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**Working Paper**

# Do Unemployment Insurance Recipients Actively Seek Work? Evidence From Randomized Trials in Four U.S. States

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IZA DP No. 128

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March 2000

Forschungsinstitut
zur Zukunft der Arbeit
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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

## Do Unemployment Insurance Recipients Actively Seek Work? Evidence From Randomized Trials in Four U.S. States

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), 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.

JEL Classification: C93, J65

Keywords: Social experiment, overpayment, work-search requirement

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## 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 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 US 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.

## 1. Previous Research

Since its inception in 1930, the Unemployment Insurance (UI) program has always been controversial[2]. At the center of this controversy are 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 generally do not formally validate the information provided[3] by the recipients, which 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 in the 1980s.

The study by Corson et al. (1986) used a nonexperimental design to evaluate the effects of work search rules in 10 states. The authors report that on average, claimants from states whose rules are stricter are generally more likely to search for work. They also note that in their sample, states with the stricter rules also experienced higher unemployment rates. As the authors note, their approach suffers from an identification problem: claimants in states with stricter rules might have searched harder in response to adverse labor market conditions rather than in response to the tighter UI regulations.

The Job Search Experiments, conducted in Virginia, New Jersey and Washington are analyzed in great detail in Corson et al. (1985), Corson et al. (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 and reemployment bonuses. Despite differences in the design of the experiments and in the treatments offered, the results 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 reduced 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 the results of previous experiments do no permit isolation of the effects of tighter job-

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[1] See also Kingston, Burgess, and St. Louis (1986), Wolf and Greenberg (1986).
[2] See for example Blaustein, O'Leary and Wandner (1997).
[3] See, for example, Decker (1997).

search rules or monitoring relative to the currently existing system[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.

## 2. Overview of the Existing UI Application Procedures.

Qualification for unemployment benefits is determined on the basis of rules whose extent and application differ substantially across states[5]. In general there 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 of wages 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. Being available 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, which is then reviewed for determination of eligibility. At the second visit, eligibility status is announced to the claimants, who can appeal the decision. After

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

the second visit, the first check is issued for those who qualify for benefits, and payments continue until the claimant finds a new job, exhausts benefit entitlement, or fails to satisfy the continuing eligibility criteria for a given week. Most states also require an additional visit, for an eligibility review, after the claimant has been unemployed for 6-9 weeks.

**3. Experimental Treatments, Research Design and Data Collection Procedures**

[A] 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 comply with the work-search requirements. Claimants were randomly assigned to treatment and control groups when they first applied for UI benefits. In each state, 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 treatment and control groups were handled in the same manner, according to the established state procedures[7].

At the first visit at the UI office, the applications in the treatment group were reviewed for eligibility with the new steps. Work-search requirements were further explained to 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 not monitored to assess work-search effort. Applicants in groups (1) and (2) who failed to meet these new requirements were disqualified, either temporarily or permanently. Figure 1 contains a diagram representing a typical application process in the treatment and control groups.

The additional cost of processing the claims in the treatment group was very low, about $10. Of course, if such a small expenditure can generate significant benefits, this implies that the work search requirements are not, in fact, being implemented. In fact, the purpose of these experiments was precisely to test such a claim.

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[5] An account of the various state laws is included in U.S. Department of Labor "Comparison of State Unemployment Insurance Laws.", various years.
[6] In the other Job Search Experiments, the treatments were offered later during the claim.
[7] Typically, after the second visit, claimants must mail a form to the office for each claiming period to demonstrate continuing eligibility.

The UI applications in the control group were handled following the established state procedures. After the second visit, the continuing eligibility of treatments and controls was reviewed in the same manner, using the established state procedures[8]. A result of this design is that our experiment will not allow us to detect any timing pattern in the work-search effort of UI recipients after the second visit. The direction of such 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 benefit entitlement runs out. On the other, discouragement might induce them to search less.

[B] 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 claims. Our second source of data is administrative records from each state's UI data processing department. These data contain information on claimant demographic characteristics, claim duration, and total benefits received.

The local UI offices collected information on all valid applications during the study period[9], following specific guidelines. Information was collected on the filing date, the eligibility status of claimants, and on the reason for ineligibility, if the claimant was determined to be ineligible. The UI personnel were instructed to complete one form for each claimant, using the same coding scheme. The data collected are therefore comparable across states.

The administrative data were recorded from each state's computerized data system, generally one month after the end of the study period[10]. Hence, for each state, we have information on completed and ongoing unemployment spells. The administrative data was linked to the UI office data by using the social security number of individuals. A limitation of the available administrative data is that states do not collect information in a uniform manner. Two states (Tennessee (TN) and Virginia (VA)) only record information on claimants who satisfy certain eligibility criteria. The records for Massachusetts (MA) 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. Typically, we were able to match about 80% of the UI office data to the administrative data.

[8] If some issues for the treatments' claims were not resolved after the second visit, the experimental procedures still applied in the determination process.
[9] The study period varied slightly across states in terms of duration of calendar time. See the appendix for more detail.
[10] This delay was to insure that enough time was available to process claims filed during the last days of the experimental period.

## 4. The Effectiveness of the Randomization

The population considered in this study consists of unemployed individuals applying for UI benefits and filing initial, in-person and intrastate claims during the experimental period[11]. 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[12]. 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[13], in particular to randomization bias. Randomization bias occurs when random assignment causes the population participating in a program to be different from the population 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[14].

Table 1 contains demographic characteristics for 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 aggregated in column (3), while in column (4) we display the information on individuals in 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 TN and the proportion of black claimants, which is smaller in MA.

We use a variety of approaches to study the effectiveness of the randomization process. First, we calculate t-statistics to test the null hypothesis of equality of means between the

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[11] This means that individuals filing transitional, continuing or interstate claims were excluded from our analysis.

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

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

[14] Since no other program offers wage compensation during unemployment, substitution bias is unlikely to affect our results as well.

treatment and control groups[15]. 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 three contrasts that are statistically significant at the .05 level, which is almost exactly what would be expected (that is, $.05 \times 57 = 2.85$) if assignment were random. In all cases, moreover, the differences, even when statistically significant, are small[16]. In another attempt to verify the effectiveness of random assignment, we used the 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 hypothesis of equality of the distribution functions.

These statistical tests, based on the demographic characteristics, work histories and UI 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.

**5. Analytical Framework and Expected Impacts of the Treatments.**

[A] Analytical Framework.

The standard framework to evaluate social programs is to use a model of potential outcomes[17]. Conceptually, we imagine that we can observe the outcomes of interest for each person in two exclusive states: a treated state denoted by "1" and an untreated state denoted by "0". To formalize things, 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 potential 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. 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.

[15] That is, we test the equality of means between groups (1) and (4), (2) and (4), (3) and (4).
[16] A limitation of the validity of our statistical tests is that in TN and VA, demographics are only observed for a nonrandom subset of all claimants.

$$E(\beta_i) = \overline{Y}_1 - \overline{Y}_0$$

This is the main parameter of interest in evaluating our experiments, but it is certainly not the only one. In the case of continuous outcomes, quantiles of the treatment effect distribution can also be very useful. In a model where treatment effects are heterogeneous, 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 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.

[B] 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 affect permanent and temporary disqualification rates, which will in turn affect benefit payments and claim duration.

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 voluntarily left a previous job[18]. If the new procedures included in the treatments are more effective than the standard ones in detecting initially ineligible, we should observed 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) 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 typically denied benefits for that week. The treatment did not incorporate any special reviews concerning the "able and available" requirements. Therefore, if we compare the temporary disqualification rate between claimants whose job contacts were verified (group 1) and those who were not (group 2), we get an estimate of the reduction in the qualification rate induced by more careful enforcement of the work-search rules. If claimants do not comply with the work search

[17] See Heckman, LaLonde and Smith (1999) for an extensive review of social program evaluation.
[18] Claimants who receive other sources of disqualifying income (for example social security benefits) are usually denied benefits.

requirements, on average, 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 non-complying claimants will increase temporary disqualifications, while on the other hand, the better information and/or the fear of getting caught should reduce them.

**6. Estimated Impacts 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 from 10% to 30%. The proportion of "no-shows" is typically similar across experimental groups within a state, but variable in its level across state ranging from 0.05 to 0.25[19]. We should emphasize that 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[20].

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 an entitlement to unemployment benefits based on past earnings. These statistics were calculated using the administrative data from the states' UI system. The difference across states in the sample means reflects differences in the state's UI programs (like benefits entitlement) and in the duration of the experimental period across states[21]. It is worth nothing that the claim durations and total benefits

[19] This dropout rate is similar to that of other experimental studies. See Heckman LaLonde and Smith (1999).
[20] See Ham and LaLonde (1996) and Hausman and Wise (1985).
[21] We also report state averages of some of these variables in the appendix.

typically include values of 0 for "no-shows" or permanently disqualified claimants. Again, this is to ensure that the random assignment of applicants is not contaminated by non-random attrition.

[A] Impact on the Qualification Rate

Estimates of the treatment effect on the qualification for UI benefits are contained in Table 3. 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 present regression-adjusted estimated treatment effects. The results do not differ substantially.

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 in only one state (CT). In other states, the higher fraction of "no-shows" in group (1) relative to group (2) explains 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.

The overall treatment effect is more variable across states. On the whole, the treatments reduced the qualification rate by about 3%, mainly reflecting a large 8% reduction in CT. The lower qualification rate in the treatment groups is generally due to their 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.

[B] 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 inherent difficulty is that claimants were not subjected to

the treatments after the second visit to the UI office. After the second visit, claimants 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, for example, 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 may be difficult to distinguish between the two effects.

Another problem is related to the administrative data. Since TN and VA only kept records on individuals satisfying specific eligibility criteria, some claimants who are permanently disqualified or who are no-shows, have no benefit and claim duration records. 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[22]. However, we use a simple solution to cope with this problem: in both cases, these variables should be 0[23], which can easily be imputed. This imputation scheme will also allow us to retrieve some of the mismatches between the UI office and state data. Table 4 contains the estimated treatment effects using the actual data and the data augmented by using this scheme.

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[24]. These effects are not statistically significant and are likely to overestimate the treatment effect. The bottom panel of Table 4 is consistent with that view. When we impute 0 benefits and duration to permanently disqualified claimants and no-shows, the estimated impact are smaller in magnitude.

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[22] Ham and LaLonde (1996) address a similar problem in their analysis of the effect of training on post-training wages. However, in their case, the missing variable (wages) cannot be imputed, so they have to use a selection model in order to account for the bias.

[23] In principle, it is possible that a permanently disqualified claimant's status changes during the course of a claim if the claimant appeals the decision and wins the appeal. However, the data on qualification collected by the UI personnel referred to final eligibility status, so this is not a concern here.

[24] Other studies found treatment impact that lie in the same range, but that are typically more precisely estimated.

There is another potentially misleading element in our analysis. In each state, some unemployment spells were still ongoing at the data the administrative data were collected. As a consequence, the unadjusted sample averages of 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. Nonetheless, we present survivor functions and use a censored regression model to investigate this possibility. In Figure 2, we present empirical survivor functions, which are the unconditional probabilities of claiming benefits for at least "n" weeks. As it is apparent from these figures, there are no systematic differences between these 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 all the differences greatly exceed their standard errors.

In sum, claimants in the treatment group do not appear to receive substantially smaller benefit payments. Benefit receipt differences are imprecisely estimated, but typically not large, and never significantly different from zero using conventional test criteria. At the same time, the treatments did not appear to reduce significantly the length of the claiming period. The additional costs of the treatments were about $10 per claimant. 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.

## 7. Conclusion

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

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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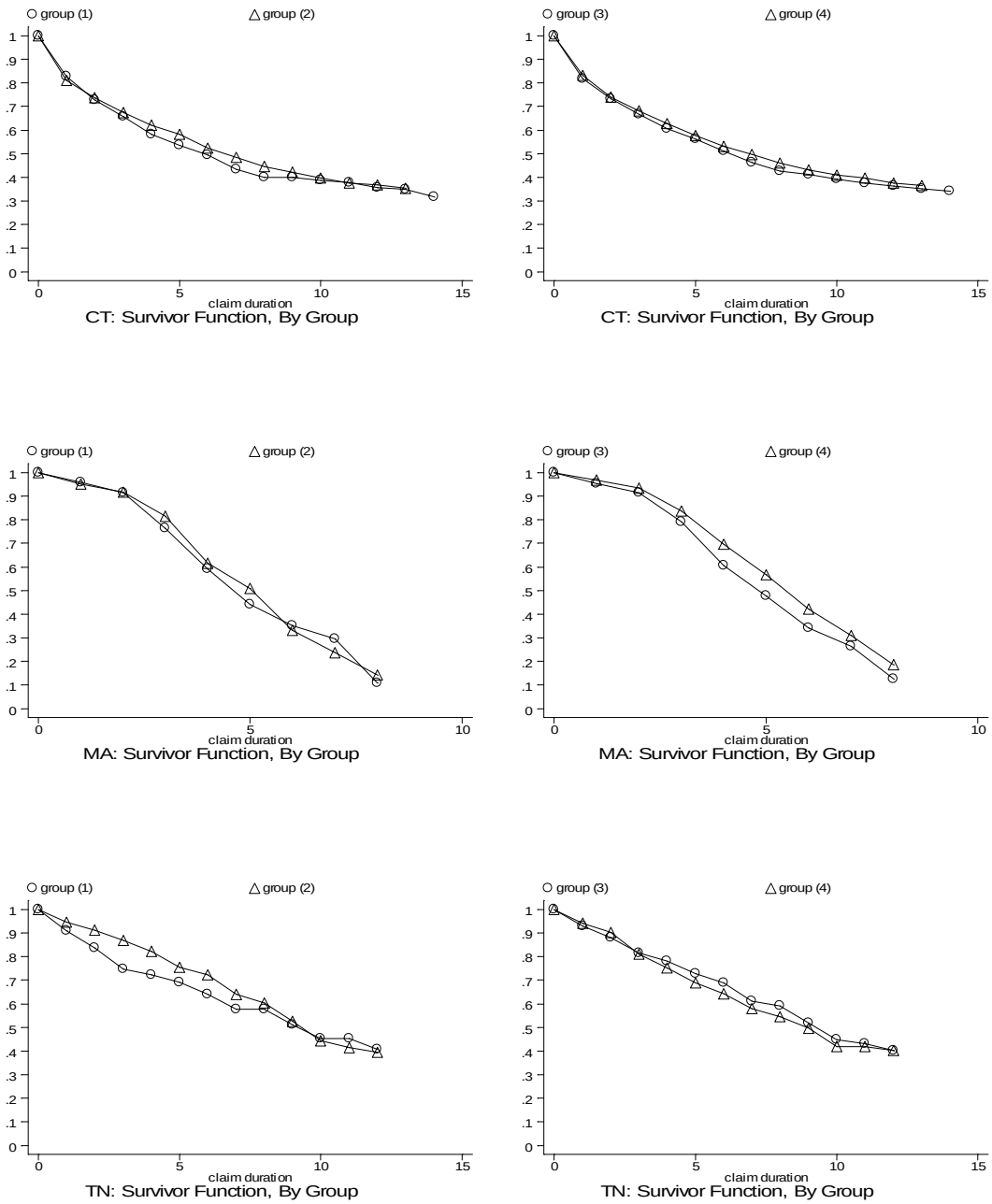
Figure 1: Typical Application Process in the Treatment and Control Groups.

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

Figure 2: Empirical Survivor Functions in the Treatment and Control Groups.



Prg: "/states/pooled/sas/means-st.sas"

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] | [12.38] | [12.39] | [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] |
| Log Base Period Earnings | 12,317 | 11,488 | 11,830 | 11,298 | 12,449 | 12,535 | 12,499 | 12,233 | 11,287 | 11,133 | 111,95 | 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 |

Notes: standard deviations in square brackets.

Table 2: Means of the Program Variables in the Treatment and Control Groups.
Prgs: "/states/pooled/sas/out0-st.sas, out1-st.sas"

| | 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 [0.49] | 0.61 [0.49] | 0.61 [0.46] | 0.69 [0.46] | 0.70 [0.46] | 0.73 [0.44] | 0.72 [0.45] | 0.71 [0.45] | 0.63 [0.48] | 0.66 [0.48] | 0.65 [0.48] | 0.68 [0.47] | 0.56 [0.50] | 0.58 [0.49] | 0.57 [0.50] | 0.53 [0.50] |
| Fraction Permanently Disqualified | 0.24 [0.43] | 0.23 [0.42] | 0.23 [0.42] | 0.20 [0.40] | 0.11 [0.32] | 0.14 [0.34] | 0.13 [0.33] | 0.13 [0.34] | 0.29 [0.45] | 0.29 [0.46] | 0.29 [0.45] | 0.26 [0.44] | 0.10 [0.31] | 0.12 [0.32] | 0.11 [0.32] | 0.11 [0.31] |
| Fraction Temporarily Disqualified | 0.08 [0.28] | 0.06 [0.24] | 0.07 [0.26] | 0.04 [0.20] | 0.02 [0.15] | 0.02 [0.13] | 0.02 [0.14] | 0.02 [0.14] | 0.02 [0.15] | 0.01 [0.12] | 0.02 [0.13] | 0.02 [0.13] | 0.08 [0.28] | 0.10 [0.30] | 0.10 [0.29] | 0.12 [0.32] |
| Fraction of "No-Shows" | 0.08 [0.27] | 0.09 [0.29] | 0.09 [0.28] | 0.07 [0.25] | 0.16 [0.37] | 0.12 [0.32] | 0.14 [0.34] | 0.14 [0.35] | 0.07 [0.25] | 0.04 [0.20] | 0.05 [0.22] | 0.04 [0.20] | 0.26 [0.44] | 0.20 [0.40] | 0.22 [0.42] | 0.25 [0.43] |
| Observations | 408 | 576 | 984 | 963 | 264 | 367 | 631 | 713 | 270 | 412 | 682 | 642 | 261 | 402 | 663 | 669 |
| Average Weekly Benefits | 116.5 [91.5] | 113.3 [82.3] | 114.6 [82.2] | 125.3 [103.88] | 128.7 [53.5] | 130.3 [53.2] | 129.5 [53.3] | 128.3 [52.4] | 96.0 [24.22] | 98.5 [23.0] | 97.4 [23.5] | 97.3 [23.3] | -- | -- | -- | -- |
| Total Benefits | 783.9 [776.8] | 794.1 [766.6] | 789.8 [766.2] | 840.1 [744.9] | 477.6 [463.2] | 488.4 [455.1] | 483.6 [458.1] | 496.03 [474.5] | 650.3 [403.9] | 738.2 [393.3] | 700.8 [399.6] | 686.1 [400.7] | 686.0 [811.0] | 656.6 [747.1] | 668.4 [772.9] | 592.3 [702.7] |
| Claim Duration | 6.16 [4.65] | 6.39 [4.68] | 6.29 [4.67] | 6.56 [4.61] | 3.75 [3.18] | 3.75 [3.15] | 3.75 [3.16] | 3.96 [3.40] | 6.88 [3.86] | 7.37 [3.40] | 7.16 [3.62] | 6.93 [3.65] | -- | -- | -- | -- |
| 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 Viriginia did not contain information on claim duration.

Table 3: Treatment Effects on Qualification Rate.
no controls: "/states/pooled/sas/out0-st.sas"

| | Group (1) – Group (2): Effect of Job Search Verification Only | | | | | Group (3) – Group (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. The "All States" column refers to a pooled state analysis. The variances are allowed to vary across state.

Table 4: Treatment Effects on Benefit Payments and Claim Duration.
no controls: "/states/pooled/sas/out1-st.sas"

| | Group (1) – Group (2): Effect of Job Search Verification Only | | | | | Group (3) – Group(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 | -8.90 | -9.07 | -52.49 | 27.22 | 1.61 | -44.27 | -10.56 | 9.01 | 69.05 |
| | [24.85] | [48.32] | [40.33] | [27.04] | [60.55] | [17.00] | [33.83] | [28.01] | [19.67] | [39.69] |
| Claim Duration[A] | -0.19 | -0.20 | -0.01 | -0.29 | -- | -0.12 | -0.24 | -0.18 | 0.14 | -- |
| | [0.17] | [0.29] | [0.28] | [0.25] | | [0.12] | [0.21] | [0.20] | [0.18] | |
| Observations[A] | 2505 | 906 | 440 | 530 | 628 | 4988 | 1760 | 952 | 1017 | 1259 |

Notes: Standard errors in brackets. (A) Includes the 0 imputations described in the text. The "All States" column refers to a pooled state analysis. The variances are allowed to vary across state.

Table 5: Treatment Effects on Benefit Payments and Claim Duration.
no controls: "/states/pooled/sas/out1-st.sas"
Adjust for censoring by using normal censored regression.

| | Group (1) – Group (2): Effect of Job Search Verification Only | | | | | Group (3) – Group(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 | | | | | | | | | | |
| 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 | -30.5 | -24.8 | -51.9 | -- | -30.64 | -57.9 | -35.6 | 12.6 | -- |
| | [34.40] | [69.5] | [37.9] | [35.1] | | [24.06] | [49.3] | [26.4] | [25.8] | |
| Claim Duration[A] | -0.30 | -0.32 | -0.08 | -0.37 | -- | -0.18 | -0.29 | -0.40 | 0.15 | -- |
| | [0.22] | [0.43] | [0.24] | [0.32] | | [0.16] | [0.31] | [0.16] | [0.24] | |
| Observations[A] | | | | | | | | | | |
| Non-Censored | 1273 | 589 | 316 | 368 | -- | 2493 | 1127 | 668 | 698 | -- |
| Right-Censored | 604 | 317 | 124 | 163 | | 1236 | 633 | 284 | 319 | |

Notes: Standard errors in brackets. (A) Includes the 0 imputations described in the text. The "All States" column refers to a pooled state analysis. The variances are allowed to vary across state. The data from VA cannot be used since it does have claim duration or a right-censoring indicator.

Table 6: 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 |
| 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 |

Source: Green Book 1985, 1986.

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