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Do unemployment insurance recipients actively seek work? Evidence from randomized trials in four U.S. States

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Abstract

In this paper, we report the results of the only field test of which we are aware that uses randomized trials to measure whether stricter enforcement and verification of work search behavior alone decreases unemployment claims and benefits paid in the U.S. unemployment insurance (UI) program. These experiments, which were implemented in four U.S. sites in Connecticut, Massachusetts, Virginia and Tennessee, were designed to explicitly test claims based on nonexperimental data, summarized in Burgess and Kingston (An Incentives Approach to Improving the Unemployment Compensation System, W. E. Upjohn Institute for Employment Research, 1987), that a prime cause of overpayments is the failure of claimants to actively seek work. Our results provide no support for the view that the failure to actively search for work has been a cause of overpayment in the UI system.
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1. Introduction

In the last two decades, U.S. policies have moved from the use of incentives to the use of sanctions to promote work effort in social programs. This shift in orientation in public policies has been documented by Jencks (1992), who, like Murray (1984), argues that it has been based, in part, on the perception that these programs are riddled with abuse. Surprisingly, except for anecdotes, there is very little systematic

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evidence of the extent to which sanctions applied to abusive use of social entitlements result in greater work effort.

In this paper, we report the results of the only field test of which we are aware that uses randomized trials to measure whether stricter enforcement and verification of work search behavior alone decreases unemployment claims and benefits paid in the U.S. unemployment insurance (UI) program. These experiments, which we implemented in four sites in Connecticut, Massachusetts, Virginia, and Tennessee, were designed to explicitly test claims based on nonexperimental data, summarized in Burgess and Kingston (1987),[1] that a prime cause of overpayments is the failure of claimants to actively seek work.

Our results provide no support for the view that the failure to actively search for work has been a cause of overpayments in the UI system. These results provide a much-needed complement to the results of other UI system experiments reported by Meyer (1995), who first brought these unique field experiments to broad attention. The treatments in the experiments Meyer (1995) surveys, which he reports were cost effective, incorporated elements of both work search verification and a system designed to teach workers how better to search for jobs. The experiment reported here incorporated only the element of work search verification, and we find that the treatments provided no benefits. Taken together, the results of both sets of experiments imply that providing workers with subsidized job search assistance may be a relatively inexpensive way to provide cost effective, but small, benefits to both workers and society.

In the remainder of the paper we first set the stage for our analysis with a brief description of previous research on UI work search rules and the details of operation of the current U.S. system. We next discuss our experimental design, the nature of the experimental treatment, and our data collection procedures. Since randomization is so important for our estimation procedure, and since there is some evidence that several field experiments have not been properly randomized, we next report tests of the effectiveness of our simple randomization technique. Finally, we report the effect of the experimental treatment on claimant qualification rates, benefit payments, and claim durations. We conclude with a brief discussion of the implications of our findings.

2. Previous research

Since its inception in 1930, the UI program has always been controversial.[2] The controversy is often attributed to the potential disincentives created by the program: UI reduces the cost of searching for a job while unemployed, which might prolong the length of insured unemployment. In order to reduce these disincentives, states typically impose work-search requirements on UI recipients. However, state agencies do not always formally validate the information provided by the recipients,[3] which

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[1] See also Kingston et al. (1986) and Wolf and Greenberg (1986).
[2] See Blaustein et al. (1997).
[3] See, for example, Decker (1997).

raises questions about the efficacy of the work-search requirements. These concerns and others led the U.S. Department of Labor to fund a series of experimental and nonexperimental research projects during the 1980s.

The study by Corson et al. (1988) is based on a nonexperimental design and evaluates the effects of work search rules in 10 states. The authors report that on average, claimants from states with stricter rules are more likely to search for work. They also note that in their sample, states with the stricter rules also experienced higher unemployment rates. This evidence does not necessarily imply that tighter regulations increase search effort since claimants in states with stricter rules might have searched harder in response to adverse labor market conditions.

The Job Search Experiments, conducted in Virginia, New Jersey and Washington are analyzed in great detail in Corson et al. (1985, 1989) and Johnson and Klepinger (1991). The Virginia and Washington experiments incorporated elements of job-search assistance and tighter job-search requirements (or better monitoring of job-search) in some of the treatment groups. The New Jersey experiment tested the effects of job-search assistance programs and reemployment bonuses. Despite some differences in the design of the experiments and in the treatments offered, the results from these studies suggest that job-search assistance and stricter job-search requirements reduced weeks of UI receipt by about one-half of a week, relative to the standard state procedures. The lower average claim duration implied a reduction in total benefits received of about $80 per claimant, which generally exceeded the additional costs of the treatments.

As noted by Meyer (1995), an important limitation of these experiments is that they combine additional job-search services and better enforcement of the job-search rules, which makes it difficult to determine what aspects of the experiments induced the change in outcomes. Therefore, we cannot infer from the previous experiments the net effects of stronger job-search requirements or monitoring relative to the existing regulations.[4] The experiments described in this paper share some common characteristics with the previous ones. For example, the treatments provided the claimants with better information on continuing eligibility regulations. However, they also have important differences, notably in the treatments offered and in the population considered. These differences will allow us to assess the effects of stricter enforcement of the existing eligibility requirements on claim outcomes, including qualification rates, benefit payments and claim durations.

3. Overview of the existing unemployment insurance application procedures

Qualification for unemployment insurance benefits is determined on the basis of rules whose extent and application differ substantially across states.[5] In general there

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[4] However, the Washington experiment provides evidence that eliminating the work-search requirements increased claim durations by about 3 weeks.

[5] An account of the various state laws is included in U.S. Department of Labor "Comparison of State Unemployment Insurance Laws," various years.

are three key requirements that a UI applicant must satisfy in order to qualify for the receipt of benefits. The applicant must have (1) sufficient labor force attachment prior to job separation; (2) an involuntary job separation; and (3) the ability and willingness to seek and accept suitable employment.

The initial eligibility determination is based on elements (1) and (2). First, applicants must satisfy the monetary requirements by having earned a specific amount during a "base period". Second, applicants must demonstrate that they were separated from their jobs through no fault of their own. Unemployed individuals disqualified under these rules may be disqualified from the receipt of benefits for a fixed number of weeks or for the entire duration of the spell.

The continuing eligibility rules relate to element (3) and specify that the claimant must (i) be "able and available" to work; and (ii) undertake active search for work. Individuals are considered able to work if their physical and mental condition is appropriate. Availability for work generally means being in the labor market area and having the necessary transportation during the filing week. All states require that unemployed workers register at a local unemployment office (or employment service office) as evidence of active job search. In addition, in almost all states, claimants must provide evidence of employer contacts each week. These requirements must be satisfied for each week during which benefits are claimed. Failure to satisfy any of these conditions for a given week makes the claimant ineligible for benefits during that week.

The application process typically involves two visits to a local UI office. At the initial visit, applicants provide information that is then reviewed for determination of initial eligibility. At the second visit claimants learn their eligibility status, and they can make an appeal if they are ruled eligible. After the second visit, the first check is issued for those who qualified for benefits, and the payments continue until the claimant finds a new job, exhausts his benefit entitlement, or fails to satisfy the continuing eligibility criteria for a given week. Most states also require an additional visit after the claimant has been unemployed for 6–9 weeks, for an eligibility review.

4. Experimental treatments, research design and data collection procedures

4.1. The treatments

The goal of our experiments was twofold. One goal was to evaluate the effectiveness of new eligibility reviews in detecting initially ineligible claimants. A second goal was to determine the extent to which UI recipients actually satisfy the work-search requirements. Claimants were randomly assigned to treatment and control groups when they first applied for UI benefits. In each of the four states, the treatment consisted of a number of additional verifications of initial and continuing eligibility, prior to the issuance of the first check.[6] After the second visit, claims in the

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[6] In the other Job Search Experiments, the treatments were offered later during the claim.

treatment and control groups were handled in the same manner, according to the established state procedures.[7]

At the first visit to the UI office, the applications in the treatment group were reviewed for eligibility with the new steps. Work-search requirements were further described to the applicants in the treatment group by providing them with written notification. At the second visit, the nature of 2 treatments differed. In the first treatment group (group 1), the job contacts reported by the claimants were actually verified by the personnel of the UI office in a telephone interview with the employers. In the second treatment group (group 2), the standard procedures applied, that is, the list of contacted employers was reviewed, but no direct contact was made with the listed employers. We emphasize that the applicants in the treatment groups 1 and 2 received the same information concerning the mandatory job-search contacts and the potential verification of such contacts. The applicants in groups 1 and 2 who failed to meet these new requirements at either stage of the treatment were disqualified, either temporarily or permanently. Fig. 1 contains a diagram representing a typical application procedure for the individuals in the treatment and control groups.

The additional processing costs per claim associated with the treatments varied from $1 to $15 (in 1984 nominal dollars) across the four states. The additional costs were mainly due to the added staff time required to go through the supplemental procedures. These cost differences reflect the disparity in average hourly compensation (including fringe benefits) of the local UI office personnel, as well as the variation in the time necessary to complete the additional steps across states.[8]

Of course, if such small expenditures can generate significant benefits, this implies that the work search requirements are not, in fact, being implemented. The purpose of these experiments was precisely to test such a claim.

The applications in the control group were handled following the established state procedures. After the second visit, the continuing eligibility of the treatments and controls was reviewed in the same manner, following the established state procedures.[9] As a result of this design, our experiment will not identify any timing pattern in the work-search effort of UI recipients after the second visit. The direction of such job-search patterns is not clear a priori. On the one hand, claimants might increase search effort as the spell lengthens in order to find a new job before their benefit entitlement runs out. On the other hand, discouragement might induce them to search less.

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[7] After the second visit, claimants typically return a form to the UI office by mail for each claiming period to demonstrate continuing eligibility.

[8] Time differences can arise if some of the supplemental steps for initial eligibility (not the work-search verification component) were already part of the existing state procedures. Table 7 reports the average additional costs associated with the treatments in each state.

[9] If some issues for the claims in the treatment groups were not resolved after the second visit, the experimental procedures still applied in the determination process.

Group	First Visit	Second Visit (one week later)
Treatment (Group 1) [20%]	• Standard initial eligibility reviews. • Additional initial eligibility reviews: (i) Verification of the reason for job separation with previous employer (ii) More emphasis on obtaining accurate base period earnings information • Additional information on work-search requirements.	• Verification of reported job-search contacts with employer. • Standard continuing eligibility reviews.
Treatment (Group 2) [30%]	• Standard initial eligibility reviews. • Additional initial eligibility reviews: (i) Verification of the reason for job separation with previous employer (ii) More emphasis on obtaining accurate base period earnings information • Additional information on work-search requirements.	• Standard continuing eligibility reviews.
Control (Group 4) [50%]	• Standard initial eligibility reviews.	• Standard continuing eligibility reviews.

Fig. 1. Typical application process in the treatment and control groups.

4.2. Data sources and collection

The data used in the analysis comes from two sources. First, the UI office personnel in each site collected data on the progress and outcome of the claims.

These data were complemented with administrative records. The administrative data contains information on demographic characteristics, claim duration, and total benefits received.

The local UI offices collected information on all valid applications during the study period,[10] following specific guidelines. The personnel collected information on the filing date, the eligibility status, and on the reason for ineligibility. The personnel were instructed to complete one form for each claimant, using the same coding scheme for each state. These data are therefore perfectly comparable across states.

The administrative data were recorded from each state's computerized data system, generally 1 month after the end of the study period.[11] This provides detailed information on completed and ongoing unemployment spells for each individual in the study groups. The administrative data was linked to the UI office data using the social security number (SSN) of individuals. The processing of UI administrative data is not uniform across states. Two states (Tennessee and Virginia) only recorded information on claimants who satisfied certain eligibility criteria. The records for Massachusetts only cover the second half of the study period. Moreover, as is often the case in such studies, the match between the UI office and the state data was not perfect. We were typically able to match about 80% of the UI office data to the administrative data. The unmatched records were not used in the analysis.

5. The effectiveness of the randomization

The population considered in this study consists of unemployed individuals applying for benefits and filing initial, in-person, and intrastate claims during the experimental period.[12] Applicants who did not satisfy these criteria were excluded from the analysis. Randomization into the treatment and control groups was based on the 7th digit of the applicant's Social Security Number.[13] It is well known that the last four digits of social security numbers are not assigned deterministically, so this method provides a unique, but random, identification for each applicant at a trivial cost. Nevertheless, in view of reports that some field experiments have not been properly randomized (see Meyer, 1995), we report tests of the effectiveness of this method below.

Social experiments are also subject to other potential limitations, in particular to randomization bias.[14] Randomization bias occurs when random assignment causes the population participating in a program to be different from the population

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[10] The length and calendar time of the study period varied slightly across states. See Table 7 for more details.

[11] This delay in the data collection procedure was to ensure that enough time was available to process the claims filed during the last days of the experimental period.

[12] This excludes individuals filing transitional, continuing or interstate claims.

[13] Applicants with an even digit (0,2,4,6,8) were assigned to the control group. Applicants whose digit equaled 1 or 5 were assigned to treatment group (1) while those with 3,7 or 9 were assigned to treatment group (2).

[14] See Heckman (1992) and Heckman and Smith (1995).

participating when the program operates normally. Since in our experiment, randomization is staged at the initial claim filing, for the normal inflow of applicants (apart from the minor exceptions listed above), randomization bias should not be a major problem.[15]

Table 1 contains the demographic characteristics of the individuals in each of the study groups. Corresponding to our discussion of the experimental design, we present the data for four groups. The sample statistics for the claimants in the treatment group who had their work search verified (group 1) are listed below column 1, while those for the claimants who received the treatments, but not the work search verification (group 2) are listed below column 2. The individuals in the two treatment groups are combined in column 3, while in column 4 we report the averages for the control group.

The administrative records contain background information on three aspects of the applicants: demographic characteristics (age, gender and race), prior work history (base period earnings), and UI entitlement (weekly benefit amount). As Table 1 indicates, the level of these variables is similar across the states and between the study groups, with the exception of the weekly benefit amount, which is smaller in Tennessee, and the proportion of black recipients, which is smaller in Massachusetts.

We use a variety of techniques to evaluate the effectiveness of the randomization process. First, we calculate t-statistics to test the null hypothesis of equality of means between the treatment and control groups.[16] In almost all cases, the results (not reported) fail to reject the null hypothesis of equality of means, which suggest that randomization was effective. In 57 contrasts there are 3 contrasts that are statistically significant at the 5% level, which is almost exactly what should be expected if assignment were random (that is, $0.05 \times 57 = 2.85$). Moreover, in all cases the differences, even when statistically significant, are small.[17] In another attempt to verify the effectiveness of random assignment, we used Kolmogorov–Smirnov statistic and the Wilcoxon-signed rank test to test the null hypothesis of equality of the distribution functions of the continuous variables between the treatment and control groups. In all cases, we were unable to reject the null hypothesis of equality of the distribution functions.

These statistical tests, based on the demographic characteristics, work histories and entitlement of the claimants, strongly suggest that the treatment and control groups were drawn from the same population. Therefore, we will use standard analysis-of-variance methods to estimate the treatment effects.

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[15] Since no other program offers wage compensation during periods of unemployment, substitution bias is also unlikely to affect our results.

[16] That is, we test the equality of the means for each variable in Table 1 for groups (1) and (4), (2) and (4), and (3) and (4).

[17] The validity of the statistical tests for Tennessee and Virginia is limited by the fact that we observe the demographic characteristics only for a nonrandom subset of all claimants.

Table 1
Means of the demographic variables in the treatment and control groups

	Connecticut				Massachusetts				Tennessee				Virginia			
	Treatment			Control	Treatment			Control	Treatment			Control	Treatment			Control
	(1)	(2)	(3)=(1)+(2)	(4)	(1)	(2)	(3)=(1)+(2)	(4)	(1)	(2)	(3)=(1)+(2)	(4)	(1)	(2)	(3)=(1)+(2)	(4)
Age	35.7	35.3	35.5	35.6	38.2	37.2	37.6	36.8	34.1	35.2	34.8	34.0	37.6	37.3	37.4	35.6
	[12.3]	[12.4]	[12.4]	[12.4]	[14.2]	[13.3]	[13.6]	[12.8]	[12.0]	[11.4]	[11.7]	[11.4]	[10.6]	[11.1]	[10.9]	[10.3]
Fraction male	0.71	0.69	0.70	0.69	0.59	0.68	0.64	0.68	0.59	0.58	0.59	0.62	0.65	0.64	0.64	0.57
	[0.45]	[0.46]	[0.46]	[0.46]	[0.49]	[0.47]	[0.48]	[0.47]	[0.49]	[0.49]	[0.49]	[0.49]	[0.48]	[0.48]	[0.48]	[0.50]
Fraction nonwhite	0.38	0.45	0.42	0.41	0.06	0.06	0.06	0.07	0.27	0.36	0.33	0.33	0.34	0.17	0.23	0.26
	[0.49]	[0.50]	[0.49]	[0.49]	[0.24]	[0.23]	[0.24]	[0.25]	[0.45]	[0.48]	[0.47]	[0.50]	[0.48]	[0.38]	[0.42]	[0.44]
Base period earnings	12,317	11,488	11,830	11,298	12,449	12,535	12,499	12,233	11,287	11,133	11,195	10,662	—	—	—	—
	[10,979]	[10,590]	[10,678]	[10,110]	[9,838]	[10,059]	[9,946]	[8,564]	[7,081]	[6,889]	[6,961]	[6,998]				
Weekly benefit amount	127.1	119.9	122.9	118.1	124.3	126.1	125.3	128.2	92.7	90.0	91.1	90.3	132.4	131.3	131.7	130.5
	[57.4]	[58.6]	[58.2]	[58.2]	[58.7]	[57.3]	[57.8]	[55.9]	[30.2]	[34.0]	[32.5]	[31.1]	[26.9]	[28.5]	[27.8]	[27.81]
Observations	393	559	952	925	100	138	238	289	198	287	485	426	104	187	291	281

Standard deviations in brackets. All dollar amounts are in 1984 nominal dollars.

6. Analytical framework and expected impact of the treatments

6.1. Analytical framework

The standard framework to evaluate social programs is based on a model of potential outcomes.[18] Conceptually, we imagine that for each individual we can observe the outcomes of interest in two exclusive states: a treated state denoted by "1" and an untreated state denoted by "0". To proceed, we denote the outcome of interest by Y and the treatment status indicator by d. In that model, each individual is represented by a vector (Y_{0i}, Y_{1i}, d_i). The realized outcome depends on the treatment assignment of each individual:

$$Y_i = Y_{0i} + \beta_i d_i,$$

where $\beta_i = Y_{1i} - Y_{0i}$, is the treatment effect specific to person i. Of course, we never observe the same individual in the both the treated and untreated states, so individual-level treatment effects cannot be measured. However, under the assumption of random assignment, the treatment status of each individual is statistically independent of each pair of potential outcomes. Therefore, the difference in mean outcomes in the treatment and control groups is an unbiased estimate of the average treatment effect:

$$\mathrm{E}(\beta_i) = \bar{Y}_1 - \bar{Y}_0.$$

This is typically the main parameter used to evaluate social experiments, but it is certainly not the only interesting one. In the case of continuous outcomes, quantiles of the treatment effect distribution can also provide useful information. In a model with heterogeneous treatment effects, the identification of other parameters of the treatment effect distribution—like its quantiles—requires additional assumptions. The fundamental problem is that randomized experiments only recover the marginal distribution of the potential outcomes. Thus, any parameter that depends on the joint distribution cannot be estimated without making further assumptions. Heckman and Smith (1995) and Heckman et al. (1997) propose several approaches to deal with this problem. We experimented with some of these techniques, and like Heckman et al. (1997), we found that the nonparametric bounds did not yield informative estimates of the quantiles of the distribution of treatment effects for our application.

6.2. Expected impacts of the treatments

The treatments were expected to affect the receipt of UI benefits by their impacts on initial and continuing eligibility of claimants. In particular, the treatments should have an impact on the permanent and temporary disqualification rates, which will in turn influence benefit payments and claim duration.

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[18] See Heckman et al. (1999) for an extensive review of the econometric techniques related to program evaluation.

Permanent disqualifications are generally due to a failure to satisfy the initial eligibility requirements. Typically, the claimant earned too little during the base period or left a previous job voluntarily.[19] If the new procedures included in the treatments are more effective in detecting initially ineligible individuals, we should observe higher permanent disqualification rates in the treatment groups. Therefore, differences in the permanent disqualification rate between the treatment groups 1 and 2 and the control group 4 will provide a measure of the efficacy of the new initial eligibility reviews. However, since individuals in the treatment groups 1 and 2 are subjected to the same additional initial eligibility reviews, we should not observe any systematic difference in the permanent disqualification rate between them.

Temporary disqualifications are mainly due to continuing eligibility issues. Claimants who do not satisfy the "able and available" or the work-search requirements for a given week are denied benefits for that week. The treatment did not incorporate any special reviews concerning the "able and available" requirements. The difference in the temporary disqualification rate between the treatment groups 1 and 2 will therefore provide an estimate of the effect of the stronger enforcement of the work-search requirements. If claimants did not comply with the work-search requirements, we should observe a higher temporary disqualification rate in group 1 relative to group 2. The expected overall treatment effect (the contrast between groups 3 and 4) on temporary disqualifications is more ambiguous. On the one hand, the detection of noncomplying claimants will increase temporary disqualifications in the treatment group. On the other hand, the improved information on the eligibility rules should reduce the number of temporary disqualification in the treatment group.

7. Estimated impact of the treatments

Table 2 contains the sample means and standard deviations of the program variables for the treatment and control groups. These sample statistics are the basis of our analysis. The top panel contains the sample means of the variables pertaining to the eligibility for UI benefits. For each group we report the qualification rate, the permanent disqualification rate, and the temporary disqualification rate. Finally, we present the fraction of claimants who did not report at the second visit at the UI office, which we label as "no-shows". These proportions were calculated using the data provided by the local UI offices. The fraction of claimants qualifying for benefits in the first week is quite similar across Connecticut, Massachusetts, and Tennessee, ranging between 0.65 and 0.75, but somewhat lower in Virginia, ranging between 0.55 and 0.60. There is more variation across states in the disqualification rates. This variation reflects differences in the extent and application of the rules across states. Within each state, the fraction of permanent disqualifications is typically higher than the fraction of temporary disqualifications, but their levels vary

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[19] Individuals who receive other sources of disqualifying income (for example social security benefits) are usually denied benefits.

Table 2
Means of the program variables in the treatment and control groups

	Connecticut				Massachusetts				Tennessee				Virginia			
	Treatment			Control	Treatment			Control	Treatment			Control	Treatment			Control
	(1)	(2)	(3)=(1)+(2)	(4)	(1)	(2)	(3)=(1)+(2)	(4)	(1)	(2)	(3)=(1)+(2)	(4)	(1)	(2)	(3)=(1)+(2)	(4)
Fraction qualified	0.60	0.61	0.61	0.69	0.70	0.73	0.72	0.71	0.63	0.66	0.65	0.68	0.56	0.58	0.57	0.53
	[0.49]	[0.49]	[0.46]	[0.46]	[0.46]	[0.44]	[0.45]	[0.45]	[0.48]	[0.48]	[0.48]	[0.47]	[0.50]	[0.49]	[0.50]	[0.50]
Fraction permanently disqualified	0.24	0.23	0.23	0.20	0.11	0.14	0.13	0.13	0.29	0.29	0.29	0.26	0.10	0.12	0.11	0.11
	[0.43]	[0.42]	[0.42]	[0.40]	[0.32]	[0.34]	[0.33]	[0.34]	[0.45]	[0.46]	[0.45]	[0.44]	[0.31]	[0.32]	[0.32]	[0.31]
Fraction temporarily disqualified	0.08	0.06	0.07	0.04	0.02	0.02	0.02	0.02	0.02	0.01	0.02	0.02	0.08	0.10	0.10	0.12
	[0.28]	[0.24]	[0.26]	[0.20]	[0.15]	[0.13]	[0.14]	[0.14]	[0.15]	[0.12]	[0.13]	[0.13]	[0.28]	[0.30]	[0.29]	[0.32]
Fraction of no-shows	0.08	0.09	0.09	0.07	0.16	0.12	0.14	0.14	0.07	0.04	0.05	0.04	0.26	0.20	0.22	0.25
	[0.27]	[0.29]	[0.28]	[0.25]	[0.37]	[0.32]	[0.34]	[0.35]	[0.25]	[0.20]	[0.22]	[0.20]	[0.44]	[0.40]	[0.42]	[0.43]
Observations	408	576	984	963	264	367	631	713	270	412	682	642	261	402	663	669
Average weekly benefits	116.5	113.3	114.6	125.3	128.7	130.3	129.5	128.3	96.0	98.5	97.4	97.3	—	—	—	—
	[91.5]	[82.3]	[82.2]	[103.88]	[53.5]	[53.2]	[53.3]	[52.4]	[24.2]	[23.0]	[23.5]	[23.3]				
Total benefits	783.9	794.1	789.8	840.1	477.6	488.4	483.6	496.03	650.3	738.2	700.8	686.1	686.0	656.6	668.4	592.3
	[776.8]	[766.6]	[766.2]	[744.9]	[463.2]	[455.1]	[458.1]	[474.5]	[403.9]	[393.3]	[399.6]	[400.7]	[811.0]	[747.1]	[772.9]	[702.7]
Claim duration	6.16	6.39	6.29	6.56	3.75	3.75	3.75	3.96	6.88	7.37	7.16	6.93	—	—	—	—
	[4.65]	[4.68]	[4.67]	[4.61]	[3.18]	[3.15]	[3.16]	[3.40]	[3.9]	[3.4]	[3.6]	[3.7]				
Observations	303	460	790	758	165	202	367	441	134	181	315	308	231	343	574	568

Notes: Standard deviations in square brackets. The administrative records for Virginia do not contain information on claim durations. All dollar amounts are in 1984 nominal dollars.

from 10% to 30%. The proportion of no-shows is similar across experimental groups within a state, but variable in its level across state ranging from 0.05 to 0.25.[20] We should emphasize that the no-shows and permanently disqualified claimants are kept in the sample throughout the analysis. Exclusion of such claimants would create non-random sample attrition, which can lead to serious biases, even with experimental data.[21]

The lower panel of Table 2 contains the sample averages of the variables pertaining to the benefits received and claim duration. We report the average weekly benefits received, total benefits received and claim duration. Note that the average weekly benefit received can differ from the weekly benefit amount reported in Table 1, which measures entitlement to unemployment benefits based on past earnings. These statistics were calculated using the administrative data collected from the states' UI system. The differences across states in the sample means might reflect differences in the state's UI programs (like benefits entitlement) and also in the duration of the experimental period.[22] It is worth nothing that the claim durations and total benefits include values of 0 for no-shows or permanently disqualified claimants. Again, this is to ensure that the random assignment is not contaminated by nonrandom attrition.

7.1. Impact on the qualification rate

Table 3 reports the estimates of the treatment effect on the qualification for UI. Since the randomization process was effective, we use simple analysis-of-variance methods to estimate the treatment effects. In a previous version of this paper (Ashenfelter, Ashmore and Deschênes, 1998), we also reported regression-adjusted estimates of the treatment effects. Given the effectiveness of the randomization, the point estimates and standard errors are essentially the same. The results are analyzed in the following manner: we present the contrast between groups 1 and 2, and between groups 3 and 4. The first contrast isolates the effect of work-search verification while the second measures the overall treatment effect (i.e. the combined effect of work-search verification and of the additional initial eligibility reviews and information).

As Table 3 indicates, the verification of reported job contacts reduced the qualification rate by about 2%. If the claimants did not comply with the work-search requirements, we would have expected the reduction in the qualification rate to be caused by an increase in temporary disqualifications. This appears to be the case for Connecticut. In the other states, the higher fraction of no-shows in group 1 relative to group 2 explains most of the differences in the qualification rate for benefits, none of which are statistically significant. Since the members of groups 1 and 2 received the same treatment at the initial visit, the higher fraction of no-shows in group 1 is not of major concern here.

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[20] These dropout rates are similar to those of other experimental studies (see e.g., Heckman et al. (1999)).
[21] See Ham and LaLonde (1996), and Hausman and Wise (1985).
[22] We also report the state-level averages of some of those variables in Table 7.

Table 3
Treatment effects on the qualification rate

	Groups 1–2: effect of job search verification only					Groups 3–4: overall treatment effect				
	All states	CT	MA	TN	VA	All states	CT	MA	TN	VA
Qualification rate	−0.020	−0.012	−0.029	−0.026	−0.022	−0.025	−0.083	0.007	−0.039	0.041
	[0.018]	[0.032]	[0.036]	[0.038]	[0.039]	[0.012]	[0.022]	[0.025]	[0.026]	[0.027]
Permanent disqualification rate	−0.007	0.007	−0.023	−0.007	−0.013	0.017	0.033	−0.004	0.029	0.004
	[0.015]	[0.027]	[0.027]	[0.035]	[0.025]	[0.010]	[0.019]	[0.018]	[0.024]	[0.019]
Temporary disqualification rate	0.007	0.023	0.006	0.008	−0.018	0.005	0.030	−0.000	0.000	−0.020
	[0.008]	[0.017]	[0.011]	[0.010]	[0.023]	[0.006]	[0.010]	[0.007]	[0.007]	[0.017]
Fraction of no-shows	0.021	−0.018	0.046	0.025	0.053	0.002	0.020	−0.003	0.009	−0.025
	[0.012]	[0.018]	[0.028]	[0.017]	[0.033]	[0.008]	[0.012]	[0.019]	[0.012]	[0.023]
Observations	2960	984	631	682	663	5947	1947	1344	1324	1332

Notes: Standard errors in brackets. All dollar amounts are in 1984 nominal dollars.

The overall treatment effect is more variable across states. The combined effect for all states is a reduction in the qualification rate of about 3%, mainly reflecting the large 8% reduction in Connecticut. The lower qualification rate in the treatment groups is generally due to a higher permanent disqualification rate. This indicates that the initial eligibility review was the most effective component of the treatments in detecting ineligible claimants. Thus, we can conclude that in our data, the main reason for payments to ineligible claimants, at least in the initial weeks of an unemployment spell, appears to be related to job separation and monetary issues rather than failure to actively seek work.

7.2. Impact on benefit payments and claim duration

The treatment effects on benefit payments and claim duration are more difficult to measure than the effects on the qualification rate. The fundamental difficulty is that claimants were not subjected to the treatments after the second visit to the UI office. After the second visit individuals in the treatment and control groups only needed to submit a form for each claiming week, and these forms were not subject to any formal review. Therefore, the differences in average benefit payments and claim duration between the treatment and control groups may result from one of two factors. First, there may be immediate effects of the treatments on the qualification rate during the first week of eligibility. If the treatment increases temporary or permanent disqualifications, on average, claim durations will be shorter in the treatment group. Second, the treatments may have a long-lasting effect resulting from a better understanding of the continuing eligibility regulations, which again should reduce claim duration and benefit payments in the treatment group. In any case, the "initial" and "long-lasting" effects of the treatments should reduce claim duration and benefit receipt, but it is not possible to distinguish between the two types of effects.

Another problem is related to the administrative data. Since Tennessee and Virginia only maintained records on individuals satisfying specific eligibility criteria, some claimants who are permanently disqualified or no-shows have no record in the administrative files. For these two states, a simple comparison of the mean benefit payments (or claim duration) is likely to overestimate the treatment effect. If the qualification rate is lower in the treatment group, a higher fraction of low benefits (or duration) claimants will be excluded in the treatment group relative to the control group.[23] Since permanently disqualified individuals (or no-shows) are not entitled to receive payments from the UI system, we can simply impute values of 0 for the outcomes of those individuals.[24] Table 4 reports the estimated treatment effects using

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[23] Ham and LaLonde (1996) address a similar problem in their analysis of the effect of training on post-training wages.

[24] Simple imputation schemes are not always possible. For example, in the application of Ham and LaLonde (1996), the missing variable (post-training wages) cannot be imputed, so they have to use a selection model in order to account for the bias.

Table 4
Treatment effects on benefit payments and claim duration

	Groups 1–2: effect of Job search verification only					Groups 3–4: overall treatment effect				
	All States	CT	MA	TN	VA	All States	CT	MA	TN	VA
Average benefits	0.85	3.23	−1.60	−2.43	—	−5.50	−10.70	1.21	0.11	—
	[3.91]	[6.56]	[6.62]	[2.68]		[2.95]	[5.08]	[4.43]	[1.87]	
Total benefits	−11.33	−10.18	−10.79	−87.91	29.44	2.00	−50.28	−12.45	14.66	76.09
	[30.29]	[55.31]	[48.14]	[45.34]	[65.83]	[20.53]	[38.43]	[33.01]	[32.06]	[43.72]
Claim duration	−0.23	−0.23	−0.01	−0.49	—	−0.15	−0.27	−0.21	0.23	—
	[0.22]	[0.34]	[0.33]	[0.41]		[0.15]	[0.24]	[0.23]	[0.29]	
Observations	2046	790	367	315	574	4121	1548	808	623	1142
Total benefits[a]	−10.15	−10.18	−10.79	−52.49	27.22	1.61	−50.28	−12.45	9.01	69.05
	[24.85]	[55.31]	[48.14]	[27.04]	[60.55]	[17.00]	[38.43]	[33.01]	[19.67]	[39.69]
Claim duration[a]	−0.19	−0.23	−0.01	−0.29	—	−0.12	−0.27	−0.21	0.14	—
	[0.17]	[0.34]	[0.33]	[0.25]		[0.12]	[0.24]	[0.23]	[0.18]	
Observations[a]	2315	790	367	530	628	4632	1548	808	1017	1259

Notes: Standard errors in brackets.
[a] Includes the imputations described in the text. All dollar amounts are in 1984 nominal dollars.

the actual data (without the imputations) and the data augmented using this scheme.[25]

As can be seen from Table 4, job-search verification during the first week of unemployment reduced claim duration by about one-quarter of a week. This translated into a reduction of total benefit payments of about $10. The combined effect of all the treatments also appeared to reduce claim duration by one-quarter of a week. However, the difference in benefit payments ranges from -50 to $75, none of which are significant at the 5% level. As expected, the estimated impacts are smaller in magnitude when we impute the missing benefits and duration for the permanently disqualified claimants and no-shows in Tennessee and Virginia.

There is another potentially misleading element in our analysis. In each state, some unemployment spells were still ongoing at the time the administrative data were collected. Consequently, the unadjusted averages for total benefit payment and claim duration will underestimate the true averages. However, if the censoring probabilities are the same in the treatment and control groups, this should not bias the estimated treatment effect. Nevertheless, we present survivor functions and use a Tobit censored regression model to investigate this possibility. In Fig. 2, we present empirical survivor functions, which are the unconditional probabilities of claiming benefits for at least "n" weeks. It is apparent from these figures that there are no systematic differences between the survivor functions in the treatment and control groups. Conventional log-rank tests on the equality of the survivor function also suggest the same conclusion. The results of the censored regression estimation, which are reported in Table 5, are typically larger in magnitude than those found in Table 4, but again all the estimated effects greatly exceed their standard errors (Tables 6 and 7).[26]
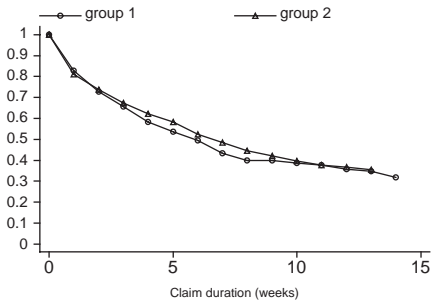
In sum, claimants in the treatment group do not appear to receive substantially smaller benefit payments. Benefit receipt differences are imprecisely estimated, typically not large, and never significantly different from zero using conventional test criteria. At the same time, the treatments did not appear to significantly reduce the length of the claiming period. The additional costs of the treatments ranged between $1 and $15 per claimant. For the 20 millions new UI claims issued in 1985, these additional costs would correspond to anywhere between 1 and 20% of the total administrative cost of the UI system for that year. The policy implication of this simple comparison is that stricter enforcement of the eligibility rules of the type we tested would probably not result in large enough savings for the UI system to justify the cost.
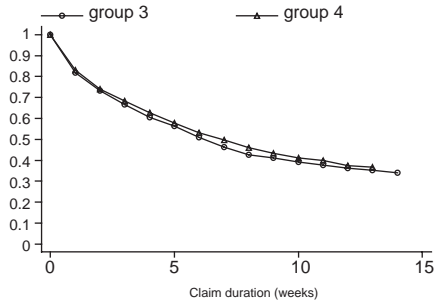
8. Conclusion

The results of the randomized trials reported in this study cast doubt on the efficacy of many assertions about abusive behavior in the U.S. unemployment

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[25] Since only the data for Tennessee and Virginia are imputed, the estimated treatment impacts are unchanged for Connecticut and Massachusetts.
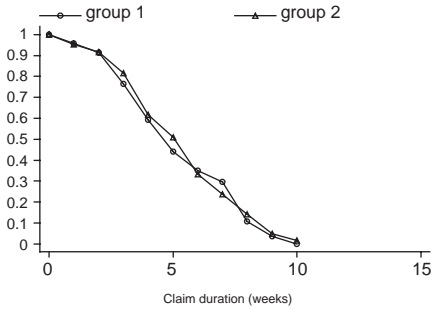
[26] In Table 6 we also report the Tobit regression estimates conditional on the observed characteristics of the claimants (age, race and gender). The results lead to same conclusion as in Table 5.
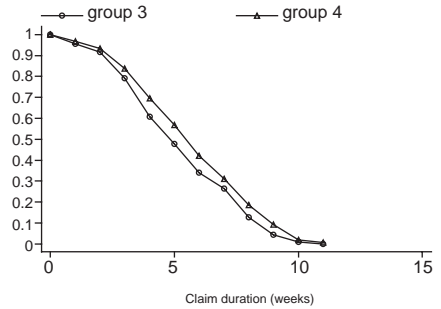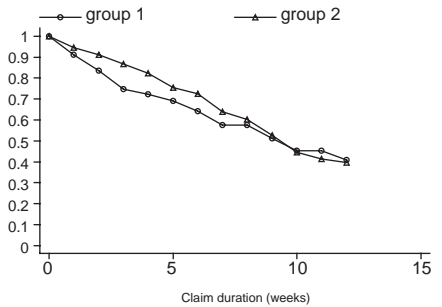
Fig. 2. Empirical survivor functions in the treatment and control groups.

insurance system. We found some evidence that, in one of the four states we studied, tighter checks on eligibility may have a small effect on initial benefit payments. However, even in this state, eligibility checks led to little or no effect on total benefit payments or the duration of unemployment claims. Most important, we found no evidence that verification of claimant search behavior led to shorter claims or lower total benefit payments.

Any program evaluation would be incomplete without a discussion of the additional costs of the treatments. While the additional costs of the treatments were

Table 5
Treatment effects on benefit payments and claim duration. (Adjusted for censoring using a Tobit censored regression)

| | Groups 1–2: effect of job search verification only | | | | | Groups 3–4: overall treatment effect | | | | |
	All States	CT	MA	TN	VA	All States	CT	MA	TN	VA
Total benefits	−50.12	−39.1	−32.3	−101.8	—	−41.90	−69.3	−41.2	25.4	—
	[48.65]	[85.4]	[47.6]	[78.75]		[33.28]	[60.1]	[32.7]	[55.54]	
Claim duration										
	−0.40	−0.39	−0.11	−0.75	—	−0.25	−0.35	−0.50	0.30	—
	[0.31]	[0.52]	[0.30]	[0.74]		[0.22]	[0.37]	[0.21]	[0.51]	
Observations	2046	790	367	315		2979	1548	808	623	
Non-censored	868	473	243	152	—	1743	915	524	304	—
Right-censored	604	317	124	163		1236	633	284	319	
Total benefits[a]	−35.83	−39.1	−32.3	−51.9	—	−30.64	−69.3	−41.2	12.6	—
	[34.40]	[85.4]	[47.6]	[35.1]		[24.06]	[60.1]	[32.7]	[25.8]	
Claim duration[a]	−0.30	−0.39	−0.11	−0.37	—	−0.18	−0.35	−0.50	0.15	—
	[0.22]	[0.52]	[0.30]	[0.32]		[0.16]	[0.37]	[0.21]	[0.24]	
Observations[a]	1688	790	367	531		3373	1548	808	1017	
Non-censored	1084	473	243	368	—	2137	915	524	698	—
Right-censored	604	317	124	163		1236	633	284	319	

Notes: Standard errors in brackets.
[a] Includes the imputations described in the text. All dollar amounts are in 1984 nominal dollars.

Table 6
Treatment effects on benefit payments and claim duration. (Adjusted for censoring using a Tobit censored regression, conditional on demographics)

	Groups 1–2: effect of job search verification only					Groups 2–4: overall treatment effect				
	All States	CT	MA	TN	VA	All States	CT	MA	TN	VA
Total benefits	−49.0	−58.1	28.3	−72.2	—	−36.8	−76.1	−11.5	14.4	—
	[46.8]	[82.8]	[59.1]	[45.3]		[32.3]	[58.1]	[41.1]	[31.8]	
Claim duration	−0.34	−0.47	0.29	−0.46	—	−0.25	−0.39	−0.47	0.01	—
	[0.30]	[0.52]	[0.35]	[0.41]		[0.21]	[0.37]	[0.23]	[0.29]	
Observations	2046	790	367	315		2979	1548	808	623	
Non-censored	868	473	243	152	—	1743	915	524	304	—
Right-censored	604	317	124	163		1236	633	284	319	
Total benefits[a]	−36.0	−58.1	28.31	−42.6	—	−29.0	−76.1	−11.5	−7.3	—
	[37.1]	[82.8]	[59.1]	[42.0]		[26.1]	[58.1]	[41.1]	[29.7]	
Claim duration[a]	−0.27	−0.47	0.29	−0.23	—	−0.19	−0.39	−0.47	−0.12	—
	[0.24]	[0.52]	[0.35]	[0.41]		[0.17]	[0.37]	[0.23]	[0.29]	
Observations[a]	1688	790	367	531		3373	1548	808	1017	
Non-censored	1084	473	243	368	—	2137	915	524	698	—
Right-censored	604	317	124	163		1236	633	284	319	

Notes: Standard errors in brackets. All regressions are conditional on age, race and sex main effects and their interactions.
[a] Includes the imputations described in the text. All dollar amounts are in 1984 nominal dollars.

Table 7
Complementary information on the implementation of the experiments and characteristics of UI programs across states in 1985

	Connecticut	Massachusetts	Tennessee	Virginia
Location of the experimental UI office	Hartford	Worcester	Nashville	Falls Church
Start of experimental period	01/11/85	01/25/85	01/14/85	12/08/84
End of experimental period	03/22/85	04/05/85	03/28/85	03/08/85
Date of collection of administrative data	04/05/85	04/26/85	04/12/85	03/22/85
Additional costs associated with the treatment (per claim)	$15.00	$1.00	$2.00	$1.00
State-specific characteristics of the UI program (in 1985):				
Number of initial claims[a]	211,183	421,631	492,971	305,504
Average state unemployment rate	4.9	3.9	8.0	5.6
Average weekly benefit amount	142	138	89	118
Average claim duration	10	14	11	8

Sources: Green Book 1985, 1986, Training and employment report of the secretary of labor 1988. All dollar amounts are in 1985 nominal dollars.

[a] Number of initial claims for 1986.

relatively modest on a per-person basis, they would have represented an important share of the total administrative costs of the UI system if the stricter eligibility review program had been implemented on a national scale. The policy implication of this simple comparison is that stricter enforcement of the eligibility rules of the type we tested would probably not result in large enough savings for the UI system to justify the cost.

There are, of course, many potential limitations of these results. First, the experiments were conducted as a test of alterations in the rules of only four U.S. states. Our results test only whether further work search verification in those states may be worth the costs. One interpretation of our results is that the current rules implemented in the four states we analyze are optimal, and the results might be different elsewhere. Second, the experiments were conducted at a time when the aggregate unemployment rate was considerably higher than it is today, and this might also affect the results. Only further experimentation can demonstrate whether these issues raise serious problems for the generality of the results.

Many social programs now incorporate sanctions on suspected abusive behavior, including the major welfare programs in the U.S. As with other government programs, the effectiveness of sanctions should be subject to a cost–benefit test. The results in this paper indicate that, at least in one program, the enforcement of sanctions does not appear to be worth the cost.

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