,522	634	78 (12%)	456 (72%)	100 (16%)
Job Referral*	1,002	424	74 (17%)	224 (53%)	200 (47%)
Group Orientation**	1,470	607	144 (24%)	393 (65%)	70 (12%)
Employment Workshop**	644	270	57 (21%)	144 (53%)	69 (26%)

Note: The first data column reports the number of treatment cases that received services and the second data column the number of treatment cases that received services for which service dates are available. The three right columns identify for the latter group whether participants received services before, during, or after the week when the meeting was scheduled. Job-counseling services include: work search plan, resume assistance, individual needs assessment, and job referrals. They do not include group orientations, employment workshops, or the eligibility review.

*= Individuals may have received more than one class of service and thus before/at/after meeting proportions may add to more than 100 percent. **= Only last date of service is observed and thus before/at/after meeting proportions add to 100 percent.

Source: Nevada employment service data.

Appendix B: Program Disqualifications

Week of UI Spell	Population At-Risk for Exit [sample proportion]	Disqualifications (proportion of at-risk population)		
		No Shows	Ineligibles	Total
1	4,673 [1.000]	0 (.000)	0 (.000)	0 (.000)
2	4,534 [.970]	5 (.001)	3 (.001)	8 (.002)
3	4,350 [.931]	10 (.002)	10 (.002)	20 (.004)
4	4,197 [.898]	9 (.002)	11 (.003)	20 (.005)
5	4,077 [.872]	6 (.001)	11 (.003)	17 (.004)
6	3,948 [.845]	2 (.001)	8 (.002)	10 (.003)
7	3,834 [.820]	1 (.000)	7 (.002)	8 (.002)
8	3,738 [.800]	1 (.000)	2 (.001)	3 (.001)
Total		34	52	86

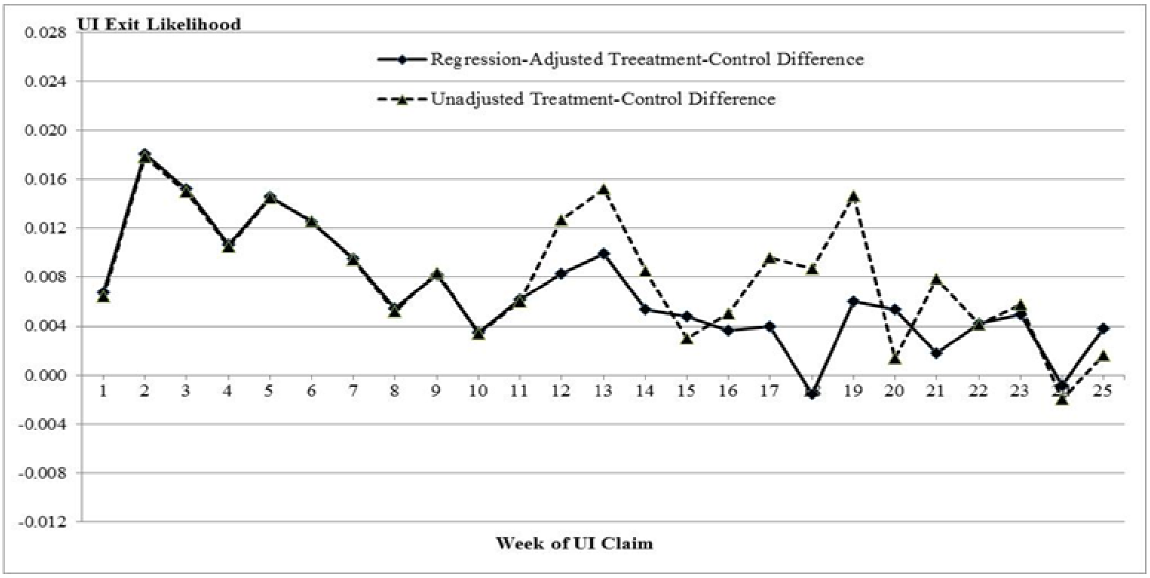
Note: The first data column reports the number of treatment cases at risk of exiting in each week with sample proportion in brackets.

No shows = number of treatment cases disqualified for failure to show up or reschedule the meeting with the proportion of at-risk population in parenthesis.

Ineligibles = number of at-risk treatment cases who were disqualified for findings of ineligibility during the meeting with the proportion of at-risk population in parenthesis.

Source: Nevada employment service data.

Appendix C: Treatment-Control Group Difference in UI Exit Likelihood, Regression Adjusted vs. Unadjusted



Regression-adjusted results, which are reported in the main body of the paper, are very close to the simple differences through week 11. The discrepancy starting in week 12 is caused by treatment-control differences in the distribution of UI weeks of eligibility (Nevada UI provides 12 to 26 weeks of authorized coverage, depending on and individual's prior work history). The differences in the distribution of UI weeks of eligibility are not due to selection (they are observed for the treatment and control groups in week 1) and are not generally statistically significant.

Appendix D: Selection Bias

Although it is not possible to estimate the selection bias for exit probability estimates in a given week due to program impacts earlier in the spell, we may identify the bounds of such bias. The approach we take is a special case of that specified in Horowitz and Mansky (2000). Denote the observed probability in week *t*>1 that a program and control case exits, conditional on the case not having exited prior to *t*, as H_t^p and H_t^c, respectively. Our measure of the program effect may be written as $H_t^p - H_t^c$. Our concern derives from the possibility that those who participated in the program may be differentially selected, since the program induces exit in prior weeks. Let us define H_t^{p*} as the probability that a program participate would exit in week *t* in the case where the program has no effect on exits in that week, but accounting for the possibility that this measure will be affected by selection prior to week *t*. The true program effect is then $H_t^p - H_t^{p*}$, the difference between the observed exit probability for program participants in week *t* and that which would be observed in the absence of any program effect. The estimated program effect may be written as the sum of the true program effect and a bias term:

[A1] $$H_t^p - H_t^c = (H_t^p - H_t^{p*}) + (H_t^{p*} - H_t^c)$$

The bias term $(H_t^{p*} - H_t^c)$ indicates the effect of selection on observed exit for program participants in week *t*.

In order to identify the extent of the bias, it is necessary to consider how many participants exit due to the program prior to week *t* and what their exit probability would have been in week *t* if they had not exited. Denote the proportion of cases that have not exited from the control and treatment groups prior to *t* as S_t^c and S_t^p, respectively (these are the proportions of the original groups in the risk sets in week *t*). As noted above, treatment cases are induced to exit the program at a higher rate prior to week *t*, that is, $S_t^c > S_t^p$. We assume that treatment cases that are in the risk set in week *t* would have been in the risk set if they had been assigned to the control. In contrast, some individuals assigned to the control group who are in the risk set at time *t* would have exited in a prior week if they had been in the treatment. Denote the UI exit likelihood of this group in week *t* as H_t^{c-p}. If there is no treatment effect in week *t*, the exit probability for the control group is just the weighted average of the exit probability for the treated group in the risk set in week *t* and the exit hazard for those who exited previously due to the treatment:

[A2] $$H_t^c = \left(\frac{S_t^c - S_t^p}{S_t^c}\right) H_t^{c-p} + \left(\frac{S_t^p}{S_t^c}\right) H_t^{p*}$$

Solving this expression for H_t^{p*} and subtracting H_t^c, we can write the bias expression as:

[A3] $$H_t^{p*} - H_t^c = \left(\frac{S_t^c}{S_t^p} - 1\right)(H_t^c - H_t^{c-p})$$

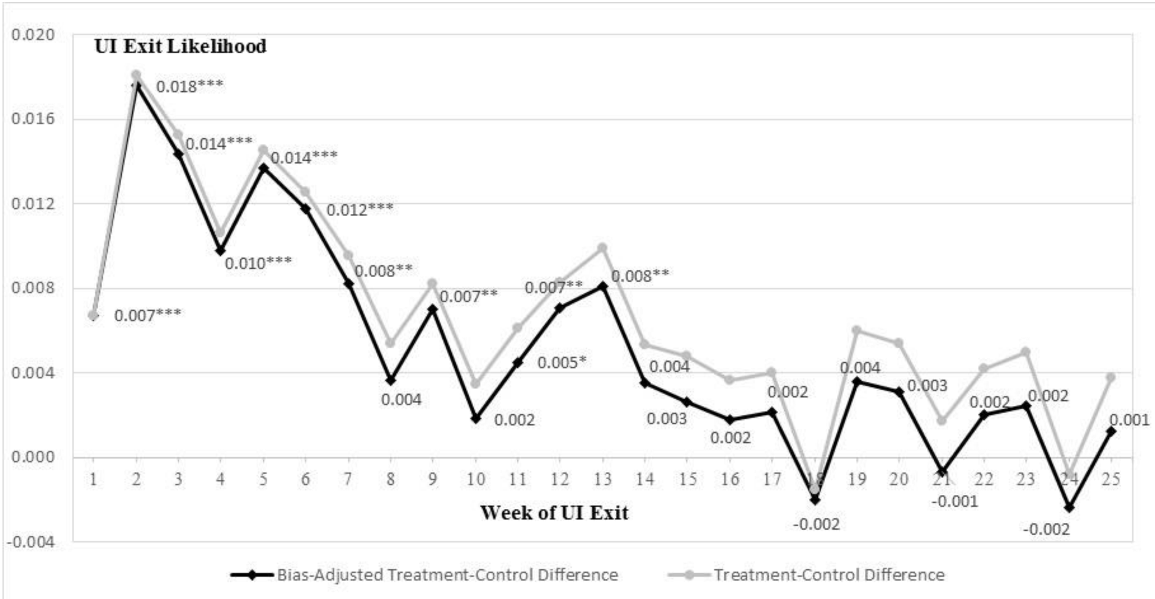
The bias is increasing with the proportion of cases that exited due to the program in prior weeks. If those in the control group who would be omitted by the treatment prior to *t* are more likely than others to exit at time *t* ($H_t^{c-p} > H_t^c$), the treatment estimate is downwardly biased. This is the assumption implicit in most formal models. No bias occurs if the exit rate for omitted individuals is the same as those in the treatment risk set in week *t*. Our concern focuses on the case where the program culls out those who are less likely to exit later ($H_t^{c-p} < H_t^c$), inducing a positive bias. To determine whether such bias could be large enough that observed estimates are wholly spurious, we make the extreme assumption that those who exited in prior weeks because of the program would have had no chance of exiting in week *t*, that is, we assume that $H_t^{c-p} = 0$.

Based on [A3], we find that this measure of potential bias is modest in each week 2-6 relative to estimated impacts, ranging from .0001 to .0011, and accounting for less than 12 percent of the estimated effect in any week. In weeks 7-11, the potential bias in a given week ranges from .0013 to .0016, whereas the average treatment effect is over .006. The measure of the maximum bias in each week from weeks 12-25 is between .0017 and .0030, in all cases less than the average effect of .0040.[25] However, selection effects of this size after week 6 would seem to be implausible, because they are based on the assumption that all those induced to exit by the program up to the prior week would continue to receive UI if they had not exited previously. On the contrary, those who respond to service effects in a given week would presumably be likely to exit even if they had not exited. More plausible would be the assumption that those induced to exit by the program in weeks 1-6, when threat effects were of primary importance, would not exit in later weeks if they had continued to receive UI. Assuming that later program selection induces no bias, it is straightforward to estimate the bias in subsequent weeks. In fact, we find that our estimates of the maximum bias are less than .0011 in each of weeks 7 to 25.

Finally, the graph below presents the bias-adjusted effects on the UI exit likelihood (black solid line), reporting the actual value and statistical significance of the estimates using bootstrap methods. As seen, bias-adjusted effects are slightly lower than the unadjusted effects (grey solid line), with 11 of the 13 estimates in weeks 1-13 statistically significant at the 10 percent level or better. Bias-adjusted estimates in weeks 7-17 were positive, with statistically significant effects obtained in weeks 7, 9, 11, 12, and 13. This illustrates that – even under extreme assumptions about the potential size of the bias – the positive program effects on UI exit after program activities ended were substantively important.

[25] Note that, beginning in week 12, cases that exit include those eligible for exactly that number of weeks. After week 11, we adjust the bias estimate in each week to omit cases that are not eligible for additional weeks of UI. Note, our estimates of program effect also control for weeks of eligibility, so this adjustment is implicit in our reported estimates.

Bias-Adjusted Treatment-Control Group Difference in UI Exit Likelihood



Note: Black line presents the bias-adjusted treatment-control differences in UI exit likelihood, based on equation [A2]. Reported is the estimated bias-adjusted effect, with ***, **, * denoting statistical significance at the 1, 5, and 10 percent level using bootstrap standard errors (500 replications). Grey line presents the treatment-control differences with no bias adjustment.

APPENDIX REFERENCES

Horowitz, J. L. and Manski C. F. (2000). Nonparametric Analysis of Randomized Experiments with Missing Covariate and Outcome Data, *Journal of the American Statistical Association*, 95(449), 77-84.