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DISCUSSION PAPER SERIES

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ABSTRACT

Do Reemployment Programs for the Unemployed Work for Youth? Evidence from the Great Recession in the United States*

We present experimental evidence on the effects of four U.S. reemployment programs for youth Unemployment Insurance (UI) recipients during the Great Recession. The three programs that emphasized monitoring and service referrals reduced UI receipt but had minimal effects on employment and earnings; these programs mainly induced the early exit of participants. The fourth program, which combined mandatory job counseling with monitoring, caused the largest reductions in UI receipt and clearly increased employment and earnings. Both early participant exits and effective job counseling underlie these impacts. We conclude that policymakers should require job counseling for youth UI recipients during recessions.

JEL Classification: J6, H4

Keywords: youth, Great Recession, REA, WPRS, job counseling, active labor market policies, unemployment, Unemployment Insurance, program evaluation

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I. INTRODUCTION

In response to the Great Recession, U.S. policymakers appropriated substantial funds for programs to help unemployed workers improve the quality and quantity of their job-search efforts, and thereby to speed their return to employment while easing the financial burden on the Unemployment Insurance (UI) system. This paper presents experimental evidence on the efficacy of U.S. reemployment programs for youth UI recipients (i.e., those under the age of 25) during the Great Recession. We consider four programs – two operating in Florida, one in Idaho, and one in Nevada – which collectively represent nearly the entire range of job-search-related interventions offered to, or imposed on, UI recipients during the recession. Similar to reemployment programs that have operated in the U.S. over the past 40 years, these four programs were not designed specifically to serve the unemployed youth, but rather targeted the general UI population. Prior studies of such programs in the U.S. context, including studies of the four programs analyzed here (Michaelides and Mueser, 2018; 2019), examine program effects for the entire UI population, which is dominated by adults. In addition to its laser focus on whether reemployment programs are effective for the unemployed youth, this paper adds value by highlighting different aspects of the problem and exploring alternative methods.

Several factors motivate our interest in the period of the Great Recession. First, we have a strong prior that job-search-related interventions perform differently during a recession, with relatively fewer vacancies and more unemployed workers, than during a boom. Sharper job-search skills may have a large payoff in a tight market but little payoff in a weak one; monitoring may increase effort when the additional effort required to secure a job is small but lead marginal workers to exit the labor market when it is large. In addition, the expected counterfactual outcomes differ in weak labor markets, as demonstrated in the context of training programs by

Lechner and Wunsch (2009). As such, we hesitate to generalize estimates from better times to this period. Second, the characteristics of UI recipients vary substantially over the business cycle (e.g., Black *et al*., 2003a; Michaelides and Mueser, 2012). Programs whose effects vary with participant characteristics would have different aggregate effects over the cycle for compositional reasons alone.

Similarly, several factors motivate our focus on youths. First, they have higher unemployment rates than adults, making them of special policy interest, especially given the possibility of lingering effects from negative initial job market experiences. Second, the cyclicality of youth unemployment exceeds that of older workers, implying that they suffer differentially in downturns. In the Great Recession, the unemployment rate for those under 25 years of age peaked at over 20 percent in 2010, roughly double the national rate.

Third, in this policy domain, unlike some others such as job training, youths receive the same programs as adults. Yet we have many reasons to expect that the generic programs for UI recipients that we study would affect youths differently than adults. Youth recipients have more limited experience with the job-search process and more often lack information about specific job requirements and how their own skills and preferences fit in. As a result, the informational services these programs provide (or provide referrals to) may have relatively more value for youths. More narrowly, youths will have a higher probability of encountering these programs for the first time, when we might expect them to have their largest effect. Youths might also find the formal eligibility review process intimidating in a way that older (and wiser) UI recipients do not. The limited evaluation literature on these programs typically provides only aggregate impact estimates, rather than youth-specific ones.

Fourth, the literature on active labor market programs often finds different effects of similar services for youths and adults, as with the Job Training Partnership Act experimental impact estimates summarized in Bloom *et al*. (1997). Moreover, the literature offers a rather dismal general view that programs for youths just do not work as well as programs for adults; see, for example, the reviews by Heckman *et al*. (1999) and Barnow and Smith (2016). The meta-analyses of Card *et al.* (2010; 2018) also find lower impacts for programs serving only youths, relative to programs serving only adults or both youths and adults. This legacy of differential findings further motivates our focus on youths.

All four of the programs we analyze randomly assigned eligible UI recipients to a treatment group subject to program requirements or a control group that was not subject to program requirements. Our analyses use state UI administrative data on individuals under the age of 25 who started collecting UI from July to December 2009 and were subject to random assignment. The data include characteristics such as sex, age, and education, as well as outcomes such as UI claim duration, benefit amounts collected, employment, and earnings. We provide experimental estimates of the effects of each program on UI duration and benefits collected. We compare the latter to program operating costs to illuminate the effects of the program on the UI system's budget in each state. We also provide experimental impact estimates on earnings and employment, which we use as inputs to our informal social cost-benefit discussion.

Our conceptual framework lays out three mechanisms through which our programs might affect these outcomes: first, threat (or deterrent) effects, when news of impending requirements spurs program exit (and, hopefully, earlier job-finding); second, monitoring effects, when participants get disqualified for failure to attend the eligibility reviews or when the reviews discover participants are not compliant with job-search requirements; and, third, services effects, when the services provided by these programs improve the quality and quantity of participant job-search efforts. To decompose the overall effects (at least qualitatively) into components associated with each mechanism, we conduct a non-experimental analysis of program effects on the conditional probability of exiting UI in each week of the claim, as we expect threat effects to drive impacts early in the claim and services effects to drive impacts later in the claim. We measure monitoring effects directly using administrative records on disqualifications.

To briefly foreshadow, we find that all four programs reduced UI claim durations as well as UI benefit payments. For the Idaho and Florida programs, these impacts resulted mainly from disqualifications due to failed or missed eligibility reviews combined with early exits due to threat effects. For the Nevada program, we find similar early exit effects combined with later improvements in UI exits, employment, and earnings; we ascribe the latter effects to the job counseling services that the Nevada program provided but the others did not.

We organize the remainder of the paper as follows. Section II discusses youth unemployment and U.S. reemployment policy during the Great Recession, along with existing studies of the effects of U.S. programs for unemployed youth. Section III describes the four programs and Section IV lays out the conceptual framework we use to think about their effects. Section V details our data sources and provides descriptive statistics for program and control group members. Section VI reviews our impact estimates, while Section VII addresses costs and benefits. Section VIII considers how to account for the substantial differences in impacts among the programs we study. Finally, Section IX concludes by comparing our youth estimates to those for adults in the same programs and drawing out implications for policy.

II. BACKGROUND

Youth Unemployment and the Great Recession

Youth workers nearly always have higher unemployment rates than adult workers in the U.S.

and other developed countries (e.g., Scarpetta *et al.*, 2010; Bell and Blanchflower, 2011; OECD, 2016). This pattern has several causes, including that youth workers have limited labor market experience, less information about available jobs, and fewer job-search skills. As a result, they are more likely than older workers to learn after taking a job that it represents a poor match with their abilities and skills, which leads to higher job turnover, implying more frequent unemployment spells and a higher average unemployment rate (e.g., Marchand, 1999; Martin, 2009; Bell and Blanchflower, 2011). Moreover, restricted employment options combined with limited financial responsibilities (e.g., no rent, no loans to repay, no dependents to care for), may discourage youths from engaging in robust job-search efforts; instead, they may rely on parental financial support and possibly return to school (e.g., Card and Lemieux, 2000; Robson, 2010; Bell and Blanchflower, 2011; Clark, 2011).

Furthermore, economic downturns hit harder and linger longer for youth workers (e.g., Blanchflower and Freeman, 2000; Verick, 2009; Scarpetta *et al.*, 2010; Choudry *et al.,* 2012). Youth workers typically have lower levels of firm-specific human capital than adult workers, making them more susceptible to labor market shocks (e.g., Martin, 2009; Verick, 2009; 2011). When employers make layoff decisions, they often let young, inexperienced workers go because they embody less extensive employer investments, or simply because of higher separation costs for adult workers with longer job tenure (e.g., Lazear, 1990; Nickell, 1997; Bertola *et al.*, 2002; Pages and Montenegro, 2007; Bell and Blanchflower, 2011).

Policymakers care about youth unemployment in particular because it may cause adverse long-term effects by "scarring" future employment prospects and earnings (e.g., Arulampalam, 2001; Gregg, 2001; Burgess *et al.*, 2003; Gregg and Tominey, 2005; Mroz and Savage, 2006; Kahn, 2010). Youth unemployment may also cause non-monetary welfare losses, with evidence suggesting that unemployment episodes at younger ages damage the self-esteem of workers and lead to antisocial behavior, including engagement in criminal activities (e.g., Korpi, 1997; Goldsmith *et al.*, 1997; Narayan and Smyth, 2004).

Figure 1 compares the unemployment experience of youth (under 25 years old) with prime-age (25-44 years old) and older (45+ years old) workers for the period 2000-2015, which brackets the Great Recession.[1] Youth unemployment rates exceeded the rates of other age groups prior to the recession and exhibited the largest increases during the recession. From peak to trough, the youth unemployment rate increased by 8.8 percentage points compared with a 5.6 percentage-point increase for prime-age and older workers. Youth workers also experienced a larger downward shift in labor force participation and full-time employment. Youth unemployment duration increased too, displaying a similar proportional increase to that for other age groups. Finally, the proportion of unemployed youths receiving UI benefits remained much below that for older workers, although it increased substantially during the recession.

U.S. Reemployment Policy during the Great Recession

In response to the Great Recession, the U.S. Congress allocated funds to enhance the capacity of the public workforce system to serve adult and youth jobseekers. The American Recovery and Reinvestment Act of 2009 (ARRA) allocated $400 million to states for 2009 and 2010 (in addition to the $724 million annual funding under continuing Wagner-Peyser legislation) to support provision of job-search services.[2] Several provisions of the ARRA focused on youth programs, including the expansion of employer tax credits to hire

[1] This figure uses age at last birthday as of the CPS interview date– age under 25 years identifies individuals who are at least 16 years of age and less than 25; age 25-44 identifies individuals who are at least 25 years of age and less than 45; and age 45+ identifies individuals who are at least 45 years of age.
[2] Source: US Department of Labor Detailed Budget Documentation, FY 2009 (https://www.dol.gov/dol/budget/index-2009.htm) and FY 2010 (https://www.dol.gov/dol/budget/index-2010.htm).

disadvantaged youth and additional support for the Workforce Investment Act (WIA) summer youth programs and other youth training and employment activities (Trutko and Barnow, 2013). The ARRA also included UI provisions, such as extensions of benefit duration for up to 99 weeks through activation of the Emergency Unemployment Compensation (EUC)[3] and Extended Benefits (EB)[4] programs, and full Federal financing of the EB program.

Policymakers also supported the expansion of the two main pre-existing federal reemployment programs: the Worker Profiling and Reemployment Services (WPRS) and Reemployment and Eligibility Assessment (REA) programs. Created in 1993, WPRS requires states to identify UI recipients most likely to exhaust benefits and refer them to job-search services.[5] The expectation was that early exposure to services would help those with employability issues to find jobs and exit UI quickly (Dickinson *et al.,* 1999; Berger *et al.,* 2001). In 2008, before the added funding took effect, the 50 state WPRS programs referred about 1.3 million UI recipients to services. Due to the added funding and higher demand for benefits during the recession, WPRS referrals rose to about 2 million annually in 2009 and 2010.[6]

REA was created by DOL in 2005 to encourage state workforce agencies to conduct reviews to assess whether UI recipients were actively searching for a job while collecting benefits (Benus *et al.,* 2008; Poe-Yamagata *et al.*, 2012; Michaelides *et al.*, 2012). This program requires UI recipients to undergo an in-person eligibility review at a public employment office. Those

[3] EUC is a federally-funded program that enables states with high unemployment rates to provide UI recipients who exhaust regular UI benefits with up to an additional 14-53 weeks of benefits.
[4] EB is a permanently authorized program, normally financed jointly by states and the federal government, which enables states with high unemployment rates to provide recipients who exhaust other benefits with up to an additional 20 weeks of benefits.
[5] The stringency and enthusiasm of WPRS implementation has varied across states and within states over time. See, for example, the discussions in Wandner (2010) and in Michaelides and Mueser (2020).
[6] The added funding also led to an increase in the number of UI recipients receiving actual services. In 2008, of the 1.3 million WPRS participants, 382,888 participated in job-search workshops and 141,806 received job counseling. Of the 2 million WPRS participants in 2010, 665,020 participated in workshops and 340,281 received counseling. Source: U.S. Department of Labor (http://workforcesecurity.doleta.gov/unemploy/profile.asp).

deemed ineligible during the review due to a failure to actively search for a job as required by state UI laws were disqualified from collecting benefits. Prior to the recession, REA programs operated in nine states (U.S. Department of Labor, 2005); as a response to the recession, DOL allocated $76 million to support the implementation of REA in 33 states and encourage states to offer job-search services to those who passed the review (U.S. Department of Labor, 2010).

Evidence Base

A wide variety of programs in the U.S. aim to help youth obtain labor market success, but few have endured rigorous evaluations, and when they do the results typically disappoint. Consider first the experimental evaluations of two flagship programs, the Job Corps program and the Job Training Partnership Act (JTPA) program. The Job Corps program, which dates back to the 1960s and takes the Civilian Conservation Corps of the 1930s as its inspiration, provides about a year of GED preparation, job skills training, and life skills training in a (usually) residential setting. An experimental evaluation at the end of the last century found declines in crime during the residential period and increases in earnings and employment for up to three years following program participation. However, there are no long-term earnings gains for most groups, and the program fails a cost-benefit test (Schochet *et al.*, 2008; Schochet, 2018).

The JTPA program received an experimental evaluation in the late 1980s that produced separate impact estimates for the program component aimed at out-of-school youth ages 16 to 21. Sadly, the experiment found no detectable effects on earnings for female youth and marginally negative effects for male youth. On a slightly more positive note, a careful non-experimental study of the Workforce Investment Act (WIA), the programmatic successor to JTPA, Heinrich *et al.* (2008) in the WIA *adult* program experienced improved earnings over the five years following participation, results similar to those for all adults.[7]

The Job Corps, JTPA, and WIA serve quite different youth populations in terms of age, education, and labor market attachment than the programs we consider, and spend (or spent, for JTPA and WIA) a lot more money on them as well.[8] As noted above, most U.S. reemployment programs for the unemployed target the general UI population, which is dominated by adult workers. They also feature modest, inexpensive interventions, and so even an optimistic prior expects relatively modest effects. This in turn implies that credible evaluations of these programs will require large samples (and probably random assignment as well). We now remark on several such evaluations aimed at the full UI population; we do not know of any evaluations that focus specifically on youth UI recipients as we do here.

Meyer (1995) reviews experimental studies of five job-search programs operating through the 1980s. The programs all shortened participants' UI spells, although their effects on employment were less clear. Studies of various reemployment programs in the 1990s by Decker *et al.* (2000), Black *et al*. (2003b), and Klepinger *et al.* (2002) confirmed that such programs reduce the amount of time participants spend collecting UI.[9] Participants exiting UI to avoid program requirements (i.e., threat effects) drive these impacts.[10] Studies of the Nevada REA program showed that the program reduced UI receipt and had positive effects on employment in the short-term (Michaelides and Mueser, 2018) and in the long-term (Manoli *et al.,* 2018). Michaelides and Mueser (2019) consider the same programs we do here for the full UI recipient populations in each state (which were dominated by adults), and emphasize the potential for job

[7] The WIA Gold Standard Experiment did not include the youth component of the program (Fortson *et al.,* 2018).
[8] Barnow and Smith (2016) offer a broad overview of U.S. employment and training programs and their evaluations.
[9] Decker *et al.* (2000) also showed that program effects on UI duration for participants under the age of 35 were similar to or greater than the effects for participants 35 years old or older.
[10] Such threat effects also appear for European programs that serve adults (e.g., Filges and Hansen, 2017).

counseling to produce effects on UI claim duration, employment, and earnings (in addition to any threat effects). Michaelides and Mian (2020) find that the Nevada REA program has similar impacts in the tight labor market of 2014-15. Finally, Klerman *et al.* (2020) evaluate REA programs in Indiana, New York, Washington, and Wisconsin in the mid-2010s and find impacts on UI duration, employment, and earnings in all four states.

Several studies provide experimental evidence on the effects of reemployment programs for unemployed youth in Europe. Programs that imposed intensive monitoring requirements with the aim of increasing search intensity had no effects on unemployment duration and employment in Denmark (Maibom *et al.*, 2014), Hungary (Micklewright and Nagy, 2010), and Sweden (Engström *et al.*, 2012).[11] The Danish authors summarize their findings as "further intensification of an already quite intensive effort for youth did not increase employment." Schemes that involved job counseling show greater promise. For example, programs that combined monitoring activities and direct job counseling in Sweden (Hägglund, 2014) and Denmark (Graversen and van Ours, 2008) had positive effects on job finding rates and exits from unemployment. Programs that involved job counseling and limited monitoring activities yielded no effects on employment in Sweden (Bennmarker *et al.*, 2013) but increased employment in France (Crépon *et al.*, 2013).[12] Caliendo and Schmidl (2016) surveyed seven additional non-experimental studies of European programs; for programs combining monitoring and counseling, they reported positive effects on employment in five studies, and null effects in one. The one study focusing on monitoring alone had positive effects on employment in the short run but

[11] These results parallel the findings for state UI search requirements in the U.S. in Toohey (2017).
[12] The youth impacts on employment from the experimental evaluations of multiple individual (one arm) or group (another arm) meetings that focused on job counseling in Maibom, *et al*. (2017) turn out negative but imprecise due to the small number of youth in the experiment.

negative effects in the long run.[13]

The European results do not readily generalize to our U.S. context for three main reasons. First, though they have declined somewhat in recent decades, important institutional differences remain between European labor markets (even relatively flexible ones as in Denmark) and the U.S. market. Second, most of the European programs described above operated during periods of relatively low unemployment. Third, most of the European programs involved multiple on-going meetings with caseworkers, rather than just one or two, in a context where caseworkers often have substantially more power over the unemployed than U.S. caseworkers do.

Overall, we observe a conspicuous gap in the literature regarding the effects of U.S. reemployment programs for unemployed youth. Existing U.S. studies focus primarily or exclusively on the general UI population, which is dominated by adult workers. The related European literature, though useful in a broad sense, does not do the job either. This study aims to fill the gap by considering four representative programs providing job-search-related interventions to youth in the U.S. during the Great Recession.

III. PROGRAM DESCRIPTIONS

During the Great Recession, all 50 states operated WPRS, with 33 states also operating REA programs. This study examines the effects for youth UI recipients of the Florida WPRS program and the REA programs in Florida, Idaho, and Nevada near the depth of the recession.

Florida

Florida operated both WPRS and REA programs during the recession. It called its WPRS program PREP (for "Priority REmployment Planning"); henceforth we do too. Each week,

[13] Non-experimental studies from Europe benefit from rich administrative data that makes claims of identification based on conditional independence more compelling. Rosholm (2014) provides a succinct overview of the (mainly) European literature on caseworkers and the unemployed. McCall *et al,* (2016) survey the broader European literature on active labor market programs at length.

regional workforce offices randomly assigned eligible new UI recipients (those not on temporary layoff, active in training programs, or attached to a union hiring hall) to the PREP program, the REA program, or the control group, based on the availability of program slots in their region.[14] UI recipients assigned to PREP received a notification letter in week two of their UI spell (i.e., when they collected their second UI weekly payment) informing them of the requirement to attend an orientation meeting at a public employment office in order to receive information about job-search services and scheduling an initial meeting date and time. Those assigned to REA received a similar letter in week two, but for an eligibility review meeting.

PREP participants who failed to attend the meeting had an opportunity to reschedule, and there were no repercussions for those who ultimately failed to attend. In contrast, REA participants who did not attend the meeting scheduled in the letter (or a rescheduled meeting within three weeks of the initial date) were disqualified from collecting UI.[15] The REA program also disqualified participants deemed non-compliant with UI work search requirements during the eligibility review.[16] After the meetings, Florida did not require PREP and REA participants to attend additional meetings or receive any services. Those assigned to the control group received no letter and had no obligations under either program but were subject to the usual UI work search requirements.[17]

[14] In 2009, 18 of the 24 Florida workforce regions implemented both PREP and REA; these regions covered 85 percent of unemployed workers in the state. The remaining six regions operated PREP but not REA. The proportion of new UI recipients assigned to each program in each week depended on their region's available resources.
[15] In practice, disqualification meant that a participant could not collect UI benefits until the end of the benefit year associated with the current claim (which lasted 365 days from the day the claim was filed). At the end of the benefit year, the participant could submit a new UI claim.
[16] State laws required UI recipients to be available for work, be actively searching for a job, and not reject suitable employment. UI recipients were also responsible for keeping track of their employer contacts, in case the UI agency wanted to verify that they were actively searching for a job.
[17] During the period of our data, Florida had a non-specific search requirement ("make a thorough and continued effort") as did Nevada ("contact several different employers each week"). Idaho had a worker-specific requirement (the "number of job contacts you must make each week was given to you at the time you filed").

Idaho

Idaho maintained both WPRS and REA programs during the recession but served only about 2 percent of services-eligible UI recipients via WPRS. The state randomly assigned the remaining services-eligible recipients to the REA program or to the control group. Those assigned to REA were sent a notification letter in week one of their UI spell (when they collected their first UI payment) asking them to complete an online review on the IdahoWorks website by week four; the online review collected information on their work search activities, including employer contacts. In week five, participants still collecting UI who either failed to complete the online review or whose responses led the state to deem them ineligible were disqualified from collecting UI. Like Florida, Idaho excused those enrolled in job-search services or training from the review requirements.

The Idaho UI agency then selected about 5 percent of those who completed the online review for telephone verification of their employer contacts and another 20 percent for an in-person review; this selection process emphasized clients with “suspicious” online responses. The remaining 75 percent had no further contact with the REA program. The state contacted those selected for the in-person review by phone in week five to set up an appointment and typically scheduled the in-person reviews in weeks six and seven. As with the online reviews, failure to appear for the in-person review, or appearing and revealing ineligibility, led to disqualification. Those who passed the in-person review received the news that they faced no further requirements under the REA program.

Nevada

Nevada operated both REA and WPRS during the recession, with REA operating in the workforce regions covering the Las Vegas and Reno metropolitan areas and WPRS operating in the remainder of the state.[18] Each week, the Nevada UI agency randomly assigned new eligible UI recipients to the REA program or to the control group based on the number of available slots in each region. The program group received a notification letter in week one of their UI spell instructing them to attend a meeting at a public employment office in weeks two to four of their UI spells. During that meeting, participants underwent an eligibility review to confirm that they were searching for a job and were otherwise satisfying UI requirements. Like the other states, Nevada disqualified those deemed non-compliant and those who did not show up for (or reschedule) the review from collecting additional UI payments.

Those who passed the review received job-counseling services during the same meeting. These services included an individual skills assessment, where program staff helped participants identify which types of jobs to pursue given their skills and experience. In addition, the program provided resume development assistance, as needed, and helped participants register in and learn how to use the state's labor exchange system. Participants also received information about available jobs as well as direct job referrals in cases where counselors could identify jobs that suited participants' profiles. After the meeting, participants learned that they had no further program obligations but could on their own initiative receive additional services, including job-search workshops and group orientations at public employment offices. Those assigned to the control group did not receive any notifications and had no obligations under REA but remained free to engage with generally available services (e.g., via the employment service or the workforce system) and remained subject to the usual UI work search requirements.

<u>External validity</u>

The programs we study represent a wide range of U.S. job-search-related interventions in

[18] Tabulations using the 2009 American Community Survey show that the Los Vegas and Reno metropolitan areas covered the overwhelming majority of unemployed workers in the state during the study period.

place during the Great Recession. Florida PREP closely parallels the structure of most state WPRS programs, which provided information and referrals to job-search services but did not typically mandate participation in services. Unlike other WPRS programs, Florida PREP used random assignment to determine participation rather than targeting the program toward UI recipients with high predicted probabilities of benefit exhaustion as determined by the state's profiling model. Florida REA looked like most of the 33 state REA programs that operated during the recession, which focused exclusively on eligibility reviews and did not mandate participation in job-search services.

The use of online tools for the eligibility reviews distinguishes Idaho REA from REA programs operating in other states, including Florida, which relied exclusively on in-person reviews. To our knowledge, Idaho REA is the only job-search 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. Nevada REA was the only state REA program that followed DOL's directives to both conduct in-person eligibility reviews and require those who passed the review to receive job counseling. Thus, Nevada REA had more intensive requirements than REA programs operating in other states during the recession, including Florida and Idaho, which mandated the eligibility review but did not mandate service participation.[19] Nevada REA was also more intensive than state WPRS programs, including Florida PREP, which provided service referrals but did not mandate an eligibility review or participation in services.

Overall, we think our findings generalize in a broad sense to other states within the context of the Great Recession, keeping in mind both the programmatic variation just described as well as

[19] Note that, to comply with the provisions of the Bipartisan Budget Act of 2018, states are currently in the process of adopting program structures very similar to those of the Nevada REA program. The Act requires states that operate the RESEA program, which replaced REA in 2015, to include mandatory job-search services in addition to an eligibility review.

other contextual factors such as program management quality and geographic differences in the timing and depth of the recession itself. Notably, all four programs considered here featured less intensive interventions than most of the European programs mentioned above, so generalization to the European context is less clear.

IV. CONCEPTUAL FRAMEWORK

We follow the literature in thinking about the mechanisms via which our programs might affect programmatic and labor market outcomes in the context of standard models of job search, as in the classic text of Pissarides (2017). Within that broad context, we identify three specific mechanisms at play in our context: *threat* effects, *monitoring* effects, and *services* effects.

Threat effects arise when the letters that treatment cases receive notifying them of required meetings and/or services provide new information to them, information that lowers the utility associated with unemployment. This leads workers to increase their search effort and/or to lower their reservation wage, which leads in turn to (on average) shorter UI spells and a quicker return to employment (though possibly at a lower wage than otherwise). Indeed, some workers may already have new jobs lined up, in which case the threat effect leads them to move the start date for those jobs forward. Particularly for youth, who may still live with their parents, reductions in the utility associated with collecting UI via the "hassle costs" of required meetings and services could also lead to labor market exit, or to continued job search without collecting UI. Black *et al*. (2003b) first highlighted the empirical importance of threat effects in the U.S. context in their study of the Kentucky WPRS program.

The three REA programs may produce *monitoring effects*, our term for the reductions in UI receipt caused by the disqualification of those who either fail to appear for their eligibility reviews in Florida or Nevada or who fail to complete the online review and follow-up activities (when required) in Idaho. Treatment cases may also face disqualification for non-compliance with job-search requirements. Of course, the threat of an impending eligibility review may lead some treatment cases to upgrade their search efforts prior to the scheduled meetings. We could reasonably call the impacts resulting from such behavior either threat effects or monitoring effects; we choose to limit monitoring effects to explicit disqualifications, which implicitly categorizes the anticipatory behavior as due to threat effects. Florida PREP has no monitoring effects, as it did not include eligibility reviews and did not disqualify those who failed to appear at (nominally) required meetings.

All four programs may produce *services effects* via program features that enhance participants' job search. Information on available jobs may allow participants to target their search efforts more effectively. Information on likely good and bad job matches via resume reviews, skills assessments and the like may do so as well. We can think of these services as increasing the number and/or quality of the offers generated by a given amount of search effort. And simple encouragement from a sympathetic caseworker may matter too, inducing participants to conduct a more intensive and consistent search. All should manifest as shorter claims and increased employment, but only in the period after receipt of services.[20] As noted above, these services may have differentially large impacts on youths due to their inexperience with job search, job-search-related services, and with the labor market more broadly. Finally, we might expect the Nevada REA program to display the largest services effects both because it mandated direct exposure to services and because the required services emphasized providing participants with information on available jobs and focusing their search on jobs compatible with their skills.

[20] Our programs might also cause (via caseworker referrals) a minor increase in participation in training programs such as those provided under WIA. We would expect this mechanism to lead to longer UI spells but perhaps higher earnings after completion of training (and thus after our data run out).

V. DATA

Prior to the Great Recession, the unemployment rates in Florida, Idaho, and Nevada resembled the national rate but, during the recession, Florida and Nevada experienced sharper increases in both total and youth unemployment rates; see Figure A1 in the online appendix. Youth unemployment rates peaked at about 24 percent in both Florida and Nevada compared with 20.4 percent nationally. The youth unemployment rate in Idaho remained slightly lower than the national rate except in 2011. In all three states, youth unemployment rates began to decline in 2011 and slowly returned to (roughly) pre-recession levels by 2015.

Our sample includes all youth (under 25 years old at the time of UI application)[21] who started collecting UI benefits from July through December 2009 in Florida, Idaho, and Nevada, and who were eligible for random assignment for participation in the reemployment programs.[22] Depending on their employment histories, Florida, Idaho and Nevada UI recipients were eligible to collect 9-26 weeks, 10-26 weeks, and 12-26 weeks of regular UI benefits, respectively. Because the state unemployment rates exceeded the thresholds for activating the EUC and EB programs, recipients who exhausted regular UI benefits could also apply for up to an additional 53 weeks of EUC and for up to an additional 20 weeks of EB.

Analyses of program effects rely on state UI claims data and wage records. UI claims data report individual characteristics, including sex, race (except for Nevada), ethnicity, education, and most recent occupation along with program assignment. The data also report benefit

[21] We define age as calendar year of UI claim start minus calendar year of birth.

[22] Of the 18 Florida workforce regions that implemented both PREP and REA, seven regions assigned all eligible youth UI recipients to either PREP or REA (i.e., none to the control group) and one region assigned fewer than 3 percent of eligible youth to either PREP or REA. Our analyses rely on the remaining 10 regions, which assigned 16-47 percent of eligible youths to PREP, 17-56 percent to REA, and 15-51 percent to the control group. These 10 regions covered about 60 percent of youth unemployed workers in the state during the study period. Idaho implemented REA statewide, so our sample covers all REA-eligible youth UI recipients in the state, omitting the small number who were assigned to WPRS. Nevada implemented REA in the Las Vegas and Reno regions, with WPRS operating in the rest of the state.

entitlements under the UI claim and the number of weeks and benefit amounts collected under the regular UI and EUC programs. Unfortunately, we could not obtain information on benefits collected under EB, so our analyses only consider receipt of regular UI and EUC benefits. Using these data, we construct several measures of UI receipt, including the number of UI weeks collected (regular UI and EUC), benefit amounts collected (regular UI and EUC), whether individuals exhausted regular UI, whether individuals collected any EUC benefits, and whether individuals exhausted EUC benefits.

Our UI wage record data report quarterly earnings from employers in the state in the eight calendar quarters prior to the start of the UI claim associated with program assignment, the quarter in which the claim started, and in the four calendar quarters after the start of the UI claim. We code a calendar quarter employment variable that defines employment as having positive earnings in the quarter. Note that UI wage records do not include earnings from jobs in other states,[23] state or federal government jobs, self-employment, or black or grey market activities; see Hotz and Scholz (2002), Wallace and Haveman (2007), and Greenberg and Barnow (2019) for discussions of the bugs and features of administrative outcome data.

Table 1 presents the characteristics of youth UI recipients in the study samples.[24] The Florida sample includes 6,524 services-eligible youth – about 32 percent were assigned to PREP, 40 percent to REA, and 28 percent to the control group. In Idaho, 1,956 eligible youth were subject to random assignment, of which 79 percent were assigned to the program. About 16 percent of the 2,767 eligible youth in the Nevada were assigned to the REA program.

[23] Tabulations of the 2010 ACS data show that in Nevada, 1.3% of employed youth and 1.8% of employed adults were employed in another state (mainly in California), in Idaho, 3.1% of employed youth and 5.0% of employed adults were employed in another state (mainly in Washington and Oregon), and in Florida, 0.8% of employed youth and 1.3% of employed adults were employed in another state.
[24] We follow common practice in calling our populations "samples" and presenting standard errors and statistical tests in line with this mislabeling; philosophically inclined readers should imagine meta-populations.

In Florida, about 55 percent of youth UI recipients were white, 22 percent were black, and 10 percent were Hispanic, reflecting the diverse workforce in the state. In Idaho, about 80 percent were white, with black youths making up less than 1 percent; nearly 16 percent were Hispanic. Race was not reported in the Nevada data, but about a quarter of youth UI recipients were Hispanic. Perhaps surprisingly, the proportion of the population with some college education in Idaho exceeds that in Nevada or Florida. The occupational distribution reflects the prevalence of the entertainment industry in Nevada, with relatively higher proportions of youth previously employed in white collar, low skill jobs. Idaho had the largest proportion of youth in blue collar, low skill jobs.

As shown in Figure 1, during the 2009 study period, only about 15 percent of unemployed youth workers received UI benefits, well below the rate for older workers. Many unemployed youth do not qualify for UI benefits because they lack sufficient prior work experience[25] or prior earnings, or because they worked in jobs not covered by the UI system. Others do not apply for UI benefits even when eligible. As a result, our study samples most likely represent highly selected subsamples of the overall population of unemployed youths.[26] Table 2 presents individual earnings in the eight quarters prior to UI entry along with UI benefit entitlements, all in nominal dollars. Nevada youth UI recipients could collect (on average) a total of 81.1 weeks of benefits with a $18,817 cumulative entitlement, compared with 73.8 weeks and a $14,541 cumulative entitlement in Florida, and 71.0 weeks and $14,579 cumulative entitlement in Idaho.

We examine covariate balance in our experiments by estimating linear probability models of

[25] In most states, these requirements pertain to employment in the five quarters prior to UI application.
[26] Online appendix Table A1 summarizes the characteristics of youths with some work experience (though not necessarily enough for UI eligibility) in 2009. Comparisons with Table 1 show that women and whites were under-represented in the UI population. In contrast, unemployed youth with a high school diploma and those previously employed in white collar, high skill and blue collar, high skill occupations were over-represented in the UI population.

assignment to each program group in each state. Online appendix Table A2 displays the estimates. We agree with Deaton and Cartwright (2018), who note the odd interpretation of statistical tests in contexts, such as this one, where institutional knowledge implies the truth of the null. Following their lead, we discount the statistical tests and focus on the magnitudes of the estimated imbalances, which end up substantively small in our context. In addition, we take the precaution of examining the sensitivity of our estimates to the inclusion of the covariates in our experimental impact regressions.

Employment service data report when required meetings were scheduled, whether participants met requirements, and who got disqualified for failure to show up or failing the eligibility review. Table 3 presents the meeting schedule for each program, including the proportions of participants who completed the meetings and who were disqualified. In Florida, PREP and REA meetings were mostly scheduled in weeks 4-6 of the UI claim, while Nevada REA meetings were mostly scheduled in weeks 2-6. Nearly two thirds of Florida PREP participants attended the orientation. Florida REA and Nevada REA realized higher completion rates, with nearly nine in every ten participants attending required meetings. Idaho only scheduled one in five participants for an in-person interview, with almost all scheduled in weeks 6-7. The bottom panel of the table shows that between 0.7 and 1.2 percent of REA youth participants across states got disqualified because they did not show up for the eligibility review. In addition, 0.4 to 0.5 percent were disqualified due to non-compliance with UI work search requirements.[27]

Table 4 compares the services received by youth UI recipients in Nevada REA and the

[27] Note that the number of completions plus the number of disqualifications do not add up to the total participant population in the Florida and Nevada REA programs; completions were not available for Idaho REA. The reason is that treatment cases that did not complete REA requirements in Florida, Idaho, and Nevada but received job-search and/or training services on their own initiative were exempt from REA requirements and thus were not disqualified.

control group using state administrative data; sadly, we could not obtain similar information for the Florida or Idaho programs. About 61 percent of program cases received at least one job-counseling service, compared with only 8 percent of control cases. Program cases had much higher take-up rates than control cases for each type of counseling service; importantly, about 17 percent of treatment cases received a direct job referral, compared with only about 3 percent of control cases. Treatment cases attended many more group orientation meetings to learn about job-search services and workshops to obtain basic job-search skills training as well. These figures show that Nevada REA created a meaningful treatment contrast by inducing youth UI recipients to participate in reemployment services.

VI. IMPACT ESTIMATES

We obtain our impact estimates via OLS estimation of the linear model

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

where *i* indexes UI recipients, Y_i denotes an outcome, $T_i \in \{0,1\}$ indicates random assignment to the program group, X_i is a vector of exogenous (i.e., not affected by treatment) covariates with associated coefficient vector c, and u_i is a mean zero error term. We interpret our estimate of *b* as the mean impact of assignment to the program group, that is, the average treatment effect, where the treatment is understood to include receipt of the letter. This is the same as the mean impact of the "intention to treat" in the terminology of the program evaluation literature. We do not estimate Local Average Treatment Effects for program services both because we lack data on service receipt in some states and, more importantly, because we expect the assumption implicit in the usual interpretation of those estimates, namely that the treatment has no effect on those who do not receive services, to fail due to the threat effects described above.

The conditioning variables include the individual characteristics listed in Table 1 along with prior earnings, indicators for weeks of regular UI eligibility, weekly benefit entitlement, indicators for week of UI entry, indicators for workforce regions, and interactions between week of UI entry and workforce region.[28] Given random assignment, the conditioning serves primarily to increase the precision of our estimated treatment effect. Inspired by Lin (2013), we privilege the conditional estimates and present unconditional estimates in a sensitivity analysis.

Effects on UI Receipt

Table 5 presents our estimated impacts on the number of weeks of regular UI, EUC, and total (regular UI plus EUC) benefits collected, the total dollar value of regular UI, EUC, and total benefits collected, regular UI benefit exhaustion, collection of any EUC benefits, and EUC benefit exhaustion. Each entry in the table corresponds to a separate estimation of [1] for the row outcome and the column program. Each cell provides the impact estimate in levels with associated standard error as well as the impact estimate expressed as a percentage of the control mean. Online appendix Table A3 provides descriptive statistics on control group outcomes.

Our estimates show that all four programs reduced UI spell durations and benefit amounts collected, with the magnitudes varying across programs. On average, Nevada REA reduced UI claim duration by 4.16 weeks and benefit amounts by $639, or by 13 and 9 percent relative to the control group means, respectively. Florida REA yielded larger reductions in claim duration and benefit amounts collected than Florida PREP, but both programs had smaller effects than Nevada REA. The effects of Idaho REA resemble those of Florida REA in percentage terms.

We also find that all four programs reduced regular UI benefit exhaustion as well as take-up of EUC benefits. Three of the four programs reduced the likelihood of exhausting EUC benefits, although the Nevada estimate lacks statistical significance. Estimated effects on regular UI

[28] The workforce region and week indicators are included to account for differential random assignment ratios among regions in Florida and Nevada.

exhaustion and receipt of EUC equal or exceed those on EUC exhaustion, implying little effect on UI exits at later stages of participants' claims. As we do not observe payments made under EB, we likely underestimate the effects on actual duration and benefit amounts collected for the programs that reduced EUC exhaustion. In particular, if EB were included, we think our estimated impacts on UI weeks received in Florida and Nevada would increase by at most 0.2 to 0.4 in absolute value and our estimated impacts on benefits collected would increase by at most \$40 to \$90 in absolute value.[29]

Effects on Employment and Earnings

Table 6 presents estimates from estimating [1] with the various measures of employment and earnings constructed from the UI wage records as outcomes. Our discussion focuses on employment and earnings outcomes in the four calendar quarters after program entry; for completeness, we also present effects for the quarter when individuals enter the program.[30] The two Florida programs generally have modest positive impacts on employment and earnings, a couple of which differ statistically from zero. Idaho REA has negative earnings impacts in three quarters and negative employment impacts in two, though none are statistically significant. Once again, in marked contrast, Nevada has very large impacts on both outcomes in all quarters;

[29] Under the assumption that program and control individuals who exhausted EUC had the same expected EB duration, each program's effect on EB receipt would be proportional to its effect on EUC exhaustion. Using information on the weeks of EB eligibility individuals would have had if they had exhausted EUC, we estimate an upper bound on the bias for total weeks (and benefits) collected by multiplying the effect on EUC exhaustion times EB eligibility weeks (and amount). These calculations lead to estimated biases of -0.44 weeks (-0.028 times 15.68 weeks) and \$86 in benefits (-0.028 times \$3,084 benefit amount) in Florida PREP, -0.38 weeks (-0.024 times 15.68 weeks) and \$74 in benefits (-0.024 times \$3,084 benefit amount) in Florida REA, and -0.23 weeks (-0.014 times 16.71 weeks) and \$42 in benefits (-0.014 times \$3,025 benefit amount) in Nevada REA. Similar calculations for Idaho REA, which had a positive but close-to-zero effect on EUC exhaustion, produce estimated biases of +0.09 weeks (0.006 times 14.5 weeks) and +\$18 in benefits (0.006 times \$2,978 benefit amount). Actual average weeks of EB received (for all UI recipients) in the second half of 2010 (conditional on receiving any) equal 17.4 in Florida, 12.8 weeks in Idaho, and 9.0 in Nevada.

[30] Assuming that UI recipients apply for benefits approximately uniformly over the quarter, they would be subject to the program for less than half of the period of the quarter on average. Any observed effects would be further reduced insofar as it may take a few weeks before program effects begin to occur. Hence, we had a strong prior that effects in the quarter of entry would not be large enough to be meaningful. Our analyses confirm this prior.

taking the sum over the four quarters, Nevada REA increased mean earnings by 18 percent relative to the control group. Though no more precise than the Idaho estimates, the magnitudes of the Nevada estimates allow us to clearly reject the null that they equal zero in all cases.

Impacts on earnings combine impacts on employment, on hours conditional on employment, and on wages conditional on employment. As knowledge regarding the relative importance of each impact margin illuminates the underlying causal mechanisms in important ways, many evaluations of labor market programs attempt to decompose overall impacts along these lines. Such attempts must grapple with an important selection issue that arises from the fact that labor market interventions may affect both the composition of employment and earnings conditional on employment. We lack data on hours, so we attempt a simpler decomposition into employment and earnings conditional on employment. Furthermore, we lack a credible source of exogenous variation in post-random-assignment employment within treatment arms, and so we offer a descriptive decomposition, interpreted with care, followed by a bounding exercise. In particular, we decompose the difference in earnings for program and control cases as follows:

$$\bar{Y}_T - \bar{Y}_C = \bar{E}_T \cdot \bar{W}_T - \bar{E}_C \cdot \bar{W}_C = (\bar{E}_T - \bar{E}_C) \cdot \bar{W}_T + \bar{E}_C \cdot (\bar{W}_T - \bar{W}_C) \qquad [2]$$

where $\bar{E}_T$ and $\bar{E}_C$ denote proportions employed, and $\bar{W}_T$ and $\bar{W}_C$ denote average earnings conditional on employment, for the program and control groups, respectively. The first product on the right side captures the difference in earnings due to the impact on employment (the "employment component") while the second product captures the difference due to the impact on earnings conditional on employment (the "earnings component").

To see the selection problem in our context, decompose the second term in [2] as:

$$\bar{E}_C \cdot (\bar{W}_T - \bar{W}_C) = \bar{E}_C \cdot (\bar{W}_T' - \bar{W}_C) + (\bar{E}_T - \bar{E}_C) \cdot (\bar{W}_T'' - \bar{W}_T), \qquad [3]$$

where $\bar{W}_T{}'$ denotes average earnings conditional on employment for workers who would be employed in either the program group or the control group while $\bar{W}_T''$ denotes the average earnings conditional on employment of those workers who would be employed in the program group but not in the control group. This representation assumes (plausibly in our context) that the program does not switch anyone from employment to non-employment. The first term in [3] represents the causal effect on earnings conditional on employment for workers employed in both the program and control groups, and the second term represents the selection effect, i.e., average earnings conditional on employment for workers employed in the program state but not the control state. Lee (2009) formalizes and provides standard errors for the intuitive bounds that assume that the workers drawn into employment by the treatment come from either the top or the bottom of the observed program group earnings distribution; i.e., that $\bar{W}_T''$ takes on either the largest or the smallest possible value consistent with the data.

Table 7 provides estimates of the employment and earnings components from [2] for each of the four calendar quarters after random assignment. The Florida and Idaho programs yield a collection of substantively small and relatively imprecise estimates; the one pattern worth noting is the negative sign on 10 of the 12 earnings component estimates for these programs. As noted above, a negative earnings component does not require that assignment to the program group would lower earnings for any one individual due to the possibility of selection. In sharp contrast, for Nevada we estimate large, positive, and statistically significant employment components in all four quarters, as well as large, positive earnings components that achieve statistical significance in the last two quarters. These estimates imply that the program increased average earnings of employed treatment group members, either by increasing the earnings of those who would work regardless of treatment assignment, or by drawing individuals with higher earnings into employment, or both.

Table 7 also presents estimated upper and lower bounds based on the program impacts on quarterly earnings for those who would work whether assigned to the program group or the control group based on Lee (2009). In most cases, the bounds include zero, but with some notable exceptions. In particular, the Florida PREP program reduced quarter two earnings between \$233 and \$362 and quarter four earnings between \$111 and \$254. In contrast, the Florida REA program increased quarter three earnings between \$13 and \$362. In both cases, though, we have disappointingly large standard errors. The bounds for the Nevada REA program always include zero, which implies that we cannot rule out the view that the positive overall earnings component for this program results from selection of high earners into work.

Effects on Conditional UI Exit Probabilities

As described in Section IV, three main mechanisms drive the effects we observe: threat effects, monitoring effects, and services effects. The data do not allow us to directly measure the threat or services effects but they do provide information on the number of participants disqualified each week following their eligibility review, which serves as our proxy for monitoring effects. As in Black *et al*. (2003b), we use temporal variation in program effects on exit from UI within spells to sort among threat effects and services effects, with the threat effects expected early on in the spell and the services effects later on, after receipt of services and completion of program requirements. Operationally, we estimate treatment-control differences in the probability of exiting UI in a given week, conditional on not exiting in a prior week, using this linear probability model:

$$H_{ti} = a_t + b_t \cdot T_i + X_i \cdot c_t + v_{ti}. \qquad [4]$$

In [4], $H_{ti} \in \{0,1\}$ indicates that individual i exited UI in week t of their claim, while T_i and X_i have the same meanings as in [1]. We estimate [4] for weeks $t = 1, \ldots, 25$, in each case using only claims still in progress at the start of week t. In technical terms, we estimate the parameters of the (assumed linear) discrete time hazard function $\Pr(H_t = 1 | H_1 = 0, \ldots, H_{t-1} = 0, T, X)$, which captures the conditional probability that a UI claim ends at duration *t* given that it did not end prior to *t*. This setup allows the effect of the program (b_t) to vary with *t* and thus to inform us about the relative substantive importance of threat and services effects.

Because we condition the estimation of [4] on having a spell in progress at duration *t*, we lose the interpretational simplicity of random assignment for $t > 1$. As pointed out in Black *et al.* (2003b), for $t > 1$ the subsets of observations from both the treatment group and the control group reflect selective exit from UI, with the result that simple comparisons of the two groups combine the causal effect of treatment on the exit hazard with the selection effect. We address this selection issue by including conditioning variables X_i in [4] and allowing their coefficients c_t to vary with claim duration. Under a conditional independence assumption, this suffices to solve the selection problem. As the available covariates do not, ex ante, obviously suffice for conditional independence, we also rely on the analyses in Michaelides and Mueser (2018; 2019) who find little evidence of dynamic selection in our context.[31]

Figure 2 summarizes the results from this analysis. The plot for each program displays the estimated effect and associated 95 percent confidence intervals. The plots for the three REA programs also display the proportion of claims disqualified during the review. Florida PREP had positive effects on exits in weeks 3-7, which reach their statistically significant maximum at 1.1 percentage points in week seven. The week seven effect reflects participant exits immediately after the meetings were scheduled. As PREP had no monitoring effects, the week seven effect likely reflects treatment cases exiting to avoid program requirements. Later weeks show a mix of

[31] They estimate frailty models and also conduct a bounding exercise in the spirit of Lee (2009).

positive and negative effects, not large or precisely estimated enough to distinguish statistically from zero.[32]

Florida REA had a statistically significant positive effect in week four, after treatment group cases received notification of program requirements and during the first week of eligibility reviews. About half the effect reflects disqualifications; the remaining component likely results from treatment cases leaving UI to avoid the review. Positive effects in weeks 5-7 (solid line) lack statistical significance and largely reflect disqualifications of no-shows and ineligibles (dashed line). Starting in week nine, estimates turn out mostly small and noisy, other than the 1.2 and 1.5 percentage-point effects in weeks 16 and 25, respectively. This pattern signals the modest empirical importance of services effects for this program.[33]

The Idaho program had a positive and statistically significant effect of 1.9 percentage points in week five, immediately following the week four deadline for completing the online review. The dashed line indicates that disqualifications account for about half of this effect; the remaining component likely arises from threat effects. In weeks 6-8, program cases continued to exit UI at higher rates than control cases, but effects become smaller and not statistically distinguishable from zero and disqualifications play a proportionately smaller role. After week eight, just seven of 17 estimates are positive and only a 2.0 percentage-point effect in week 11 is statistically significant. Overall, the estimates after week eight, when we would expect to see any service effects, suggest that threat and monitoring effects mattered most for Idaho REA.[34]

Finally, Nevada REA has large positive impacts in weeks 2-5, the period just after treatment

[32] Using the estimated effects from [4], we find a program effect on the cumulative probability of exiting UI of 1.3 percentage points in weeks 1-8 and 4.1 percentage points in weeks 1-25.
[33] Using the estimated effects from [4], we find a program effect on the cumulative probability of exiting UI of 1.6 percentage points in weeks 1-8 and 6.5 percentage points in weeks 1-25.
[34] Using the estimated effects from [4], we find a program effect on the cumulative probability of exiting UI of 4.8 percentage points in weeks 1-8 and 6.3 percentage points in weeks 1-25.

cases receive their notification letter in week one and during which most program meetings were scheduled. The overall effects, which range from 0.9 to 4.2 percentage points, far exceed the disqualification rates, which range from 0.1 to 0.5 percentage points, so voluntary exits dominate. We also obtain positive effects on UI exits in weeks 11-16, including statistically significant effects of 1.8, 2.9, and 3.1 percentage points in weeks 12, 13, and 16, respectively, with another positive and statistically significant effect in week 23. Taken as a whole, we interpret the pattern of estimates for Nevada REA as indicating the importance of services received as well as of monitoring and threat effects.[35]

Sensitivity Analyses

Freedman (2008) raises concerns about the use of parametric linear models to reduce variance and address any covariate imbalance that remains following random assignment, as we do in our primary estimates presented above. To console readers who may share these concerns, we calculate two alternative sets of impacts. The first set of estimates replaces the linear model with inverse propensity weighting; the online appendix describes how we constructed these estimates and displays them in Table A5. The second set of alternative estimates consists of simple mean differences with no conditioning; these estimates appear in online appendix Table A6. Both sets of alternative estimates closely resemble those described above.

VII. COSTS AND BENEFITS

Full cost-benefit analyses of the four programs we study lie well beyond the scope of this paper; instead, we offer some cost estimates and a brief sketch of what a more complete analysis would contain and of where we think it would end up.[36] The bottom row of Table 5 presents

[35] Using the estimated effects from [4], we find a program effect on the cumulative probability of exiting UI of 12.3 percentage points in weeks 1-8 and 22.4 percentage points in weeks 1-25.

[36] See e.g., McCall *et al*. (2016) for an extended discussion of cost-benefit analysis for ALMPs.

direct costs per program group member for three of the programs and bounds on the cost of the remaining one; the table notes detail the source of the numbers. Especially for the Florida and Idaho programs, the numbers tell a simple story: these programs cost remarkably little. In fact, all four programs produced average UI savings that exceeded program costs.

From the standpoint of a social cost-benefit analysis, UI payments wash out as a transfer. Thus, we compare the costs to the earnings impacts that, in a simple model, represent increases in total output. Comparing the cost numbers to the point estimates of the impacts on the sum of earnings in the four calendar quarters after random assignment (given in Table 6) indicates that the increase in total output exceeded average program costs in the Florida and Nevada programs but not in the Idaho program. These comparisons allow us to conclude that the Florida and Nevada programs all pass social cost-benefit tests while the Idaho program fails.

Four additional factors potentially cloud this clear and simple picture. First, the cost numbers omit the marginal social cost of public funds (MSCPF), which includes the direct cost of operating the tax system and the indirect costs of the distortions it induces. Because of the small direct costs of the program, even the larger values for the MSCPF offered in the literature do not change the cost-benefit lesson. Second, we follow the literature in ignoring the value of leisure; see Greenberg and Robins (2008). Third, we omit consideration of other outcomes possibly affected by the programs, such as the health of the participant, outcomes of the participant's family members, and so on. We expect most of these to sit on the positive side of the ledger, so that including them would only make the cost-benefit performance of the programs stronger.

Finally, reemployment programs may have spillover effects on individuals with no ties to the participants. In our context, we expect primarily displacement effects (rather than effects working through skill prices). Lise *et al*. (2004) provide a conceptual framework while Crépon *et* *al.* (2013) and De Giorgi (2005) provide contrasting empirical estimates. The literature on spillovers is small and its findings mixed; the weak light it provides suggests that taking account of spillovers would non-trivially reduce, but not eliminate, the excess of benefits over costs.

VIII. INTERPRETING DIFFERENCES IN IMPACTS AMONG PROGRAMS

Our impact estimates reveal important substantive differences in the nature and size of the impacts of the four programs we study; in this section we interpret these differences in terms of the causal mechanisms laid out in Section 3. To start, recall that Florida PREP had smaller effects on UI duration and benefit amounts collected than Florida REA or Idaho REA. In these three programs, detectable positive effects on employment and earnings did not accompany the reductions in UI receipt. Our analyses of UI exits show that these programs primarily led youth UI recipients to exit UI around the time that program activities were scheduled. These early UI exits consisted mostly of voluntary participant exits, combined with disqualifications based on the eligibility review in Florida REA and Idaho REA. For these three programs, we find only small effects on UI exits after participants fulfill the program requirements, effects that do not translate into employment gains. Overall, our findings suggest that these three programs pushed youths to exit the program to avoid program requirements rather than providing them with services that materially aided their job search.

These findings broadly comport with results for adults in earlier studies of U.S. programs that included services referrals (Black *et al.*, 2003b; Decker *et al.*, 2000), or monitoring activities (Klepinger *et al.*, 2003; Michaelides and Mueser, 2019), but no strong services components. In this prior work, positive effects on early UI exit tended to go hand-in-hand with positive short-term effects on employment and earnings, suggesting that the programs reduced morally hazardous behavior among UI recipients not actively searching for a job or with readily available job options. We find limited evidence that the Florida and Idaho programs improved employment outcomes for youth UI recipients, which makes this story less plausible in our context. Indeed, some European literature – e.g., Micklewright and Nagy (2010), Engström *et al*. (2012), Maibom *et al*. 2014, and Caliendo and Schmidl (2016) – raises the possibility that the added scrutiny imposed by the programs may so discourage some participants that they exit UI even without good job options.[37]

Among our four programs, only Nevada REA clearly helped youth UI recipients exit UI more quickly *and* find jobs and improve their earnings. When viewed in light of the absence of clear employment effects for the Idaho and Florida programs, this pattern points to an alternative mechanism in which mandatory job counseling, which only the Nevada program provided, increases the quality (and perhaps intensity) of participants' job-search activities. Evidence from European programs that engage unemployed youth with job counseling, either stand-alone or in combination with monitoring (e.g., Graversen and van Ours, 2008; Crépon *et al.*, 2013; Hägglund, 2014; Caliendo and Schmidl, 2016), lends some support to this story.

Treatment effect heterogeneity provides a possible alternative explanation for the larger impacts of the Nevada REA program. In this story, reemployment programs have larger effects on youths with certain characteristics, and Nevada REA has relatively more youths with those characteristics. To the extent that we observe relevant characteristics, we can test this explanation by reweighting the Nevada sample to have the same distribution of observed characteristics as each of the other three program samples using the methodology laid out in DiNardo *et al*. (1996). The online appendix details our application of their scheme and our

[37] In their review of the European literature on programs targeting unemployed youth, Caliendo and Schmidl (2016) note: "A potential downside of [monitoring schemes] is that they may result in a direct withdrawal from the labor market when monitoring and sanctions are imposed too fiercely."

findings, which are presented in Table A7. We find no evidence that the relatively large effects in Nevada are driven by treatment effect heterogeneity associated with observed characteristics.

IX. CONCLUSIONS

This study considers the effects of four broadly representative reemployment programs on youth UI recipients in the U.S. during the Great Recession. All four programs reduced the average duration of UI claims and yielded savings for state UI programs. While we would expect effects on labor market outcomes to accompany reductions in time on UI, only the Nevada program led to substantive improvement in participants' employment and earnings. Further analyses indicate that the Florida and Idaho programs mainly caused early exits via disqualifications due to failed or skipped eligibility reviews combined with voluntary exits due to threat effects. In contrast, while the Nevada REA program also led some youth to exit UI early in their spells for the same reasons as in the other states, it alone had substantive effects on UI exits after most participants had met their service requirements. We think these later effects result from the job counseling services that the Nevada program provided but the others did not.

One of our motivations for this study concerns the use of a one-size-fits-all-ages policy in this domain. Table 8 directly addresses this motivation by presenting program effects for adults corresponding to (a subset of) the ones for youth given in Tables 5 and 6, along with estimated impact differences and their standard errors. Observed differences for adults and youths in effect estimates on the total weeks receiving benefits or the dollar value of benefits are not consistently signed, nor are any of the differences statistically significant. For Idaho and Nevada, effects on earnings are substantially greater for adults, although the difference is only statistically significant for Idaho. Overall, the basic pattern of the relative effects for the four programs is similar for youth and adults.

Taken together, our findings have important policy implications. First, reemployment programs that focus on monitoring activities but do not include a job counseling component represent a poor programmatic choice during recessions. Second, programs that provide unemployed youth with job counseling early in their UI spells can help them achieve better labor market outcomes. Such programs clearly represent a fiscal “win” for state UI systems and should easily pass a social cost-benefit test. Third, and finally, we find only very modest evidence that a one-program-fits-all-ages strategy represents a policy error in this domain.

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TABLE 1
Characteristics of Youth UI Recipients
Eligible for Reemployment Programs

	Florida	Idaho	Nevada
Sample Size	6,524	1,956	2,767
PREP	0.322	--	--
REA	0.398	0.785	0.162
Control	0.281	0.215	0.838
Female	0.447	0.344	0.436
White	0.553	0.797	--
Black	0.215	0.006	--
Other race	0.232	0.198	--
Hispanic	0.099	0.156	0.261
Disabled	0.013	0.032	0.063
No high school diploma	0.124	0.146	0.216
High school diploma	0.713	0.496	0.535
Some college/college degree	0.162	0.358	0.249
White collar, high skill†	0.200	0.096	0.090
White collar, low skill	0.386	0.235	0.428
Blue collar, high skill	0.260	0.300	0.258
Blue collar, low skill	0.154	0.369	0.224

Note: Reported are sample proportions.
† Occupation of prior employment: *White collar, high skill* includes management, healthcare practitioner, business and financial, computer and mathematical, architecture and engineering, and life, physical and social science, and legal occupations; *white collar, low skill* includes office and administrative support, sales, education, training, and library, healthcare support, arts and entertainment, and community and social services occupations; *blue collar, high skill* includes production, transportation, installation, maintenance, and repair, protective services, and military occupations; and *blue collar low skill* includes construction and extraction, food preparation and serving, building cleaning and maintenance, personal care and services, and agricultural occupations. GED recipients are included in the "no high school diploma" category.
Source: State UI claims data.

TABLE 2
Prior Earnings and UI Eligibility of Youth UI Recipients
Eligible for Reemployment Assistance Programs

	Florida	Idaho	Nevada
Prior earnings			
Quarter 1 prior to entry	3,945 (2,754)	4,183 (3,242)	4,247 (3,404)
Quarter 2 prior to entry	3,994 (2,635)	3,568 (2,698)	4,133 (3,229)
Quarter 3 prior to entry	3,973 (2,686)	3,063 (2,849)	3,954 (3,230)
Quarter 4 prior to entry	3,723 (2,821)	3,597 (2,832)	3,861 (3,356)
Quarter 5 prior to entry	3,361 (2,760)	4,086 (3,392)	3,765 (3,400)
Quarter 6 prior to entry	3,033 (2,821)	2,754 (2,722)	3,037 (3,133)
Quarter 7 prior to entry	2,765 (2,732)	2,316 (2,525)	2,789 (3,233)
Quarter 8 prior to entry	2,615 (2,708)	2,527 (2,631)	2,579 (3,010)
Regular UI weeks eligibility	19.7 (4.8)	18.6 (5.5)	21.3 (4.8)
Regular UI cumulative entitlement ($)	3,889 (1,954)	3,817 (2,154)	4,952 (2,718)
EUC weeks eligibility	40.2 (9.8)	37.9 (11.2)	43.3 (9.9)
EUC cumulative entitlement ($)	7,932 (3,994)	7,785 (4,389)	10,048 (5,472)
EB weeks eligibility	13.9 (4.5)	14.5 (4.3)	16.5 (3.8)
EB cumulative entitlement ($)	2,720 (1,486)	2,978 (1,678)	3,818 (2,071)
Total weeks eligibility	73.8 (17.5)	71.0 (20.9)	81.1 (18.5)
Total cumulative entitlement ($)	14,541 (7,246)	14,579 (8,219)	18,817 (10,259)

Note: Reported are sample means with standard deviations in parentheses.
Source: State UI claims data (UI eligibility measures); state UI wage records (prior earnings).

TABLE 3
Meeting Schedule, Completions, and Disqualifications for Program Cases

	Florida PREP	Florida REA	Idaho REA	Nevada REA
Total	2,099	2,595	1,535	447
UI week 1	--	--	--	--
2	13 (1%)	35 (1%)	--	98 (22%)
3	39 (2%)	66 (2%)	--	142 (32%)
4	797 (38%)	990 (38%)	--	99 (22%)
5	848 (40%)	978 (38%)	--	67 (15%)
6	402 (19%)	526 (20%)	184 (12%)	27 (6%)
7	--	--	92 (6%)	9 (2%)
8	--	--	21 (1%)	5 (1%)
9	--	--	7 (<1%)	--
10	--	--	4 (<1%)	--
Completions	1,370 (65%)	2,309 (89%)	N/A	389 (87%)
Disqualifications				
No-shows	--	19 (0.7%)	18 (1.2%)	5 (1.1%)
Ineligibles	--	11 (0.4%)	8 (0.5%)	2 (0.4%)
Total	--	30 (1.1%)	26 (1.7%)	7 (1.5%)

Note: Reported are the numbers of program cases with the sample proportions in parentheses. For Florida and Idaho, the dates specify the original scheduled meetings; we lack data on actual meetings dates for postponed meetings. For Nevada, the dates include postponements, so the date we use indicates either when the meeting actually occurred or the final “missed” date. The values in the last four rows do not sum to 100% because some treatment cases exit UI prior to the meeting date and others are exempt from the meeting requirement due to service receipt.
Source: Employment service data.

TABLE 4
Service Take-Up Rates, Program vs. Control Group, Nevada REA

	Program	Control	*Difference*
Any job-counseling service	0.613	0.081	*0.532 [0.019]****
Work search plan	0.515	0.050	*0.465 [0.016]****
Resume assistance	0.248	0.021	*0.228 [0.011]****
Individual needs assessment	0.293	0.032	*0.262 [0.013]****
Job referral	0.169	0.033	*0.136 [0.013]****
Group orientation	0.237	0.031	*0.206 [0.013]****
Job-search workshops	0.098	0.009	*0.089 [0.008]****

Note: Job-counseling services include the work search plan, resume assistance, individual needs assessment, and job referrals. They do not include group orientations, job-search workshops, and the eligibility review.
Source: Nevada employment service data.
*** $p<0.01$.

TABLE 5
Effects on Unemployment Insurance Receipt

	Florida PREP	Florida REA	Idaho REA	Nevada REA
Weeks on UI				
Regular	-0.07 (0.22) [<-1%]	-0.20 (0.23) [-2%]	-0.90 (0.26)*** [-6%]	-2.10 (0.38)*** [-12%]
EUC	-1.05 (0.57)* [-6%]	-1.68 (0.57)*** [-9%]	-0.76 (0.79) [-7%]	-2.06 (0.97)** [-15%]
Total	-1.12 (0.73) [-3%]	-1.88 (0.74)** [-5%]	-1.67 (0.94) [-6%]	-4.16 (1.19)*** [-13%]
Benefits Collected				
Regular UI	-10 (55) [<-1%]	-31 (55) [-1%]	-225 (66)*** [-7%]	-373 (108)*** [-10%]
EUC	-179 (123) [-5%]	-325 (124)*** [-9%]	-150 (186) [-6%]	-266 (244) [-8%]
Total	-168 (165) [-2%]	-356 (166)** [-5%]	-375 (225)* [-7%]	-639 (312)** [-9%]
Exhausted Regular UI	-0.028 (0.016)* [-4%]	-0.026 (0.016)* [-4%]	-0.055 (0.027)** [-9%]	-0.138 (0.026)*** [-23%]
Collected EUC	-0.025 (0.016) [-4%]	-0.035 (0.016)** [-5%]	-0.048 (0.027)* [-10%]	-0.113 (0.027)*** [-23%]
Exhausted EUC	-0.028 (0.013)** [-14%]	-0.024 (0.013)* [-12%]	0.006 (0.017) [+5%]	-0.014 (0.019) [-9%]
Cost per Participant†	$21-34	$54	$12	$201

Note: Each cell contains the average treatment effect, robust standard error in parentheses, and the treatment effect expressed as a percentage of the control group mean in brackets. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.
† Calculated as follows: 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. Florida REA – REA grant amount in 2009 divided by the number of REA referrals in 2009. Nevada REA – REA grant amount plus Wagner-Peyser grant amount used to support the program in 2009 divided by the number of REA referrals in 2009.

TABLE 6
Effects on Employment and Earnings

	Florida PREP	Florida REA	Idaho REA	Nevada REA
Employed				
Quarter of entry	-0.010 (0.012) [-1%]	-0.004 (0.012) [-<1%]	0.016 (0.019) [+2%]	-0.006 (0.0184) [-1%]
Quarter 1 after entry	0.009 (0.016) [+3%]	0.008 (0.016) [+2%]	0.013 (0.028) [+3%]	0.096 (0.027)*** [+22%]
Quarter 2 after entry	0.032 (0.017)* [+8%]	0.026 (0.017) [+7%]	0.036 (0.027) [+6%]	0.086 (0.027)*** [+17%]
Quarter 3 after entry	0.028 (0.017)* [+6%]	0.027 (0.017) [+7%]	-0.018 (0.027) [-3%]	0.047 (0.026)* [+8%]
Quarter 4 after entry	0.026 (0.017) [+5%]	0.016 (0.017) [+3%]	-0.023 (0.027) [-3%]	0.040 (0.025) [+7%]
Earnings				
Quarter of entry	-35 (56) [-2%]	15 (58) [+1%]	2 (107) [+<1%]	-43 (96) [-2%]
Quarter 1 after entry	49 (59) [+6%]	38 (60) [+1%]	-53 (86) [-6%]	154 (113) [+14%]
Quarter 2 after entry	46 (78) [+3%]	39 (81) [3%]	68 (115) [+4%]	332 (137) ** [+20%]
Quarter 3 after entry	144 (88) [+3%]	138 (91) [+8%]	-224 (158) [-9%]	405 (161)** [+19%]
Quarter 4 after entry	60 (105) [+3%]	-18 (106) [-1%]	-166 (149) [-7%]	392 (168)** [+17%]
Total, quarters 1-4	300 (282) [5%]	197 (289) [3%]	-375 (408) [-5%]	1,283 (467)*** [+18%]

Note: Each cell contains the average treatment effect, robust standard error in parentheses, and the treatment effect expressed as a percentage of the control group mean in brackets.
* p<0.1; ** p<0.05; *** p<0.01.

Table 7: Decomposition of Program Effects on Earnings
and Lee Bounds for Effects on Conditional Earnings

	Florida PREP	Florida REA	Idaho REA	Nevada REA
Quarter 1 after entry				
Decomposition of program effect				
Employment component	-6 (36)	-6 (30)	25 (49)	252 (62)***
Earnings component	-9 (43)	15 (41)	-90 (68)	-42 (79)
Lee bounds, conditional earnings				
Lower bound	-44 (160)	21 (153)	-387 (324)	-1,138 (204)***
Upper bound	44 (351)	118 (335)	-141 (189)	436 (259)*
Quarter 2 after entry				
Decomposition of program effect				
Employment component	12 (50)	-32 (50)	92 (75)	345 (90)***
Earnings component	-105 (55)*	-35 (53)	-50 (92)	105 (93)
Lee bounds, conditional earnings				
Lower bound	-362 (350)	-171 (192)	-403 (252)	-740 (220)***
Upper bound	-233 (187)	153 (291)	69 (215)	827 (259)***
Quarter 3 after entry				
Decomposition of program effect				
Employment component	10 (59)	-46 (59)	-48 (103)	252 (107)**
Earnings component	-7 (61)	52 (59)	-75 (144)	296 (120)**
Lee bounds, conditional earnings				
Lower bound	-81 (362)	13 (185)	-197 (285)	-331 (294)
Upper bound	6 (187)	362 (257)	132 (493)	957 (317)***
Quarter 4 after entry				
Decomposition of program effect				
Employment component	19 (64)	-4 (61)	-55 (93)	234 (105)**
Earnings component	-75 (78)	-82 (74)	-94 (127)	202 (123)*
Lee bounds, conditional earnings				
Lower bound	-254 (327)	-172 (200)	-232 (255)	-442 (281)
Upper bound	-111 (204)	-58 (457)	150 (418)	745 (301)**

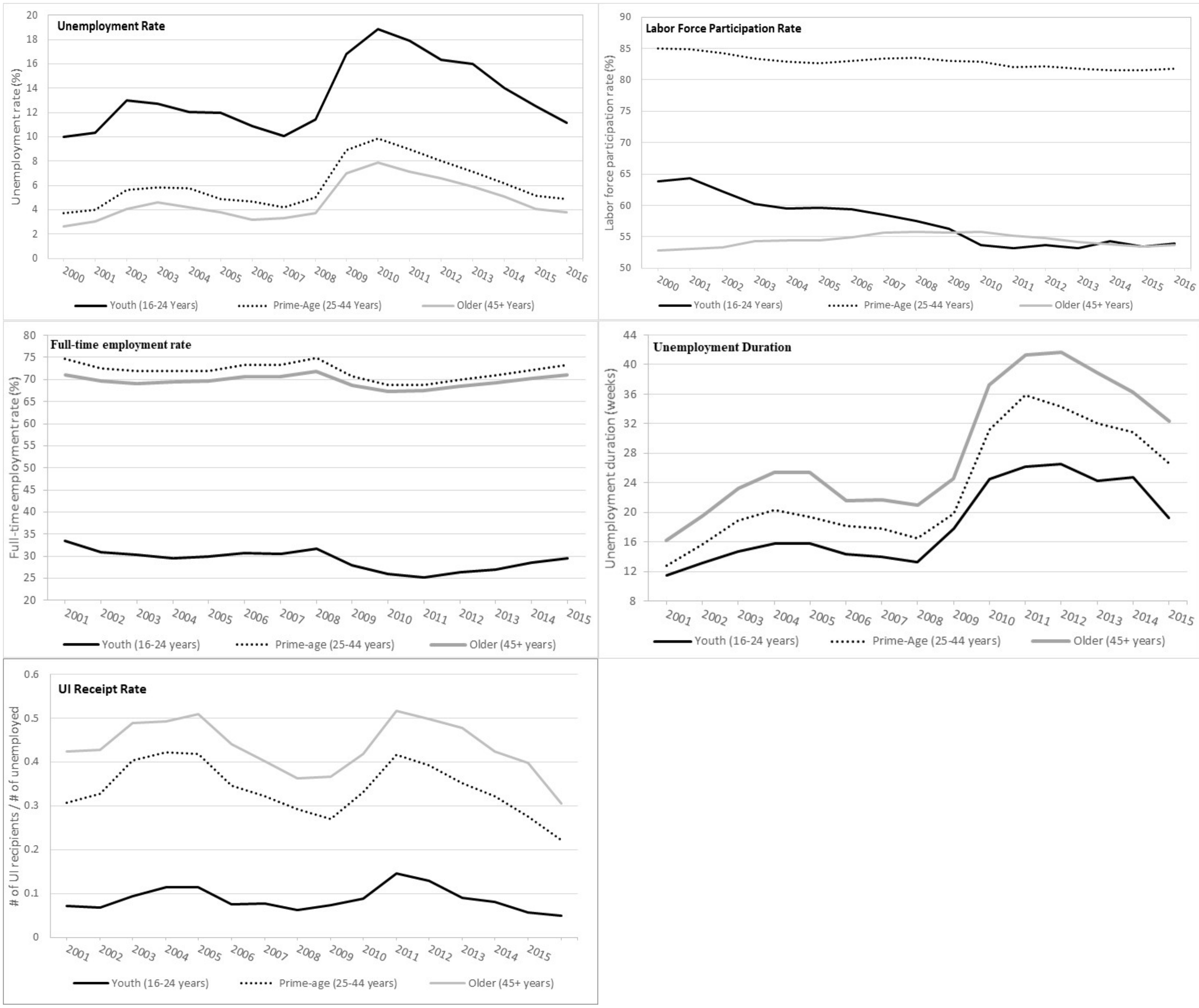
Note: The decomposition exercise does not include covariates. "Conditional earnings" refers to earnings conditional on employment (i.e., on earnings > 0). We report estimates and, in parentheses, their associated standard errors.
* p<0.1; ** p<0.05; *** p<0.01.

TABLE 8
Program Effects for Adults and Youth-Adult Differences

	Florida PREP	Florida REA	Idaho REA	Nevada REA
Total weeks on UI	-0.67 (0.27)** 0.45 [0.78]	-1.89 (0.27)*** -0.01 [0.79]	-1.23 (0.46)*** 0.44 [1.05]	-3.67 (0.47)*** 0.49 [1.28]
Total benefits collected	-183 (72)** -15 [180]	-489 (71)*** -133 [181]	-315 (142)** 60 [266]	-937 (162)*** -298 [352]
Earnings				
Quarter 1 after entry	66 (63) 17 [86]	66 (61) 28 [86]	67 (82) 120 [119]	341 (68)*** 187 [132]
Quarter 2 after entry	53 (57) 7 [97]	85 (58) 46 [100]	128 (74)* 60 [137]	499 (75)*** 167 [156]
Quarter 3 after entry	-24 (61) -168 [107]	35 (60) -103 [109]	194 (92)** 418 [183]**	517 (87)*** 112 [183]
Quarter 4 after entry	-1 (64) -61 [123]	37 (64) 55 [124]	103 (94) 269 [176]	531 (168)** 139 [238]
Total, quarters 1-4	94 (205) -206 [349]	223 (205) 26 [354]	492 (276)* 867 [493]*	1,889 (267)*** 606 [538]

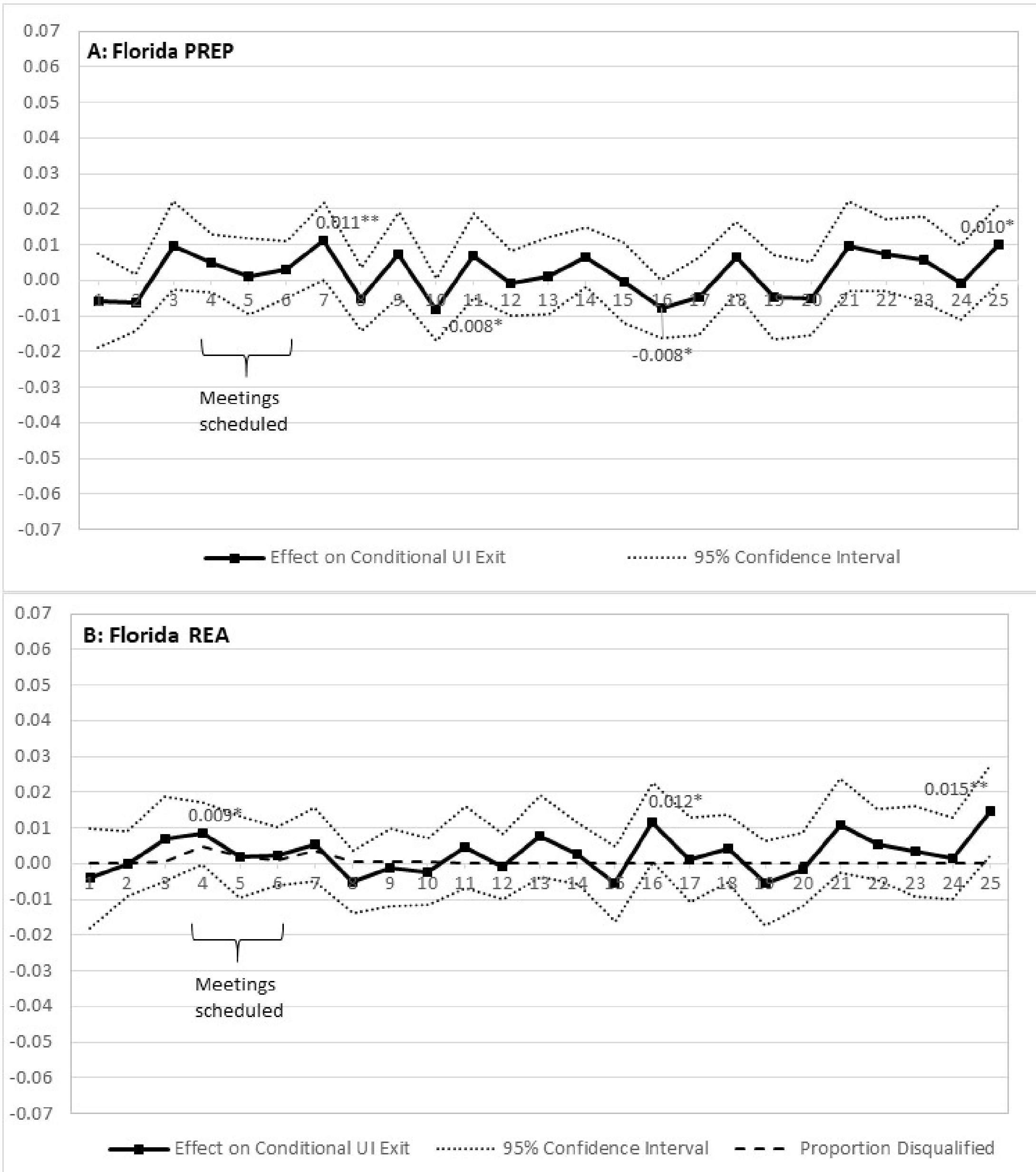
Note: The top two numbers in each cell are the estimated effects for adults and, in parentheses, the associated robust standard error. The bottom two numbers in each cell are the estimated difference in effects between adults and youth (adult estimate - youth estimate) and, in square brackets, its robust standard error.
* p<0.1; ** p<0.05; *** p<0.01.

FIGURE 1
Youth Workers during the Great Recession



Note: The unemployment rate is the number of unemployed workers divided by the number in the labor force. The labor force participation rate is the number of labor force participants divided by the population. The full-time employment rate is the proportion of employed workers who worked full time. The unemployment duration is the average number of weeks spent unemployed (not necessarily collecting UI) among unemployed workers. The UI receipt rate is the number of UI recipients divided by the number of unemployed workers. Sources: Authors' tabulations of the Current Population Survey (accessed at https://cps.ipums.org/cps/), except the number of UI recipients, which is based on U.S. Department of Labor reports (https://oui.doleta.gov/unemploy/chariu.asp).

FIGURE 2
Program Effects on the Conditional Probability of UI Exit



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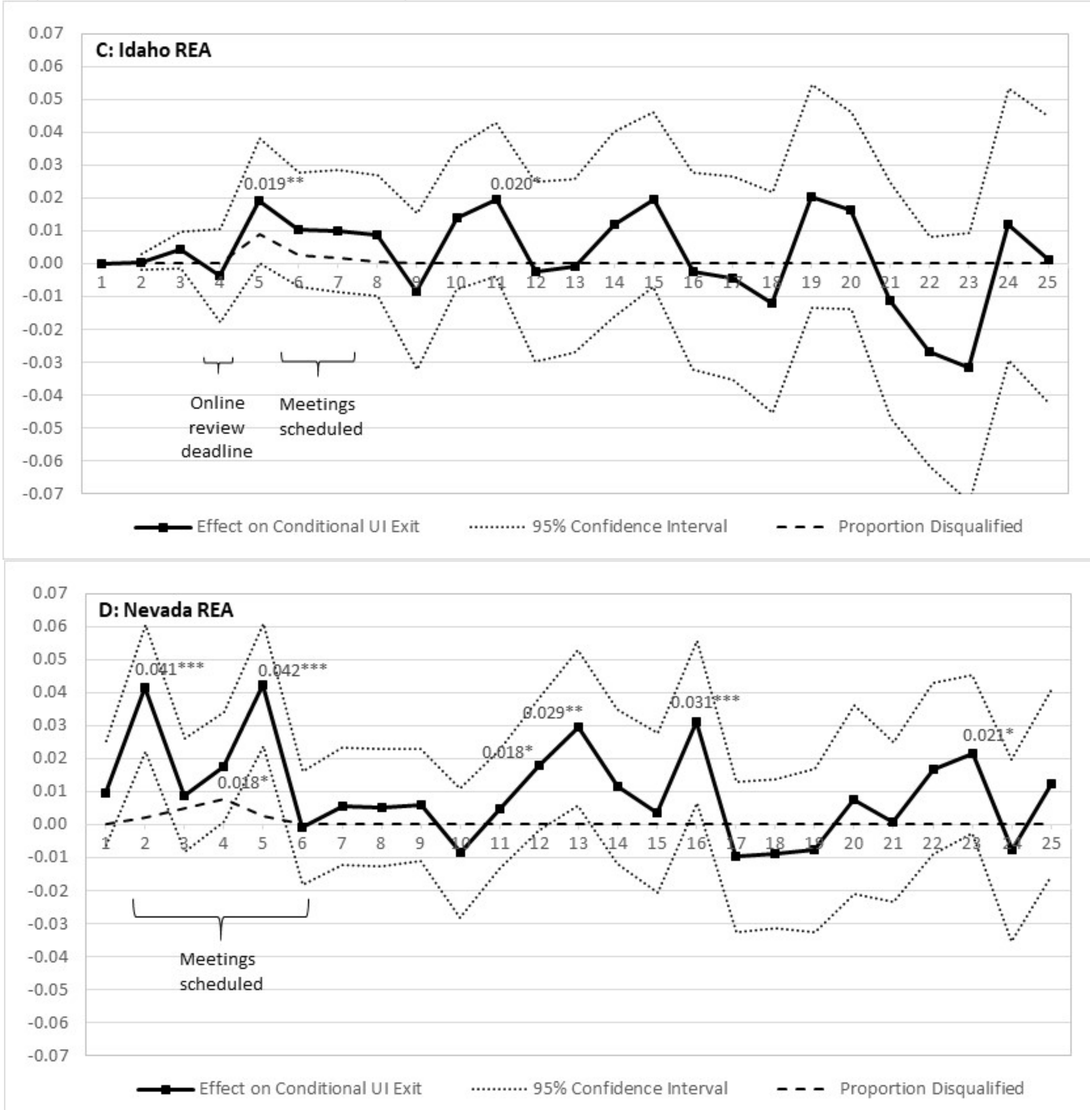


Note: Program effect on the conditional probability of UI exit (solid line) and the associated 95 percent confidence interval (dotted lines). The dashed lines for Florida REA, Idaho REA, and Nevada REA report the proportion of no-shows and ineligibles disqualified each week.
* p<0.1; ** p<0.05; *** p<0.01.

Do Reemployment Programs for the Unemployed Work for Youth?
Evidence from the Great Recession in the United States

Marios Michaelides, Peter Mueser, and Jeffrey Smith

Online Appendix

Version of May 2020

Program Effects using Inverse Probability Weighting

The evidence in Table A2 indicates that the program and control groups had similar means of observed characteristics, though in a few cases we found statistically significant differences. Although we control for all of the observed characteristics in a simple regression model when generating the impact estimates in Tables 5 and 6, to check for possible specification problems we produce another set of experimental impact estimates using inverse propensity weighting. This process involves three steps. First, we estimate the probability of program assignment based on observed characteristics (X_i), including week and workforce region, using a logistic regression model.[1] Second, we estimate the propensity score for each program and control case as $\hat{p}_i = \frac{\exp(\hat{\beta} X_i)}{1+\exp(\hat{\beta} X_i)}$, where $\hat{\beta}$ is the coefficient estimate based on the logistic regression. Third, we compute weights based on the propensity score as follows: $w_i = \frac{1}{\hat{p}_i} / \sum_{T_i=1} \frac{1}{\hat{p}_i}$ if $T_i = 1$, and $w_i = \frac{1}{1-\hat{p}_i} / \sum_{T_i=0} \frac{1}{1-\hat{p}_i}$ if $T_i = 0$, where w_i denotes the weight for individual *i* and T_i is an indicator equal to 1 if individual *i* is in the program group and 0 if individual *i* is in the control group. We dropped observations outside the common support.[2]

Table A4 presents estimates from a linear probability model of assignment to the treatment group estimated using the reweighted sample. The weighting eliminates all statistically different differences; moreover, the coefficient estimates are substantively small implying a high degree of balance for all four programs. Using the reweighted balanced samples, we estimate program effects on UI weeks, UI benefits, and earnings by simply calculating differences in mean

[1] These are the same covariates included in model (1) in the main text.

[2] For simplicity, we impose the common support by including only cases with estimated propensity scores between the smallest $\hat{p}_i$ for the treatment group and the largest $\hat{p}_i$ for the control group. Using the common support condition, we dropped: 10 program and 11 control cases for Florida PREP; 80 program and 36 control cases for Florida REA; 42 program cases for Idaho REA, and 1 program and 179 control cases for Nevada REA.

outcomes for the weighted program and control groups. The impact estimates from this exercise, shown in Table A5, are not substantively different from the estimates with linear conditioning presented in Tables 5 and 6, which implies the absence of important specification problems in this context. In addition, Table A6 presents simple mean differences in outcomes between treatment and control groups. These impact estimates do not substantively differ from those in Tables 5, 6, and A5, providing further evidence of the unimportance of covariate imbalance.

Program Effects Accounting for Characteristics of Population Served

Using the DiNardo, Fortin and Lemieux (1996) framework, we estimate the effects of the Nevada REA program using weights that adjust the characteristics distribution of the Nevada population to match the characteristics distribution of the populations served by each of the other three programs. For example, to adjust the Nevada REA population using the Florida PREP population as the baseline, we: (1) merged Nevada REA with Florida PREP data; (2) estimated a probit model on the likelihood of being in Florida PREP based on observed characteristics that are common to both programs; (3) calculated the predicted probability to construct weights for Nevada program and control group members; and (4) estimated program effects for Nevada REA using the weighted sample. A similar process was used to reweight the Nevada sample based on the Florida REA and Idaho REA samples.

Table A7 presents the estimated impacts of the Nevada REA program using the population of each of the other programs as a baseline. The reweighted estimates for the Nevada program are very similar to those presented in Tables 5 and 6 using the unweighted Nevada population. This implies that the average effect of the program does not vary very much with the characteristics we employ in the reweighting. In a series of similar analyses (not shown) we reweighted the estimates for each of the other programs; in every case we found no substantive differences. Note that while we do not find variation in conditional average treatment effects based on the variables included in our reweighting, other variables not available in our data may still capture variation in the average treatment effects of these programs.

TABLE A1
Characteristics of Experienced Unemployed Youth in 2009

	Florida	Idaho	Nevada	National
Female	0.505	0.509	0.480	0.492
White	0.739	0.911	0.713	0.759
Black	0.166	0.010	0.064	0.109
Other race	0.095	0.079	0.223	0.132
Hispanic	0.227	0.128	0.308	0.163
No high school diploma	0.171	0.245	0.203	0.195
High school diploma	0.419	0.420	0.456	0.400
Some college/college degree	0.410	0.335	0.342	0.405
White collar, high skill†	0.052	0.056	0.058	0.060
White collar, low skill	0.460	0.377	0.438	0.428
Blue collar, high skill	0.146	0.158	0.169	0.167
Blue collar, low skill	0.342	0.409	0.335	0.346

Note: Reported are sample proportions. Experienced unemployed youth includes unemployed workers under the age of 25 with prior employment experience.
† Occupation of prior employment. See Table 1.
Source: American Community Survey (accessed at https://usa.ipums.org/usa/).

TABLE A2
Linear Probability Model, Probability of Program Assignment

	Florida PREP	Florida REA	Idaho REA	Nevada REA
Female	0.027 (0.017)	0.033 (0.015)**	0.011 (0.022)	-0.002 (0.015)
White	--	--	--	--
Black	-0.009 (0.020)	0.028 (0.018)	-0.143 (0.126)	--
Other race	-0.002 (0.021)	-0.010 (0.017)	0.016 (0.033)	--
Hispanic	-0.012 (0.028)	-0.005 (0.023)	-0.037 (0.038)	0.011 (0.016)
No high school diploma	--	--	--	--
High school diploma	-0.021 (0.024)	-0.021 (0.021)	-0.016 (0.029)	-0.008 (0.018)
Some college/college deg.	-0.070 (0.030)**	-0.041 (0.026)	-0.019 (0.031)	0.075 (0.021)***
Disabled	-0.038 (0.070)	-0.050 (0.058)	0.026 (0.054)	0.022 (0.029)
White collar, high skill	--	--	--	--
White collar, low skill	-0.008 (0.022)	-0.014 (0.019)	-0.014 (0.036)	-0.015 (0.026)
Blue collar, high skill	-0.020 (0.024)	-0.007 (0.021)	0.008 (0.036)	0.041 (0.027)
Blue collar, low skill	0.003 (0.028)	0.014 (0.024)	-0.030 (0.036)	-0.011 (0.028)
Prior earnings (in $000s)				
Quarter 1 prior to entry	-0.0022 (0.0048)	-0.0002 (0.0041)	0.0162 (0.0043)***	0.0016 (0.0029)
Quarter 2 prior to entry	0.0008 (0.0060)	-0.0033 (0.0051)	-0.0153 (0.0058)***	-0.0017 (0.0036)
Quarter 3 prior to entry	-0.0008 (0.0057)	-0.0005 (0.0048)	0.0078 (0.0059)	0.0008 (0.0037)
Quarter 4 prior to entry	-0.0023 (0.0049)	-0.0010 (0.0045)	0.0029 (0.0054)	-0.0011 (0.0031)
Quarter 5 prior to entry	-0.0047 (0.0054)	-0.0002 (0.0049)	-0.0095 (0.0041)**	0.0034 (0.0037)
Quarter 6 prior to entry	0.0039 (0.0052)	0.0039 (0.0050)	-0.0011 (0.0058)	-0.0011 (0.0034)
Quarter 7 prior to entry	0.0003 (0.0056)	-0.0054 (0.0050)	0.0032 (0.0068)	0.0022 (0.0038)
Quarter 8 prior to entry	-0.0024 (0.0047)	-0.0014 (0.0041)	-0.0004 (0.0056)	0.0039 (0.0034)
Observations	3,944	4,443	1,956	2,767
R-Squared	0.1413	0.2328	0.0543	0.0684

Note: Reported are estimated parameters with standard deviations in parentheses. The models also include indicators for weeks of UI entitlement and interactions between week of UI entry and workforce region. Robust standard errors appear in parentheses.
** $p<0.05$; *** $p<0.01$.

TABLE A3
UI Receipt, Employment, and Earnings of Control Cases

	Florida	Idaho	Nevada
Weeks on UI			
Regular	16.67 (7.30)	16.26 (5.94)	16.93 (7.65)
EUC	18.42 (16.75)	10.83 (14.66)	14.01 (18.11)
Total	35.09 (21.91)	27.10 (17.89)	30.94 (22.19)
Benefits Collected			
Regular UI	3,271 (2,091)	3,346 (1,970)	3,903 (2,725)
EUC	3,560 (3,757)	2,332 (3,516)	3,186 (4,608)
Total	6,831 (5,365)	5,678 (4,831)	7,089 (6,421)
Exhausted Regular UI	0.681	0.637	0.613
Collected EUC	0.668	0.470	0.513
Exhausted EUC	0.207	0.112	0.151
Employed			
Quarter of entry	0.807	0.793	0.835
Quarter 1 after entry	0.351	0.461	0.437
Quarter 2 after entry	0.399	0.570	0.496
Quarter 3 after entry	0.458	0.637	0.566
Quarter 4 after entry	0.503	0.658	0.588
Earnings			
Quarter of entry	2,304 (2,516)	2,242 (2,353)	2,487 (2,529)
Quarter 1 after entry	830 (1,739)	909 (1,671)	1,096 (2,066)
Quarter 2 after entry	1,376 (2,502)	1,624 (2,284)	1,626 (2,521)
Quarter 3 after entry	1,719 (2,729)	2,546 (3,254)	2,086 (2,969)
Quarter 4 after entry	2,079 (3,527)	2,501 (2,961)	2,256 (3,072)
Total, quarters 1-4	6,004 (9,145)	7,581 (8,401)	7,064 (8,624)

Note: Reported are sample proportions or sample means with standard deviations in parentheses.

TABLE A4
Regression Results, Probability of Program Assignment,
Inverse Probability Weighting

	Florida PREP	Florida REA	Idaho REA	Nevada REA
Female	0.027 (.021)	0.008 (.022)	0.011 (.035)	0.001 (.033)
White	--	--	--	--
Black	-0.022 (.026)	0.038 (.027)	0.044 (.168)	--
Other race	0.008 (.025)	0.019 (.025)	-0.023 (.054)	--
Hispanic	-0.008 (.033)	-0.012 (.032)	0.003 (.062)	0.003 (.004)
No high school diploma	--	--	--	--
High school diploma	0.010 (.030)	0.010 (.033)	-0.011 (.046)	-0.015 (.040)
Some college/college deg.	-0.004 (.037)	-0.004 (.039)	0.002 (.050)	0.021 (.045)
Disabled	0.027 (.086)	0.009 (.080)	-0.014 (.088)	0.017 (.062)
White collar, high skill	--	--	--	--
White collar, low skill	-0.016 (.027)	-0.016 (.027)	-0.015 (.058)	0.001 (.056)
Blue collar, high skill	-0.027 (.029)	-0.031 (.030)	-0.026 (.058)	-0.002 (.059)
Blue collar, low skill	-0.001 (.006)	-0.004 (.036)	-0.020 (.057)	-0.000 (.062)
Prior earnings (in $000s)				
Quarter 1 prior to entry	-0.0009 (.0061)	0.0036 (.0061)	0.0028 (.0065)	-0.0013 (.0059)
Quarter 2 prior to entry	0.0055 (.0074)	0.0036 (.0075)	-0.0048 (.0095)	-0.0002 (.0075)
Quarter 3 prior to entry	0.0032 (.0071)	-0.0013 (.0070)	-0.0008 (.0096)	0.0008 (.0077)
Quarter 4 prior to entry	-0.0053 (.0066)	-0.0081 (.0067)	-0.0050 (.0089)	0.0018 (.0072)
Quarter 5 prior to entry	0.0014 (.0068)	-0.0017 (.0071)	0.0026 (.0066)	-0.0020 (.0063)
Quarter 6 prior to entry	0.0053 (.0064)	0.0061 (.0074)	-0.0052 (.0092)	-0.0014 (.0078)
Quarter 7 prior to entry	-0.0033 (.0068)	-0.0052 (.0072)	-0.0009 (.0108)	0.0022 (.0078)
Quarter 8 prior to entry	-0.0048 (.0062)	-0.0023 (.0063)	0.0023 (.0087)	-0.0005 (.0073)
Observations	3,913	4,307	1,914	2,587
R-Squared	0.0072	0.0109	0.0119	0.0032

Note: Reported are estimated coefficients rather than average derivatives. The bootstrap standard errors in parentheses capture the variance component due to the estimation of the propensity scores. The models also include indicators for weeks of UI entitlement and interactions between week of UI entry and workforce region.

TABLE A5
Program Effects using Inverse Probability Weighting

	Florida PREP	Florida REA	Idaho REA	Nevada REA
Weeks on UI				
Regular	0.08 (.25)	-0.08 (.27)	-0.96 (0.27)***	-2.00 (0.39)***
EUC	-0.70 (.61)	-1.50 (.64)**	-0.61 (0.77)	-1.79 (0.86)**
Total†	-0.62 (.80)	-1.58 (.84)*	-1.57 (0.94)	-3.79 (1.11)***
Benefits Collected				
Regular UI	24 (59)	-22 (61)	-236 (63)***	-378 (108)***
EUC	-143 (130)	-308 (134)**	-94 (184)	-241 (235)
Total†	-119 (178)	-330 (182)*	-330 (219)*	-619 (308)**
Employed				
Quarter of entry	-0.008 (0.016)	-0.009 (0.015)	0.018 (0.024)	0.003 (0.0171)
Quarter 1 after entry	0.014 (0.017)	0.020 (0.018)	0.019 (0.027)	0.088 (0.026)***
Quarter 2 after entry	0.036 (0.017)**	0.039 (0.018)**	0.060 (0.027)**	0.082 (0.025)***
Quarter 3 after entry	0.027 (0.018)	0.021 (0.019)	-0.029 (0.027)	0.047 (0.025)*
Quarter 4 after entry	0.022 (0.018)	0.017 (0.019)	-0.012 (0.027)	0.034 (0.025)
Earnings				
Quarter of entry	30 (91)	30 (99)	-23 (194)	-14 (100)
Quarter 1 after entry	35 (59)	67 (62)	-1 (90)	123 (119)
Quarter 2 after entry	36 (85)	61 (88)	150 (108)	287 (144)*
Quarter 3 after entry	159 (90)*	140 (97)	-196 (164)	410 (169)**
Quarter 4 after entry	22 (142)	-14 (135)	-89 (159)	391 (179)**
Total, quarters 1-4	252 (322)	255 (327)	-136 (415)	1,211 (497)**

Note: Reported are weighted program-control differences in means. The bootstrap standard errors in parentheses capture the variance component due to the estimation of the propensity scores.
* p<0.1; ** p<0.05; *** p<0.01.

TABLE A6
Average Treatment Effects, Models with No Covariates

	Florida PREP	Florida REA	Idaho REA	Nevada REA
Weeks on UI				
Regular	-0.05 (0.22)	-0.19 (0.23)	-0.95 (0.33)***	-2.05 (0.42)***
EUC	-0.92 (0.57)	-1.54 (0.57)***	-0.78 (0.81)	-2.13 (0.97)**
Total†	-0.97 (0.73)	-1.73 (0.74)**	-1.72 (0.99)	-4.18 (1.21)***
Benefits Collected				
Regular UI	-23 (57)	-79 (58)	-233 (111)**	-343 (150)***
EUC	-192 (124)	-349 (125)***	-163 (198)	-242 (243)
Total†	-215 (168)	-428 (170)**	-396 (274)	-585 (334)*
Employed				
Quarter of entry	-0.007 (0.012)	0.000 (0.012)	0.021 (0.019)	-0.006 (0.019)
Quarter 1 after entry	0.007 (0.016)	0.003 (0.016)	0.016 (0.028)	0.095 (0.027)***
Quarter 2 after entry	0.029 (0.017)*	0.024 (0.017)	0.040 (0.027)	0.087 (0.027)***
Quarter 3 after entry	0.026 (0.017)	0.025 (0.017)	-0.010 (0.027)	0.047 (0.027)*
Quarter 4 after entry	0.023 (0.017)	0.012 (0.017)	-0.019 (0.027)	0.044 (0.026)*
Earnings				
Quarter of entry	-37 (56)	11 (58)	0 (107)	-43 (96)
Quarter 1 after entry	-8 (60)	10 (61)	-57 (86)	183 (114)
Quarter 2 after entry	-22 (81)	-12 (82)	56 (119)	378 (139)***
Quarter 3 after entry	63 (81)	83 (93)	-136 (174)	422 (166)***
Quarter 4 after entry	-29 (109)	-97 (109)	-139 (155)	418 (171)***
Total, quarters 1-4	21 (295)	-16 (298)	-277 (434)	1,401 (480)***

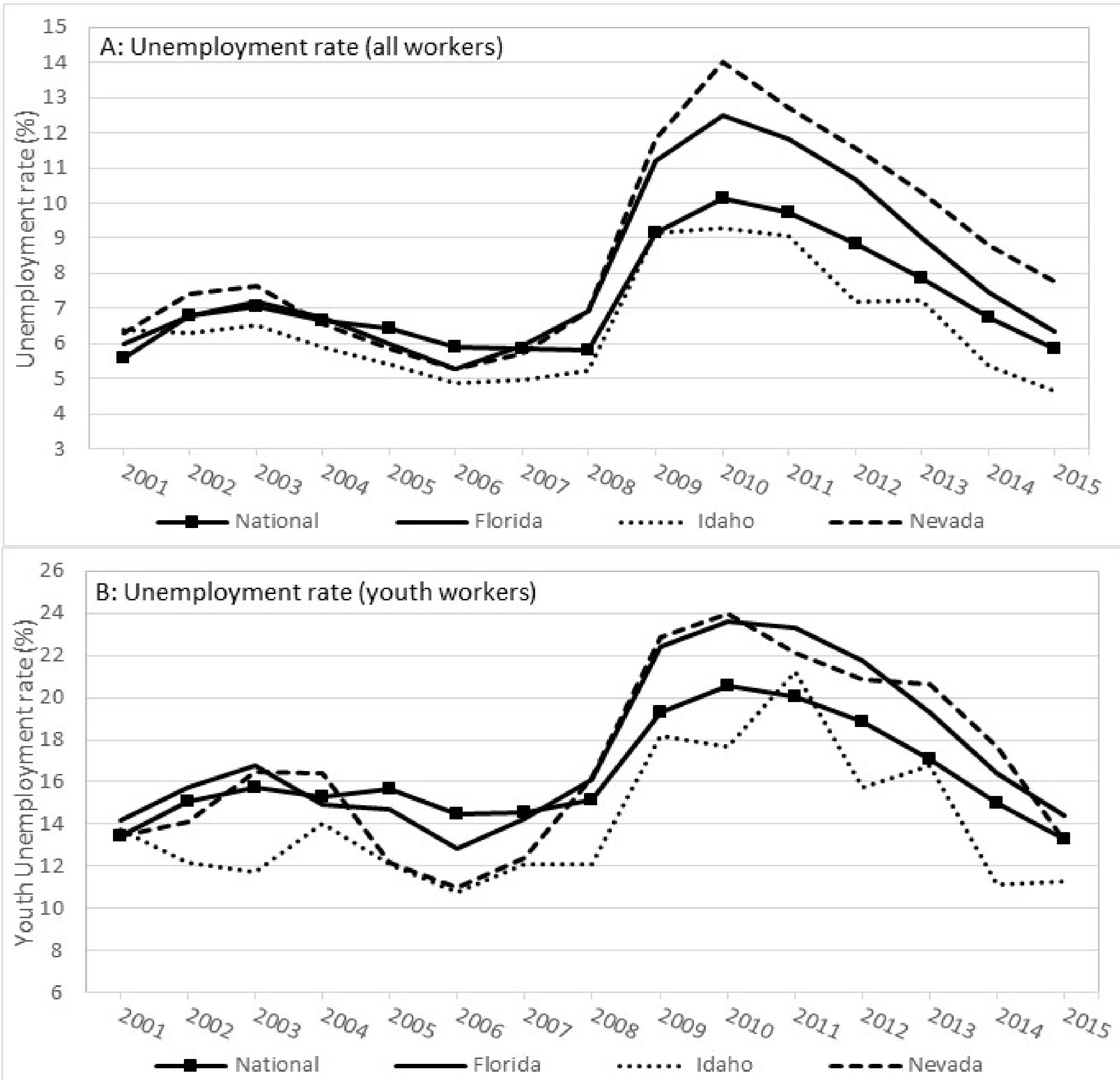
Note: Reported are average treatment effects with robust standard errors in parentheses.
* p<0.1; ** p<0.05; *** p<0.01.

TABLE A7
Nevada REA Effects, Weighted Samples

	Baseline: Florida PREP	Baseline: Florida REA	Baseline: Idaho REA
Weeks on UI			
Regular	-2.41 (0.69)***	-2.44 (0.68)***	-2.10 (0.53)***
EUC	-2.32 (1.13)**	-2.32 (1.11)**	-2.27 (1.10)**
Total†	-4.73 (1.54)***	-4.76 (1.52)***	-4.37 (1.41)***
Benefits Collected			
Regular UI	-563 (174)***	-557 (167)***	-342 (134)**
EUC	-349 (314)	-371 (302)	-276 (297)
Total†	-912 (425)**	-918 (410)**	-618 (381)*
Employed			
Quarter of entry	0.016 (0.024)	0.002 (0.020)	-0.012 (0.020)
Quarter 1 after entry	0.140 (0.037)***	0.138 (0.036)***	0.095 (0.032)***
Quarter 2 after entry	0.080 (0.039)**	0.079 (0.038)**	0.095 (0.032)***
Quarter 3 after entry	0.050 (0.039)	0.051 (0.038)	0.071 (0.032)**
Quarter 4 after entry	0.034 (0.038)	0.033 (0.036)	0.052 (.031)*
Earnings			
Quarter of entry	84 (154)	53 (129)	-5 (113)
Quarter 1 after entry	315 (153)**	284 (170)*	147 (129)
Quarter 2 after entry	336 (175)*	313 (194)*	344 (156)**
Quarter 3 after entry	144 (106)	151 (160)	374 (195)*
Quarter 4 after entry	463 (248)*	454 (274)*	457 (204)**
Total, quarters 1-4	1,258 (667)*	1,202 (768)	1,323 (573)***

Note: Reported are average treatment effects with bootstrap standard errors (that include the variance component associated with estimating the weights) in parentheses.
* p<0.1; ** p<0.05; *** p<0.01.

FIGURE A1
Unemployment Rates across Study States



Source: Authors' tabulations of the American Community Survey (accessed at https://usa.ipums.org/usa/).

Appendix References

DiNardo, J., N. Fortin, and T. Lemieux. “Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach.” Econometrica 64(5), 1996, 1001-1044.