University of Cyprus
Department of Economics

Working Paper 08-2015

The Labor Market Effects of U.S. Reemployment Policy: Lessons from an Analysis of Four Programs during the Great Recession

Marios Michaelides and Peter Mueser

Department of Economics, University of Cyprus, P.O. Box 20537, 1678 Nicosia, Cyprus
Tel.: +357-22893700, Fax: +357-22895028, Web site: http://www.ucy.ac.cy/econ/en

The Labor Market Effects of U.S. Reemployment Policy: Lessons from an Analysis of Four Programs during the Great Recession†

MARIOS MICHAELIDES (UNIVERSITY OF CYPRUS)

PETER MUESER (UNIVERSITY OF MISSOURI)

NOVEMBER 2017

ABSTRACT

We present experimental evidence on four U.S. reemployment programs targeting Unemployment Insurance (UI) recipients during the Great Recession. All programs reduced UI spells, produced UI savings that exceeded program costs, and increased employment rates. The services-referral program had the smallest effects, occurring because of voluntary participant exit from UI to avoid requirements. The two programs that reviewed participants' UI eligibility produced higher effects because they induced voluntary exits and disqualified participants not engaged in active job search. The program requiring participation in both the eligibility review and job-counseling services was the most effective, indicating that services improved participants' job-search efforts.

JEL Classifications: J6, H4.

Keywords: Great Recession, job counseling, reemployment assistance, active labor market policies, eligibility review, unemployment, Unemployment Insurance, program evaluation.

† This paper was based on data collected by IMPAQ International, LLC (IMPAQ) as part of a study funded by the U.S. Department of Labor, Employment and Training Administration (DOL/ETA). The authors are grateful to the European Research Council (ERC) for financial support. The views expressed in this paper are those of the authors and should not be attributed to DOL/ETA, IMPAQ, or ERC. Author contact information: Marios Michaelides (mariosm@ucy.ac.cy); Peter Mueser (mueserp@missouri.edu).

Introduction

Over the course of the past 25 years, policymakers in the United States have made substantial investments in programs that provide job-search assistance services to unemployed workers who collect Unemployment Insurance (UI) benefits. Programs have typically referred UI recipients to public employment offices in the early stages of their UI spells to learn about and receive services designed to help them improve their job-search skills and connect to suitable jobs (Klepinger *et al.*, 2002; Black *et al.*, 2003; Wandner, 2010). Reemployment programs in Europe – often referred to as activation programs – have included more intensive requirements, mandating regular participation in job-search services and monitoring activities (Kahn, 2012; OECD, 2013). The effects of these programs have been studied extensively. Experimental studies of U.S. programs show that they are effective in reducing average UI spells and benefits collected, but have limited effects on employment and earnings (Meyer, 1995; Decker *et al.*, 2000; Klepinger *et al.*, 2002; Black *et al.*, 2003). Numerous experimental studies of European activation programs – including in the Netherlands (Gorter and Kalb, 1996), Denmark (Graversen and van Ours, 2008; Maibom *et al.*, 2017), France (Behaghel *et al.*, 2012), and Sweden (Hägglund, 2011) – find that these programs often lead to improvements in job-finding rates.

After the start of the Great Recession, U.S. policymakers focused on new strategies to facilitate the reemployment of UI recipients and reduce the burden they imposed on the UI program. In addition to augmenting funding for existing programs, which referred selected UI recipients to job-search services, new funding was established to implement programs with in-person eligibility reviews to identify and disqualify those who were not engaged in active job search or were otherwise ineligible for benefits. However, very little is known about whether the services referrals and eligibility review programs implemented during the recession were actually effective.

The most recent experimental studies focused on U.S. programs implemented in the mid-1990s, a period characterized by low unemployment, and did not test many of the types of interventions in place during the recession. Similarly, most studies of European programs tested interventions that operated in strong labor markets and had more intensive requirements than the U.S. programs operating during the recession.

This paper presents experimental evidence on the effects of four reemployment programs, which correspond to a wide range of interventions supported by the U.S. government since the start of the Great Recession. The first program is Florida’s PREP (Priority Reemployment Planning), which asked participants to attend an orientation meeting at a public employment office to receive information about and get referrals to job-search services. The second program is Florida’s REA (Reemployment and Eligibility Assessment), which conducted in-person eligibility reviews, in which participants provided information on their job-search activities and employer contacts. The third program is Idaho’s REA, which required participants to provide information on their job-search activities and employer contacts using an online questionnaire. The program then followed up with a subset of participants, contacting employers to verify job-search actions and requiring some participants to attend in-person reviews. The fourth program is Nevada’s REA/RES (REA/reemployment services), which required participants to meet with program staff at the start of their UI spells to undergo an eligibility review *and* receive mandatory job-counseling services. Those services included a skills assessment to identify an appropriate job-search plan, assistance in developing a resume and other job-application materials, and referrals to job vacancies. In all four programs, requirements were scheduled in the first few weeks of participants’ UI spells, with no further requirements to meet with program staff, receive additional services, or undergo eligibility reviews.

Our analyses of program effects rely on state administrative data providing information on all UI recipients in the three states who started collecting benefits in the period August-November 2009 – during the Great Recession – and were subject to random assignment for participation in the reemployment programs. These data are used to measure, for both program and control cases, several measures of benefit receipt, including the number of UI weekly benefit payments received and the dollar value of benefits collected under the UI claim, and quarterly employment rates and earnings in the four quarters following assignment into the program. To estimate program effects, we use regression models that estimate program-control differences in UI receipt, employment rates, and earnings, controlling for individual characteristics and random assignment criteria.

We then undertake analyses to examine the underlying program mechanisms that may have affected participant job-search behavior and outcomes. First, these programs may have produced *threat effects*, occurring because some participants exited UI prior to scheduled program activities to avoid the anticipated cost of program participation. Second, the eligibility reviews required by three of the programs led to the disqualification of participants who did not complete the review or were deemed ineligible for benefits based on the review, what we term *monitoring effects*. Third, the four programs may have created *services effects* by providing services or motivating participants to obtain services that helped them develop more effective job-search strategies. For the three programs that disqualified participants, we observe disqualifications, providing a proxy for monitoring effects at each week of the UI spell. However, there is no direct way of measuring the importance of threat and services effects.

To identify the importance of threat and services effects, we estimate the overall program effects on the UI exit probability in each week, and compare these effects with our measure of monitoring effects. Our analyses rely on the plausible assumption that threat effects likely occurred after participants were notified of program requirements but prior to actual participation, while services effects likely occurred after participants had met program obligations. Based on this assumption and the time pattern of program effects, we assess the extent to which the results of each program can be attributed to threat, monitoring, and services effects. We also consider whether our results may be affected by dynamic selection, occurring because program requirements may have pushed some participants to exit early in their UI spells.

The remainder of this paper is organized as follows. Section 1 provides an overview of U.S. reemployment policy in the past three decades and changes occurring as a response to the Great Recession, followed by a discussion of existing evidence on the effectiveness of reemployment programs. Section 2 discusses the characteristics of the four programs and the design of the evaluation studies. Section 3 presents our data sources and Section 4 describes our methodology. Section 5 presents the analyses results and Section 6 summarizes our findings and conclusions.

1. Background

1.1 U.S. Reemployment Policy and the Great Recession

Before the Great Recession, the primary programs for supporting worker reemployment were the Worker Profiling and Reemployment Services (WPRS) program and the Reemployment and Eligibility Assessment (REA) program. Both programs targeted new UI recipients who were designated as "services-eligible," omitting those who were reported to be on temporary layoff, attached to a union hiring hall, or active in employment or training programs. WPRS was created in the early 1990s to address concerns that UI recipients were not participating in job-search services offered at public employment offices. WPRS required services-eligible UI recipients who were deemed most likely to exhaust regular benefits to: (1) register in the state's labor exchange system to match them to available employment opportunities; (2) attend workshops to learn basic job-search skills; and (3) receive individualized job-counseling services (see Dickinson *et al.*, 1999). WPRS became operational in all states in 1996 (Wandner, 2010).

By the early 2000s, UI systems had become highly automated, enabling unemployed workers to use telephone and Internet systems to apply for UI, report their work-search activities, and access basic job-search services (O'Leary and Wandner, 2005; Ridley and Tracy, 2004). This automation reduced administrative costs but raised concerns about the ability of agencies to monitor UI recipients (Wandner, 2010). To address these concerns, the U.S. Department of Labor (DOL) established REA in 2005, which required services-eligible UI recipients to undergo an eligibility review. REA's objective was to reduce UI fraud by disqualifying recipients who, during the meeting, were deemed ineligible for benefits (Benus *et al.*, 2008; Poe-Yamagata *et al.*, 2012). Nine states – including Florida, Idaho, and Nevada – adopted the program at that time (U.S. Department of Labor, 2005).

At the end of 2007, the U.S. economy entered the Great Recession, a period when the national unemployment rate climbed from 5 percent in December 2007 to a peak of about 10 percent at the end of 2009. From 2007 to 2009, new UI claims increased from 7.7 to 13.9 million, and registrations in labor exchange systems increased from 17.8 to 22.4 million (Wandner and Eberts, 2014). To alleviate the added burden brought on by the recession, the American Recovery and Reinvestment Act of 2009 allocated $400 million – in addition to the annual $724 million allocation under the Wagner-Peyser Act for 2009 and 2010 – to support job-search services and expand WPRS. Partly as a result, WPRS referrals increased from 1.3 million in 2008 to 1.9 million in 2009 and to 2.1 million in 2010. The federal government also provided $76 million to DOL to support the expansion of REA to 33 states and encourage states to offer job-counseling services to participants who passed the eligibility review (U.S. Department of Labor, 2009; 2010).

1.2 Prior Research

In the 1980s, DOL implemented various experimental-design demonstration programs to test the effectiveness of programs that required UI recipients to engage in job-search services, including use of job banks, employment workshops, and job counseling. Most of these programs were found to reduce UI receipt, although they yielded small or no effects on participants' reemployment rates and earnings (Anderson *et al.*, 1991; Johnson and Klepinger, 1991; Meyer, 1995). These early studies made little attempt to assess the underlying mechanisms that led to program effects on UI receipt and why these effects did not translate into substantive improvements in job-finding rates and earnings.

Several experimental studies published in the early 2000s provided evidence on the source of program effects. Black *et al.* (2003) showed that the Kentucky WPRS program in the period October 1994 through June 1996 reduced UI receipt by 2.2 weeks and benefit amounts collected by $143. The study found that these effects were primarily because the program caused some participants to discontinue benefits following receipt of the letter informing them of the required meeting but prior to the meeting itself. They argued that most of the program's impact was due to the threat effects of program requirements and that services played a limited role.

Two subsequent experimental-design studies found similar results. Decker *et al.* (2000) showed that two demonstration programs implemented by DOL in Florida and Washington, DC in the mid-1990s, which referred UI recipients to job-search services, reduced UI duration by up to 1.1 weeks and UI benefits collected by up to $182. Klepinger *et al.* (2002) used data from a demonstration program implemented in Maryland in 1994 to show that requiring UI recipients to participate in an employment workshop reduced UI duration by 0.9 weeks and benefits collected by $75. In both cases, almost all of the programs' effects occurred in the early stages of participants' UI spells, around the time they were notified of program requirements and prior to engaging in program activities.

Although these studies provide valuable guidance on the effects of reemployment programs, we cannot rely on their results to infer the efficacy of the types of interventions implemented during the Great Recession. First, the programs studied by this work operated in the mid-1990s, more than a decade prior to the Great Recession, a period when the national unemployment rate was in the 5-6 percent range and unemployed workers were much less likely to exhaust their UI eligibility (extending up to 26 weeks). Second, none of those programs focused on formal eligibility reviews, where UI recipients were required to meet with UI agency staff and provide proof of their job-search activities as a condition for continued benefit eligibility. Thus, there is no evidence on the potential monitoring effects stemming from the eligibility review programs operating in 33 states during the recession, including the Florida and Idaho programs examined here.

Third, those who believe that services are of potential value have pointed out that existing U.S. studies examined programs with relatively weak service components. Although many participants in the programs studied by Decker *et al.* (2000), Klepinger *et al.* (2002), and Black *et al.* (2003) received basic services (such as registration in job banks, group orientations, and employment workshops), few participants received personalized job-counseling services. The latter would include one-on-one meetings with program staff, where a participant receive a skills assessment, assistance in developing a work-search plan, assistance in developing a resume and job-application materials, and direct referrals to jobs. Thus, existing studies cannot be used to make inferences about the effects of job-counseling services, nor can they provide credible evidence on programs similar to the Nevada program examined here, which included both the eligibility review and mandated participation in job counseling.[1]

A substantial number of random assignment studies, many completed over the last decade, have evaluated programs implemented in European countries, including Denmark (Graversen and van Ours, 2008), France (Behaghel *et al.*, 2012), Germany (Krug and Stephan, 2013), the Netherlands (Gorter and Kalb, 1996), Sweden (Hägglund, 2011), and the United Kingdom (Dolton and O'Neill, 2002). These studies – which mostly concerned programs implemented in periods of low unemployment – often found that programs were responsible for large effects on unemployment exits and reemployment rates.

For the most part, European programs are more intensive than U.S. programs (including those examined here), requiring continuous participation in monitoring activities and job counseling, so the incentives of participants to escape program requirements and monitoring effects cannot be distinguished from the benefits of job counseling. One partial exception is Maibom *et al.* (2017), who show that an intensive counseling treatment in Denmark for newly unemployed workers had effects that continued even after program requirements had been satisfied. Another partial exception is Cockx *et al.* (2017), which found that monitoring activities in the Belgian UI system had small positive effects on participants' job-search behavior. Importantly, none of the European studies determined if programs had positive impacts on earnings, a key indication of whether the job counseling mandated by these programs led to improved labor market outcomes.

[1] There is evidence based on U.S. data that job-counseling services may help disadvantaged workers to improve their labor market success, but the results concern individuals receiving training (Perez-Johnson *et al.*, 2011) or disability benefits (Weathers and Bailey, 2014).

2. Program Description

The four programs examined here were selected for two reasons. First, they used random assignment procedures to allocate services-eligible UI recipients to either the program group (subject to program requirements) or the control group (not subject to program requirements). This ensures that program effects can be estimated through program-control comparisons in observed outcomes after controlling for the random assignment procedures. Second, these programs adopted different approaches, representing a wide range of programs that have been in operation in the U.S. since the start of the recession. This allows us to examine the effects of programs that have not been previously tested in the literature and provide an assessment of U.S. reemployment policy during the recession. Below is a detailed description of the four programs. Figure 1 presents a summary of program designs.

2.1 The Florida PREP and REA Programs

Florida implemented both the WPRS and REA programs during the recession. Each week, the UI agency uploaded the pool of new UI recipients who were eligible for program participation to an interface accessible by regional workforce boards (RWBs). Each RWB randomly assigned services-eligible recipients to one of three groups: WPRS, called PREP (Priority Re-employment Planning), REA, and the control group.[2] PREP participants were referred to a public employment office to participate in a group orientation where they received information and referrals to job-search services. REA participants were required to undergo an in-person eligibility review at a

[2] The proportion assigned to PREP and REA varied based on each RWB's capacity to serve participants in a given week. Our analyses consider service-eligible UI recipients in the 10 RWBs that assigned substantial numbers of eligible UI recipients to both WPRS and REA during the study period. Tabulations of the 2009 American Community Survey (ACS) show that these 10 RWBs covered 60 percent of unemployed workers in the state.

public employment office. PREP and REA participants were informed of program requirements through a notification letter sent in week 2 of their UI spell (i.e., when they collected their second UI weekly payment). The letter specified the date and time of the PREP orientation or REA review meeting, typically scheduled in weeks 4-6 of the UI spell. Control cases received no notifications and had no requirements under PREP or REA but were subject to the usual UI job-search requirements.

REA participants who were deemed ineligible based on the review were disqualified from collecting UI. Those who did not show up for and failed to reschedule the review within three weeks of the scheduled meeting were also disqualified, unless the employment service data system indicated that they had participated in job-search services or were enrolled in training. In contrast, PREP participation was not strictly enforced and benefits were not cut off for those who failed to attend or reschedule the orientation. After the PREP or REA meeting, participants were informed that they had no additional requirements under the program. However, both program and control cases could participate in services on their own initiative.

The design of Florida PREP is similar to that of most of the 50 WPRS state programs in effect during the recession, where participants were required to visit public employment offices to learn about and receive referrals to services. Unlike Florida PREP and other U.S. programs studied to date, Florida REA did not include referrals to orientation meetings or to specific job-search services. Instead, it focused exclusively on conducting eligibility reviews and disqualifying participants who were deemed ineligible during the reviews. Although this structure is similar to the structure used by the majority of the 33 states that implemented REA, it is notably different than the structure of most European programs, which mandate continuous participation in monitoring and job-counseling activities.

2.2 The Idaho REA Program

Idaho maintained both WPRS and REA programs during the study period, but WPRS was very small, serving only about 2 percent of services-eligible UI recipients.[3] The remaining services-eligible recipients were randomly assigned to the REA program or the control group. Control group cases received no targeted communications beyond those associated with normal UI receipt. Those assigned to the REA program were sent a notification letter in week 1 of their UI spell (when they collected their first UI payment) asking them to complete an online review on the IdahoWorks website by week 4, providing information on their work search activities and employer contacts. In week 5, participants who were still receiving UI but had not completed the online review and those deemed ineligible based on their responses were disqualified from collecting UI.[4]

The Idaho UI agency then selected about 5 percent of those who completed the online review for telephone verification of their employer contacts and about 20 percent for an in-person review. The remaining 75 percent had no further contact with the program. Participants who were selected for the telephone verification were not informed of their selection, except in cases where the information obtained warranted disqualification from collecting UI benefits. Those selected for the in-person review were contacted by phone in week 5 to set up an appointment; the in-person reviews were typically scheduled in weeks 6-7. Those who did not show up for the in-person review and those who were deemed ineligible during the review were disqualified.

Those who attended and passed the review were not required to receive any services and were explicitly informed that they did not have any further requirements under the REA program, but

[3] Idaho assigned to WPRS services-eligible recipients who were deemed hard-to-employ based on whether they had low education, were previously employed in low-wage jobs, and had short prior job tenure.
[4] Similar to the Florida REA program, participants who did not complete the online review but who participated in job-search services or were enrolled in training, as shown in the employment service data system, were excused.

that they were still subject to the usual UI job-search requirements. The use of online tools to conduct eligibility distinguishes Idaho REA from the REA programs operating in other states, including Florida, which relied exclusively on in-person reviews. To our knowledge, Idaho REA is the first reemployment program in the U.S. or Europe which relied primarily on online tools and did not require most participants to have face-to-face interactions with program staff.

2.3 The Nevada REA/RES Program

Nevada adopted a different approach from other states, essentially combining WPRS and REA into one program that required participants to attend a meeting with program staff in the first few weeks of their UI spell to: (1) undergo the REA eligibility review and, if deemed eligible, (2) receive mandatory staff-assisted job-counseling services (called RES).[5] Each week, the Nevada UI agency randomly assigned program-eligible recipients into the REA/RES group or the control group. A notification letter was sent to each REA/RES participant in week 1 of the UI spell (the first week of UI payments) providing a specific date and time for the required meeting, typically in weeks 2-4. REA/RES participants who did not show up for the meeting were disqualified from collecting UI unless they rescheduled the meeting. Exempted from the meeting were those who had received job-search services, were active in training, or had discontinued UI benefit receipt. Participants deemed ineligible for benefits during the meeting because they were not actively searching for a job or for other reasons were disqualified from collecting UI.

Participants who passed the eligibility review were offered job-counseling services during the same meeting, including: (1) an individual skills assessment and development of a work-search

[5] REA/RES was implemented in the workforce regions covering the Las Vegas-Henderson-Paradise and Reno metropolitan areas. Tabulations of the 2009 ACS show that these regions covered 87 percent of unemployed workers in the state during the study period. Workforce regions in the rest of the state continued to operate the WPRS program.

plan targeting the types of jobs they should be pursuing based on their skills and experience; (2) assistance in developing a professional resume and other job-application materials; and (3) referrals to employers with job openings compatible with their skills and experience. Participants were offered job-counseling services based on individual needs, so not all participants received all services. Participants also received information about other available job-search services, including job banks, orientation meetings, and employment workshops. Following the meeting, REA/RES participants were informed that they had completed program requirements, and – although they were not required to receive additional services or attend follow-up meetings – they were expected to continue to actively search for a job. Those assigned to the control group did not receive any notifications from the REA/RES program and were not required to receive any services, although they were subject to the usual UI work-search requirements.

The design of the Nevada REA/RES program is a departure from most U.S. programs studied to date. Nevada was the only one of the 33 states adopting eligibility review programs since the start of the recession that combined the review with mandatory job-counseling services. Most U.S. programs studied prior to the recession did not include the eligibility review and did not require participation in individualized job-counseling services. Nevada REA/RES had similar requirements to many European activation programs, with the notable distinction that, while the European programs mandated monitoring and job-counseling activities throughout the participants' UI spells, Nevada required participants to engage in such activities only once, at the beginning of their UI spells, with no further requirements thereafter.

3. Data Description

We use state UI claims data and wage records on all workers who started collecting UI benefits in August-November 2009 and were subject to random assignment for participation in the four programs. During the study period, the three states had their highest unemployment rates in over 25 years – 11.1 percent in Florida, 8.7 percent in Idaho, and 12.0 percent in Nevada, compared with the 9.8 percent national unemployment rate.[6] The state rates exceeded the threshold for activating the Emergency Unemployment Compensation (EUC) and Extended Benefit (EB) programs. Thus, in accord with federal rules, recipients who exhausted regular UI benefits (10-26 weeks in Florida and Idaho, and 12-26 weeks in Nevada) and had a minimum of 20 weeks of employment in the year prior to the start of the UI spell were eligible to apply for up to 53 more weeks of benefits under EUC and, in addition, for up to 20 weeks of benefits under EB.

The UI claims data provide the socioeconomic characteristics of services-eligible recipients at the start of their UI claims and the duration and amounts of benefits they were eligible to receive on their claims. The data also report the number of benefit weeks and the dollar value of benefits collected under the regular UI and EUC programs; benefits collected under EB were not available because states had not yet developed a system to track EB payments.[7] Using UI claims data, we construct the following measures of UI receipt: (1) an indicator of whether the individual exhausted regular UI benefits; (2) an indicator of whether the individual started collecting EUC; (3) an indicator of whether the individual exhausted EUC benefits; (4) the number of regular UI and EUC weeks collected; and (5) the dollar value of regular UI and EUC benefits collected. These data do not provide information on employment transitions, so they cannot be used to measure unemployment and employment spells.

Wage records are maintained by states under the UI program and provide individual

[6] Source: Bureau of Labor Statistics, https://www.bls.gov/web/laus/ststdnsadata.txt.

[7] The implication is that we do not observe the entire UI spell for those who exhausted both regular UI and EUC, and applied for EB.

information on quarterly earnings received from employers in the state. Using wage records, we construct two measures of employment for each of the four quarters prior to entry and the four quarters following entry: (1) a dichotomous variable indicating whether earnings in a quarter are positive, and (2) the dollar value of total earnings in a quarter, with those without earnings included with values of zero. These data do not provide information on earnings from employers outside the state, informal or illegal employment, or federal and military jobs. Also, the data do not include employment length or hourly wages. Despite these limitations, wage records appear to provide valid measures of program impact and have been used widely in the program evaluation literature in lieu of survey data (Black *et al.*, 2003; Wallace and Haveman, 2007; Perez-Johnson *et al.*, 2011; Michaelides and Mueser, 2017). Appendix Table A1 presents sample means of UI receipt, employment, and earnings outcomes for program and control groups.

We also have access to program data that provide partial information on participants' program-related activities. The Florida data provide the exact date when the PREP and REA meetings were initially scheduled and whether the participant attended the meeting.[8] The Idaho REA data report which participants were selected for the phone verification and for the in-person interview, but do not report whether participants completed the online and in-person reviews. The Nevada REA/RES program data provide the date when the REA/RES meeting was scheduled and whether the participant attended the meeting.[9] The data for the three programs with the eligibility review report whether a participant was disqualified for failure to complete the review or because of issues identified during the review. The Nevada data also report the specific job-search services received

[8] REA participants could reschedule the meeting to occur within three weeks after the original date. The data do not report the actual week when the meeting took place, but a code in the data indicates if the meeting did not occur.
[9] In Nevada, the specified interview date includes any postponement, so, unless the individual is coded as missing the interview, the interview occurred on the date indicated.

by treatment and control cases, so we can explore whether the program increased services take-up. Unfortunately, we were not able to obtain job-search services data for the other three programs.

Table 1 summarizes the characteristics of program-eligible UI recipients in the three states. In Florida, there were 58,416 eligible recipients; 32 percent were assigned to PREP and 40 percent to REA, with 28 percent in the control group. In Idaho, 79 percent of eligible recipients were assigned to REA, whereas in Nevada, 16 percent of eligible recipients were assigned to REA/RES. There are some differences across states in the characteristics of eligible recipients. Blacks made up about a sixth of recipients in Florida but only 1 percent of those in Idaho. (Nevada does not have a reliable measure of race.) In Florida, a smaller proportion reported completing college than in the other states, but more individuals in Florida reported white collar jobs, reflecting the service focus of the economy. Prior earnings in Florida were slightly higher than prior earnings in Nevada and 30 percent higher than earnings in Idaho.

Using observed characteristics and prior labor market outcomes, we investigate whether random assignment was properly undertaken in each state. In Florida, random assignment occurred on a weekly basis within each region, at the level of the RWB, and the number of eligible recipients assigned in each program depended on the program capacity in each region at the time of assignment. To test Florida's random assignment process, we ran two linear probability models, predicting PREP and REA assignment with individual characteristics and measures of prior labor market outcomes, RWB and week indicators, and RWB-week interactions. In Idaho and Nevada, we ran similar regressions but, since random assignment occurred at the state level on a week-by-week basis, the inclusion of region indicators or region-week interactions was not necessary. Results (see Appendix Table A2) show that only three of 74 coefficients are statistically significant at the 10 percent level and none at a higher level, a count that could easily be due to chance. These results suggest that observed characteristics and prior labor market outcomes do not contribute to the program assignment prediction, consistent with the view that participants were randomly selected. Importantly, estimated parameters are very small in size, implying that any treatment-control imbalances in characteristics do not cause substantive differences in outcomes once week (and region by week effects in Florida) are controlled.

Table 2 provides information on the scheduling of program requirements and completion rates. The vast majority of PREP and REA meetings in Florida were scheduled in weeks 4-6. In Idaho, all participants were required to complete the online review by week 4 and about one-fifth were selected for an in-person interview; most in-person interviews were scheduled in weeks 6-7. In Nevada, about a fifth of REA/RES meetings were scheduled in week 2, with 95 percent of meetings scheduled by week 6.[10] Florida and Nevada data show that most participants completed the in-person eligibility reviews, and about 60 percent of Florida PREP participants attended the orientation. Completion data were not available for Idaho.

Program data also provide information on disqualifications occurring because treatment cases failed to complete the eligibility review or because of findings of ineligibility during the review. These data show that a small proportion of program cases were disqualified in the three programs with an eligibility review – 1.4 percent in Florida REA, 2.6 percent in Idaho REA, and 2.1 percent in Nevada REA/RES. The timing of disqualifications for each program, and whether the disqualifications were for failure to undergo the review or because of findings of ineligibility during the review, are provided in Appendix Tables A3-A5.

Nevada REA/RES data also provide information on the specific services received by program

[10] Recall that the Nevada meeting schedule includes postponements, whereas the other schedules do not.

and control cases. As seen in Appendix Table A6, about two-thirds of REA/RES participants received at least one job-counseling service (work search plan, resume assistance, or job referral), compared with only about 10 percent of control cases. REA/RES participants were also significantly more likely than control cases to receive each job-counseling service offered during the REA/RES meeting, including assistance in developing a work search plan (55 percent), resume development assistance (25 percent), and a direct job referral (21 percent). These figures show that participation in individualized job-counseling services was much higher in the Nevada REA/RES program than in most U.S. programs operating prior to the recession.[11] Moreover, Nevada REA/RES program cases were more likely than control cases to participate in orientations and employment workshops, although these services were not mandated by the program.

4. Methods

4.1 Effects on UI Receipt, Employment, and Earnings

Given that random assignment in Florida was within region and week, regression models that estimate program-control differences in outcomes controlling for region, week, and their interaction provide estimates of program effects. Random assignment in Idaho and Nevada occurred weekly at the state level, so estimates of program effects can be obtained using regression models that control for week. To improve precision, we obtain estimates from regression models including all relevant measured characteristics:

[11] For example, the programs examined by Black *et al.* (2003) and Klepinger *et al.* (2002) referred most participants to basic job-search services and did not require participation in individualized job-counseling. Black *et al.* (2003) report that less than half the participants received at least one basic job-search service, with very few participants receiving job-counseling. Klepinger *et al.* (2002) reports that only about 30 percent of those required to participate in a job-search workshop actually attended. Partial exceptions are the Florida and Washington, D.C. demonstration programs examined by Decker *et al.* (2000), where job-counseling services were required; however, fewer than 40 percent of participants received at least one individualized job-counseling service.

$$Y_i = \alpha T_i + X_i\beta + F_i\gamma + u_i \qquad [1]$$

The independent variable (Y_i) is the outcome for individual i. T_i is an indicator that equals 1 if the individual was assigned to the program and 0 otherwise. Control variables in vector X_i include a constant, individual characteristics (gender, race, education, and age), prior employment (occupation category and prior earnings), and UI eligibility (logarithm of weekly benefit entitlement and dummy variables indicating weeks of regular UI eligibility). F_i is a vector of fixed effects for the week the individual filed the UI claim and workforce region in which the individual was residing; the Florida models also include week-region interactions. u_i is a zero-mean disturbance term. Greek letters are parameters to be estimated. The model is estimated separately for each program, using all program and control cases; thus, parameter α estimates the program's average treatment effect on the outcome.

4.2 Program Mechanisms and Time Patterns of Effects

The workings of the four programs can be, in part, identified by examining the time patterns of effects, so as to identify the relative importance of: (1) *threat effects*, occurring because participants exited UI voluntarily to avoid program requirements; (2) *monitoring effects*, occurring because the program disqualified participants who were deemed ineligible during the review or because they did not complete the review; and (3) *services effects*, occurring because participants received services that helped them improve their job search. Any program effects occurring in the early stages of the UI spell after participants were informed of program requirements but before meeting those requirements are attributed to the threat of program requirements. Around the time of the eligibility review, we expect that both threat and monitoring effects would be relevant, reflecting continued concern with program requirements and findings of ineligibility. Finally, any impacts of services are likely to occur in the period after participants had satisfied program requirements and the interactions with the program had ended, as participants undertook job search. In addition, if the eligibility review induced concern about potential future monitoring during this period, such concerns might also increase search intensity.

The types and timing of these effects is expected to vary across programs. Florida PREP could have produced threat and services effects, but could not produce monitoring effects because the program did not evaluate eligibility and there were no disqualifications of no-shows. Florida REA would have produced threat effects initially, when meetings began to be scheduled, with monitoring effects becoming progressively more important as participants attended – or missed – meetings and faced potential disqualification. Similarly, Idaho REA is expected to produce threat effects, with monitoring effects growing more important as participants faced the requirement of an online review and a portion were chosen for telephone verification or in-person reviews. Services effects are less plausible for the Florida or Idaho REA programs because neither involved services components, but if they exist, they would occur after participants satisfied eligibility review requirements. Because the Nevada REA/RES program mandated both the eligibility review and job-counseling services, it could have produced a combination of all three effects.

To infer the underlying mechanisms that led to program effects, we examine program-control differences in the probability of exiting UI in each week, conditional on not exiting in a prior week. We use the following linear probability model:

$$H_{ti} = \delta_t T_i + X_i \varepsilon_t + F_i \zeta_t + v_{ti} \qquad [2]$$

The independent variable (H_{ti}) is the UI exit probability for individual *i* at week *t*, contingent on continuing to receive UI up through week *t*. Control variables T_i, X_i, and F_i are the same as in model [1] and v_{ti} is a zero-mean disturbance term. The model is estimated separately for each week using all program and control cases that were at risk of exiting in that week (i.e., excluding those that exited in prior weeks). This general model structure allows for the program effect on the UI exit probability (δ_t) to vary over time, which is consistent with the expectation that the timing of effects may depend on program design. The model also allows for the effects of individual characteristics on UI exit to change over time; this minimizes the possibility that dynamic selection based on observed heterogeneity influences the estimated treatment parameters.

Using the patterns of program effects over time based on model [2] allows us to provide evidence on the relative importance of threat, monitoring, and services effects. Recall that we observe the proportion of program cases that were disqualified each week for failure to complete the review or for findings of ineligibility (see Appendix Tables A3-A5). The disqualification rates provide proxies for the direct effects of monitoring in each week (see Appendix B1 for a formal discussion). Using disqualification rates to measure monitoring effects aids our efforts to identify the importance of threat and services effects. In practice, based on the assumption that voluntary exits would dominate before or during the period when participants faced program requirements, any differences between the program's estimated effect on UI exit and the proportion of disqualifications early in the UI spell provide measures of threat effects. We expect, however, that participants' exits later in the spell, after most participants had completed program requirements, would be largely due to the value of services. Our comparisons across programs further clarify the basis for these effects.

5. Results

5.1 Effects on UI Receipt, Employment, and Earnings

Table 3 presents estimates of the average treatment effects on UI receipt measures based on model 1. The same table presents the average treatment effect as a percentage of the control group mean (see Appendix Table A1 for treatment and control means). Estimates confirm that all four programs reduced UI benefit receipt, although there is a great deal of variation in effects. Florida PREP had the smallest effects, reducing exhaustion of regular UI benefits by 1.2 percentage points, a 2 percent decline relative to the control mean. Similarly, the program reduced the likelihood of EUC receipt by 1.1 percentage points (2 percent). These effects are three times as great for the Florida REA. Idaho REA effects are similar to the Florida REA effects, while Nevada REA/RES effects are greater still – the program reduced regular benefit exhaustion and the likelihood of EUC receipt by 9.2 percentage points each, corresponding to a 13 percent and 15 percent decline relative to the control mean, respectively. Similarly, participants in all four programs were less likely than control cases to exhaust their EUC entitlement, with statistically significant effects for Florida REA and Nevada REA/RES.

For other UI receipt measures, although differences between programs are less extreme, the patterns correspond closely to these. The total number of benefit weeks received (regular plus EUC) was reduced by about 2 percent in the Florida PREP program, by 4-5 percent in the Florida and Idaho REA programs, and by 10 percent in the Nevada REA/RES program. The program-induced percentage reduction in the total dollar value of payments for each program is very similar to the percentage reduction in weeks collected. Since we do not observe whether those who exhausted EUC benefits collected EB payments, it is likely that we underestimate effects on actual duration and total benefit amounts.[12]

[12] In the case of EUC, it is easy to show that the programs reduced receipt primarily by inducing participants to leave prior to exhausting regular benefits. Assuming this is true for EB (i.e., that the program effect on EB would be by reducing the number who begin receiving it), the maximum possible bias can be calculated under the assumption that

The bottom row of Table 3 provides each program's average cost, calculated by dividing the allocated program funding in 2009 by the total number of participants. For each program, the reduction in benefits collected per participant far exceeds average cost per participant, suggesting that the programs yielded positive returns on investment. Florida PREP and Florida REA registered average total UI savings of $159 and $453 per program participant, which exceeded average costs by a ratio of 4.7 and 8.4, respectively. Nevada REA/RES, which was by far the costliest of the four programs, yielded $976 in average UI savings, a ratio of 4.9 relative to average costs. Idaho REA – which had much lower average costs than the other programs – yielded the highest return ratio, with UI savings exceeding costs by more than 24 times.

Table 4 presents program effects on employment and earnings based on model [1]. All four programs had positive effects on employment rates. Florida PREP increased employment by about one percentage point in each quarter. Relative to Florida PREP, the Florida and Idaho REA programs produced slightly smaller or similar effects in quarter 1, but their effects on employment were about twice as large in quarters 2-4. The Nevada REA/RES effects were substantial, in the range of 6 to 8 percentage points, far exceeding the effects of the other programs. Estimates of effects on earnings follow the same pattern as for employment, although differences between the Nevada program and the others are somewhat greater. Estimated effects of Florida PREP were small and not statistically significant, whereas Florida REA had positive effects in the range of 3-5 percent. These results indicate that Florida REA was more successful than Florida PREP in pushing participants to exit UI quickly and find employment. The Idaho REA program had slightly higher effects on quarterly earnings than Florida REA, in the range of 4-8 percent.

all recipients who exhausted EUC started collecting EB and exhausted their full EB eligibility. In this case, we underestimate the effects on UI duration and benefits by 0.07 weeks and $17 for Florida PREP; 0.19 weeks and $46 for Florida REA; 0.14 weeks and $36 for Idaho REA; and 0.33 weeks and $102 for Nevada REA/RES.

The Nevada REA/RES program led to a $1,740 (21 percent) increase in participant earnings over the four quarters of the observation window, exceeding the effects of the other three programs by more than three-fold. This provides support for the view that not only did the Nevada program have longer-lasting impacts, but it also contributed in a more substantial way to the labor market success of participants than did the other programs.

5.2 Timing of UI Exit: Threat, Monitoring, and Services Effects

Figure 2 provides graphs that identify the impact of each program on the likelihood of discontinuing UI, conditional on not exiting in a prior week, based on model [2]. Each graph presents the estimated program effect on the UI exit probability and the corresponding 95 percent confidence interval; for convenience, effect sizes are provided when they are statistically significant. In addition, for the three programs that disqualified recipients, we present the rate of disqualification in each week, which provides a measure of the importance of monitoring effects.

The Florida PREP results are notable because the only statistically significant estimate is for week 3. After week 3, estimates are generally very small compared to standard errors and are about as likely to be negative as positive, suggesting that the program had very little effect on the likelihood of leaving UI later in the spell. Since the only program effect occurred during the period when threat effects are expected to be dominant, and there are no indications of positive effects following that period, program effects must reflect voluntary participant exit from UI to avoid anticipated program requirements.

Results show that Florida REA had more substantial effects and for a longer period, with positive effects starting in week 3 and extending through week 10; in six of these weeks, effects are statistical significant. As with Florida PREP, requirements were scheduled in weeks 4-6, so the program's effect in week 3 reflects almost exclusively threat effects. It appears that the Florida PREP and REA letters produced similar threat effects, despite differences in specific program requirements. Effects in weeks 4 through 10 (grey solid line) correspond closely to the level of disqualifications (black dashed line).[13] After week 10, there were no further disqualifications, and estimated effects are small relative to standard errors and about as likely to be negative as positive. Monitoring effects, as reflected in disqualifications, appear to fully explain the finding that Florida REA had higher overall effects than Florida PREP.

The Idaho results show that there were no program effects in weeks 1-3, but effects were positive from week 4 to week 7. Since there were no disqualifications in week 4, the week 4 effect is attributable to threat effects, reflecting participant exits in anticipation of the online review requirement. The effect is very similar to threat effects observed for the two Florida programs. The largest effect (1.4 percentage points) occurred in week 5, around the time when the audits of the online reviews began and disqualifications of ineligibles and non-completers were made. Comparison of total program effects (grey solid line) and the rate of disqualifications (black dashed line) in weeks 5-7 indicate that effects in this period are largely attributable to monitoring.[14] After week 7, when interactions of most participants with the program had ended and disqualifications rates were very low, estimated effects are small and not statistically significant.

The Nevada REA/RES program effect estimates differ from those for the other programs in three respects. First, every estimate in weeks 1-13 is positive and all but one are statistically significant, implying longer-lived effects than the other three programs. Second, the effects of the

[13] Of those disqualified, about two thirds were disqualified because they missed the meeting and failed to reschedule it, and about a third because they were judged ineligible on the basis of the meeting (see Appendix Table A3).
[14] Overall, it appears that monitoring effects stemmed primarily from disqualifications of participants who did not complete the online or in-person review, with disqualifications of those judged ineligible based on the review playing a secondary role (Appendix Table A4).

Nevada REA/RES are substantially greater than the effects of the other three programs in almost every week through week 13. Third, the program had substantial positive effects after the period when participants' interactions with the program had ended. The increased UI exit associated with the program in weeks 1-2 is largely a function of participants dropping out of UI in anticipation of program requirements (threat effects), given that very few disqualifications occurred in that period. Comparison of the grey solid and black dashed line implies that between a third and a half of total program effects in weeks 3-7 are attributable to monitoring effects, with about an equal number of disqualifications of no-shows and ineligibles (see Appendix Table A5). In weeks 3-4, the program effects not due to disqualifications may be largely due to participants exiting voluntarily to avoid the meeting. However, in weeks 5-7, most remaining participants had completed their meetings, so such withdrawals likely played a declining role. Beginning in week 8, over 80 percent of individuals had completed meetings and there were no disqualifications in week 9 or later. Moreover, participants who attended meetings were told that there were no additional requirements, so participant concerns with the burden of the program would be expected to play a much smaller role.

We conclude that although threat and monitoring effects are important for the Nevada program, since substantial effects occurred after participants had complied with program requirements, the job-counseling services themselves also played a role. Insofar as participants obtained services that improved the efficiency of their job search and led them to seek additional services, the rates of movements off of UI and into jobs could have been elevated for an extended period. This conclusion is supported by the fact that the other three programs examined here – which did not mandate participation in job counseling – did not have observable effects after program requirements had ended. The conclusion that job-counseling services may have provided direct aid to participants' job-search efforts is also supported by our finding that the Nevada program had substantive positive effects on employment and earnings, which much exceeded the effects of the other three programs.

5.3 Dynamic Selection

The models we use to estimate program effects on UI exit control for the effects of observed characteristics on the exit likelihood over time, so they account for dynamic selection based on observed characteristics. However, if there is unobserved heterogeneity, our estimates of program effects may be biased if those who exited early because of the program would have had different subsequent exit probabilities than those who remained. All formal models that have been used to account for such heterogeneity use a structure that assumes program effects early in the spell cause a downward bias in later estimated program effects (e.g., Geerdsen, 2006; Graverson and van Ours, 2008). This occurs in the case when participants who exit UI early because of the program would have been more likely to exit at a later point in the program's absence. On the other hand, at least in theory, if those who left UI early because of the program would have been *less* likely to exit after the initial stages of the claim in the absence of the program based on factors not captured by observed characteristics, then estimated effects for subsequent weeks would be upwardly biased (Black *et al.*, 2003).

Although we cannot directly measure the effects of dynamic selection, we can characterize the potential bias for the estimated effect in any given week. Appendix B2 derives a general expression for the bias, although the implications differ for each of our four programs. In the case of Florida PREP, the only statistically significant and substantively meaningful effect occurs in week 3, when participants receive the letter outlining program requirements but before they receive any services. Our concern focuses on the possibility that the failure to find effects in subsequent weeks is due to selection. In fact, our calculations of the potential bias show that, even if those who exit UI in week 3 would be very likely to later exit due to the program, there would be essentially no observable change in subsequent program effect estimates. Thus, the failure to find program impacts after week 3 cannot be attributed to dynamic selection. In the case of Florida REA, after an initial threat effect, Figure 2 makes clear that the small effects observed through week 10 are very likely entirely due to disqualifications. Again, the potential bias in estimated effects is small relative to standard errors. Similarly, our calculations of the potential bias in Idaho show that it is unlikely that the program had substantive effects in week 8 or later, after program requirements had ended.

In the case of the Nevada program, the question is whether the substantial estimated effects in weeks 7-13, which we have attributed to the value of job-counseling services, may be spurious. The bias would be positive if those who departed as a result of the program in prior weeks – due largely to threat and monitoring effects – had lower exit rates after week 6 than those who did not exit. The maximum bias would occur if the subsequent exit rate for such cases was zero. Our analyses show that even this measure of maximum potential bias is small relative to estimates of program effects. In weeks 7-13, the potential bias ranges between .0009 and .0011, whereas the range of the average treatment effect is between .0050 and .0100 in all weeks except week 10. Essentially, our analyses suggest that only about 11-16 percent of the estimated program effects in weeks 7-13 could be due to bias based on dynamic selection. These results suggest that selection effects could play at most a limited role in explaining estimated program effects in later weeks.

5.4 General Equilibrium Effects

Our discussion to this point ignores general equilibrium issues, and it should be acknowledged that if participants in our programs displace non-participants searching for jobs, program effects estimated here might be greater than those that would be obtained were these programs implemented for the full population. One might assume that during a recession, when the number of jobs is limited, such displacement would be particularly likely. In fact, some studies find substantial evidence of displacement effects, particularly for programs that target the unemployed youth (Crepon *et al*., 2013; Toohey, 2015). In contrast, Martins and Possoa e Costa (2014) conclude that displacement effects of reemployment programs that target the entire unemployed population are not important during an economic downturn. Similarly, other studies find that job-search assistance and employment subsidies have positive impacts even when they are provided to a large share of unemployed workers, suggesting the displacement effects are minor (Blundell et al., 2004; De Giorgi, 2005).

6. Conclusion

This study provides experimental evidence on the efficacy of four interventions that represent a wide range of reemployment programs operating in the U.S. during the Great Recession. Florida PREP is representative of WPRS services-referral programs operating in all 50 states during the recession. The Florida REA program is similar to eligibility-review programs operating in more than two thirds of the states since the start of the recession. The sole component of most REA state programs was an eligibility review conducted early in participants' UI spells, and participants were not required to receive job-search services or participate in further monitoring activities. Idaho REA differed from existing programs in that it used an online tool to conduct eligibility reviews and only required a minority of participants to have face-to-face interactions with program staff. Nevada REA/RES required participants to undergo the in-person eligibility review *and* receive mandatory job-counseling services in the early stages of their UI spells. The Nevada program differed from other U.S. programs, which either provided services referrals or required the eligibility review, and from European programs, which generally required continuous participation in job counseling and monitoring activities throughout the UI spell.

We find that all four programs reduced average UI spells, produced UI savings that exceeded the costs of the interventions, and improved participants' employment rates. However, there was notable variation in the magnitude of the effects among the four programs. Florida PREP produced relatively small effects on UI receipt and employment, and no effects on earnings. The two REA programs in Florida and Idaho produced greater effects on UI receipt and employment than Florida PREP, and also had positive effects on earnings. Nevada REA/RES was much more successful than the other three programs on all dimensions.

Analyses of the time patterns of program effects provide further evidence on the mechanisms that led to the success of each program and help explain variation in program results. Florida PREP effects are entirely attributable to threat effects, occurring because participants exited UI after receiving notification of program requirements and prior to attending the orientation meeting. The absence of any effects following the period when most orientation meetings were scheduled suggests that the program did not provide services of direct value to participants' job search. This conclusion is confirmed by the fact that the program had very limited effects on employment and earnings, and is consistent with the findings of recent research on similar U.S. programs implying the primacy of threat effects.

Florida REA had almost identical threat effects, but the program also produced monitoring effects in the period when the program disqualified participants who did not show up for the review or were found ineligible based on the review. Positive effects on employment and earnings point to the possibility that participants who were disqualified often had access to employment opportunities. Results for the Idaho REA indicate similar threat effects prior to the deadline for completing the online review, but the largest effects occurred in the weeks following this deadline and are primarily attributable to disqualifications of those who did not complete the online review, failed to attend the in-person interview, or did not pass the telephone verification standard. None of the two REA programs had any effects after the period when monitoring activities were underway and associated effects are observed, suggesting that the eligibility review did not push participants to conduct a more effective job search.

The Nevada REA/RES results tell a different story, as the program produced larger and longer-lasting effects on UI exit than the other programs. In the initial period when participants received notification about program requirements and were scheduled to undergo in-person meetings, program effects are primarily attributable to threat and monitoring effects, reflecting participant exits to avoid requirements and disqualifications of no-shows and ineligibles. Notably, the rate of disqualifications and the weeks over which they occurred were similar to those of the Florida REA but were lower than those in the Idaho REA. Even during the period when disqualifications are important, voluntary exits play a larger role than in the two REA programs, suggesting that the mechanism underlying program effects may differ.

A substantial share of the program's effects occurred after a large majority of participants had fulfilled program requirements, when threat and monitoring effects were unlikely to be important. This, combined with the fact that the program had much larger and more persistent effects on employment and earnings, suggests that the mandatory job-counseling services offered to participants were effective in helping them achieve better labor market outcomes. In short, although the Nevada program produced threat and monitoring effects similar to those of the other programs, it also appears to have induced subjects to undertake more effective job search. While we cannot dismiss the possibility that participants experienced continuing concerns about future program requirements, it is notable that none of the other programs exhibited effects lasting beyond the period when explicit requirements were in effect.

These findings expand the evidence base on the efficacy of reemployment programs in important ways. Most of the U.S. and European programs studied to date concerned programs that operated during periods of low unemployment and often had different requirements than the programs examined here. Thus, existing work provides limited guidance on whether reemployment programs can be effective during economic downturns, and little evidence on the effects of the particular types of interventions that were operating in the U.S. during the Great Recession. The general finding that all four programs reduced UI spells, produced savings that covered program costs, and improved employment rates indicates that reemployment programs can be cost-effective interventions in a weak economy. This finding is particularly important from a policy perspective given the substantial support these programs received during the recession. In fact, between half and three-quarters of the savings in benefits induced by each of the programs reflect declines in payments of EUC benefits that were available during the recession, showing that that value of inducing early exit may be larger during times of extended coverage.

Our analyses of the importance of underlying mechanisms in explaining program results during the recession further contribute to the evidence base. We find that all four programs increased the perceived cost of collecting UI benefits, pushing some to voluntarily exit UI to avoid participation. Although these threat effects are similar to those identified by previous work, our findings establish that such effects are operable when the labor market is weak. Hence, even in an environment with scarce job options and extended UI coverage, minimal requirements may be effective in pushing some to exit UI.

It is also evident that programs that involve eligibility reviews may produce additional effects by identifying and disqualifying those who are not conducting an active job search or are otherwise ineligible. In fact, monitoring effects appear to be more important than pure threat effects in explaining the impacts of eligibility-review programs during the recession. The results of the Idaho program imply that online tools may be used in conjunction with in-person reviews to reduce program costs, possibly with little impairment to program effectiveness. When budget constraints preclude hiring staff to review participant eligibility, it may be worth adopting a relatively inexpensive online review program.

Perhaps the most important finding is that mandatory job-counseling services may push participants to develop job-search strategies that help them improve their employment outcomes even for those who do not obtain a job immediately. This finding provides support for the view that adding a mandatory job-counseling services component to existing eligibility-review programs operating in the U.S. may enhance program efficacy, leading to higher UI savings and better employment outcomes for participants. This also suggests that the intensive job-counseling services typically provided by European programs may play an important role in aiding the job-search efforts of participants during a recession.

References

Anderson P., Corson W., and Decker D. (1991). The New Jersey Unemployment Insurance Reemployment Demonstration Project: Follow-Up Report, UI Occasional Paper 91-1, U.S. Department of Labor, Washington, DC.

Behaghel L., Crépon B., and Gurgand M. (2012). Private and Public Provision of Counseling to Job-Seekers: Evidence from a Large Controlled Experiment, IZA Discussion Paper No. 6518.

Benus J., Poe-Yamagata E., Wang Y., Blass E. (2008). Reemployment and Eligibility Assessment Study, ETA Occasional Paper 2008-02, U.S. Department of Labor, Washington, DC.

Black, D.A., Smith, J.A., Berger, M.C., and Noel, B.J. (2003). Is the Threat of Reemployment Services More Effective than the Services Themselves? Evidence from Random Assignment in the UI System, *American Economic Review*, 93(4), 1313-1327.

Blundell, R., Meghir, C., Cost Dias, M. and Van Reenen, J. (2004). Evaluating the Employment Impact of a Mandatory Job Search Program. *Journal of the European Economic Association*, 2 (4), 569-606.

Cockx, B., Dejemeppe, M., Launov, A. and van der Linden, B. (2017). Imperfect Monitoring of Job Search: Structural Estimation and Policy Design. *Journal of Labor Economics*, forthcoming.

Crepon, B., Duflo, E., Gurgand, M., Rathelot, R. and Zamora, P. (2013). Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment. *Quarterly Journal of Economics*, 128(2), 531-580

Decker, P.T., Olsen, R.B., Freeman, L., and Klepinger, D.H. (2000). Assisting Unemployment Insurance Claimants: The Long-Term Impacts of the Job Search Assistance Demonstration, Mathematica Policy Research, No. 8170-800.

De Giorgi, G. (2005). Long-Term Effects of a Mandatory Multistage Program: The New Deal for Young People in the UK. Institute for Fiscal Studies Working Paper No. 5.

Dickinson, K.P., Decker, P.T., Kreutzer, S.D., and West, R.W. (1999). Evaluation of Worker Profiling and Reemployment Services: Final Report, Research and Evaluation Report 99-D, U.S. Department of Labor, Washington, DC.

Dolton, P. and O'Neill, D. (2002). The Long-Run Effects of Unemployment Monitoring and Work-Search Programs: Experimental Evidence from the United Kingdom, *Journal of Labor Economics*, 20(2), 381-403.

Geerdsen, L.P. (2006). Is There a Threat Effect of Labour Market Programmes? A Study of ALMP in the Danish UI System. *The Economic Journal*, 116(513), 738-750.

Gorter, C. and Kalb, R. J. K. (1996). Estimating the Effect of Counseling and Monitoring the Unemployed Using a Job Search Model, *Journal of Human Resources*, 31(3), 590-610.

Graversen, B.K, and van Ours, J.C. (2008). How to Help the Unemployed Find Jobs Quickly: Experimental Evidence from a Mandatory Activation Program, *Journal of Public Economics*, 92(10-11), 2020-2035.

Hägglund, P. (2011). Are There Pre-Programme Effects of Active Placement Efforts? Evidence from a Social Experiment, *Economics Letters*, 112(1), 91-93.

Johnson, T.R., and Klepinger, D.H. (1991). Evaluation of the Impacts of the Washington Alternative Work Search Experiment, UI Occasional Paper 91-4, U.S. Department of Labor, Washington, DC.

Kahn, L. (2012). Labor Market Policy: A Comparative View on the Costs and Benefits of Labor Market Flexibility. *Journal of Policy Analysis and Management*, 31(1), 94-110.

Klepinger, D.H., Johnson, T.R., and Joesch J.M. (2002). Effects of Unemployment Insurance

Work-Search Requirements: The Maryland Experiment, *Industrial Relations and Labor Review*, 56(1), 3-22.

Krug, G., and Stephan, G. (2013). Is the Contracting-Out of Intensive Placement Services More Effective than Provision by the PES? Evidence from a Randomized Field Experiment, IZA Discussion Paper No. 7403.

Maibom J., Rosholm, M., and Svarer, M. (2017). Experimental Evidence on the Effects of Early Meetings and Activation, *The Scandinavian Journal of Economics*, 119(3), 541-570.

Martins, P,S. and Pessoa e Costa, S. (2014). Reemployment and Substitution Effects from Increased Activation: Evidence from Times of Crisis. IZA Discussion Paper No. 8600.

Meyer, B. (1995). Lessons from the U.S. Unemployment Insurance Experiments, *Journal of Economic Literature*, 33(1), 91-131.

Michaelides, M., and Mueser P. (2017). Are Reemployment Services Effective? Experimental Evidence from the Great Recession. University of Cyprus, Department of Economics, Working Paper 06-2015.

OECD (2013). Activating Jobseekers: Lessons from Seven OECD Countries, in *OECD Employment Outlook 2013*, 127-190, Paris: OECD.

O'Leary, C.J., and Wandner, S.A. (2005). Do Job Search Rules and Reemployment Services Reduce Insured Unemployment? Upjohn Institute Working Paper 05-112.

Perez-Johnson, I., Moore, Q., and Santillano, R. (2011). Improving the Effectiveness of Individual Training Accounts: Long-Term Findings from an Experimental Evaluation of Three Service Delivery Models. Final Report, Mathematica Policy Research, 2011.

Poe-Yamagata, E., Benus, J., Bill, N., Michaelides, M., and Shen, T. (2012). Impact of the Reemployment and Eligibility Assessment (REA) Initiative. *ETA Occasional Paper* 2012-08, U.S. Department of Labor, Washington, DC.

Ridley N., and Trace W.A. (2004). State and Local Labor Exchange Services. In: Labor Exchange Policy in the United States, Balducchi, D.E., Eberts, E.W., and O'Leary, C.J. (eds), Kalamazoo, MI: W.E. Upjohn Institute for Employment Research, pp. 73-99.

Toohey, D. (2015) Job Rationing in Recessions: Evidence from Work-Search Requirements. Unpublished, University of Delaware.

U.S. Department of Labor, OPA News Release, No. 05-0343-NAT, March 2005.

U.S. Department of Labor, ETA News Release No. 09-0893-NAT, July 2009.

U.S. Department of Labor, ETA News Release No. 10-0488-NAT, April 2010.

Wallace, GL., and Haveman R. (2007). The Implications of Differences between Employers and Worker Employment/Earnings Reports for Policy Evaluation. *Journal of Policy Analysis and Management*, 26(4), 737-753.

Wandner, S.A. (2010). Solving the Reemployment Puzzle: From Research to Policy, Kalamazoo, Michigan: Upjohn Institute for Employment Research, Kalamazoo, Michigan.

Wandner S.A., and Eberts, R.W. (2014). Public Workforce Programs during the Great Recession. *Monthly Labor Review*, July 2014.

Weathers, R.R., and Bailey, M.S. (2014). The Impact of Rehabilitation and Counseling Services on the Labor Market Activity of Social Security Disability Insurance (SSDI) Beneficiaries. *Journal of Policy Analysis and Management*, 33(3), 623-648.

Table 1: Characteristics of Services-Eligible UI Recipients

	Florida	Idaho	Nevada
Total	58,416 (100%)	12,645 (100%)	21,898 (100%)
PREP	18,510 (32%)	--	--
REA	23,471 (40%)	9,950 (79%)	--
REA/RES	--	--	3,496 (16%)
Control	16,435 (28%)	2,695 (21%)	18,402 (84%)
Men	33,422 (57%)	7,769 (61%)	12,426 (57%)
Women	24,994 (43%)	4,876 (39%)	9,472 (43%)
White	35,194 (60%)	10,336 (82%)	--
Black	10,194 (17%)	79 (1%)	--
Other Race	13,028 (22%)	2,230 (18%)	--
No High School Diploma	7,843 (13%)	1,694 (13%)	3,480 (16%)
High School Diploma	31,352 (54%)	4,827 (38%)	9,475 (43%)
Some College	9,955 (17%)	4,510 (36%)	6,286 (39%)
College Degree	9,266 (16%)	1,614 (13%)	2,657 (12%)
16-24 Years	6,550 (11%)	1,956 (15%)	2,767 (13%)
25-34 Years	13,663 (23%)	3,442 (27%)	5,526 (25%)
35-44 Years	13,605 (23%)	2,603 (21%)	4,972 (23%)
45-54 Years	13,672 (23%)	2,671 (21%)	4,782 (22%)
55-64 Years	8,421 (14%)	1,559 (12%)	2,807 (13%)
65+ Years	2,505 (4%)	414 (3%)	1,044 (5%)
White Collar, High Skill	17,796 (30%)	2,236 (18%)	4,219 (19%)
White Collar, Low Skill	16,749 (29%)	3,049 (24%)	7,040 (32%)
Blue Collar, High Skill	12,830 (22%)	2,986 (24%)	4,980 (23%)
Blue Collar, Low Skill	11,041 (19%)	4,374 (35%)	5,659 (26%)
Regular UI Entitlement	5,428 (1,973)	5,198 (2,603)	7,054 (3,031)
Regular UI Weeks Allowed	22.5 (4.6)	20.0 (5.6)	22.9 (4.5)
Earnings, 1st Quarter prior to entry	7,412 (8,089)	5,582 (5,210)	7,108 (7,038)
Earnings, 2nd Quarter prior to entry	7,718 (10,305)	5,446 (5,271)	7,267 (7,102)
Earnings, 3rd Quarter prior to entry	7,878 (8,129)	5,204 (5,850)	7,422 (7,305)
Earnings, 4th Quarter prior to entry	7,775 (8,591)	5,986 (5,754)	7,482 (7,440)

Note: Number of services-eligible recipients with sample proportion in parentheses; for regular UI entitlement, regular UI weeks allowed, and prior earnings, the sample means with standard deviations in parentheses are reported.

Table 2: Program Scheduling of In-Person Meetings and Completion Rates

	Florida PREP	Florida REA	Idaho REA	Nevada REA/RES
Treatment Cases (Proportion of Total)	18,510 (100%)	23,471 (100%)	9,950 (100%)	3,496 (100%)
UI Week	Scheduled Meeting			
1	--	--	--	--
2	--	--	--	774 (22%)
3	237 (1%)	966 (4%)	--	1,133 (32%)
4	8,897 (48%)	10,484 (45%)	--	754 (22%)
5	5,700 (31%)	7,214 (31%)	--	453 (13%)
6	3,202 (17%)	4,202 (18%)	1,365 (14%)	211 (6%)
7	474 (3%)	605 (3%)	359 (4%)	84 (2%)
8	--	--	130 (1%)	53 (2%)
9	--	--	80 (1%)	19 (<1%)
10	--	--	72 (1%)	2 (<1%)
11	--	--	49 (<1%)	1 (<1%)
12+	--	--	--	12 (<1%)
Treatment Cases Attending Meeting	11,179 (60%)	20,397 (87%)	N/A	2,800 (80%)

Note: Number of treatment cases with sample proportion in parentheses. For Nevada REA/RES, dates include any postponements. For the other programs, the date indicates the initially scheduled date.

Table 3: Average Treatment Effects on UI Receipt

	Florida PREP	Florida REA	Idaho REA	Nevada REA/RES
Exhausted Regular Benefits	-.012 (.005)** [-2%]	-.036 (.005)*** [-5%]	-.037 (.010)*** [-5%]	-.092 (.009)*** [-13%]
Collected EUC	-.011 (.005)** [-2%]	-.034 (.005)*** [-5%]	-.032 (.011)*** [-6%]	-.092 (.009)*** [-15%]
Exhausted EUC	-.004 (.003) [-3%]	-.011 (.003)*** [-7%]	-.008 (.007) [-5%]	-.019 (.007)*** [-10%]
Weeks on UI				
Regular	-.13 (.08)* [-1%]	-.47 (.07)*** [-3%]	-.43 (.10)*** [-2%]	-1.71 (.13)*** [-9%]
EUC	-.57 (.19)*** [-3%]	-1.42 (.19)*** [-7%]	-.95 (.35)*** [-6%]	-2.03 (.35)*** [-11%]
Total†	-.70 (.25)*** [-2%]	-1.89 (.24)*** [-5%]	-1.38 (.41)*** [-4%]	-3.74 (.42)*** [-10%]
Benefit Amounts Received ($)				
Regular UI	-29 (19) [-1%]	-114 (19)*** [-3%]	-77 (30)** [-2%]	-489 (45)*** [-8%]
EUC	-130 (47)*** [-3%]	-339 (46)*** [-7%]	-213 (99)** [-5%]	-487 (110)*** [-9%]
Total†	-159 (61)*** [-2%]	-453 (60)*** [-5%]	-290 (117)** [-3%]	-976 (137)*** [-9%]
Observations	34,945	39,906	12,645	21,898
Cost per Participant††	$21-34	$54	$12	$201

Note: Average treatment effect with standard error in parentheses. Brackets identify the average treatment effect as a percentage of the control group mean. ***, ** =significant at the 1%, 5% level.

†Regular plus EUC; does not include EB.

†† Calculated as follows:

(1) Florida PREP – lower bound: Wagner-Peyser grant amount in 2009 divided by the number of Wagner-Peyser participants in 2009; upper bound: Wagner-Peyser grant amount in 2009 divided by number of PREP participants.
(2) Florida REA – REA grant amount in 2009 divided by the number of REA referrals in 2009.
(3) Idaho REA – REA grant amount in 2009 divided by the number of REA referrals in 2009.
(4) Nevada REA/RES – REA grant amount plus Wagner-Peyser grant amount used to support the program in 2009 divided by the number of REA/RES referrals in 2009.

Table 4: Average Treatment Effects on Employment and Earnings

	Florida PREP	Florida REA	Idaho REA	Nevada REA/RES
Employed				
In Quarter 1 after entry	.010 (.005)** [+3%]	.011 (.005)*** [+3%]	.005 (.011) [+1%]	.066 (.009)*** [+17%]
In Quarter 2 after entry	.009 (.005)* [+3%]	.017 (.005)*** [+5%]	.021 (.011)** [+4%]	.076 (.009)*** [+19%]
In Quarter 3 after entry	.007 (.005) [+2%]	.022 (.005)*** [+6%]	.027 (.011)** [+5%]	.059 (.009)*** [+13%]
In Quarter 4 after entry	.011 (.005)** [+3%]	.018 (.005)*** [+4%]	.023 (.011)** [+4%]	.058 (.009)*** [+12%]
Earnings				
In Quarter 1 after entry	32 (50) [+2%]	74 (48) [+4%]	64 (67) [+6%]	294 (59)*** [+20%]
In Quarter 2 after entry	42 (47) [+2%]	110 (47)** [+5%]	131 (61)** [+8%]	461 (66)*** [+24%]
In Quarter 3 after entry	3 (50) [+0%]	99 (48)** [+4%]	169 (76)** [+7%]	502 (76)*** [+21%]
In Quarter 4 after entry	7 (53) [+0%]	75 (51) [+3%]	92 (77) [+4%]	482 (81)*** [+19%]
Total, Quarters 1-4	85 (162) [+1%]	370 (159)** [+4%]	455 (222)** [+6%]	1,740 (232)*** [+21%]
Observations	34,945	39,906	12,645	21,898

Note: Average treatment effect with standard error in parentheses. Brackets identify the average treatment effect as a percentage of the control group mean. ***, ** =significant at the 1%, 5% percent level.

Figure 1: Program Designs in Florida, Idaho, and Nevada

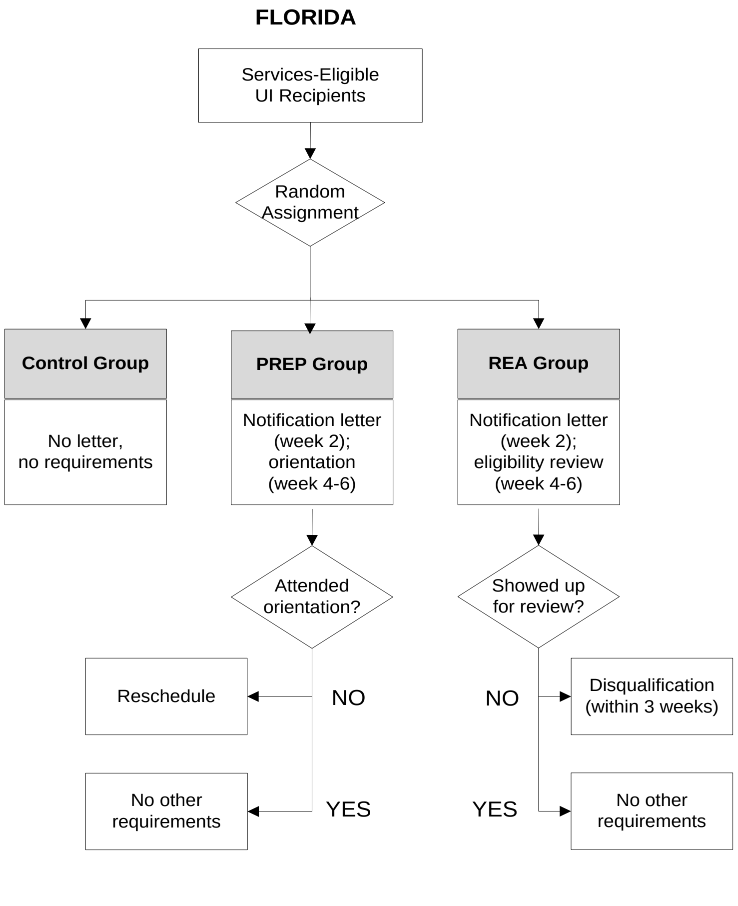
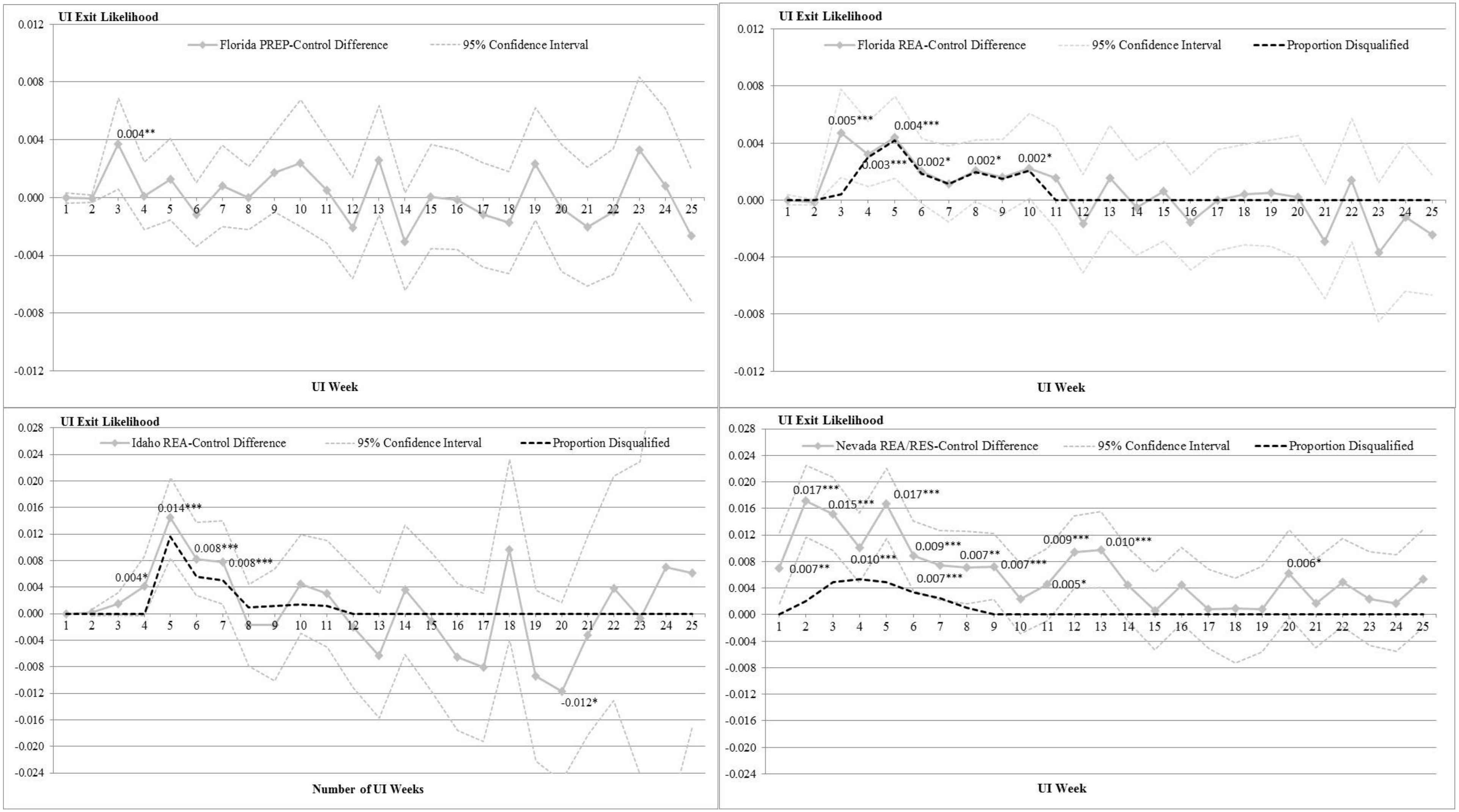


Figure 2: Regression-Adjusted Program Effects on the UI Exit Likelihood



Note: Grey solid line is the regression-adjusted program-control difference in the UI exit likelihood; grey dotted lines encompass the 95% confidence interval. The black dotted line is the proportion of treatment cases disqualified each week, as applicable. ***, **, *= significant at the 1%, 5%, 10% level.

Appendix A

Table A1: UI Receipt, Employment, and Earnings Outcomes

	Florida			Idaho		Nevada	
	PREP	REA	Control	REA	Control	REA/RES	Control
Exhausted Regular UI Benefits	.696	.689	.708	.630	.668	.587	.682
Collected EUC Benefits	.679	.672	.690	.508	.540	.510	.608
Weeks on UI							
Regular	18.9 (7.7)	18.7 (7.7)	19.1 (7.6)	16.8 (6.4)	17.3 (6.0)	17.1 (8.5)	19.0 (7.8)
EUC	20.7 (18.1)	20.7 (18.1)	21.1 (18.1)	13.8 (17.2)	14.7 (17.5)	15.4 (19.1)	17.7 (19.1)
Total†	39.6 (23.6)	39.3 (23.6)	40.2 (23.4)	30.6 (21.2)	32.0 (21.1)	32.5 (24.5)	36.7 (23.4)
Benefit Amounts Received ($)							
Regular UI	4,506 (2,334)	4,431 (2,326)	4,543 (2,214)	4,286 (2,474)	4,442 (2,404)	5,375 (3,496)	5,870 (3,396)
EUC	4,797 (4,557)	4,759 (4,555)	4,926 (4,567)	3,628 (5,038)	3,894 (5,152)	4,810 (6,422)	5,318 (6,261)
Total†	9,469 (6,270)	9,303 (6,271)	9,190 (6,274)	7,914 (6,773)	8,336 (6,789)	10,185 (8,872)	11,188 (8,490)
Exhausted EUC	.146	.149	.148	.146	.152	.169	.189
Employed							
In Quarter 1 after entry	.356	.346	.345	.423	.418	.466	.396
In Quarter 2 after entry	.353	.343	.346	.507	.488	.491	.407
In Quarter 3 after entry	.392	.388	.386	.546	.519	.519	.451
In Quarter 4 after entry	.422	.414	.411	.564	.540	.546	.479
Earnings							
In Quarter 1 after entry	1,818 (5,165)	1,710 (4,995)	1,731 (4,796)	1,117 (3,423)	1,070 (2,472)	1,757 (3,666)	1,435 (3,173)
In Quarter 2 after entry	2,069 (4,538)	2,013 (4,742)	2,023 (4,584)	1,828 (3,031)	1,737 (2,903)	2,395 (4,144)	1,882 (3,606)
In Quarter 3 after entry	2,512 (4,814)	2,471 (4,872)	2,495 (4,899)	2,548 (3,828)	2,414 (3,758)	2,951 (4,611)	2,379 (4,204)
In Quarter 4 after entry	2,839 (5,163)	2,753 (5,185)	2,797 (5,168)	2,622 (3,770)	2,556 (3,807)	3,146 (4,965)	2,588 (4,443)
Total, Quarters 1-4	9,238 (16,393)	8,947 (16,782)	9,045 (16,461)	8,115 (11,222)	7,776 (10,766)	10,248 (14,411)	8,284 (12,843)

Note: Reported is the sample proportion, or sample mean with standard deviation in parentheses. †Regular plus EUC; does not include EB.

Table A2: Program Assignment Probability Regression Results

	[1] Florida PREP	[2] Florida REA	[3] Idaho REA	[4] Nevada REA/RES
Men	-.009 (.006)	-.008 (.005)	.003 (.008)	.004 (.005)
Black	.009 (.007)	.010 (.007)	-.029 (.046)	--
Other Race	-.007 (.007)	-.003 (.006)	.006 (.010)	--
No High School Diploma	.016 (.011)	.013 (.009)	.018 (.016)	-.015 (.010)
High School Diploma	.013 (.008)	.013 (.007)*	.001 (.013)	-.017 (.009)
Some College	.014 (.009)	-.005 (.008)	.011 (.012)	-.012 (.009)
College Degree	--	--	--	--
<25 Years	--	--	--	--
25-34 Years	-.006 (.010)	.009 (.009)	.008 (.012)	.000 (.009)
35-44 Years	-.005 (.010)	.002 (.009)	.005 (.013)	-.008 (.009)
45-54 Years	-.005 (.010)	.006 (.009)	.020 (.013)	.017 (.011)
55-64 Years	-.008 (.011)	-.004 (.009)	-.001 (.014)	-.018 (.010)*
65+ Years	-.014 (.015)	-.001 (.013)	-.018 (.022)	-.015 (.013)
White Collar, High Skill	--	--	--	--
White Collar, Low Skill	-.007 (.007)	-.007 (.006)	-.003 (.012)	-.004 (.007)
Blue Collar, High Skill	.006 (.008)	.003 (.007)	.012 (.012)	.008 (.008)
Blue Collar, Low Skill	.003 (.009)	.007 (.008)	-.004 (.012)	-.012 (.008)
Log Benefit Entitlement	-.005 (.006)	-.011 (.009)	-.013 (.008)	.012 (.007)
Earnings, 1st Quarter prior to entry	-.000 (.001)	.000 (.001)	-.002 (.001)*	-.001 (.001)
Earnings, 2nd Quarter prior to entry	-.001 (.001)	-.001 (.001)	.002 (.001)	-.001 (.001)
Earnings, 3rd Quarter prior to entry	.001 (.001)	.001 (.001)	.001 (.001)	.000 (.001)
Earnings, 4th Quarter prior to entry	.000 (.001)	.000 (.001)	-.000 (.001)	.001 (.001)
Constant	.271 (.075)***	.768 (.064)***	.897 (.070)***	.178 (.044)***
R-squared	.0671	.0754	.0033	.0081
Observations	34,945	39,906	12,645	21,898

Note: Dependent variable is program assignment. Estimated parameters are presented with heteroscedasticity-robust standard errors in parentheses. All specifications include fixed effects for weeks of regular UI payments allowed and week the UI spell started. The Florida models also include fixed effects for workforce region and region-week interactions. ***, *= statistically significant at 1%, 10% level.

Table A3: Florida REA Disqualifications

Week	At-Risk Population	Disqualifications (proportion of at risk population)		
		No-shows	Ineligibles	Total
1	23,471	0 (.0000)	0 (.0000)	0 (.0000)
2	22,817	0 (.0000)	0 (.0000)	0 (.0000)
3	22,506	1 (.0000)	8 (.0004)	9 (.0004)
4	21,921	39 (.0018)	27 (.0012)	66 (.0030)
5	21,646	50 (.0023)	41 (.0019)	91 (.0042)
6	21,162	25 (.0012)	14 (.0007)	39 (.0018)
7	20,887	19 (.0009)	5 (.0002)	24 (.0011)
8	20,497	31 (.0015)	9 (.0004)	40 (.0020)
9	20,253	25 (.0012)	5 (.0002)	30 (.0015)
10	19,868	38 (.0019)	3 (.0002)	41 (.0021)

Note: Number of treatment cases disqualified each week, with the proportion of at-risk population in parenthesis.

Table A4: Idaho REA Disqualifications

Week	At-Risk Population	Disqualifications (proportion of at risk population)		
		No-shows†	**Ineligibles††**	**Total**
1	9.950	0 (.0000)	0 (.0000)	0 (.0000)
2	9.950	0 (.0000)	0 (.0000)	0 (.0000)
3	9.947	0 (.0000)	0 (.0000)	0 (.0000)
4	9.928	0 (.0000)	0 (.0000)	0 (.0000)
5	9,807	88 (.0090)	26 (.0027)	114 (.0116)
6	9,575	44 (.0046)	9 (.0009)	52 (.0055)
7	9,398	38 (.0040)	9 (.0010)	47 (.0050)
8	9,179	5 (.0005)	4 (.0004)	9 (.0010)
9	8,996	3 (.0003)	7 (.0008)	10 (.0011)
10	8,592	4 (.0005)	8 (.0009)	12 (.0014)
11	8,003	3 (.0004)	6 (.0007)	9 (.0011)

Note: Number of treatment cases disqualified each week, with the proportion of at-risk population in parenthesis.
†= No-shows in week 5 represent disqualifications for failure to complete online review; no-shows in weeks 6-11 represent disqualifications for failure to attend the in-person interview.
††= Ineligibles in week 5 represent disqualifications for findings of ineligibility based on phone verification; ineligibles in weeks 6-11 represent disqualifications for findings of ineligibility based on phone verification or in-person interview.

Table A5: Nevada REA/RES Disqualifications

Week	At-Risk Population	Disqualifications (proportion of at risk population)		
		No-shows	**Ineligibles**	**Total**
1	3,496	0 (.0000)	0 (.0000)	0 (.0000)
2	3,397	3 (.0009)	4 (.0012)	7 (.0021)
3	3,273	8 (.0024)	8 (.0024)	16 (.0049)
4	3,158	9 (.0028)	8 (.0025)	17 (.0054)
5	3,071	9 (.0029)	6 (.0020)	15 (.0049)
6	2.969	8 (.0027)	2 (.0007)	10 (.0034)
7	2,892	6 (.0021)	1 (.0003)	7 (.0024)
8	2,821	2 (.0007)	1(.0004)	3 (.0011)

Note: Number of treatment cases disqualified each week, with the proportion of at-risk population in parenthesis.

Table A6: Service Take-Up Rates, Nevada REA/RES Program

	Treatment	Control	*Difference*
Job-Counseling Services	.675	.100	*.575 [.006]****
Work Search Plan	.549	.063	*.487 [.006]****
Resume Assistance	.253	.027	*.226 [.004]****
Job Referral	.211	.042	*.169 [.005]****
Orientation	.302	.036	*.266 [.005]****
Employment Workshops	.132	.015	*.117 [.003]****

Note: Proportions receiving each service. Job-counseling services indicates whether participants received work-search plan assistance, resume development assistance, or a job referral. ***=statistically significant at 1% level.

Appendix B

B1. Direct Impact of Monitoring: Disqualifications

For the three programs that reviewed participants' UI eligibility, we can identify those individuals who were disqualified, providing a proxy for the direct effects of monitoring. The total effect of each program in a given week can be decomposed into an effect that is due to disqualifications and an effect due to voluntary exit, whether reflecting concerns with program requirements or improved labor market opportunities. In equation [2] in the main text, the coefficient δ_t is an estimate of the difference between the exit rate for participants in week *t* (H_t^p) and the exit rate for the control group (H_t^c), conditional on not having exited in a prior week. Denoting the probability of exit due to disqualification during week *t* as H_t^D and the probability of exiting voluntarily as H_t^V, we wish to divide the total program effect into a part due to voluntary and involuntary exits. Insofar as disqualifications may be selective, it is necessary to know what the exit rate would have been for program participants who were disqualified, if they had been assigned to the control group. If this exit rate is zero, we can write:

$$H_t^p - H_t^c = H_t^D + [H_t^V - H_t^c] \qquad \text{[B1]}$$

H_t^D is an approximation for the program effect in week *t* due to disqualifications, and the term in brackets identifies the program effect due to voluntary exits. If some of those who were disqualified in the week would have left in absence of the program, the bias in [B1] will be small unless disqualifications target individuals who are particularly likely to exit. The dashed lines in Figure 2 report H_t^D as a proxy for the program effects due to disqualifications.

B2. Dynamic Selection

Once we control for the structure of random assignment, treatment-control differences in exit probability for the initial week represent unbiased program effects. However, estimates for later weeks may be biased if those who exited early because of the program would have had different subsequent exit probabilities than those who remained.

We can characterize the potential bias for the estimated effect in a given week as follows. Consider the observed probability that a control case exits in week *t*>1, conditional on it not having exited prior to *t*, as H_t^c. 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 samples in the risk sets in week *t*). We assume that treatment cases are induced to exit the program at a higher rate prior to week *t*, i.e., $S_t^c > S_t^p$, and 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 control cases that are in the risk set at time *t* would have exited in a prior week if they had been in the treatment group. Denote the UI exit likelihood of this group in week *t* as H_t^{c-p}. Under the assumption that the program has no effect at time *t*, we may write the difference between the participant exit probability in the case where the program has no effect and the exit probability of the control group as

$$H_t^{p*} - H_t^c = \left(\frac{S_t^c}{S_t^p} - 1\right)(H_t^c - H_t^{c-p}) \qquad \text{[B2]}$$

The asterisk indicates that the exit likelihood for program participants is calculated under the assumption that there is no treatment effect at time *t*. Equation [B2] provides a measure of the bias in the estimate, indicating how the estimated exit likelihood for the treatment group at time *t* is affected by selection due to program effects in prior weeks.[15] If the departure rate for those who responded to the program is greater than that of the average control group member ($H_t^{c-p} > H_t^c$), the bias is negative, implying that program effect estimates are negatively biased. In contrast, if the program culls out those who are less likely to exit later ($H_t^{c-p} < H_t^c$), the bias will be positive.

Our analysis of the Florida and Idaho programs is focused on the possibility that selection bias may hide program effects on exit probabilities later in the UI spell. In fact, even if we assume that $H_t^{c-p} = 1$, the possible bias in the estimates of program effects would not have been large enough to hide a statistically significant program effect.

In the case of Nevada, our concern is whether observed positive program effects may be due to selection. In order to consider the most conservative case, we set $H_t^{c-p} = 0$. Since, beginning in week 12, cases that exit include those eligible for exactly that number of weeks, 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 control for weeks of eligibility, so this adjustment is implicit in reported estimates. As reported in the text, even with this conservative assumption, the bias is modest relative to estimated effects.

[15] To derive this expression, note that, if there is no treatment effect in week *t*, the exit likelihood for the control group is just the weighted average of the exit likelihood for the treated group in the risk set in week *t* and the exit likelihood for those who exited previously due to the treatment: $H_t^c = \left(\frac{S_t^c - S_t^p}{S_t^c}\right) H_t^{c-p} + \left(\frac{S_t^p}{S_t^c}\right) H_t^{p*}$