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## Evaluating public employment programs with field experiments: A survey of American evidence

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# Evaluating Public Employment Programs with Field Experiments: A Survey of American Evidence

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# Evaluating Public Employment Programs with Field Experiments: A Survey of American Evidence

Upjohn Institute Working Paper 17-279

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## ABSTRACT

Research in the 1970s based on observational data provided evidence consistent with predictions from economic theory that paying unemployment insurance (UI) benefits to involuntarily jobless workers prolongs unemployment. However, some scholars also reported estimates that the additional time spent in subsidized job search was productive. That is, UI receipt tended to raise reemployment wages after work search among the unemployed. A series of field experiments in the 1980s investigated positive incentives to overcome the work disincentive effects of UI. These were followed by experiments in the 1990s that evaluated the effects of restrictions on UI eligibility through stronger work search requirements and alternative uses of UI. The new century has seen some related field experiments in employment policy, and reexamination of the earlier experimental results. This paper reviews the experimental evidence and considers it in the context of the current federal-state UI system.

**JEL Classification Codes:** J65, J68, J48

**Key Words:** field experiments, public employment policy, unemployment insurance, UI, employment service, ES, job search assistance, JSA, targeting employment services, profiling, WPRS, self-employment, short-time compensation, work sharing

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## Evaluating Public Employment Programs with Field Experiments: A Survey of American Evidence

Policies to support labor markets in the United States are mainly initiatives of the federal government. Historically, states and localities have been reluctant to act independently in employment policy for fear of competitively disadvantaging resident industries with added costs. Federal leadership has permitted individual states to address important labor market problems with a diminished risk of job loss.

The Wagner-Peyser Act of 1933 established the U.S. Employment Service (ES), the Social Security Act of 1935 established the federal-state system of unemployment insurance (UI), and Depression-era public works programs enacted by the Works Progress Administration put millions of men to work. These three New Deal programs signaled the start of federal employment policies, which have been refined over the years based on program experience in states and local areas. Public administration relies on best practice as a guide. Modern public management looks to program evaluation as a guide to improve policy. In the area of employment policy, since the 1980s the states have truly served as laboratories of democracy. States have tested promising policy improvements for employment programs by applying classical experimental methods with randomized controlled trials on large samples of program-eligible persons.

This paper summarizes the knowledge accumulated from a wide variety of field experiments conducted on elements of U.S. employment programs over the past 40 years. To set the context for this discussion, the next section briefly considers the advantages and

disadvantages of the experimental approach. The following four sections provide summaries of the research on employment programs and incentives tried by the ES-UI partnership to promote return to work by job-ready persons with recent labor market experience: cash reemployment bonuses, job search assistance (JSA) and the UI work test, targeted JSA, and employer incentives. The final section offers a summary and some comments on the relevance of lessons for the UI system today from these field experiments.

## **The Appeal of Field Experiments**

Classically designed field experiments involving randomized controlled trials (RCT) are the gold standard for estimating the impact of changes to public programs. If random assignment is achieved, modeling of behavior and complex econometric methods are not needed to obtain reliable program impact estimates.<sup>1</sup> With large samples randomly assigned to treatment and control groups, observable and unobservable characteristics of the two groups should not differ on average, so any difference in outcomes can be attributed to the program change. Average program impacts can be measured as the simple difference between the means of the samples of program participants and of control group members on outcomes of interest. Since this process is easy to understand, impact estimates computed in this way can be influential for public policy.

When there is nonrandom assignment to either a program participant group or the comparison group, then proper estimation of program impacts requires statistical methods of

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<sup>1</sup> Heckman, Lalonde, and Smith (1999) enumerate the assumptions implicit in such a view of random-assignment field experiments as a means for model-free impact estimation.

correction to offset potential sample selection bias. A commonly used approach, which could be called “belt and suspenders,” is to compute mean differences in regression models, including observable characteristics to correct for sample differences.<sup>2</sup> However, this is often not sufficient to correct for biases due to unobservable differences between groups. A popular solution to this problem was proposed by Heckman (1976), who asserted that sample selection could be characterized as an unobservable variable that distinguishes program participants from nonparticipants. Other approaches involve strategically selecting a comparison group by matching characteristics of program participants with nonparticipants who appear to be otherwise similar. Such matching may be done either on a set of characteristics or on a single summary measure of several characteristics known as a propensity score (Heckman, LaLonde, and Smith 1999). Recent research has exploited naturally occurring events that cannot be manipulated by participants and lead to discontinuities in outcomes of interest (Lee and Lemieux 2010). These regression discontinuity methods are now regarded as second best to the gold standard of RCT in field experiments. Because of the focus on a particular discontinuity, the RCT methods usually estimate local average treatment effects.

Policy decisions concerning questions of whether to continue, expand, reduce, or cancel government employment programs require information about the net benefits of government spending. Cost-benefit analysis requires measurement of net impacts. Net impact evaluations are not without potential problems, even if the evaluation is done under the ideal conditions of a field experiment. The first type of potential pitfall threatens the *internal validity* of the experiment. Such problems include errors in random assignment to treatment and control

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<sup>2</sup> The term “belt and suspenders” refers to redundant systems that afford mutual backup in case one fails.

groups, as well as inconsistent experimental conditions. The first of these can lead to heterogeneity in characteristics between treatment and control groups. The second means that the same treatment conditions were not successfully repeated in all cases. Even when randomization is successful, problems can result from dropout bias, wherein a customer assigned to an experimental treatment did not in fact receive the service. The logical complement to dropout bias is called substitution bias, wherein a control group member actually receives the treatment, although this might not be observed (Heckman et al. 2000).

The second group of evaluation pitfalls concern *external validity*. These issues affect the ability to transfer impact estimates from the evaluation context to the real-world policy context. *Time horizon effects* can occur when treatment subjects understand that an experimental service is only temporary rather than permanent. *Learning effects* can take place within a community during the course of an evaluation, causing later enrollees to act differently from those enrolled around the time the experiment begins. *Entry effects* not observed during an evaluation can emerge when an appealing service becomes generally available to a population of potential customers, thereby increasing program take-up and system costs. *Hawthorne effects* are responses to treatments that are not due to the content of service, but simply to special attention being paid to participants.<sup>3</sup> *Displacement effects*, which may be the most critical external

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<sup>3</sup> A Hawthorne effect is the initial improvement in a process of production caused by the obtrusive observation of that process. The effect was first noticed in the Hawthorne Works plant of the Western Electric Company in Cicero, Illinois, during studies of workplace behavior in the 1920s and '30s. Production increased not as a consequence of actual changes in working conditions introduced by the plant's management, but because management demonstrated interest in such improvements. A reexamination of the Hawthorne data has called into question whether such an effect actually occurred during the original studies (Jones 1992).

validity concern, occur when treatment-assigned subjects improve their outcomes at the expense of others in the community who are not part of the evaluation sample.<sup>4</sup>

As Zvi Griliches said, “If the data were perfect, collected from well-designed randomized experiments, there would be hardly room for a separate field of econometrics” (Orr 1999, p. 187). The following review mentions few exceptions to the classical assumptions of experimental design and does not delve into any corrections that might have been done before reporting final program impact estimates. The focus here is on average program effects. That is, it focuses on the effect of treatment upon the treated, assuming good experimental designs were properly implemented.

## **Reemployment Bonuses**

Economic theory suggests that paying unemployment insurance (UI) benefits to involuntarily jobless workers prolongs unemployment. The static neoclassical theory of choice by a consumer-worker under certainty and the dynamic theory of job search under uncertainty both suggest that the presence of UI will lengthen unemployment durations beyond what they would be otherwise (Cox and Oaxaca 1989; Krueger and Meyer 2002). A utility-maximizing consumer-worker considering reemployment while receiving UI will choose to supply less labor because the opportunity cost of leisure is lower than in the absence of UI. An unemployed job

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<sup>4</sup> This discussion of impact estimation and most of the studies reviewed here focus on partial equilibrium effects of interventions. That is, they assume away external validity issues that include general equilibrium effects such as entry and displacement effects. Some evaluations have directly measured these effects (Davidson and Woodbury 1993).



seeker facing uncertain draws from wage-offer distribution will have a higher reservation wage while receiving UI than in the absence of jobless compensation.

A series of papers in the 1970s provided empirical evidence of a UI work disincentive. Feldstein (1974) cites extremely high wage replacement rates for UI in some states, compounded by the tax exempt status of UI payments. He argues that moral hazard from this social insurance induced beneficiaries to exaggerate the involuntary nature of their joblessness so as to prolong unemployment.<sup>5</sup> Using estimating equations derived from a job search model and data from the National Longitudinal Survey, Ehrenberg and Oaxaca (1976) estimate that a 10 percentage point increase in the UI wage replacement ratio would increase unemployment durations by 1.5 weeks. They also report evidence that such an extended job search is productive, in that it increases reemployment wages by more than 7 percent. Around the same time, Classen (1977) reports similar empirical evidence, based on data from Arizona and Pennsylvania, that higher UI benefits increased the duration of unemployment, but she does not find evidence of higher reemployment wages after prolonged job search.<sup>6</sup> However, recent research by Nekoei (2014), using administrative data from Austria and a regression discontinuity design, estimates that a nine-week extension of UI eligibility increases the average reemployment wage by 0.5 percent.

The payment of UI during times of involuntary unemployment is an important part of the social safety net, and the automatic countercyclical role of UI is important to the macro economy. For these reasons, the evidence of work disincentive effects from paying UI benefits

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<sup>5</sup> Solon (1985) estimated that the 1979 federal tax reform that made UI benefits taxable shortened average insured unemployment durations by about one week.

<sup>6</sup> Decker (1997, pp. 293–294) reported the range of published estimates to be between 0.3 and 1.5 weeks' longer duration of UI receipt for a 10 percentage point increase in the UI replacement rate.

led to a search for ways to improve reemployment incentives in the system.<sup>7</sup> A series of field experiments was conducted to evaluate positive reemployment incentives in UI. Between 1984 and 1989, four reemployment bonus experiments targeted at UI recipients were conducted in the United States. These experiments provided various levels of lump-sum payments to UI recipients who took new, full-time jobs within 6 to 12 weeks of their benefit application and held those jobs for at least three to four months. The purpose of these interventions was to learn more about the behavioral response of UI recipients to changes in the UI program. Experiments that offered reemployment bonuses were designed to find an incentive that would speed the return to work in a manner that would benefit employees, employers, and the government, and would be cost effective. UI claimants would be better off if they returned to work sooner and found jobs that were similar and paid similar wages to the jobs that they would take in the absence of a bonus offer. Employers would be better off if they had lower UI payroll taxes. The government would be better off if the cost of the bonus were offset by a decrease in UI benefit payments to unemployed workers and an increase in income and other tax contributions by workers during their longer period of employment.

### ***Illinois UI Incentive Experiment***

The first bonus experiment was conducted in Illinois during 1984–1985 and was sponsored by the Illinois Department of Employment Security. Its goal was to examine the theoretical and empirical economic implications of a reemployment bonus offer to UI claimants and the potential for developing a cost-effective bonus program. The Illinois design provided a

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<sup>7</sup> Oaxaca and Taylor (1986) estimate the macro stabilizing effects of UI on local economies.

\$500 bonus amount, equivalent to about four weeks of UI benefit payments—i.e., four times the UI weekly benefit amount (WBA). To collect a bonus payment, treatment group members needed to become reemployed within 11 weeks of filing their UI claims (Table 1 summarizes the design and impact estimates for the reemployment bonus experiments).

The estimated impact of the Illinois reemployment bonus offer to UI claimants was a reduction in the duration of UI-compensated unemployment by 1.15 weeks (Woodbury and Spiegelman 1987). This reduction was so great that the reemployment bonus was cost-effective to the UI Trust Fund, generating a benefit-cost ratio of 2.32. At the same time, participants suffered no reduction in postunemployment wages, which indicates that the bonus offer did not reduce job quality.

### ***New Jersey UI Reemployment Experiment***

Independent of the Illinois experiment, the U.S. Department of Labor (USDOL) sponsored a New Jersey UI experiment that included a reemployment bonus treatment group. This project was designed and became operational in 1985 and 1986, before the results from the Illinois experiment became available. As such, the New Jersey experiment was not designed to replicate or validate the Illinois experiment. The New Jersey bonus offer was designed so that the amount of the offer was tied to a claimant's remaining UI benefit entitlement. Thus, the amount paid was larger in cases of more rapid reemployment. The initial bonus offer was one-half of the claimant's remaining entitlement at the time of the offer. This offer amount remained constant for the first two full weeks after the initial offer. Thereafter, the amount of the bonus

offer declined by 10 percent of the original amount per week, falling to zero by the end of the eleventh full week of the bonus offer. Initial bonus offers in New Jersey averaged \$1,644, which was about nine times the UI weekly benefit amount.

The evaluation of the New Jersey experiment suggested that the reemployment bonus, as it was implemented in New Jersey, generated modest savings in UI. Since the cost of offering and paying the bonuses exceeded the modest UI savings, the New Jersey bonus was not cost effective from the perspective of the UI system.

### ***Pennsylvania and Washington Reemployment Bonus Experiments***

In 1987, with the evaluation of the Illinois experiment completed and the New Jersey experiment operations over, USDOL sponsored two additional reemployment bonus experiments. In contrast to the Illinois experiment, these later trials generated much more modest results. In the Pennsylvania and Washington experiments, the bonus offers were set as multiples of the worker's weekly benefit level. This design was adopted because in the Illinois experiment, claimants receiving less than the UI maximum weekly benefit responded more strongly to bonus offers than those constrained by the maximum (O'Leary, Spiegelman, and Kline 1995). The Pennsylvania and Washington experiments tested benefit levels that bracketed the Illinois bonus amount ( $4 \times \text{WBA}$ ) and tested qualifications both similar to the earlier offers and about half as great.

The resulting designs provided for four treatment groups in Pennsylvania and six in Washington (Table 1). Each treatment specified a bonus level (high and low in Pennsylvania; high, medium, and low in Washington) and a qualification period or duration of the bonus offer (short and long in both states). The reemployment period of four months was the same for all treatments. While half of the 10 treatments in Pennsylvania and Washington were cost effective to claimants, society, and the government sector as a whole, only two of the treatments were cost effective for the UI system (Decker and O’Leary 1995).

The relatively weak response to the bonus offers in Pennsylvania and Washington led to a reexamination of the powerful Illinois results. It was discovered that within the designed experiment, a second experiment had unintentionally taken place. In 1984, as Illinois was recovering from a major recession, the availability of Federal Supplemental Compensation (FSC) was terminated. This resulted in about half of the claimants studied having 38 weeks of UI benefit eligibility, with the remainder being eligible for only 26 weeks of regular UI benefits. It turns out that the mean bonus response of  $-1.15$  weeks in Illinois was made up of a response of  $-1.78$  weeks for those eligible for FSC and  $-0.54$  weeks for those not eligible (Davidson and Woodbury 1991). The mean response of  $-0.54$  for the non-FSC sample in Illinois is close to the response observed in Pennsylvania and Washington, where the entitled duration of benefits was also similar.

Among the individual treatments, the impact on weeks of UI benefits ranged from  $-0.05$  for the offer in Washington involving a low bonus amount and a short qualification period to  $-1.78$  for the bonus offer to FSC-eligible claimants in Illinois. Impacts for Pennsylvania tended

to fall between those for Illinois and Washington. Overall, a cash bonus can be expected to modestly shorten spells of insured unemployment—the mean effect of the offers made in the three states yielded about a one-half week’s reduction in UI benefits.

The degree of response to the bonus offer was also examined for important subgroups within the sample. Results from Pennsylvania and Washington suggest that UI claimants in low unemployment areas and claimants whose prior employment was in manufacturing tended to respond more strongly to the bonus. However, close inspection of subgroup results reveals one overarching finding: there is no difference between any pair of subgroups shown that is both statistically significant at conventional confidence levels and consistent across the three experiments. The implication of this finding is quite striking—the reemployment bonus has a remarkably even impact on various subgroups of workers, whether delineated by gender, age, race, industrial sector of employment, level of local unemployment, or level of the weekly benefit amount.

### ***Pennsylvania and Washington Targeted Reemployment Bonuses***

O’Leary, Decker, and Wandner (2005) investigated whether targeting reemployment bonus offers to unemployment insurance (UI) claimants identified as most likely to exhaust benefits would reduce benefit payments.<sup>8</sup> They showed that targeting bonus offers with profiling

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<sup>8</sup>Targeted reemployment bonuses were also tested in a field experiment (Wandner 2012) as part of personal reemployment accounts (PRAs). However, the design of the bonus offers under PRAs was not similar to the earlier experiments, and the bonus take-up was low among UI beneficiaries who accepted a PRA offer. Furthermore, across the seven states where targeted PRAs were tried, only 45 percent of PRA money was paid out in reemployment bonuses. A larger share of PRA money was paid for supportive services (Kirby et al. 2008).

models similar to those in state Worker Profiling and Reemployment Services (WPRS) systems can improve cost effectiveness.<sup>9</sup> However, estimated average benefit payments do not steadily decline as the eligibility screen for targeting is gradually tightened by the probability of UI exhaustion. They find that narrow targeting is not optimal. The best candidate to emerge is a low bonus amount with a long qualification period, targeted to the half of profiled claimants most likely to exhaust their UI benefit entitlement.

Two potential behavioral effects might reduce cost effectiveness for an operational program (Meyer 1995). First, an actual bonus program could have a displacement effect. Displacement occurs if UI claimants who are offered a bonus increase their rate of reemployment at the expense of other job seekers not offered a bonus. Second, there is also the risk that an operational bonus offer program could induce an *entry effect*. That is, the availability of a reemployment bonus might result in a larger proportion of unemployed job seekers entering the UI system.

If entry and displacement effects are sizable, actual program cost effectiveness will be lowered. However, targeting offers of a low bonus amount coupled with a long qualification period to only those most likely to exhaust UI should reduce both these risks. Targeting would introduce uncertainty that a bonus offer would be forthcoming upon filing a UI claim, which should reduce the chance of a large entry effect. Also, targeting should reduce any potential for displacement, since a smaller proportion of claimants would receive the bonus offer.<sup>10</sup>

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<sup>9</sup>More on WPRS is provided below in the section on targeted job search assistance.

<sup>10</sup> Davidson and Woodbury (1993) estimate that a nontargeted bonus offer to all UI claimants could increase unemployment durations among those not eligible for UI by between 0.2 and 0.4 weeks.

## **The UI Work Test and Job Search Assistance**

Unemployment insurance provides temporary partial wage replacement to the involuntarily unemployed. Proper administration of this principle assures that UI is social insurance and not a dole. Unemployment insurance eligibility rules require that beneficiaries are strongly attached to the labor force and temporarily jobless through no fault of their own. To initially qualify for UI, a claimant must satisfy both monetary and nonmonetary eligibility requirements. Monetary eligibility for UI is determined by base period earnings.<sup>11</sup> The nonmonetary eligibility rules specify that the job separation must be involuntary. These rules prohibit quits and discharge for causes justifiable by an employer, such as frequent tardiness, unexplained absences, misconduct, or poor job performance. To maintain continuing UI eligibility, beneficiaries also must be able, available, and actively seeking full-time work. Assessment of compliance with the UI work test is normally administered by the Employment Service (ES), which works in cooperation with state UI agencies. An influential audit of UI payment accuracy done for the U.S. Department of Labor reported that a large fraction of overpayments in the UI system were due to failure to satisfy work search requirements (Burgess and Kingston 1987). This influential study spawned a series of evaluations of the UI work test and associated job search requirements.

The UI work test normally involves beneficiaries certifying on their biweekly continued claim form that they have actively searched for work. Most states require beneficiaries to name two or three specific employers contacted about work in the past two weeks. Job search

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<sup>11</sup> The UI base period is normally the first four of the previous five completed calendar quarters before the date of claim for benefits. For claimants not eligible based on earnings in the standard base period, earnings in an alternate base year (ABY)—the four most recently completed calendar quarters—are considered for monetary eligibility in 41 states.



assistance (JSA) comprises a bundle of services available from the public labor exchange, which may include resume preparation assistance, job finding clubs, provision of specific labor market information, development of a job search plan, and orientation to self-service resources (job vacancy listings, resume preparation, word processor competency testing, and telephones for contacting employers). In the evaluations of JSA that have been done, job search workshops are treated as a distinct service. Evaluations of the UI work test and JSA overlap.

Four specific evaluations of JSA have been particularly influential in shaping public labor exchange policy. The designs, samples, and findings from these studies are given in Table 2. All evaluations were done as field experiments involving random assignment. Among other offerings of the public employment service, job referrals and placements have not applied an experimental design because of the untenable design requirement of withholding from the control group basic services having universal entitlement. Consequently, JSA evaluations have focused on UI claimants and have usually involved providing additional services.

It is well documented that in performing its income replacement function, UI acts as a disincentive to rapid return to work (Decker 1997). The work test that links the UI and ES programs in the United States is an institutional mechanism for monitoring whether UI beneficiaries are available and actively seeking work. The JSA evaluations have investigated various approaches to improving the effectiveness of the work test for UI.

### ***Charleston Claimant Placement and Work Test Experiment***

The first field experiment addressing aspects of the UI work test in the United States began enrollment in February 1983 in Charleston, South Carolina (Corson, Long, and Nicholson 1985). Random assignment of 5,675 initial UI claimants to three treatment groups and a control group was completed in December 1983. The experiment was designed to evaluate new procedures intended to improve the UI work test and enhance ES practices. The three treatments tested represented successively larger bundles of services. This design permitted researchers to draw contrasts between the three treatment groups themselves as well as between the treatment groups and the single control group.

Claimants assigned to the control group were given the customary work test, which involved informing claimants that ES registration was required but involved no systematic monitoring of this requirement. The three treatments in Charleston were as follows:

1. A strengthened work test, requiring that an ES registration notice be sent after the first UI benefit check was paid. Payment of the second check would be suspended for failure to register with the ES. This measure required establishment of improved data-sharing systems between UI and ES.
2. A strengthened work test, plus enhanced placement services, including a personal placement interview within one week of the first UI check, a job referral or an outreach attempt to contact a prospective employer (job development), and training in using the job vacancy

listings. Treatment-assigned claimants were also told they would be called for special services again once they drew nine weeks of benefits.

3. A strengthened work test, enhanced placement services, plus job search workshops that included a three-hour workshop and, after four weeks of UI benefits, a workshop on labor market information.

The strengthened work test had the greatest impact. It alone shortened the duration of compensated joblessness by more than half a week; the impact estimate was  $-0.55$  weeks of UI benefits. This effect was statistically significant, but not significantly different from the estimated effect of the second treatment. The addition of enhanced placement services resulted in an impact estimate of  $-0.61$  weeks, or an insignificant increase over the strengthened work test alone. The impact estimate for the third treatment, which added job search workshops, was  $-0.76$  weeks of UI benefits, a modest incremental effect over either of the other treatments.

Impacts of the treatments were concentrated among men who averaged impacts of greater than  $-1.0$  weeks for all treatments, and among workers in the construction industry, who had impacts of over  $-4.0$  weeks. The relatively low cost of treatments resulted in jaw-dropping benefit-cost ratios in excess of 4. That is, more than four dollars in UI benefit payments were saved for every dollar spent on the work test, JSA, and job search workshop services. The third treatment, which involved the largest number of components, had an average cost of only \$17.58 in 1983 dollars.

In 1969, the UI trust fund was added to the federal unified budget. Conservation of UI funds consequently improved the overall budget picture. In the 1980s political environment of huge federal deficits, the Charleston Claimant Placement and Work Test Experiment drew attention to the strengthened work test, JSA, and job search workshops as appealing policy tools. These instruments offered the potential of providing positive services while conserving UI trust fund dollars.

### ***Washington Alternative Work Search Experiment***

Effects of the UI work test and related services of the public labor exchange were further investigated by a field experiment with random assignment between July 1986 and August 1987 in Tacoma, Washington, job service centers. A total of 6,763 UI claimants were assigned to one of three treatments, and 2,871 claimants were assigned to the control group, which followed the existing Washington state work search policy.

The standard work search rule required three employer contacts per week plus an eligibility review interview 13 to 15 weeks after the initial claim was filed. This eligibility review interview involved a one-hour group session followed by a 15-minute individual interview. The focus of both sessions was on UI eligibility. The three treatments in Tacoma were as follows:

1. Exception reporting—elimination of the UI work test. Claimants were not required to file the standard biweekly continued UI claim form, and they were

told that UI payments would continue until the claimant voluntarily reported a change in employment circumstances, such as return to work or an increased level of earnings.

2. New work search policy—individualized work search requirements, including a group eligibility review interview followed by an intensive one-on-one follow-up interview.
3. Intensive services—individualized work search requirements (Treatment 2), plus a two-day job search workshop after four weeks (two days of classroom instruction plus 10 hours of phone canvassing), plus a group eligibility review interview after 12 weeks with a focus on employability development, plus individual follow-up.

Suspension of enrollment into the first treatment was done earlier than planned because the larger-than-expected response could easily be detected with a sample much smaller than designed. Claimants relieved of the work test and continued claim filing were estimated to have increased their receipt of UI benefits by 3.34 weeks—a statistically significant effect. This impact was bigger for women with children and men without children, and for married women and unmarried men.

The new work search policy, which provided custom-tailored services and schedules, had the effect on UI benefit receipt of adding 0.17 weeks and was statistically indistinguishable from the existing standard work-search rule, which is applied uniformly to all claimants.

Treatment 3, which was customized and had a job search workshop after four weeks and an eligibility review interview after 12 weeks, had a statistically significant impact of  $-0.47$  weeks. Impacts were bigger for women without children and unmarried women. An analysis of the timing of the components of this treatment and claimant response (at 4 and 12 weeks) was combined with analysis of the timing of the standard treatment given the control group (at 13 to 15 weeks) and response to that analysis. This combination provided new insight into claimant behavior. In both cases, it was more likely for beneficiaries to stop UI receipt before a scheduled intervention, rather than after the service was provided. Such a response might be termed an “invitation effect.”

This led to the conclusion that the timed elements of the work test—job search workshop and eligibility review interview—acted more like a stick prodding return to work than a carrot providing a reward for achieving that end. The researchers speculated that the response to Treatment 2 had no identifiable peaks in the timing of exit from UI receipt because the individually customized schedule attenuated the observed response to an invitation to have an eligibility review interview.

Needless to say, exception reporting was estimated to be very costly. Individualized requirements generated no differential impact. An invitation to attend either an eligibility review interview or a job search workshop shortens duration, with the latter having a bigger effect. Exit rates are lower during and after the eligibility review interview and job search workshop, suggesting it is the requirement to attend rather than the value of the session that shortens duration.

Lachowska, Meral, and Woodbury (2015) recently examined long-term evidence from the Tacoma experiment by merging Washington UI program administrative data from nine additional years after the original one-year follow-up period. They focused on the treatment that removed the work test, and they estimated that nearly all the costs were borne by the UI system in the year of the experimental program change. Long-term effects averaged out to zero, but subgroup analysis by job separation reason yielded an important result for those permanently separated from jobs. For this group, the 10-year follow-up suggested that the standard UI work search requirement yielded significantly faster reemployment and greater long-term employment stability. Those excused from the work test got reemployed about 1.40 calendar quarters later and had job tenure of about 1.65 quarters shorter than the comparison group.

### ***Maryland UI Work Search Experiment***

Enrollment into the Maryland UI work search experiment was conducted in six public labor-exchange offices around the state throughout the calendar year of 1994 (Klepinger et al. 1998). A combined sample of 23,758 new monetarily eligible UI claimants were enrolled into the experiment.

The standard work search policy was given to the control group. This requires two job-search contacts per week, which must be reported on the biweekly UI continued claim form but are not verified. The four alternative treatments tested were these:

1. Report four weekly employer contacts, which are not verified.
2. Contact two employers per week, but claimants need not report the two contacted.
3. Report two weekly employer contacts, which are not verified, plus attend a four-day job search workshop early in the unemployment spell.
4. Report two weekly employer contacts, plus claimants are told contacts would be verified.

Requiring four employer contacts per week yielded a statistically significant impact of  $-0.7$  weeks of UI benefits. This reduction in duration resulted even in the absence of any verification of the offers. Requiring two employer contacts per week but removing the requirement to report the two contacts resulted in a statistically significant increase in UI benefit durations of 0.4 weeks. The impact of requiring two employer contacts per week, which were not verified, plus attendance at a four-day job search workshop early in the unemployment spell, was  $-0.6$  weeks of UI. As in the Tacoma experiment, this impact was due to increasing the hassle associated with staying on UI, not to increasing claimants' job search skills. Notably for employers, this third treatment also reduced the probability of a claimant's returning to his or her previous employer.

Requiring reporting of two employer contacts, plus telling claimants that their two contacts would be verified, shortened UI benefits by 0.9 weeks. Conducting verification at a rate of 10 percent appeared to suffice as an adequate threat. Notably, the impact of this fourth treatment occurred during the first spell of joblessness. Similarly, the first treatment generated the bulk of its response during the first spell of joblessness in the benefit year. The effects of



Treatments 1, 3, and 4 were not associated with lower reemployment earnings. However, eliminating the work-search reporting requirement, as in Treatment 2, raises reemployment earnings by a statistically significant 4 percent.

A second control group facing the standard work test was also tracked, but claimants assigned to this group were told that their behavior was being tracked as part of an experiment. This was done to permit testing for the presence of a Hawthorne effect. This is relevant in ensuring external validity of the evaluation. If part of the treatment response to a new work test is simply due to added attention on the work test, then such an effect could quickly dissipate after actual implementation. Impact estimates computed as a contrast between the participant group and each of the two control groups were virtually identical, suggesting the absence of any Hawthorne effect.<sup>12</sup>

### ***Connecticut, Tennessee, Massachusetts, and Virginia Benefit-Rights Interviews Experiment***

Ashenfelter, Ashmore, and Deschênes (2005) conducted a field experiment on work search activity by UI claimants in four states. The control group followed the regular procedure for UI applicants in the states of Connecticut, Tennessee, Massachusetts, and Virginia. The procedure specified that applicants must apply in person for UI at public employment offices. To be initially eligible, applicants must demonstrate labor force attachment by sufficient recent

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<sup>12</sup> A 1987 employment service reform in the United Kingdom called “Restart” was evaluated by Dolton and O’Neill (1996, 2002). They found evidence that, over the short term, requiring JSA appears to act as a stick, prodding UC beneficiaries back to work, but over the long term an earlier JSA intervention supports higher success in the labor market and higher earnings—evidence that JSA can have valuable content for job seekers.

earnings and show that their job separation was involuntary. Claimants also were given three additional eligibility requirements: 1) a benefit rights interview, in which a staff member explained the continuing work search requirements; 2) a requirement to visit the office two weeks after UI application to learn of their initial eligibility; and 3) a requirement to report on their active work search since application.

The experiment included two treatment groups. For both groups, the first office visit included an enhanced benefit rights interview that involved immediate telephone verification with the previous employer about the reason for job separation, thorough immediate checking of prior earnings through administrative wage records or applicant-provided pay stubs, and additional information about the requirements for continuing an active job search. During the second office visit, the first treatment group (which received 40 percent of all treatments) had their reported employer job search contacts validated by telephone with employers, while the second treatment group (which received 60 percent of all treatments) did not. The second group received only the standard continuing eligibility review that was also given to the first treatment group and the control group during their second office visit.

There were about 1,900 experimental subjects and the same number of controls. No treatment impacts in the individual states were statistically significant at the 95 percent confidence level. In data pooled across the four states, the combined treatment claimant groups showed a statistically significant 5 percent decrease in the likelihood of qualifying for benefits in the first week. However, there were no statistically significant effects on benefit amounts or duration, once qualified. The authors concluded that the results of this experiment failed to

confirm the benefits of stricter continuing work-search enforcement. Nonetheless, the results highlight the value of properly checking initial eligibility.

### ***Michigan Reemployment and Eligibility Assessment Nudge***

The high unemployment levels and long durations of UI receipt occurring in the 1975 recession led the U.S. Department of Labor to renew its emphasis on active job search by UI beneficiaries. Guidelines for an eligibility review program were issued by USDOL to all state employment security agencies in 1976, and beginning in 1977 states were allotted funds for operating eligibility review programs. The eligibility review programs required states to do two things: 1) continuously review whether UI beneficiaries had satisfied the requirements for being able, available, and actively seeking work, and 2) actively promote reemployment of UI beneficiaries with services. Over time, the use of eligibility review programs dwindled in many states, along with federal funding for staff to provide services.

In 2005, USDOL renewed and expanded the concept of eligibility review programs by providing \$30 million in funding, divided among 21 states, to provide reemployment and eligibility assessment (REA) grants. The REA program requires that UI beneficiaries must report in person to a One-Stop Career Center for staff-assisted services, and that those assessments must include four elements: 1) a review of continued eligibility and referral to adjudication if a potential issue is identified, 2) the provision of labor market information, 3) development or review of a work search plan, and 4) a referral to employment services, or to occupational or skills training when appropriate. Nine of the 21 REA states were selected to

participate in a quasi-experimental evaluation. In the end, only data from Minnesota were sufficient for a reliable evaluation study. The Minnesota data suggested that the REA program reduced the duration of UI benefit receipt by 1.2 weeks (Benus, Poe-Yamagata, et al. 2008a). A follow-up evaluation involving random trials in Nevada provided evidence that the REA reemployment services were effective (Michaelides et al. 2012). Funding to states for REA has risen steadily, from \$50 million in 2009 to \$68.7 million in 2014.

The W.E. Upjohn Institute for Employment Research worked with Mathematica and Ideas42 on a random assignment experiment evaluating an additional feature of REAs for unemployment insurance (UI) beneficiaries in the four-county workforce development area administered by Michigan Works! Southwest, a One-Stop agency affiliated with the Upjohn Institute. The U.S. Department of Labor recently awarded Michigan funding for REA activities in five Michigan workforce areas, including Michigan Works! Southwest, which covers the counties of Kalamazoo, St. Joseph, Branch, and Calhoun. The core REA is a call-in program in which continuing UI beneficiaries call in to validate that they are satisfying work-search eligibility requirements, and so the agency can provide them with additional reemployment services. Failure to schedule and complete an REA interview results in suspension of UI weekly benefits.

The Michigan REA started in January 2015, and random assignment for the experiment in the Michigan Works! Southwest counties began in March 2016. Before random assignment, only about half of REA-assigned beneficiaries were completing REA. Randomly assigned REA beneficiaries in southwest Michigan were given additional nudges designed on principles of

behavioral economics (Babcock et al. 2010). The nudges took the form of a series of e-mails providing information and reminders to participate in REA services. The nudges reminded REA beneficiaries about three required REA appointments. A follow-up set of three “persistence” e-mails was also sent to encourage and reinforce job search activity after the third REA visit to a Michigan Works! office. The persistence e-mails provided links to office locations and phone numbers, schedules of local services, and testimonials from previous service recipients.<sup>13</sup> Interventions were delivered to the treatment sample from March to September 2015.

Part of the standard Michigan UI work test is registering online with the ES system. That requires entering a personal e-mail address, and those addresses were linked to the weekly REA list to operationalize the random trials. The actual sample inflow for randomization was smaller than the expected 40 new participants per week in just Kalamazoo. Since the workforce area also included Battle Creek, Coldwater, and St. Joseph, those areas were added to yield a total of about 40 new REA referrals weekly by the end of the experiment. The study found that “UI claimants who were sent email messages were more likely to start the REA program by scheduling their first session. UI claimants who received email messages were also more likely to complete the REA program. Once individuals attended their first REA session, they were equally likely to complete the program regardless of whether they had received emails or not” (Darling et al. 2016, p. 1).

### **Targeted Job Search Assistance**

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<sup>13</sup> Only one recipient of a persistence nudge e-mail opted out of the reminder and reinforcement service.

Targeting of JSA surfaced as a policy option during the 1990s, following the massive economic restructuring and worker dislocation of the previous decade. Earlier research had identified JSA as a cost-effective tool for promoting return to work. The question of whether JSA would be effective for those at risk of long-term unemployment was evaluated in the context of a major field experiment in New Jersey (Corson et al. 1989). Together with earlier evidence on JSA cost effectiveness, results from the New Jersey experiment supported establishment of the Worker Profiling and Reemployment Services (WPRS) system, which required targeted JSA (Wandner 1994).

Two subsequent experiments have evaluated the effectiveness of targeted JSA. The first was undertaken around the time of WPRS start-up, with special accommodations made to ensure experimental integrity (Decker et al. 2000). The other evaluation, which involved randomization at the margin, was done in the context of the operating WPRS program in Kentucky (Black et al. 2003). In this section, we briefly review the design and findings of these studies. A summary of results is given in Table 3.

### ***New Jersey UI Reemployment Experiment***

Enrollment in the New Jersey UI Reemployment Experiment was done between July 1986 and June 1987 (Corson et al. 1989). The sampling frame for random assignment was calibrated to target the evaluation to dislocated workers claiming UI benefits. Characteristics screens were set to construct the sampling frame.

These conditions required that a claimant must do these five things: 1) receive a first UI payment, which must occur within five weeks of applying for benefits; 2) be at least 25 years of age; 3) have worked for the pre-UI claim employer for at least three years; 4) not be on standby awaiting return to the claimant's previous job with a specific recall date; and 5) not be a union hiring hall member.

The first three of these eligibility conditions permitted the offer of an intervention early in the jobless spell; the second two out of the these first three ensured that subjects of the experiment were well-established labor force members separated from a long job attachment; and the last two conditions provided the potential for interventions to affect job search plans. Claimants who are awaiting recall to their previous job and members of union hiring halls are not required by the UI system to engage in active job search.

Random assignment sent 2,385 claimants to the control group and 8,675 to one of three treatment groups. All three treatments included JSA, the first consisting of JSA alone. The second treatment added job training to JSA.<sup>14</sup> The third treatment added a cash reemployment bonus to JSA. The bonus was for reemployment within 11 weeks of the claim and was a cash payment of half the remaining UI entitlement, with the initial offer good for two weeks and then declining by 10 percent per week. The bonus was not paid if return to work was a recall, or if the job was temporary, seasonal, part-time or with a relative. For all three treatment groups, at

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<sup>14</sup> A relocation allowance was also available in Treatment 2, but it was rarely used.

five weeks into the claim all treatments had been given: JSA orientation, skills and aptitude testing, JSA workshop, and an assessment or counseling interview.

During the benefit year, the weeks of UI benefit receipt declined by  $-0.47$ ,  $-0.48$ , and  $-0.97$  for the three treatments, respectively. All of these impact estimates carried statistical significance. The cumulative impacts on weeks of UI benefit receipt over the six years after the initial benefit claim were  $-0.76$ ,  $-0.93$ , and  $-1.72$  for the three treatments, and the estimated impact from the third treatment was statistically significant (Corson and Haimson 1995).

The New Jersey UI Reemployment Experiment demonstrated that JSA targeted to claimants likely to be long-term unemployed had the same cost-effective impact as that found for other groups of UI claimants—about half a week shorter UI receipt. The encouraging results for the bonus treatment led the U.S. Department of Labor to further investigate the ideal design for a reemployment bonus offer (Decker and O’Leary 1995).

### ***D.C. and Florida Job Search Assistance Experiment***

The Emergency Unemployment Compensation Act of 1991 authorized the U.S. Department of Labor to conduct the Job Search Assistance Experiment. The experiment was designed to evaluate whether providing early JSA to claimants identified by statistical models as likely to exhaust their UI benefit entitlement would be cost effective (Decker et al. 2000). During the planning stages of the evaluation, which was to be run in the District of Columbia and the state of Florida, federal legislation leap-frogged public policy analysis.



In 1993, President Clinton signed Public Law 103-152, which required state employment security agencies to establish and use a system of profiling all new claimants for regular UI benefits. The Worker Profiling and Reemployment Services (WPRS) system was intended to identify UI claimants who are most likely to exhaust their regular benefits, so they may be provided early reemployment services to make a faster transition to new employment.

The WPRS established a two-stage process. First, UI recipients who are expecting recall or who are members of a union hall are dropped. These groups are excluded because they are not expected to undertake an active independent job search. Second, remaining UI recipients are ranked by their likelihood of exhausting regular unemployment insurance benefits. Beneficiaries are then referred to early reemployment services in the order of their profiling score until the capacity of local agencies to serve them is exhausted.

The JSA experiment proceeded with enrollment in Florida between March 1995 and March 1996 in 10 sites around the state where regular WPRS operations were temporarily delayed. Random assignment in Florida involved 8,071 claimants. In Washington, DC, the experiment counted as the federal district's WPRS implementation. Random assignment enrollment to the JSA experiment was done in all public labor exchange offices throughout the District between June 1995 and June 1996, and involved 12,042 claimants.

The JSA experiment established an eligible pool of claimants using a two-stage process: 1) exclude job-attached and union hiring hall members, then 2) evaluate the probability of

exhausting UI entitlement and target those with the highest probabilities for the evaluation.

These claimants were randomly assigned to the control group or one of three treatment groups.

There were three treatments:

1. Structured job search assistance (SJSA): orientation, testing, job search workshop, one-on-one assessment interview. Failure to participate could result in denial of UI benefits.

Two additional visits with staff to report job search progress.

2. Individualized job search assistance (IJSa): orientation and one-on-one assessment interview. Individual plan is developed which may include additional mandatory services.

3. Individualized job search assistance with training (IJSa+): identical to IJSa, plus a coordinated effort with Economically Dislocated Worker Adjustment Act staff to enroll the customer in training.

The impacts of the three treatments on weeks of UI compensation in the benefit year in Washington, D.C., were  $-1.13$ ,  $-0.47$ , and  $-0.61$ , respectively; all were estimated to have statistical significance. Estimates of the same parameters in Florida were  $-0.41$ ,  $-0.59$ , and  $-0.52$ , all of which were also statistically significant. Both evaluations indicated that reemployment occurred at wage rates similar to previous levels. The treatments had generally positive and significant effects on earnings in Washington, DC, but no impact on participant earnings in Florida.

Structured JSA emerged as the most cost-effective intervention examined. The authors of the evaluation report attributed the generally larger impacts observed in Washington, D.C., to stricter enforcement of JSA participation requirements. They recommended making particular JSA services mandatory and maintaining clear linkages between UI and ES in the new One-Stop environment under the Workforce Investment Act (WIA).

### ***Kentucky Targeted Reemployment Services***

While Kentucky was included among the states studied in the national evaluation of WPRS, an independent assessment of WPRS in Kentucky based on an experimental design arrived at a much different conclusion. The profiling model used in Kentucky was developed by economists at the Center for Business and Economic Research at the University of Kentucky (Berger et al. 1997). In working with the Kentucky Department for Employment Services on the WPRS system, they advocated a methodology for assignment to WPRS that provided ready data for an experimental evaluation of WPRS effectiveness.

Kentucky divides the predicted UI exhaustion distribution into 20 groups spanning 5 percentile points each. Every week the local WPRS capacity is reached within one of the 20 groups. That group is referred to as a profiling tie group. In Kentucky, profiled WPRS customers within profiling tie groups are randomly assigned either to WPRS or to a control group. This is viewed as an appropriate rule for referral to WPRS from a group of UI claimants having scores that are not statistically significantly different. It also provides the basis for evaluation of WPRS based on random trials.

From the profiling tie group, experimental and control groups were formed by the random trials to conduct an evaluation of the WPRS in Kentucky (Black et al. 2003). Data were collected starting with the very beginning of WPRS implementation in Kentucky—from October 1994 through June 1996. The profiling tie groups yielded a total sample of 1,981 claimants, with 1,236 of these assigned to mandatory WPRS job search assistance. Compared to the total population of 48,002 profiled and referred Kentucky claimants in that period, the means of observable characteristics (age, schooling, gender, race, prior earnings, weekly benefit amount) for the experimental treatment group were not statistically significantly different from those in the control group.

The impact estimates for WPRS in Kentucky were dramatic. On three outcomes of interest, the estimated impacts were  $-2.2$  weeks of UI,  $-\$143$  UI benefits, and a  $\$1,054$  increase in earnings during the UI benefit year. The difference in these estimates from the national WPRS evaluation were most likely due to the fact that Black et al. (2003) essentially confined their contrasts within profiling tie groups, thereby achieving a closer counterfactual. The authors noted that the reduced duration was mainly due to no-shows for the profiling services, but it may be the case that these UI beneficiaries simply returned to work earlier. On the other hand, Dickinson, Decker, and Kreutzer (2002) and Dickinson et al. (1999) compared those assigned to WPRS who had the highest probability of benefit exhaustion against all those profiled but not referred, including many with very low exhaustion probabilities. This meant the comparison group in the national evaluation was likely to have shorter mean benefit durations than program participants, even in the absence of WPRS services.

The extraordinary foresight of the Kentucky Department of Employment Services to include randomization in assignment to WPRS should be a model for all state and local Employment Service delivery agencies. In setting up WPRS administrative rules, the Kentucky agency realized the value of evaluation research and used that orientation to help resolve the resource allocation problem. When resources are limited, randomization in program assignment can always be viewed as an equitable mechanism. It has the added benefit of providing for strong evaluation evidence.

### **Employer Incentives**

Field experiments to induce hiring, job creation, self-employment, or job retention are summarized in this section, including the Dayton wage subsidy experiment; the Illinois UI employer incentive experiment; the Washington and Massachusetts UI self-employment experiments; project GATE (Growing America Through Entrepreneurship) to assist entrepreneurs in Maine, Minnesota, and Pennsylvania; and the work-sharing experiment in Iowa and Oregon. A summary of design elements and results from these evaluations is given in Tables 4 and 5.

In standard usage, a *wage subsidy* is a payment directly to an employer to partially offset the wage costs for a newly hired employee, while a *wage supplement* means a payment directly to a worker. There is much less evidence about the latter, but results pertaining to the wage subsidy suggest a supplement may be more effective (Card and Robins 1998). The main appeal

of the wage supplement is that it is unlikely to create the type of stigma that employers may attribute to workers for whom they receive wage subsidies. The importance of the Earned Income Tax Credit (EITC) may be largely that it is paid directly to working families without any employer knowledge.

Among the four tests of wage subsidies in the United States, two operated as government programs run through the tax system and two worked as voucher experiments. During the late 1970s and early 1980s, the New Jobs Tax Credit (NJTC) and the Targeted Jobs Tax Credit (TJTC) allowed employers to reduce tax payments by a fraction of the amount paid to workers hired under the programs. Hamermesh and Rees (1984) report that NJTC subsidies were drawn for one-third of all the new jobs created during the period it was in effect. However, Perloff and Wachter (1979) estimate that the NJTC resulted in just 3 percent more jobs than would have been created without the program. The TJTC was intended to increase employment among certain targeted disadvantaged groups. Hollenbeck and Wilke (1991) found that the TJTC increased labor market success of “nonwhite male youth, but is stigmatizing for eligible individuals from other race/sex groups.” This finding that a wage subsidy acts as a stigma also emerged from the experimental studies.

### ***Dayton Wage Subsidy Experiment***

A targeted wage subsidy was operated as a field experiment with random trials in 1980–1981 by the U.S. Department of Labor in Dayton, Ohio. The evaluation involved two treatments: 1) a hiring tax credit (with 247 in the sample) and 2) a lump-sum cash subsidy

payment (299 in the sample), plus a control group (sample size of 262) of otherwise similar employers. Burtless (1985, p. 106) writes that “the results show conclusively that workers known to be eligible for targeted wage subsidies were significantly less likely to find jobs than were otherwise identical workers whose eligibility for subsidies was not advertised.” Burtless (1985, p. 105) speculates that “the vouchers had a stigmatizing effect and provided a screening device with which employers discriminated against economically disadvantaged workers.”

### ***Illinois UI Hiring Incentive Experiment***

Another experiment testing an intervention that amounted to a wage subsidy was not restricted to economically disadvantaged workers but may have also stigmatized job seekers. Woodbury and Spiegelman (1987) report that for the Illinois Reemployment Bonus Experiment, cash bonuses paid directly to persons who gain reemployment have a powerful effect in reducing the duration of unemployment, while if a cash payment for hiring a job seeker is made to employers, the effect is almost nil. Employers may be reluctant to hire workers who present a voucher for payment from the state because it signals that the workers may have “hidden” characteristics that hinder their finding employment without a state subsidy.

Most programs for the unemployed are either income-support or labor-supply enhancing; the wage subsidy is a labor-demand stimulus. But apparently regardless of the form of delivery of the subsidy to employers, it has a stigmatizing effect on workers. An obvious alternative is the wage supplement, which is paid directly to workers. This type of program has even been

recommended to help welfare recipients (who might face the most severe stigma) gain reemployment.<sup>15</sup>

### ***Washington and Massachusetts UI Self-Employment Experiments***

Self-employment initiatives for unemployed persons have been operating in Europe since 1979.<sup>16</sup> Seventeen countries belonging to the Organisation for Economic Co-operation and Development (OECD) have programs patterned after either the French model, which grants a lump sum to the unemployed who plan to become self-employed, or the British model, which gives a series of periodic support payments during the start-up phase of self-employment.<sup>17</sup> The British model amounts to a waiver of the work search requirements for continued receipt of periodic unemployment compensation payments. American experiments tested the French model in Washington State and the British model in Massachusetts (Benus et al. 1995).

Self-employment assistance in Massachusetts was assessed by randomized controlled trials between 1990 and 1993. The treatment group increased self-employment, reduced the length of their unemployment, and increased their total time in employment—including self-employment plus wage and salary employment. The experiment also had a substantial positive impact on participants' earnings. In a benefit-cost framework, self-employment assistance was estimated to be cost effective for project participants, society as a whole, and the government

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<sup>15</sup>See for example Lerman (1985).

<sup>16</sup>Background information on the European experience with and the American experiments in self-employment for unemployed persons can be found in Wandner (1994).

<sup>17</sup>The French model is followed in Luxembourg, Norway, Portugal, Spain, and Sweden, while the British model is used in Australia, Belgium, Canada, Denmark, Finland, Greece, Ireland, Italy, the Netherlands, and Germany.



sector as well. Overall, the self-employment assistance provided in the demonstration significantly increased participants' total time in employment (i.e., the combination of self-employment and wage and salary employment) after being randomly assigned to the project. Including time spent in self-employment and wage and salary employment, participants were employed 1.9 months longer than the control group. Total earnings of the average project participant increased by \$5,940 over the amount earned by the average control-group member over the three-year follow-up period.

The Washington UI Self-Employment Demonstration (SEED) involved random-assignment enrollment to treatment and control groups in Washington State between September 1989 and September 1990, with business services available for participants through March 1991 (Benus et al. 1995). A total of 755 new claimants were enrolled in SEED at the six sites and were offered demonstration services; 752 new claimants who applied to SEED were assigned to the control group. The SEED treatment followed the French-style lump-sum payment method, with the offer being the remainder of a UI beneficiary's entitlement at the start of self-employment efforts along with business start-up and development efforts. The first Washington telephone survey was conducted, on average, 21 months after random assignment.

Only about 4 percent of targeted Washington UI claimants met the initial eligibility requirements of attending an orientation and submitting an application. Compared to the control group, treatments achieved the following 10 things: 1) spent about 4.0 months more in self-employment; 2) earned more than control subjects from self-employment during the follow-up period; 3) had reduced likelihood of wage and salary employment; 4) spent about one month

less in wage and salary employment; 5) earned significantly less from wage and salary employment; 6) had similar earnings from wage and salary and self-employment during the observation period; 7) had higher rates of employment; 8) reduced the length of the first unemployment spell; 9) excluding the lump-sum payment, had reduced UI benefit receipt during the first benefit year; 10) including the lump-sum payment, had higher total UI payments during the first benefit year.

### ***Maine, Minnesota, and Pennsylvania Entrepreneurship Experiment***

Growing America Through Entrepreneurship (GATE) studied the value of helping new entrepreneurs start and expand their own small businesses (Benus, McConnell, et al. 2008). Enrollment was done in state employment offices or kiosks between Fall 2003 and Summer 2005 in seven sites in Maine, Minnesota, and Pennsylvania. In the end, only data from Minnesota was sufficient for analysis. GATE offered three services: 1) individual assessment session; 2) training in general business, in legal and personnel issues, and in business accounting computer software; and 3) individual meetings with business counselors about business plans and loan applications. A total of 4,198 Minnesota GATE applicants were randomly assigned to either the treatment or the control group. The response rate to the follow-up survey was 82 percent at 18 months after enrollment. Survey data were merged with administrative records on UI payments and quarterly wage records covering the 12 months before and after random assignment.

Project GATE generated a small but significant impact on business ownership in the 18-month follow-up. By the third quarter after random assignment, 43 percent of the program group

reported owning a business, a statistically significant 6 percentage points more than the comparison group. The advantage for participants dwindled to 3 percentage points (statistically significant at the 0.10 level) at the 18-month follow-up. There were no significant effects on total employment with rates (self-employment plus wage and salary employment) at about 70 percent in the first quarter after random assignment and 85 percent eighteen months after random assignment. However, compared to the control group, GATE participants were more likely to be self-employed and less likely to be employed in wage and salary jobs.

Control-group members earned slightly more than participants from wage and salary jobs, and about the same through self-employment, so that GATE participants earned somewhat less during the 18-month follow-up. The wage and salary difference was \$1,800 based on the survey, but only \$200 less based on UI quarterly wage records (not statistically significant). Both groups earned about \$6,000 over the 18-month follow-up period. GATE increased receipt of UI benefits by about one week, or about \$340 per person for all participants, and by about \$605 for those already receiving UI benefits when they applied to GATE. There were no program impacts on the receipt of public assistance or other income.

Overall, results from the GATE study suggest the following things:

- Self-employment services can be effectively offered at One-Stop Career Centers.
- Increased business ownership might not lead to increased self-employment earnings in the short run.
- Self-employment can lead to a loss of earnings from wage and salary jobs in the short run.

- Self-employment programs improve outcomes for UI recipients more than others.

### ***Iowa and Oregon Work-Sharing Experiments***

Short-time compensation (STC), commonly known as work sharing, is one of the very few public employment policies to directly support labor demand.<sup>18</sup> Under STC, work reductions are shared among employees by reducing work hours instead of laying off some workers. The STC program partially replaces lost earnings by paying a percentage of the entitled UI weekly benefit amount equal to the percentage reduction in weekly work hours. Currently, 28 states have STC plans, and in those states STC is used relatively infrequently compared to regular UI (Balducchi 2015). The Middle Class Tax Relief and Job Creation Act of 2012 had features promoting STC in the states, including temporary reimbursement by the federal government to states of STC benefits paid for three years (PL 112-96, Title III, Subtitle D). If STC were available in all states, in recession periods it could be used as a channel of fiscal policy by supplementing emergency federal extended unemployment benefits. A field experiment started in 2014 aimed to identify effective strategies to promote broader employer use of STC.

The experiment involved randomized controlled trials in Iowa and Oregon designed to increase employer awareness and adoption of STC in lieu of temporary layoffs, and thereby reduce regular UI claims and weeks of UI compensation. The evaluation sought to answer research questions about 1) program awareness and interest, 2) program use, 3) costs, and 4)

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<sup>18</sup> This summary is adapted from the evaluation design report (Houseman et al. 2017).

other lessons. The federal reimbursement feature was part of the Iowa treatments, but not the Oregon treatments. However, because the Middle Class Tax Relief and Job Creation Act required that benefit charges from STC and regular UI be treated the same, the new law affected the UI tax treatment of many Oregon employers with prior STC experience.

Before the experiment began, the only posted STC program information generally available to Iowa employers was one brief section in the UI employer handbook. The researchers worked with Iowa state officials to create a webpage and to develop informational materials, including a brochure, fact sheet, and list of frequently asked questions about STC. Stratified random assignment to treatment and control groups was done for Iowa employers with more than five employees. Sampling strata were defined by employment size, UI claims history, industry, and location. Treatment-group employers were given program information and directed to the program staff and website by a series of postal mailings. The Iowa interventions were delivered over 12 months starting September 2014 and involved two mass mailings to all treatments in September 2014 and May 2015, an insert with the annual tax rate notice that is mailed out in November, and quarterly mailings to treatment employers who had UI claims against them in the previous calendar quarter.<sup>19</sup> The offer of federal reimbursement for STC benefits was available up until February 22, 2015. When the experiment started in September 2014, only 14 STC plans were operating in Iowa.

Before the experiment, Oregon had relatively well-developed materials on STC, but had not systematically advertised the program to employers. As with Iowa, the researchers worked

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<sup>19</sup> There was an error in the tax rate notice STC insert that contaminated about 20 percent of the control group and delayed the mailing by two months to 80 percent of the treatment group.

with the state to improve and update these informational materials. In the Portland metropolitan area, an RCT design similar to that in Iowa was implemented, and a quasi-experimental design evaluation was implemented in the balance of the state.<sup>20</sup> During the design phase for the experiment, USDOL set up a technical work group of advisers for the study, including social scientists and state and federal practitioners. One recommendation from the technical work group to increase employer usage of STC was not amenable to an RCT evaluation: the work group recommended a cultural change to saturate the environment with information about STC on mass media and public announcements made through political channels, business and community organizations, and employee groups.

In the Portland metropolitan area, as in Iowa, the researchers constructed a stratified sample of all employers and randomly assigned them to treatment and control groups. It was judged that the Portland metro area had enough users of UI and STC that the sample size would be sufficient to permit estimation of statistically significant effects big enough to be of policy interest. The Oregon Employment Department divides the state into 15 state Worksource Regions for the purposes of delivering services. The quasi-experimental design used Worksource Regions located outside of the Portland metro area as the basis for employer assignment: all employers located in one set of Worksource Regions were given interventions, while all employers located in the other set were not. These were referred to as the “treatment” region and

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<sup>20</sup>The Portland area alone has nearly as many STC-eligible employers as the whole state of Iowa. The Iowa RCT involved about 14,000 employers in the treatment and control groups, while in Oregon the count was about 10,000 in each group.

the “comparison” region, respectively.<sup>21</sup> As in Iowa, the Oregon interventions were administered for a one-year period. In Oregon, the treatment period started in late October 2014.

Use of STC by Iowa employers did not change appreciably after the interventions began, but in regression models controlling for observable characteristics, there was a small but significant increase in starting new STC plans by employers in the treatment group. Furthermore, the pattern of weekly STC payments in Iowa suggests that employers tried to take advantage of temporary federal payment of STC benefits. The informational efforts had positive and significant effects in Oregon, and there was some evidence that employers with prior STC experience may have started new plans in response to the information. Part of the response by experienced STC employers may have resulted from the news that STC payments in Oregon would affect employer UI tax rates the same as regular UI payments.

## **Summary and Relevance to UI Today**

As social insurance, UI pays compensation to labor force members who are involuntarily separated from their jobs while they are actively seeking work. The program embodies elements of both private insurance and social assistance. While benefit levels are related to prior earnings, they do not completely replace lost earnings, but pay an amount that is directly related to prior wage levels up to a socially determined adequate weekly maximum. The elements most

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<sup>21</sup> Following Bloom (2000), the minimum detectable effect in the Oregon QED evaluation will be larger than in the RCT evaluation by a factor approximated by the square root of  $[1/(1-R^2_A)]$ , where,  $R^2_A$  is the coefficient of determination from the regression of the QED treatment indicator on characteristics of employers in the treatment and control samples.

reflecting private insurance principles involve using work-search requirements to test initial and continuing eligibility for benefits.

Research in the 1970s recognized the moral hazard risks of work disincentives resulting from paying UI benefits and estimated the effects to be between 0.5 and 1.5 weeks for a 10 percent increase in the wage replacement rate. This work led to a series of UI-related field experiments to identify improved administrative practices and incentives to control system costs and improve beneficiary outcomes. The reemployment bonus experiments in the 1980s estimated that offers would reduce UI durations by an average 0.5 week and be modestly cost effective. Simulations based on the bonus experiments found that a bonus amount smaller than the average, when tested and targeted to the half of UI-eligible beneficiaries who are most likely to exhaust UI, achieved a 0.5 week reduction more cost effectively. Field experiments estimating the effects of strengthening UI work-search requirements estimated duration reductions between 0.5 and 1.0 week. An experiment removing the work test saw durations jump by 3.3 weeks. The UI work test involves connecting the unemployed to job search assistance. Experimental evaluations of targeted job search assistance estimated that durations were shortened by between 0.5 and 2.2 weeks.

Field experiments evaluating hiring incentives offered to employers have generally not found cost-effective policy options, mainly because of low employer take-up. However, some smaller UI programs show promise as labor demand policies—particularly when properly targeted. Field experiments that paid UI as self-employment assistance with a work-search waiver during the business start-up phase, and targeted to those most likely to exhaust UI, were



found to be cost-neutral to the UI system and often lead to second-order employment effects though hiring. Work sharing, or short-time compensation (STC), which pays employees a fraction of their weekly UI equal to the proportionate reduction in work hours, can help employers control layoff costs and retain talent during business downturns. A recent field experiment suggests employers will sometimes use STC instead of layoffs when they know how STC works.

The federal-state UI program is now gradually rebuilding system reserves after the Great Recession. Many states were left with billions in debt from paying regular benefits, even though the federal government fully paid for benefit extensions at unprecedented levels. Some states are retreating from accepted standards of UI adequacy with the expectation that the federal government will once again intervene when an unemployment crisis emerges. The potential duration of regular UI benefits is no longer at least 26 weeks in all states. However, after welfare reform, all social policy is now employment policy. Making and maintaining connections to the workforce is the only path to self-sufficiency. Policymakers are looking for improvements to the public employment system that will be cost-neutral. The experiments reviewed in this paper offer a menu for further improvements.

Active reemployment services and targeted assistance are ways to serve more workers within a given UI benefits budget. Financing of services and financing of benefits have both been hampered by the inadequate federal taxable wage base of \$7,000, which has not increased since 1982. Financing of Wagner-Peyser employment services, and in many states adequate financing of regular UI benefits, is limited by the federal taxable wage base. In 1939, this wage

base matched the Social Security wage base, but the two have not remained in balance over the years: the latter has grown to \$118,500, while the former has barely budged. Relying on the recent field experiment on REAs in Nevada, the federal government is attempting to strengthen and expand state use of UI eligibility reviews and rejuvenate UI worker profiling. We are now in a transition phase where REA is being integrated with WPRS so that job search activation and reemployment services are targeted to UI beneficiaries most at risk of long-term unemployment and benefit receipt. As reviewed in this paper, elements of the REA/WPRS effort—now called Reemployment Services and Eligibility Assessments (RESEA)—have been found effective in field experiments. The UI system has been weakened by financing challenges, but incremental improvements have often been guided by evidence from classically designed random trials. This approach requires an understanding of how the system works and of the incentive and administrative structure. It also requires up-front public investment for evaluation studies. However, the results can yield an employment security system that is a stronger part of the social safety net for all Americans.

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<b>Table 1 Reemployment Bonus Experiments' Impacts on Benefit-Year Weeks of UI (standard errors in parentheses)</b>		
Illinois		
Bonus amount	Qualification period	Impact estimate
\$500	11 weeks	-1.15** (-0.27)
New Jersey		
Bonus amount	Qualification period	Impact estimate
Half the remaining UI entitlement, with the initial offer good for two weeks and then declining by 10% per week.	11 weeks	-0.69** (-0.23)
Pennsylvania		
Bonus amount		12 weeks (long)
3 × WBA (low)	-0.65 (-0.34)	-0.36* (-0.28)
6 × WBA (high)	-0.44* (-0.31)	-0.82** (-0.27)
Declining		-0.33 (-0.30)
Washington		
2 × WBA (low)	-0.06 (-0.30)	-0.50* (-0.29)
4 × WBA (medium)	-0.19 (-0.30)	-0.14 (-0.30)
6 × WBA (high)	-0.62* (-0.33)	-0.73** (-0.34)

\* significant at the 0.10 level; \*\* significant at the 0.05 level.

**Table 2 Experiments on Job Search Assistance and the UI Work Test**

Authors (year published)	Title	Design	Sample	Findings
Corson, Long, and Nicholson (1985)	Evaluation of the Charleston Claimant Placement and Work Test Demonstration	T1: Stronger work test T2: T1 plus enhanced placement services T3: T2 plus Job Search Workshop (JSW) C: Customary work test	Charleston, SC, February to December 1983: T: 4,247, C: 1,428	T1: -0.55* wks. UI T2: -0.61** wks. UI T3: -0.76** wks. UI Impacts greater on men and construction workers.
Johnson and Klepinger (1991, 1994)	Evaluation of the Impacts of the Washington Alternative Work Search Experiment	T1: Exception reporting T2: New work search policy T3: Intensive services C: Existing work search policy	Tacoma, WA, July 1986 to August 1987: T: 6,763, C: 2,871	T1: +3.34** wks. UI T2: +0.17 wks. UI T3: -0.47* wks. UI Exits increased preceding required service participation.
Klepinger et al. (1998)	Evaluation of the Maryland Unemployment Insurance Work Search Demonstration	T1: Report four employer contacts weekly T2: Two contacts required weekly, but no reporting T3: Report two contacts weekly plus a four-day JSW T4: Report two contacts weekly and both verified C1: Standard policy: report two contacts weekly but not verified C2: Standard policy, but told data were to be used in an evaluation study	Maryland: Six offices, January 1, 1994, to December 31, 1994; Combined sample, 23,758 monetarily eligible new initial UI claimants.	T1: -0.7** wks. UI T2: +0.4* wks. UI T3: -0.6** wks. UI T4: -0.9** wks. UI Impacts identical against either control group, suggesting no Hawthorne effect present. Treatments 1, 3, and 4 had no earnings impacts. Treatment 2 raised earnings by 4** percent.
Ashenfelter, Ashmore, and Deschênes (2005)	Do Unemployment Insurance Recipients Actively Seek Work? Evidence from Randomized Trials in Four U.S. States	T1 & T2: 1st visit to verify eligibility and separation T1: 2nd visit to verify job search C: 1st visit std. eligibility review, 2nd visit std. eligibility review	Application and reviews in person: T1: 760; T2: 1,140; C: 1,900	T1 & T2: -5% initial UI eligibility rate. T1: no statistically significant effect on continuing eligibility or duration of benefits.

NOTE: T: treatment; C: control group; JSW: job search workshop. \* significant at the 0.10 level; \*\* significant at the 0.05 level.

<b>Table 3 Experiments on Targeted Job Search Assistance</b>				
Authors (year published)	Title	Design	Sample	Findings
Corson et al. (1989)	New Jersey Unemployment Insurance Reemployment Demonstration Project	T1: JSA T2: JSA plus training or relocation assistance T3: JSA plus a cash bonus C: Eligibility: first UI payment, age, tenure, temporary layoffs, union	New Jersey: July 1986 to June 1987: T: 8,675 C: 2,385	T1: -0.47** wks. of UI T2: -0.48** wks. of UI T3: -0.97** wks. of UI 6-year T1: -0.76 wks. of UI 6-year T2: -0.93 wks. of UI 6-year T3: -1.72** wks. of UI
Decker et al. (2000)	Assisting Unemployment Insurance Claimants: The Long-Term Impact of the Job Search Assistance Demonstration	T1: Structured JSA T2: Individualized JSA T3: T2 plus training C: Not on standby or a union hiring hall member, and predicted likely to exhaust UI entitlement	DC and Florida: DC: June 1995 to June 1996, 8,071 claimants. FL: March 1995 to March 1996, 12,042 claimants.	DC T1: -1.13** wks. of UI DC T2: -0.47** wks. of UI DC T3: -0.61** wks. of UI  FL T1: -0.41** wks. of UI FL T2: -0.59** wks. of UI FL T3: -0.52** wks. of UI
Black et al. (2003)	Is the Threat of Reemployment Services More Effective Than the Services Themselves? Experimental Evidence from the UI System	T: WPRS profiled and referred to early JSA reemployment services C: Profiled and in the same predicted UI exhaustion cohort as T, but not referred to JSA	Kentucky: October 1994 to June 1996, T: 1,236; C: 745.	In the benefit year  T: -2.2 weeks of UI T: -\$143 in UI benefits T: \$1,054 in earnings

NOTE: T: experimental treatment group; P: participant group; C: experimental control group or comparison group; JSW: job search workshop.

\* significant at the 0.10 level; \*\* significant at the 0.05 level.



<b>Table 4 Experiments on Employer Incentives</b>				
Authors (year published)	Title	Design	Sample	Findings
Burtless (1985)	Are Targeted Wage Subsidies Harmful? Evidence from a Wage Voucher Experiment	T1: Hiring tax credit T2: Hiring cash subsidy C: No assistance All groups drawn from AFDC and general assistance recipients.	Dayton, OH December 1980 to May 1981 T1: 247 T2: 299 C: 262	T1: -7.6% employed T2: -7.9% employed  Few subsidy vouchers submitted by employers (27.1% of those employed).  No impact on wage rates.
Woodbury and Spiegelman (1987)	Bonuses to Workers and Employers to Reduce Unemployment: Random Trials in Illinois	T: \$500 paid to the employer if hired within 11 weeks of UI claim and continuously employed for four months.  C: Eligible for regular UI.	Illinois (22 Job Service offices), mid-1984 to early 1985 T: 3,963 C: 4,186	T: -\$164 UI for white females, no impact on white males or blacks. Low take-up: 22.8% qualified for bonus, but employers only converted voucher to cash for 2.8% of treatment assigned.
Benus et al. (1995)	Self-Employment Programs: A New Reemployment Strategy, Final Report on the UI Self-Employment Demonstration	TW: lump-sum cash-out of UI entitlement after self-employment training. TM: UI work search waiver after completing self-employment training.  C: UI-eligible, attended self-employment orientation and applied.	WA: Sept. 1989 to March 1991 TW: 755 CW: 752  MA: May 1990 to April 1993 TM: 614 CM: 608	TW: more self-employment, less regular employment, higher self-employment income, lower earnings from regular employment, net earnings gain.  TM: more self-employment, more regular employment, higher earnings from both regular and self-employment.  In both states, self-employment was most successful for educated males aged 35-55.

NOTE: T = experimental treatment group; C = experimental control group; JSW= job search workshop.

<b>Table 5 Effects of Self-Employment Assistance Programs</b>						
Authors (year)	Method	Sample	Intervention	Self-employed	Any employment	EI/UI receipt
Benus et al. (1995)	RA: Profiled high exhaustion probability	Massachusetts inflow 1990–1993 treatments: 614; controls: 608	British-style SEA: weekly UI pay with work search waiver	at 19, 31 mos., +11%, +5%	at 19, 31 mos., +1%, –4%	–1.8 wks.
Benus et al. (1995)	RA: Profiled high exhaustion probability	Washington state inflow 1989–1990 treatments: 755; controls: 752	French-style SEA: lump-sum balance of UI entitlement	at 21, 33 mos., +16%, +12%	at 21, 33 mos., –9%, –8%	–7.6 wks.
Benus, McConnell, et al. (2008)	RA: GATE participants got 13 hours more SE training than comparison group.	Inflow at One-Stop centers: 2003 to 2005; Seven sites: Pennsylvania (2), Minnesota (2), Maine (3)	Assessment, classroom training, 1-on-1 business counseling, help with loan applications	At 3 qtrs., +6%; at 6 qtrs., +3%	No statistically significant effects, but more employed in SE than in wage and salary work.	+1 wk., +2 wks. if on UI first