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Randomized Trials in Four U.S. States
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ABSTRACT

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. Surprisingly, except for anecdotes, there is very little systematic evidence of the extent to which sanctions applied to the abusive use of social entitlements result in greater work effort. In this paper we report the results of randomized trials designed to measure whether stricter enforcement and verification of work search behavior alone decreases unemployment (UI) claims and benefits. These experiments were designed to explicitly test claims based on non-experimental data, that a prime cause of overpayment is the failure of claimants to actively seek work. Our results provide no support for the view that the failure to actively seek 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 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 describe our experimental design, the nature of the experimental treatment, and our data collection procedures. Since randomization is so important

[1] See also Kingston, Burgess, and St. Louis (1986) and Wolf and Greenberg (1986)

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.

1. Research Design, Experimental Treatment, and Data Collection Procedures

A. The Treatments

In each state, the treatment consisted of a number of new steps in the processing of initial claims for unemployment insurance prior to the issuance of the first check[2]. The new steps were implemented via supplemental questionnaires and reviews, as additions to or modifications to the pre-existing procedures. Claimants in the control group were processed via the pre-existing procedures. As a result, the number of new procedures in the treatment varied across states, reflecting the differences between the pre-existing procedures in these states. A diagram representing the application processing procedures in the treatment and control group is contained in Figure 1, and provides a helpful way to describe the experiments.

As Figure 1 indicates, there were, in fact, two treatment groups:

Overall treatment group (Group 1). The overall treatment group was treated differently from the control group in two ways. First, as Figure 1 indicates, some processes that were provided to the control group at the second visit to the UI office were instead provided at the first visit. The main part of the process that was provided earlier to the treatment group was the administration of the Benefit Rights Interview (BRI), wherein the claimant's responsibility to actively search for work as well as other responsibilities are explained.

Second, some processes were provided to all members of the overall treatment group that were not provided to the control group at all. All the members of the overall treatment group (Group 1) received an expanded initial eligibility questionnaire designed to explore whether the

[2] This is an important difference from the design of other Work Search Experiments where the treatment was in general provided after issuance of the first check or later.

claimant meets the able and available requirement[3] or has other relevant income. In addition, at the initial visit, the active search for work requirement was emphasized to the claimant, and the claimant received a series of forms (the work search plan, the work search instruction sheet and the work search report form). The claimant was instructed to begin an immediate active work search and was informed that such search is subject to verification. In addition, at the initial visit, the treatment group received and completed a work history form, listing all relevant base-period employment and wages. This form was used to make an extensive review of the accuracy of the monetary determination at the time of the second visit to the UI office.

Work Search Verification Treatment (Group 3). For part of the control group (Group 3) an additional aspect of the treatment involved an actual verification that the claimant undertook the job search. To do this, at the second visit to the UI office the completed work search report form was reviewed to evaluate the appropriateness of the claimant's work search. In addition, job search contacts of a random sample of the treatment group were verified by telephone before the first check was issued.

Table A-1 in the appendix summarizes the major differences across states between the experimental procedures and the existing state's procedures. Applicants in the treatment groups who failed to meet these new requirements were disqualified for benefits, either temporarily or permanently. As a consequence, compliance with the treatment is not an issue here as claimants must go through the procedures (experimental or control) in order to receive their benefits. In order to eliminate the possibility of Hawthorne effects, which can arise when individuals change their behavior because they know it is under study, the claimants were never informed that they were taking part in a randomized experiment.

B. Randomization

The population group considered for assignment to the treatment group consists of enrolling applicants filing initial, in-person, intrastate UI benefit claims[4] during the

[3] This means that the claimant must be able, ready and willing to accept full time work with reasonable efforts.
[4] Individuals filing transitional, continuing or interstate claims were excluded.

experimental period, which varied slightly across states[5]. Randomization into the treatment and control groups was based on the 7th digit of the applicant's Social Security number. 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 (see Meyer (1995)) have not been properly randomized, we report tests of the effectiveness of this method below. The period of enrollment into treatment and control groups ran from December of 1984 to April of 1985.

C. Data Sources and Collection

The data used in the analysis come from two sources. Data on the outcome of claims applications at the initial visit to the UI office (in particular qualification, disqualification, amount of first check) and on claimants characteristics were collected by the UI office personnel. The local offices collected information on all valid applications during the study period. The experimental effects associated with these short-run outcomes are estimated using this data set.

Data on longer run outcomes of the unemployment spells, such as total benefits received and claim duration, were obtained from each state's central data processing department. These longer run outcomes were in general measured one month after the end of the study period, though that period of time varied in each state. For that reason, we will refer to the claim duration variable as the *observed* claim duration since our measure is not always a measure of the length of a completed claim. The analysis of duration variables and the measurement of treatment effects in the context of random assignment are not as straightforward as is sometimes the case.

For example, using data from the National Supported Work (NSW) Demonstration, Ham and Lalonde (1996) showed that samples of individuals in new spells of employment after the treatment (training program) has been randomly assigned to unemployed workers were not random subsets of the experimental sample. In our context, this should not be a serious problem because individuals in the control and treatment group remain in the same state (that is, unemployed) while the treatment is provided to the individuals in the treatment group.

[5] See the appendix for more information on the experimental periods.

Nonetheless there is a serious problem with our long-run outcomes samples in two of the four states because of the data collection system used. In both Tennessee and Virginia, the longer run outcomes were recorded only for claimants whose claim was still active at the date of measurement, so that claim duration is right-censored[6]. If the treatment has any sort of impact in reducing the duration of unemployment, then the probability of observing a censored unemployment spell will differ between the treatment and control groups, which should not be the case under random assignment. For these two states, therefore, it is possible that the samples on longer run outcomes are not random subsets of the experimental sample. For this reason, we perform separate statistical tests of random assignment on the basis of the claimant's demographic characteristics on data coming from both short-run and long run outcomes samples. In addition, we provide some analyses that are restricted to the data for the two states where we are certain random assignment was not compromised.

2. The Effectiveness of the Randomization

Tables 1a and 1b present descriptive statistics for each of the study groups. Table 1a is based of the full sample of demographics and short-run claim outcomes obtained from each participating local UI office in each state, while Table 1b is based on the smaller sample for which we have longer run outcome measures. Corresponding to the discussion of the treatments presented above we have presented data on four separate groups. In the groups labeled "All Sample" we have provided data for the "Control" group in column (2), and data for the overall "Treatment" group in column (1). Since we selected whether a claimant was in the treatment or control group randomly, about one-half of the total sample falls in each group. Under the columns labeled "WSV" we have placed data on the groups that can be used to test for whether the work search verification treatment had an impact separate from the remainder of the treatment process. The "Treatment" group in column (3) was subject to work search verification, while the "Control" group in column (4) was subject only to the other parts of the treatment

[6] In Virginia, about 5% of the observations were not right-censored.

process. The number of observations in column (1) is therefore the sum of the number of observations in columns (3) and (4).

These tables describe the socioeconomic characteristics and mean week benefit amount (WBA). As is apparent from the tables, the means of these variables are remarkably similar across group within a state. To study the effectiveness of our randomization method we tested the hypothesis of equality of means (or proportions) for all the variables in the relevant treatment and control groups combinations, that is (1) vs. (2), (3) vs. (2) and (4) vs. (2). For the full sample of demographics and short-run claim outcomes in Table 1a, the results are consistent with randomization[7] in almost all cases. The exceptions are a significantly different age variable in the treatment and control groups in Virginia, and significant differences in the race proportions and the gender proportions in the comparison of columns (3) versus (4) in Tennessee and Virginia, respectively. Thus, in 57 contrasts there are three contrasts that are statistically significant at the .05 level, which is almost precisely what would be expected (that is, .05(57)=2.85) if assignment were random. In all cases, moreover, the differences, even when statistically significant, are very small.

In another attempt to verify the effectiveness of the random assignment, we tested the null hypothesis of equality of the distribution function of the demographic variables using a Kolmogorov-Smirnov test (see DeGroot 1975 p.558). In all cases we were unable to reject the hypothesis of equality of the distributions.

Finally, we performed the same tests reported above for the sample of long-run claim outcomes in Table 1b. In general these results are also consistent with randomization. In fact, the patterns of rejection of equality of means are almost the same as for the full sample of demographics and short-run claim outcomes.

These statistical tests, based on the observable characteristics of the treatment and control group, suggest strongly that the treatment and control groups were indeed drawn from the same population. We will therefore estimate experimental effects using standard analysis-of-variance methods. In addition, we use regression methods to estimate treatment effects in order

[7] We use a two-sided t-test for equality of means at the 5% level.

to capture any gains in efficiency that may exist. A comparison of the difference between the results before and after adjustment for regression provides another test for the presence of randomization. If the randomization was effective the two sets of estimates should not differ apart from that which may be attributable to sampling error. Nonetheless, as discussed earlier, for two states (TN and VA) data on long-run claim outcomes was collected on a potentially non-random subset of the claim outcomes, and so we report some results that eliminate these data from the results.

3. Estimated Effects of the Treatments

Table 2 displays simple summary statistics on several measures of program outcomes for the treatment and control groups. These measures of location are based on two different samples. The "Qualification Rate" panel is based on the same samples that underlies Table 1a, that is the full sample of demographic variables and short run claim outcomes coming from each local UI office. It provides details on the extent to which the treatment resulted in applicants being denied claims at various points in the process. The "Benefit Payments" and "Claim Duration" panels are based on the same sample that underlies Table 1b, that is the sample of long-run claim outcomes variables that was provided by the state's central data processing departments. In general, the long run outcome samples are smaller than the samples containing demographic variables only.

Tables 3 to 6 display the various responses of the claimants to the experimental procedures. For each program variable related to the experimental procedures we present four measures of the experimental effects. For each variable of interest, we present differences in means (or quantile)[8] and estimated regression coefficients that control for additional characteristics. Under random assignment the estimates of the experimental effects obtained by regression methods should not differ from the estimates obtained by comparison of means (apart from sampling error), but could be more precise. Since in our applications most of the variation in the dependant variable is unexplained (the regressions have R^2 less than .05), the improvement in the precision of the estimates is negligible. It should be noted that it is not possible to isolate the specific

impact of the three components of the treatment (eligibility, monetary determination and work search). We can achieve partial identification by looking at contrasts that contain a unique component of the treatment.

The first column is the overall treatment effect, the difference between the "All Sample" treatment and control outcomes. This treatment effect is the combined result of the three elements of the experimental procedures. The remaining columns measure the part of the overall treatment effect associated with work search verification. The second column presents the difference in means between the claimants in the treatment group whose job contacts were not verified (4) and the claimants in the control group (2), who by definition were not given the work search verification part of the treatment. This difference measures the result of receiving the eligibility and monetary determination elements of the experimental procedures plus the effect of stressing the rules of work search as a requirement for claiming UI benefits. The third column presents the difference in means between the claimants in the treatment group whose job contacts were actually verified (3) and the claimants in the treatment group who did not have their job contacts verified (4). Since claimants in groups (3) and (4) receive the same treatment, except for the actual work search verification, this difference measures the specific impact on claim outcomes of actually verifying the job contacts of claimants. The fourth column presents the difference in means between the claimants in the treatment group whose job contacts were actually verified (3) and the claimants in the control group (2), who by definition were not given the work search part of the treatment. This measures the combined impact of informing the claimant and actually verifying the appropriateness of the claimant's work search, in addition to the eligibility and monetary determination parts of the experimental procedures.

As this discussion illustrates, it is possible to uniquely identify only one component of the experimental treatment. The difference in responses between experimental groups (3) and (4) yields an estimate of the effect on outcomes of actually verifying the appropriateness of the work search behavior of the claimants. Since individuals in the treatment group either get their job search contacts verified (group (3)) or not (group (4)), we can always write the mean of an

[8] It should be noted, as we will discuss below, that difference in quantiles are not in general unbiased estimates of the

outcome in the treatment group (1) as $X_1 = pX_3 + (1-p)X_4$, where p is the fraction of the treatment group which gets job search contacts verified. Then the overall treatment effect[9], that is the mean outcome in the treatment group minus the mean outcome in the control group $(X_1 - X_2)$, can be written as $(X_4 - X_2) + p(X_3 - X_4)$. The second part of the mean difference $p(X_3 - X_4)$ reflects the importance in the overall treatment of actually verifying the appropriateness of the work search behavior of the claimants. The first part of the mean difference $(X_4 - X_2)$ reflects the importance in the overall treatment effect of the combination of all other parts of the experimental procedures, without the verification of the job search contacts. This simple formula permits partial identification of the specific impacts of the 3 elements of the experimental procedures.

The object of interest in the evaluation of experiments is the effect of the treatment on the distribution of outcome variables. Unless we observe the same individuals in the treated and untreated states (which is not the case in most instances) we cannot observe the experimental effect for any particular individual. One implication of randomization is that the outcome distribution for individuals in the treatment group summarizes the outcome distribution that individuals in the treatment group would have had if they had been in the control group, that is, if they had not received the treatment. As a result, the difference in means of outcomes in the treatment and control group is the mean of the (unobserved) distribution of experimental impacts. Without further assumptions, the same conclusion is not true for other parts of the experimental effects distribution. For example, the difference in the median outcomes in the treatment and control group is not in general equal to the median impact of the experiment. A discussion of this issue is presented in Heckman (1992) and Heckman, Smith and Clements (1997). Under the assumption of "common experimental effect", where the experimental impact is assumed to be the same for all the participants, the difference in the medians (or any other quantile) recovers the median impact. This assumption is unlikely to hold in our experiment[10]. For example, there is no reason to believe that the work search component of the experimental procedures should reduce the length of the unemployment spell of all the claimants in the treatment group by the same

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quantiles of the experimental effects distribution as it is the case for means.

[9] This decomposition is only valid when we look at the difference in means.

[10] Heckman, Smith and Clements (1997) propose some methods to test the "common experimental effect" model.

amount. As a consequence, when looking at the tables, the reader should not interpret the differences in quantiles as unbiased estimates of the quantiles of the impact distribution. [11] Nevertheless, the difference between the quantiles in the treatment and control distributions does provide an appropriate estimate of the effect of the treatment on the distribution of the outcome variables.

A. Impact on the Qualification Rate

The impact of the experimental procedures on the qualification rate mainly reflects the effectiveness of the eligibility component of the experiment, which focussed on detecting ineligible claimants via an extended questionnaire. Thus, if current procedures are ineffective in detecting non-eligible claimants, the most immediate effect of the eligibility review component of the experimental procedures should be to increase disqualifications and reduce those qualified for benefits in the first week of eligibility in the treatment group. We analyze differences in the treatment and control groups for the following outcomes: the fraction of claimants qualifying for benefits in the first week, the fraction of claimants disqualified permanently (for the most part based on the reason for separation or the failure to have significant earlier earnings), the fraction of claimants disqualified temporarily (which mainly reflects "able and available" issues, or transitory income or work search issues), and the fraction of "no-shows" (those claimants not formally disqualified who did not show up at their scheduled second visit to the UI office). The analysis of the "no-shows" variable allows us to measure the relative importance of endogenous stratification (see Hausman and Wise (1985)) in our experimental design.

The top panel of Table 2 presents the sample proportions of the qualification-related variables for all the study groups. The fraction of claimants qualifying for benefits in the first week of eligibility is quite similar among Connecticut, Massachusetts, and Tennessee, but somewhat higher in Virginia. There is much more variation across states in the three other short run claim outcome variables. In some states, almost all of the disqualifications are permanent (CT, MA, TN), while in VA almost all disqualifications are temporary. The fraction of "no-shows" is in general small, except in MA, where it is around 15%.

[11] As in the example in Heckman, Smith, and Clements (1997), the nonparametric bounds procedure they suggest

The top panel of Table 3 displays the program treatment effects on the qualification rate. Any effect of the experimental procedures on the qualification rate should mainly reflect the eligibility review component of the experimental procedures. The bottom panel presents the same differences in proportions, but from a linear probability model that controls for various factors (which are listed in the stub of the table). It is clear from Table 3 that the experimental procedures had very moderate effects on the qualification rate outcomes. In each state (except in CT), the overall experimental effects (column 1) are small in magnitude and not significantly different from 0. The experimental effect on the qualification rate in the first week is of the expected negative sign in all states, except in VA, where it is positive. The effect on the qualification rate in the first week is only significant in CT where applicants in the treatment were 8% less likely to qualify for benefits during the first week of eligibility. Using the decomposition of means formula presented above, we can conclude that in CT most of the overall treatment effect on qualification in the first week is explained by the combination of the three elements of the experimental procedures, and not by the actual verification of work search requirements. This suggests that the eligibility questionnaire was effective in detecting ineligible claimants.

The experimental effects on temporary and permanent disqualifications provide further information on the effect of the treatment on benefit receipt. The effect of the experiment on disqualifications is of the expected positive sign in all states, except again in VA, where it is negative. These results indicate that the experimental procedures tend to increase disqualifications, but by a very small amount that is not statistically significant. The treatment effects related to the "no-show" variable are in all cases small in magnitude (2% or less) and in most cases not significant. This indicates that the probability of claimants withdrawing themselves from the experiment is not correlated with the assignment in the treatment and control group. Hence the potential problem of endogenous stratification can be left aside in the following discussion. Regression adjusted results are presented in the bottom panel of Table 3. The results are almost identical to those contained in the top panel.

B. Impact on Benefits Payments

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provides very little information on the size of treatment effects in our data.

The impact of the experimental procedures on the benefit payments reflects the interaction between the effectiveness of the review of monetary determination and the work search components of the experimental procedures. The former aims at identifying overpayments due to incorrect monetary determination while the later aims at reducing the unemployment claim length by increasing the cost of inappropriate job search. We analyze differences in the treatment and control groups for the following outcomes: total benefits received (up until the point of measurement of the long-run outcomes), average weekly benefits, and the amount of the first check issued. We also look at various quantiles (25^{th} , 50^{th} and 75^{th}) of the total benefits variable[12]. Since some claims will still be active at the date of measurement, we do not consider the mean of the total benefits variable to be an ideal estimate of the mean of total benefits received for a completed claim. It is likely to underestimate total benefits. Total benefits and average weekly benefits, both of which are determined by total claim duration and by the weekly benefit amount (WBA), should be affected by the two components of the experimental procedures mentioned above. The amount of the first check issued should only be affected by the review of monetary determination. Thus the most immediate effect of the review of monetary determination and work search components of the experimental procedures should be to decrease total benefits, average weekly benefits, and the first check issued in the treatment group.

Table 4 presents estimates of the experimental effects on benefit payments. None of the mean or median treatment effects is statistically significant. Moreover, in TN and VA, the effects are opposite of the sign expected, as claimants in the treatment group received on average higher total benefits for the observed duration of the claim. The same observation applies to the average weekly benefits variable, though a significant negative treatment effect of 12$ a week is observed in CT. Since the amount of the first check issued is independent of the observed claim duration, any experimental effect on that variable is a result of the effectiveness of the review of the monetary determination component of the experimental procedures. The effect on the first check issued is only significant in CT where applicants in the treatment group received 8 dollars

[12] We use these various quantiles in addition to the means since they are less influenced by extreme values.

less than their counterparts in the control group. For the rest of the participating states, the experimental effects are insignificant and of a sign opposite to that expected, i.e. claimants in the treatment group received larger amounts in their first check. There is no clear pattern that emerges when we look at the differences in various quantiles of the total benefits received distribution. The differences between the treatment and control groups tend to be bigger in the tails of the distribution (25^{th} and 75^{th} quantiles) than at the median[13]. Regression adjusted results are presented in the bottom panel of Table 4. The results are quantitatively the same as in the top panel.

In sum, claimants in the treatment group do not appear to receive substantially smaller benefit payments than claimants in the control group. The review of the monetary determination was also unsuccessful in identifying overpayments in the treatment group, as the treatment effects on the first check issued variable are positive (instead of the expected negative sign) but non-significant. The exception is CT where the first check issued is reduced by 8$ on average in the treatment group. It appears that for that state, the review of monetary determination reached its objectives.

C. Impact on Claim Duration

The direct effect of the work search component of the experimental procedures is to provide claimants an incentive to search actively for work. Thus, the most immediate effect of this component of the experimental procedures should be to reduce the length of unemployment claims. We analyze differences in means (and quantiles) in the treatment and control groups for the observed claim duration. In addition, we can decompose the difference in means in the following way:

$$X_1 - X_0 = [c_1X_{1c} + (1-c_1)X_{1nc}] - [c_0X_{0c} + (1-c_0)X_{0nc}]$$

Where X_1 and X_0 are the average observed claim duration in the treatment and control groups respectively, c_1 and c_0 are the probabilities of censoring in the treatment and control groups, X_{1c}

[13] Standard errors of the differences in quantiles were obtained by the bootstrap method, with 5000 replications. For more

and X_{0c} are the average claim duration of censored spells in the treatment and control groups and X_{1nc} and X_{0nc} are the average claim duration of non-censored spells in the treatment and control groups. Then using a decomposition of the above formula, we can write:

$$X_1 - X_0 = (c_1 - c_0)X_{1c} + [(1-c_1) - (1-c_0)]X_{1nc} + (X_{1c} - X_{0c})c_0 + [X_{1nc} - X_{0nc}](1-c_0)$$

The first two terms represent the difference in mean duration due to differences in the probabilities of censoring (and non-censoring) across the treatment and control groups, while the last two terms represent the difference in means due to the difference in average censored and non-censored claim duration in the treatment and control groups. We present each of these four components as a fraction of the overall mean difference. Since the observations on long-run outcomes are all censored in TN and VA, we cannot perform this decomposition for these two states.

The bottom panel of Table 2 presents the mean and median of the observed claim durations. Variation across states reflects the difference in the impacts of the treatment across states, but also the fact that the date of measurement after the experimental period was not uniform across states[14]. For most states, the average observed claim duration ranges from 4 to 6 weeks, while in VA, the average is substantially higher, 20 weeks. This is due to the fact that data on the long-run outcomes (including claim duration) was collected 2 months after the end of the study period, and that for this state, data were only collected on claims that were still active at the date of collection. In most cases, medians and means are quite similar. We also present the fraction with censored claims durations, which are around 40% in CT and MA.

The top panel of Table 5 displays differences in means and quantiles for the observed claim duration distribution. Any impact of the experimental procedures from this table should be associated with the work search component of the experimental procedures. We also present the results of the decomposition of means outlined above. It is clear from Table 5 that the experimental procedures had little effect (if any) in reducing the length of the unemployment

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details on that approach of computing standard errors, see Efron and Tibshirani (1993).

spells. None of the difference in means and quantiles is statistically different from 0. The decomposition of the difference in means reveals that in CT, most of the difference is explained by the difference in the probability of censoring, while in MA, most of the difference is explained by the difference in the means of censored claims. Regression adjusted results are presented in the bottom panel of Table 5, and they are very similar to the reports in the top half of the table. In sum, the experimental procedures, in particular the work search component, were unsuccessful in reducing the length of unemployment spells.

D. Pooling the Data

We present in Table 6 the analysis of the response to the treatment obtained by grouping observations from all the states[15]. We present simple differences in means (or in quantiles) as well as regression adjusted estimates. Each regression controls for observed characteristics[16] of the claimants as well as state fixed effects. The results are consistent with those presented in Tables 3-5. The only element of the experimental procedures that had a significant impact is apparently the review of eligibility, as some of the treatment effects associated with the qualification rate are statistically significant. Claimants in the treatment groups in the four states are 5% less likely to qualify for benefits in the first week and 3% more likely to be disqualified than claimants in the control groups. The responses to the treatment related to benefit received and claim duration are not significant and they are very small in magnitude. The results for CT and MA, where data censoring is not a problem, are similar.

5. 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

[14] These dates of measurement are presented in appendix A.
[15] The data are pooled without weighting.
[16] The regressions on all four states include controls for age, sex, and race only since annual income is not available for VA. The regressions for CT and MA only control for age, sex, race and annual income.

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 only in four states, 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 is not worth the cost.

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Figure 1: Typical Processing of Claims in the Treatment and Control Groups in the Field Experiment.

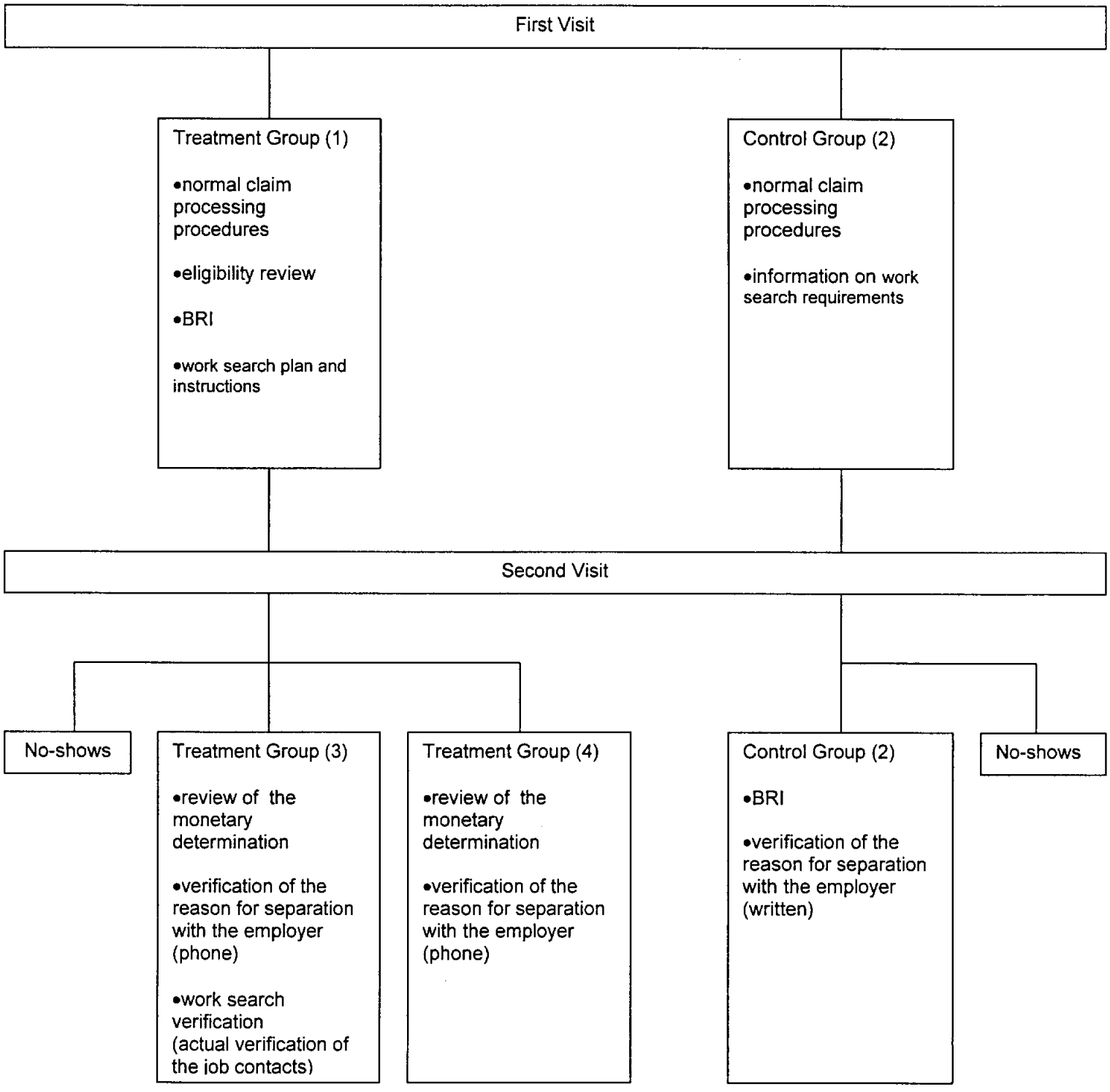

Table 1a: Descriptive Statistics
Demographic and Short-Run Claim Outcomes Sample.

	Connecticut				Massachusetts				Tennessee				Virginia			
	All Sample		WSV Only		All Sample		WSV Only		All Sample		WSV Only		All Sample		WSV Only	
	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (1)	Control (2)	Treatment (3)	Control (4)
Age	35.79	35.81	35.83	35.77	37.68	36.83	38.34	-37.22	34.79	33.99	34.27	35.15	37.57	35.61	37.90	37.38
	[0.43]	[0.43]	[0.67]	[0.55]	[0.89]	[0.77]	[1.44]	[1.13]	[0.54]	[0.55]	[0.86]	[0.68]	[0.65]	[0.62]	[1.07]	[0.82]
Fraction Male	0.71	0.71	0.72	0.71	0.65	0.68	0.60	0.68	0.58	0.63	0.58	0.59	0.64	0.57	0.66	0.63
	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]	[0.03]	[0.05]	[0.04]	[0.02]	[0.02]	[0.04]	[0.03]	[0.03]	[0.03]	[0.05]	[0.04]
Fraction White	0.61	0.62	0.64	0.59	0.95	0.94	0.95	0.96	0.68	0.69	0.73	0.64	0.77	0.74	0.67	0.83
	[0.02]	[0.02]	[0.03]	[0.02]	[0.01]	[0.01]	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]	[0.03]	[0.02]	[0.03]	[0.05]	[0.03]
Annual Earnings	12217	12183	12536	11991	12829	12419	12845	12814	11295	10862	11484	11165	-	-	-	-
	[341.4]	[353.4]	[499.9]	[463.4]	[653.6]	[513.7]	[1010.6]	[860.0]	[312.6]	[339.6]	[510.6]	[394.3]				
Weekly Benefits Amount (WBA)	128.8	126.8	132.7	126.1	125.8	128.6	125.5	126.1	91.22	90.84	93.1	89.92	131.5	130.4	132.6	130.9
	[1.79]	[1.77]	[2.74]	[2.35]	[3.77]	[3.34]	[5.96]	[4.88]	[1.49]	[1.49]	[2.17]	[2.03]	[1.66]	[1.68]	[2.67]	[2.11]
Observations	858	821	355	503	235	280	97	138	474	425	193	281	285	280	101	184

Table 1b: Descriptive Statistics
Long-Run Claim Outcomes Sample Only.

	Connecticut				Massachusetts				Tennessee				Virginia			
	All Sample		WSV Only		All Sample		WSV Only		All Sample		WSV Only		All Sample		WSV Only	
	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (1)	Control (2)	Treatment (3)	Control (4)
Age	36.31	36.42	36.26	36.34	38.04	36.82	38.69	37.58	36.88	35.68	36.50	37.15	37.46	35.62	37.94	37.20
	[0.46]	[0.48]	[0.71]	[0.60]	[0.91]	[0.77]	[1.46]	[1.17]	[0.73]	[0.71]	[1.19]	[0.92]	[0.65]	[0.62]	[1.06]	[0.82]
Fraction Male	0.73	0.71	0.74	0.72	0.66	0.69	0.61	0.70	0.56	0.65	0.53	0.58	0.64	0.56	0.65	0.64
	[0.02]	[0.02]	[0.03]	[0.02]	[0.03]	[0.03]	[0.05]	[0.04]	[0.03]	[0.03]	[0.05]	[0.04]	[0.03]	[0.03]	[0.05]	[0.04]
Fraction White	0.64	0.66	0.66	0.62	0.95	0.94	0.95	0.95	0.71	0.75	0.81	0.65	0.78	0.74	0.68	0.83
	[0.02]	[0.02]	[0.03]	[0.02]	[0.01]	[0.01]	[0.02]	[0.02]	[0.03]	[0.03]	[0.04]	[0.04]	[0.02]	[0.03]	[0.05]	[0.03]
Annual Earnings	13774	13779	14124	13525	13034	12533	12970	13079	12666	11831	12772	12591	-	-	-	-
	[362.9]	[380.6]	[522.3]	[498.0]	[657.1]	[514.9]	[1014.2]	[865.9]	[448.3]	[461.8]	[747.1]	[555.1]				
Weekly Benefits Amount (WBA)	138.2	135.3	142.1	135.4	130.9	131.4	129.5	131.8	98.14	94.97	98.45	97.93	131.6	130.4	132.7	131.0
	[1.70]	[1.73]	[2.58]	[2.25]	[3.53]	[3.21]	[5.68]	[4.50]	[1.72]	[1.91]	[2.78]	[2.19]	[1.66]	[1.68]	[2.12]	[2.12]
Observations	736	701	306	430	226	275	94	132	275	250	114	161	280	273	99	181

Standard Errors are in square brackets.

The overall treatment and control groups (1) and (2) are the result of the initial assignment. The Work Search Verification (WSV) treatment group (3) is the subset of the overall treatment group (1) that was assigned to WSV. The control group (4) is the rest of the overall treatment group (1) that was not assigned to WSV, but nonetheless received the other components of the treatment. The number of observations in column (3) and (4) should add up to the number of observations in column (1). The demographic and short-run claim outcomes sample are obtained from the participating local UI office in each state and include information on all individuals starting a new UI claim during the experimental period. The long-run claim outcomes samples are obtained from each state's central data processing department and include information on a strict subset of individuals who started a new UI claim during the experimental period. The conditions for being included in that sample vary from state to state and are discussed in the paper

Table 2: Means and Quantiles of the Program Variables in the Treatment and Control Groups.

	Connecticut				Massachusetts				Tennessee				Virginia			
	All Sample		WSV Only		All Sample		WSV Only		All Sample		WSV Only		All Sample		WSV Only	
	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (1)	Control (2)	Treatment (3)	Control (4)
Qualification Rate																
Fraction Qualified in the First Week	0.654 [0.02]	0.734 [0.02]	0.642 [0.03]	0.662 [0.02]	0.711 [0.03]	0.743 [0.03]	0.670 [0.05]	0.739 [0.04]	0.681 [0.02]	0.708 [0.02]	0.680 [0.03]	0.682 [0.03]	0.884 [0.02]	0.871 [0.02]	0.822 [0.04]	0.918 [0.02]
Fraction Disqualified Permanently	0.193 [0.01]	0.161 [0.01]	0.194 [0.02]	0.192 [0.02]	0.132 [0.02]	0.107 [0.02]	0.134 [0.03]	0.130 [0.03]	0.298 [0.02]	0.285 [0.02]	0.278 [0.03]	0.311 [0.03]	0.000 [0.00]	0.007 [0.01]	0.000 [0.00]	0.000 [0.00]
Fraction Disqualified Temporarily	0.072 [0.01]	0.046 [0.01]	0.090 [0.02]	0.060 [0.01]	0.013 [0.01]	0.014 [0.01]	0.021 [0.01]	0.007 [0.01]	0.006 [0.00]	0.005 [0.00]	0.010 [0.01]	0.004 [0.00]	0.109 [0.02]	0.114 [0.02]	0.168 [0.04]	0.076 [0.02]
Fraction of "no-shows"	0.080 [0.01]	0.058 [0.01]	0.073 [0.02]	0.085 [0.01]	0.145 [0.02]	0.135 [0.02]	0.175 [0.04]	0.123 [0.03]	0.015 [0.01]	0.002 [0.00]	0.031 [0.01]	0.004 [0.00]	0.007 [0.00]	0.007 [0.01]	0.010 [0.005]	0.005 [0.005]
Observations	858	821	355	503	235	280	97	138	474	425	193	281	285	280	101	184
Benefit Payments																
Total Benefits	789.04 [28.49]	837.86 [28.03]	794.65 [44.47]	785.06 [37.15]	563.77 [34.33]	587.97 [31.54]	579.12 [55.14]	552.83 [43.90]	691.40 [23.48]	668.03 [24.05]	649.94 [36.83]	720.76 [30.35]	1041.05 [46.37]	949.74 [43.10]	1106.56 [80.54]	1005.2 [56.60]
Median Total Benefits	599.12 [49.10]	696.42 [38.47]	607.99 [75.41]	591.00 [58.20]	531.02 [57.689]	554.00 [70.30]	528.54 [96.710]	529.27 [81.79]	661.73 [46.683]	652.02 [47.22]	594.01 [61.864]	708.86 [49.37]	659.85 [46.547]	651.89 [47.56]	591.08 [62.23]	710.79 [48.35]
Average Weekly Benefits	115.19 [3.43]	127.14 [4.20]	118.59 [5.66]	112.78 [4.28]	131.77 [4.13]	127.65 [3.75]	131.64 [6.63]	131.85 [5.31]	99.15 [1.39]	97.86 [1.45]	98.72 [2.23]	99.45 [1.78]	48.39 [2.13]	45.94 [2.18]	51.43 [3.79]	46.73 [2.56]
First Check	90.55 [2.89]	99.00 [2.90]	93.20 [4.59]	88.66 [3.72]	91.65 [5.91]	91.28 [5.42]	86.72 [9.17]	95.47 [7.74]	98.14 [1.75]	94.97 [1.91]	98.45 [2.78]	97.93 [2.19]	124.99 [2.02]	123.29 [2.08]	123.57 [3.50]	125.74 [2.48]
Observations	736	701	306	430	226	275	94	132	275	250	114	161	280	273	99	181
Observed Claim Duration																
Average Duration (weeks)	6.235 [0.173]	6.499 [0.174]	6.124 [0.269]	6.314 [0.226]	4.310 [0.235]	4.720 [0.223]	4.479 [0.378]	4.189 [0.301]	6.931 [0.210]	6.744 [0.221]	6.702 [0.351]	7.093 [0.259]	21.957 [0.281]	21.578 [0.92]	22.030 [0.481]	21.918 [0.347]
Median Duration (weeks)	5.674 [0.464]	5.981 [0.219]	5.195 [0.561]	5.854 [0.357]	4.613 [0.651]	5.556 [0.527]	5.118 [0.877]	4.311 [0.685]	6.960 [0.249]	6.554 [0.489]	6.433 [0.588]	7.075 [0.308]	23.891 [0.574]	23.252 [0.523]	24.135 [1.196]	23.835 [0.636]
Fraction Censored	0.385 [0.018]	0.405 [0.019]	0.373 [0.028]	0.393 [0.024]	0.394 [0.033]	0.379 [0.029]	0.372 [0.050]	0.409 [0.043]	1.000	1.000	1.000	1.000	0.947 [0.013]	0.946 [0.014]	0.940 [0.024]	0.951 [0.016]
Observations	736	701	306	430	226	275	94	132	275	250	114	161	280	273	99	181

Standard Errors are in square brackets.

The standard errors for the various quantiles are obtained by using the bootstrap method with 5000 replications. The overall treatment and control groups (1) and (2) are the result of the initial assignment. The Work Search Verification (WSV) treatment group (3) is the subset of the overall treatment group (1) that was assigned to WSV. The control group (4) is the rest of the overall treatment group (1) that was not assigned to WSV, but nonetheless received the other components of the treatment. The number of observations in column (3) and (4) should add up to the number of observations in column (1).

Table 3: Treatment Effect on the Qualification Rate.

	Connecticut Treatment – Control				Massachusetts Treatment – Control				Tennessee Treatment – Control				Virginia Treatment - Control			
	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)
Difference in Means																
Fraction Qualified in the First Week	-0.081* [0.022]	-0.072* [0.026]	-0.020 [0.033]	-0.092* [0.030]	-0.033 [0.039]	-0.005 [0.046]	-0.069 [0.061]	-0.074 [0.055]	-0.026 [0.031]	-0.026 [0.035]	-0.002 [0.044]	-0.028 [0.040]	0.013 [0.027]	0.047 [0.028]	-0.097 [0.043]	-0.050 [0.043]
Fraction Disqualified Permanently	0.032 [0.019]	0.032 [0.022]	0.002 [0.027]	0.034 [0.025]	0.025 [0.029]	0.024 [0.034]	0.004 [0.045]	0.027 [0.039]	0.013 [0.030]	0.026 [0.035]	-0.033 [0.042]	-0.007 [0.039]	-0.007 [0.005]	-0.007 [0.005]	-	-0.007 [0.005]
Fraction Disqualified Temporarily	0.026* [0.011]	0.013 [0.013]	0.030 [0.019]	0.044* [0.017]	0.001 [0.010]	-0.007 [0.010]	0.013 [0.016]	0.006 [0.016]	0.002 [0.005]	-0.001 [0.005]	0.007 [0.008]	0.006 [0.008]	-0.006 [0.027]	-0.038 [0.027]	0.092* [0.042]	0.054 [0.042]
Fraction of "no-shows"	0.022 [0.012]	0.027 [0.015]	-0.012 [0.019]	0.015 [0.016]	-0.009 [0.031]	-0.012 [0.035]	0.052 [0.048]	0.040 [0.044]	0.012* [0.006]	0.001 [0.004]	0.027* [0.013]	0.029* [0.013]	-0.000 [0.007]	-0.002 [0.007]	0.004 [0.011]	0.003 [0.011]
Regression Adjusted[A]																
Fraction Qualified in the First Week	-0.077* [0.021]	-0.063* [0.025]	-0.034 [0.032]	-0.095* [0.028]	-0.022 [0.038]	-0.023 [0.042]	-0.110 [0.060]	-0.153* [0.053]	-0.031 [0.030]	-0.026 [0.034]	-0.014 [0.041]	-0.037 [0.038]	0.012 [0.028]	0.043 [0.029]	-0.100* [0.041]	-0.041 [0.042]
Fraction Disqualified Permanently	0.025 [0.017]	0.010 [0.020]	0.017 [0.025]	0.026 [0.023]	0.025 [0.027]	0.022 [0.030]	-0.022 [0.041]	0.018 [0.039]	0.012 [0.029]	0.022 [0.034]	-0.024 [0.041]	-0.003 [0.037]	-0.010 [0.006]	-0.010 [0.007]	-	-0.008 [0.009]
Fraction Disqualified Temporarily	0.025* [0.011]	0.015 [0.011]	0.027 [0.016]	0.037* [0.015]	-0.018 [0.013]	-0.007 [0.011]	0.026* [0.011]	0.004 [0.015]	0.002 [0.004]	-0.001 [0.004]	0.003 [0.010]	0.009 [0.010]	-0.053 [0.026]	-0.032 [0.028]	0.093* [0.040]	0.042 [0.040]
Fraction of "no-shows"	0.021 [0.011]	0.025 [0.014]	-0.016 [0.018]	0.016 [0.015]	0.016 [0.029]	0.002 [0.034]	0.041 [0.048]	0.043 [0.041]	0.017 [0.006]	0.005 [0.003]	0.036 [0.010]	0.036 [0.010]	0.002 [0.007]	-0.001 [0.008]	0.008 [0.012]	0.005 [0.011]
Observations	1679	1324	858	1176	515	418	235	377	899	706	474	618	565	464	285	387

Standard Errors are in square brackets.

Note: The first column is the overall treatment effect, the difference between the "All Sample" treatment and control groups (1) and (2). The second column presents the difference in outcomes of groups (4) and (2), the combined effect of all the components of the treatment, except the actual verification of job contacts. The third column measures the unique impact of the work search verification as it is the difference between outcomes of claimants in the treatment group who receive WSV (group 3) and those who did not (group 4). The last column presents the difference in outcomes for groups (3) and (2), which measures the effect of receiving all the components of the treatment, including the verification of the job contacts. (A) The linear probability model includes controls for age, sex, race and annual earnings, except the regressions for VA that exclude controls for annual earnings. The regressions are estimated by WLS to correct for heteroscedasticity.

Table 4: Treatment Effects on Total Benefits Received.

	Connecticut Treatment – Control				Massachusetts Treatment – Control				Tennessee Treatment – Control				Virginia Treatment - Control			
	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)
Difference in Means[A]																
Total Benefits	-48.815	-52.801	9.587	-43.215	-24.205	-35.138	26.284	-8.854	23.372	52.730	-70.819	-18.089	93.634	59.054	97.517	156.57
	[39.964]	[46.53]	[57.943]	[52.56]	[46.622]	[54.05]	[70.481]	[63.53]	[33.610]	[38.73]	[47.722]	[43.98]	[63.074]	[70.90]]	[97.672]	[90.71]
25th Percentile of Total Benefits	-147.6*	-139.3*	1.535	-136.3*	-6.360	0.381	-3.046	-2.385	9.389	62.374	-105.60	-44.20	27.459	1.499	91.689	94.056
	[52.81]	[58.50]	[44.41]	[60.68]	[29.43]	[39.05]	[41.14]	[40.82]	[37.06]	[52.72]	[66.89]	[57.09]	[72.068]	[69.66]	[108.0]	[107.7]
Median Total Benefits	-97.30	-105.71	16.99	-87.03	-22.98	-26.49	-0.724	-26.07	9.707	58.12	-114.85	-61.35	0.406	0.537	-0.642	-0.127
	[61.59]	[70.11]	[95.44]	[84.95]	[90.71]	[107.2]	[127.0]	[118.6]	[66.24]	[67.70]	[78.47]	[77.55]	[0.546]	[0.573]	[0.666]	[0.758]
75th Percentile of Total Benefits	-73.89	-63.286	-15.903	-81.12	-40.412	-58.190	74.636	13.766	65.961	68.934	-10.602	60.112	47.370	-9.001	196.87	185.04
	[84.547]	[96.87]	[116.96]	[106.9]	[70.007]	[73.46]	[110.55]	[117.7]	[70.041]	[84.61]	[94.464]	[81.15]	[127.79]	[144.1]	[208.13]	[196.6]
Average Weekly Benefits	-11.953*	-14.362	5.809	-8.554	4.114	4.194	-0.199	3.995	1.289	1.591	-0.728	0.863	2.614	1.049	4.415	5.464
	[5.426]	[5.998]	[7.094]	[7.049]	[5.576]	[6.496]	[8.490]	[7.611]	[2.010]	[2.298]	[2.858]	[2.662]	[3.019]	[3.333]	[4.540]	[4.335]
First Check	-8.457*	-10.34*	4.533	-5.808	0.371	4.190	-8.749	-4.559	3.170	2.953	0.522	3.475	2.296	2.998	2.024	0.974
	[4.096]	[4.720]	[5.909]	[5.428]	[8.018]	[9.448]	[12.000]	[10.65]	[2.568]	[2.902]	[3.539]	[3.372]	[2.899]	[3.233]	[4.260]	[4.053]
Regression Adjusted[B]																
Total Benefits	-48.433	-44.768	-6.436	-52.320	-37.075	-48.307	35.691	-23.827	-4.099	22.490	-64.471	-40.944	66.421	23.999	101.18	128.79
	[30.088]	[43.77]	[55.241]	[49.23]	[41.939]	[49.95]	[63.146]	[55.43]	[33.097]	[37.98]	[46.495]	[43.38]	[63.040]	[70.03]	[97.67]	[86.10]
25th Percentile of Total Benefits	-53.516	-37.471	-19.403	-55.408	1.623	-18.935	6.037	60.569	10.135	86.323	-122.23	-61.39	11.250	-17.097	102.04	65.769
	[25.615]	[29.99]	[24.913]	[41.12]	[31.625]	[30.92]	[36.946]	[44.07]	[46.586]	[56.36]	[65.508]	[63.20]	[59.509]	[63.78]	[105.81]	[78.85]
Median Total Benefits	-57.644	-23.201	-87.357	-122.36	-76.812	-48.354	52.707	-54.390	15.774	60.504	-83.777	-22.558	56.938	33.60	92.686	121.81
	[53.373]	[59.54]	[76.570]	[80.57]	[83.235]	[117.6]	[121.48]	[115.5]	[55.354]	[58.67]	[65.322]	[43.92]	[73.148]	[71.17]	[104.12]	[98.89]
75th Percentile of Total Benefits	-56.670	-60.541	18.734	-40.208	-39.257	-4.325	14.781	-47.884	-36.049	-56.570	25.161	35.099	98.500	-75.00	67.895	164.00
	[70.869]	[81.12]	[106.05]	[93.19]	[59.00]	[54.00]	[99.556]	[74.52]	[58.821]	[90.15]	[78.522]	[94.04]	[139.84]	[150.3]	[243.57]	[203.0]
Average Weekly Benefits	-11.681*	-12.75*	3.205	-10.316	1.814	2.372	-0.018	0.452	0.042	0.560	-1.554	-0.658	1.560	-0.138	3.669	4.060
	[5.017]	[5.874]	[6.497]	[6.897]	[3.710]	[4.302]	[5.745]	[5.076]	[1.551]	[1.682]	[2.271]	[2.081]	[3.024]	[3.382]	[4.460]	[4.261]
First Check	-8.365*	-9.036*	2.286	-7.351	-2.206	2.444	-9.637	-8.216	0.131	0.896	-0.609	-0.453	0.919	0.474	1.264	1.842
	[3.678]	[4.180]	[5.443]	[4.729]	[6.943]	[7.899]	[10.461]	[9.239]	[1.861]	[2.095]	[2.669]	[2.460]	[2.812]	[3.189]	[4.152]	[3.965]
Observations	1437	1131	736	1007	501	407	226	369	525	411	275	364	553	454	280	372

Standard Errors are in square brackets. The standard errors for the various quantiles are obtained by using the bootstrap method with 5000 replications. Note: (A) or difference in quantiles. (B) The regressions include controls for age, sex, race and annual earnings, except VA, which excludes controls for annual earnings. The decomposition of means into the censoring and non-censoring components cannot be computed for TN and VA since all observations are censored. See the notes below Table 3 for details on the interpretation of columns (1)-(2), (4)-(2), (3)-(4) and (3)-(2).

Table 5: Treatment Effect on Observed Claim Duration.

	Connecticut Treatment – Control				Massachusetts Treatment – Control				Tennessee Treatment – Control				Virginia Treatment - Control			
	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)
Difference in Means[A]																
Average Observed Claim Duration	-0.264 [0.246]	-0.185 [0.286]	-0.190 [0.352]	-0.375 [0.321]	-0.410 [0.235]	-0.531 [0.374]	0.289 [0.483]	-0.241 [0.439]	0.187 [0.305]	0.349 [0.341]	-0.391 [0.436]	-0.042 [0.415]	-0.380 [0.405]	-0.340 [0.453]	0.112 [0.593]	-0.452 [0.563]
Decomposition of Difference in Means: (fraction of difference)																
Prob. of censoring	0.80	0.67	1.11	0.89	0.26	0.39	0.89	-0.17								
Prob. of non-censoring	-0.29	-0.25	-0.39	-0.32	-0.02	-0.04	-0.03	0.01								
Mean if censored	0.18	0.27	-0.03	0.12	0.65	0.70	0.79	0.54								
Mean if non-censored	0.31	0.30	0.32	0.31	0.11	-0.04	-0.65	0.62								
25th Percentile of Observed Claim Duration	-0.053 [0.252]	-0.048 [0.325]	0.055 [0.343]	0.013 [0.344]	-0.119 [0.439]	0.004 [0.562]	-0.067 [0.565]	-0.059 [0.571]	0.374 [0.668]	0.899 [0.527]	-1.440 [0.661]	-0.551 [0.720]	1.084 [1.034]	0.884 [1.186]	0.458 [1.301]	1.319 [1.253]
Median Observed Claim Duration	-0.307 [0.517]	-0.110 [0.426]	-0.668 [0.671]	-0.794 [0.609]	-0.942 [0.840]	-1.243 [0.867]	0.807 [1.125]	-0.448 [1.037]	0.406 [0.546]	0.537 [0.573]	-0.642 [0.666]	-0.127 [0.758]	0.640 [0.782]	0.563 [0.823]	0.300 [1.363]	0.852 [1.298]
75th Percentile of Observed Claim Duration	-0.352 [0.666]	-0.173 [0.698]	-0.349 [0.774]	-0.519 [0.748]	-0.267 [0.497]	-0.951 [0.527]	0.951 [0.538]	0.003 [0.272]	0.059 [0.288]	0.044 [0.280]	0.265 [0.551]	0.304 [0.543]	0.000	0.000	0.000	0.000
Fraction Right Censored	-0.021 [0.026]	-0.012 [0.030]	-0.020 [0.036]	-0.033 [0.033]	-0.016 [0.044]	-0.031 [0.052]	0.037 [0.066]	0.006 [0.058]	0.000	0.000	0.000	0.000	0.001 [0.020]	0.005 [0.021]	0.011 [0.029]	-0.006 [0.027]
Regression Adjusted[B]																
Average Observed Claim Duration	-0.260 [0.244]	-0.170 [0.281]	-0.215 [0.349]	-0.379 [0.316]	-0.456 [0.319]	-0.546 [0.378]	0.268 [0.473]	-0.322 [0.431]	-0.031 [0.301]	0.097 [0.348]	-0.264 [0.420]	-0.205 [0.398]	0.304 [0.394]	0.182 [0.444]	0.496 [0.576]	0.535 [0.553]
25th Percentile of Observed Claim Duration	-0.156 [0.235]	-0.224 [0.321]	0.110 [0.397]	-0.116 [0.366]	-0.076 [0.187]	-0.241 [0.209]	0.067 [0.367]	0.250 [0.326]	0.149 [0.411]	0.536 [0.391]	-0.922 [0.561]	-0.893 [0.595]	1.238 [0.782]	0.957 [0.916]	0.232 [1.252]	1.324 [1.155]
Median Claim Observed Duration	-0.476 [0.407]	-0.209 [0.438]	-0.962 [0.456]	-0.889 [0.542]	-0.670 [0.488]	-0.712 [0.640]	0.340 [0.893]	-0.567 [0.633]	0.341 [0.467]	0.462 [0.507]	0.091 [0.679]	0.422 [0.586]	0.200 [0.524]	-0.182 [0.580]	1.000 [1.156]	0.485 [0.651]
75th Percentile of Observed Claim Duration	-0.450 [0.359]	-0.431 [0.486]	-0.219 [0.648]	-0.776 [0.512]	-0.239 [0.285]	-0.708 [0.422]	0.760 [0.543]	-0.143 [0.388]	0.182 [0.354]	-0.130 [0.536]	0.684 [0.569]	0.350 [0.584]	-0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]
Observations	1437	1131	736	1007	501	407	226	369	525	411	275	364	553	454	280	372

Standard Errors are in square brackets. The standard errors for the various quantiles are obtained by using the bootstrap method with 5000 replications. Note: (A) or difference in quantiles. (B) The regressions include controls for age, sex, race and state dummies. The decomposition of means into the censoring and non-censoring components cannot be computed for TN and VA since all observations are censored. See the notes below Table 3 for details on the interpretation of columns (1)-(2), (4)-(2), (3)-(4) and (3)-(2).

Table 6: Pooled States Analysis of the Responses to the Treatment

	Difference in Means[A]				Regression Adjusted[B]				Regression Adjusted[C]: CT and MA Only			
	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)	(1) – (2)	(4) – (2)	(3) – (4)	(3) – (2)
Qualification Rate												
Fraction Qualified in the First Week	-0.047*	-0.032	-0.039	-0.071*	-0.048*	-0.029	-0.036	-0.069*	-0.068*	-0.051*	-0.043	-0.092*
	[0.015]	[0.017]	[0.021]	[0.020]	[0.014]	[0.016]	[0.021]	[0.019]	[0.019]	[0.022]	[0.028]	[0.025]
Fraction Disqualified Permanently	0.025*	0.025	0.001	0.024	0.019	0.017	-0.001	0.021	0.019	0.014	0.015	0.040*
	[0.012]	[0.014]	[0.018]	[0.016]	[0.012]	[0.014]	[0.018]	[0.016]	[0.015]	[0.017]	[0.022]	[0.020]
Fraction Disqualified Temporarily	0.011	-0.000	0.029*	0.029	0.011	-0.006	0.042*	0.018	0.021*	0.000	0.025	0.031*
	[0.007]	[0.008]	[0.011]	[0.011]	[0.006]	[0.007]	[0.012]	[0.010]	[0.008]	[0.010]	[0.013]	[0.012]
Fraction of "no-shows"	0.011	0.007	0.011	0.018	0.018*	0.010	0.012	0.031*	0.023	0.024	-0.000	0.024
	[0.008]	[0.009]	[0.011]	[0.010]	[0.007]	[0.007]	[0.010]	[0.009]	[0.011]	[0.013]	[0.017]	[0.015]
Observations	3658	2912	1852	2558	3658	2912	1852	2558	2194	1742	1093	1553
Benefit Payments												
Total Benefits	0.552	0.178	0.926	1.104	-12.247	-15.614	12.087	-5.854	-44.905	-46.206	4.311	-42.477
	[24.784]	[28.668]	[36.699]	[33.333]	[23.796]	[27.170]	[35.064]	[31.223]	[30.325]	[35.037]	[44.803]	[39.278]
25th Percentile of Total Benefits	-55.152*	-36.352	-35.542	-70.846*	-14.824	-4.000	-10.273	-26.526	-30.079	-27.055	-13.863	-32.904
	[26.610]	[39.231]	[43.737]	[33.834]	[14.327]	[20.745]	[16.957]	[23.603]	[16.397]	[20.723]	[18.513]	[26.027]
Median of Total Benefits	-27.303	-23.840	-4.488	-27.667	-21.092	1.667	6.000	-24.000	-53.023	-24.209	-64.255	-120.17*
	[42.369]	[47.150]	[58.987]	[56.884]	[33.424]	[39.403]	[42.530]	[43.529]	[44.237]	[46.651]	[59.996]	[56.767]
75th Percentile of Total Benefits	6.434	-5.663	27.017	20.313	-20.694	-36.225	49.975	16.617	-57.230	-71.749	27.110	-50.831
	[47.929]	[57.673]	[62.205]	[58.940]	[38.436]	[40.258]	[60.301]	[49.953]	[51.112]	[56.723]	[73.438]	[51.619]
Average Weekly Benefits First Check	-5.255	-7.669*	6.018	-1.652	-4.950	-6.217*	3.338	-3.076	-8.894	-9.803*	2.495	-7.854
	[2.993]	[3.318]	[4.037]	[3.950]	[2.681]	[3.096]	[3.549]	[3.734]	[4.040]	[4.726]	[5.373]	[5.560]
	-3.186	-2.958	-0.563	-3.521	-3.493	-3.745	0.827	-3.117	-6.743	-6.477	0.051	-7.058
	[2.443]	[2.793]	[3.563]	[3.291]	[2.292]	[2.596]	[3.347]	[3.037]	[3.258]	[3.716]	[4.841]	[4.201]
Observed Claim Duration												
Average Observed Claim Duration	0.000	0.287	-0.711	-0.423	-0.127	-0.074	-0.123	-0.198	-0.303	-0.265	-0.092	-0.348
	[0.273]	[0.320]	[0.394]	[0.356]	[0.156]	[0.180]	[0.225]	[0.207]	[0.199]	[0.231]	[0.289]	[0.259]
25th Percentile of Observed Claim Duration	-0.046	0.227	-0.389	-0.160	-0.032	0.024	-0.176	-0.074	-0.238	-0.261	0.081	-0.077
	[0.281]	[0.512]	[0.552]	[0.395]	[0.150]	[0.190]	[0.200]	[0.192]	[0.174]	[0.177]	[0.303]	[0.215]
Median of Observed Claim Duration	-0.323	0.009	-0.578	-0.544	-0.200	-0.077	-0.275	-0.267	-0.412	-0.398	-0.485	-0.783
	[0.634]	[0.707]	[0.649]	[0.635]	[0.280]	[0.301]	[0.340]	[0.361]	[0.345]	[0.382]	[0.527]	[0.420]
75th Percentile of Observed Claim Duration	0.160	0.511	-0.878	-0.364	0.000	0.000	0.067	0.000	-0.366	-0.452	-0.000	-0.349
	[0.690]	[0.634]	[0.607]	[0.674]	[0.099]	[0.00]	[0.179]	[0.00]	[0.271]	[0.334]	[0.420]	[0.369]
Observations	3016	2403	1517	2112	3016	2403	1517	2112	1938	1538	962	1376

Standard Errors are in square brackets. The standard errors for the various quantiles are obtained by using the bootstrap method with 5000 replications. Note: (A) or difference in quantiles. (B) The regressions include controls for age, sex, race and state dummies. (C) We presents regression results obtained by pooling only the observations from CT and MA in order to investigate the consequence of the non-randomness of the long-run claim outcomes in TN and VA on the program impacts. See the notes below Table 3 for details on the interpretation of columns (1)-(2), (4)-(2), (3)-(4) and (3)-(2).

Appendix

Differential impact of the experimental procedures across states

There are a number of differences among the four participating states with respect to existing new claim processing procedures, formats and policies. In some states, pre-existing processing procedures already included steps that are under study. Consequently the specific treatment that will be applied in each state will vary. In each of the states it will consist of a combination of some new steps and "front-loading" of some current ones. The major differences across states are summarized in Table 1 in the appendix.

Table A-1: Major Differences in the Treatment Across States.

Component of Treatment	CT	MA	TN	VA
Eligibility:				
new questions	13	?	14	11
front-loaded questions	12	?	0	11
Work Search:				
work search plan	new	new	old	new
work search instructions	new	new	new	new
work search report	?	?	old	?
work search verification	new	new	new	new
Monetary Determination:				
work history form	new	old	old	?
verification of the reason for separation with employer (phone)	new	new	new	new
Study Period	01/85 – 03/85	02/85 – 04/85	01/85 – 03/85	12/84 – 03/85
Date of collection of long-run outcomes data	04/29/85	05/01/85	05/01/85 ???	05/28/85
Number of visits before first check	2	3	2	2
Number of employees at the participating local UI office*	33	25	13	11
Location	Hartford	Worcester	Nashville	Falls Church

Note: In rows 2 and 3 a NEW means that the element is new to the state processing procedures, a OLD indicates that the state procedure already included a similar step. We denote ambiguities by a "?". The number of employees refers to the employees involved in the experiment.