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# Is the Threat of Reemployment Services More Effective Than the Services Themselves? Evidence from Random Assignment in the UI System

By DAN A. BLACK, JEFFREY A. SMITH, MARK C. BERGER, AND BRETT J. NOEL\*

*We examine the effect of the Worker Profiling and Reemployment Services system. This program “profiles” Unemployment Insurance (UI) claimants to determine their probability of benefit exhaustion and then provides mandatory employment and training services to claimants with high predicted probabilities. Using a unique experimental design, we estimate that the program reduces mean weeks of UI benefit receipt by about 2.2 weeks, reduces mean UI benefits received by about \$143, and increases subsequent earnings by over \$1,050. Most of the effect results from a sharp increase in early UI exits in the treatment group relative to the control group. (JEL J650)*

The UI system is widely believed to provide incentives for workers to lengthen their spells of

unemployment by providing a subsidy to their job search and leisure. This paper examines the behavioral effects of a new program that “profiles” Unemployment Insurance (UI) claimants based on the predicted length of their unemployment spell or the predicted probability that they will exhaust their UI benefits. Established in 1993 and formally called the “Worker Profiling and Reemployment Services” (WPRS) system, the program forces claimants with long predicted UI spells or high predicted probabilities of benefit exhaustion to receive employment and training services early in their spell in order to continue receiving benefits.<sup>1</sup>

We consider the effects of the profiling program on claimant behavior using data from Kentucky. Our data embody a unique experimental design. The randomization in our data occurs only to satisfy capacity constraints and only at the margin. UI claimants are assigned “profiling scores” that take on integer values

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<sup>1</sup> See Stephen Wandner (1997), U.S. Department of Labor (1999), or Randall Eberts et al. (2002) for more detailed descriptions of the program and of how it varies across states.

from 1 to 20, with higher scores indicating claimants with longer expected durations. The requirement to receive reemployment services is allocated by profiling score up to capacity. Within the marginal profiling score—the one at which the capacity constraint is reached—random assignment allocates the mandatory services. Thus, if there are ten claimants with a profiling score of 16 but only seven slots remain, seven claimants are randomly assigned to the treatment group and three are assigned to the control group. Donald T. Campbell (1969) terms this experimental design a “tie-breaking experiment.” Donald L. Thistlethwaite and Campbell (1960) first advocated it as a means of evaluating the impact of receiving a college scholarship.<sup>2</sup> To our knowledge, our experiment is the first to use this design.

We have three major findings. First, using our unique data, we evaluate the WPRS system for persons at the profiling score margin. We estimate that for this group, the program reduces mean weeks of UI benefit receipt by about 2.2 weeks, reduces mean UI benefits received by about \$143, and increases subsequent earnings by about \$1,000. Given its very low cost, the program easily passes standard cost-benefit tests.

Second, the dynamics of the treatment effect provide important evidence about *how* the program works. The treatment group has significantly higher earnings in the first two quarters after filing their UI claims than the control group, while there are no significant differences in the third through sixth quarters. This suggests that the earnings gains result primarily from earlier return to work in the treatment group. Moreover, examination of the exit hazard from UI suggests that much of the impact results from persons in the treatment group leaving UI upon receiving notice of the requirement that they receive reemployment services, rather than during or after the receipt of those services. Thus, the program induces some job-ready

claimants to exit quickly, thereby reducing the extent of moral hazard in the UI program.

Third, we evaluate the use of profiling scores based on expected UI claim duration as a means of allocating the treatment. If this is an efficient method of treatment allocation, we would expect to find that the impact of treatment increases in the profiling score. Instead, we find little evidence of any systematic relationship between the estimated impact of treatment and the profiling score. This suggests that such profiling does not increase the efficiency of treatment allocation and indicates the potential value of further research on econometric methods of treatment allocation before extending profiling to other programs.

The paper proceeds as follows: In the next section, we describe the Kentucky WPRS system and the design of the experiment. Section II lays out the econometric framework for our investigation. Section III presents our empirical findings and Section IV concludes.

### I. How the WPRS System Works

States are afforded a great deal of leeway in the design and implementation of their WPRS systems. In Kentucky, the Department of Employment Services contracted with the Center for Business and Economic Research (CBER) of the University of Kentucky to develop an econometric model of expected UI spell duration.

CBER estimated the profiling model using five years of UI claimant data and variables obtained from various administrative and public use data sets. The profiling model contains local economic and labor market conditions along with worker characteristics.<sup>3</sup> U.S. Department of Justice regulations prevent states from using

<sup>2</sup> We thank Joshua Angrist for bringing these citations to our attention. Campbell (1969) notes the relationship between the “tie-breaking experiment” and the regression discontinuity design. See Angrist and Alan B. Krueger (1999) and James J. Heckman et al. (1999) for discussions of the regression discontinuity design.

<sup>3</sup> See Berger et al. (1997) for a more detailed description of the model. The profiling model has moderate success in predicting claimants who will exhaust their UI benefits. Berger et al. report that selection based on the profiling model results in a treated group whose members receive 78.3 percent of their possible benefits while random assignment would result in a treated group whose members receive only 66.6 percent of their possible benefits. “Perfect” assignment based on realized spell lengths would yield a treatment group whose members receive about 93 percent of their potential benefits.

sex, age, race, ethnicity, and veteran status in their profiling models. While the econometric profiling model provides a continuous measure of the expected number of weeks of benefit receipt, CBER provides the Department of Employment Services with a discrete profile score ranging from 1 to 20. Claimants predicted by the profiling model to exhaust between 95 and 100 percent of their unemployment benefits receive a score of 20, claimants predicted to exhaust between 90 and 95 percent of their unemployment benefits receive a 19, and so on. The WPRS system was implemented in October of 1994; we make use of UI spells starting between that date and June 30, 1996.

The Kentucky WPRS system begins with claimants providing information about their employment history and characteristics while filing their claims. For claimants found to be eligible for profiling, the Kentucky DES provides CBER with data from the claimants' intake forms.<sup>4</sup> CBER then provides local Department of Employment Services' offices with the profiling scores of claimants in their area and the list of those chosen to receive reemployment services. Finally, those claimants selected to receive reemployment services are contacted through the mail to inform them of their rights and responsibilities under the program. A copy of the letter sent by the Department of Employment Services appears as Exhibit 1 on the AER web site (<http://www.aeaweb.org/aer/contents/>). In the letter, the claimant is told:

You have been indentified as a dislocated worker and selected under the UI Claimant Profiling Program to receive job search assistance services. You are obligated under the law to participate. Failure to report or participate in reemployment services without justifiable cause may result in denial of your unemployment insurance benefits.

This program is designed to provide job search assistance services to those UI claimants identified as being most likely to need assistance in finding new employ-

ment. We will assess your needs and work with you to decide which services may increase your chances of finding a good job. Services may include counseling, job search workshops, testing, job referral and placement, or if needed, referral to more intensive services, such as training.

Because of capacity constraints, local offices at some times during the year are not able to serve the entire population of claimants, making it necessary to ration entry into the program. CBER allocates program slots at each local office, serving those claimants with the highest profiling scores. In the marginal score group, where there are enough slots to serve some but not all claimants with a given score, CBER randomly assigns persons to either a treatment group required to participate in reemployment services as a condition of continued UI receipt or a control group exempt from this requirement. We call these sets of claimants "profiling tie groups," or PTGs—groups of claimants in a given office filing claims in a given week who have the marginal profiling score for that office in that week. This design differs from typical experimental evaluations of employment and training programs wherein all program applicants are randomly assigned.

Unfortunately for the sample size available for our analysis but fortunately for the claimants, the Kentucky economy was extremely strong from October 1994 to June 1996, the period for which we currently have data. As a result, local offices were often able to treat the entire claimant population. Indeed, of the 57,779 claimants in this period, 48,002 were selected for treatment, or slightly over 83 percent. Of the 2,748 potential PTGs, there are only 286 actual PTGs, ranging in size from 2 to 54. The mean size of a PTG is 6.9, with a median of 4, a 25th percentile of 3, and a 75th percentile of 8. Profiling scores within the PTGs range from 6 to 19, with the median and the mode at 16.<sup>5</sup> Combining all of the PTGs yields

<sup>5</sup> Most of the variation in the marginal profiling score among the PTGs consists of variation across local offices. A regression of the marginal profiling score on a vector of local office indicators using PTGs as the unit of observation explains 64 percent of the variation in profiling scores.

<sup>4</sup> Individuals who have a definite recall date or who are hired through a union hall are exempt from profiling.

TABLE 1—DEMOGRAPHIC CHARACTERISTICS OF TREATMENT AND CONTROL GROUPS:  
KENTUCKY WPRS EXPERIMENT, OCTOBER 1994 TO JUNE 1996

|                               | Control group        | Treatment group      | <i>p</i> -values for tests of differences in means | Population with profiling score 6 to 19, not in experiment | Treated population, not in experiment |
|-------------------------------|----------------------|----------------------|--|--|---------------------------------------|
| Age                           | 37.0<br>(10.9)       | 37.1<br>(11.1)       | 0.717  | 37.2<br>(11.3)   | 37.4<br>(11.2)                        |
| Years of schooling            | 12.3<br>(2.10)       | 12.6<br>(2.14)       | 0.221  | 12.4<br>(2.03)   | 12.4<br>(2.06)                        |
| White male                    | 0.564                | 0.518                | 0.095  | 0.519  | 0.517                                 |
| White female                  | 0.352                | 0.372                | 0.060  | 0.394  | 0.399                                 |
| Nonwhite male                 | 0.040                | 0.055                | 0.433  | 0.044  | 0.042                                 |
| Nonwhite female               | 0.044                | 0.055                | 0.691  | 0.044  | 0.042                                 |
| Earnings in year before claim | \$19,759<br>(13,677) | \$19,047<br>(13,636) | 0.666  | \$18,612<br>(13,344)                                       | \$19,171<br>(14,612)                  |
| Weekly benefit amount         | \$168.35<br>(68.90)  | \$167.36<br>(64.70)  | 0.747  | \$169.90<br>(66.01)  | \$173.26<br>(64.76)                   |
| N                             | 745                  | 1,236                | —  | 54,649   | 46,766                                |

*Notes:* Standard deviations are given in parentheses. Means are unweighted. Tests for differences in means are for the treatment and control groups and are based on a linear regression that also conditions on the 286 PTGs. The treated population consists of all claimants assigned to the profiling treatment, not just those in the PTGs. All claimants are eligible for 26 weeks of UI benefits.

*Source:* Authors' calculations are from the Kentucky WPRS Experiment.

a treatment group of 1,236 claimants and a control group of 745 claimants. Thus, the experimental design uses only about 6 percent of the treated population and 7.6 percent of the untreated population. Table 1 compares the characteristics of the treatment and control groups as well as the populations of treated claimants who are not randomly assigned and of other claimants with profiling scores between 6 and 19. Based on these characteristics all of the groups look very similar.

Figure 1 provides a time line for the typical claimant, although there is considerable heterogeneity among claimants in the timing of these events. Unemployment insurance checks are usually sent fortnightly in Kentucky. The first check is received in week two of the spell. The letter in Exhibit 1 is typically received after the first check but before the second—that is, in week three or four. Claimants need to contact the UI office in week three or four to verify their

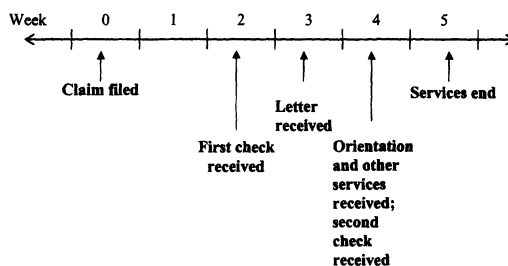


FIGURE 1. TIMELINE FOR TYPICAL UI CLAIMANT IN KENTUCKY WPRS PROGRAM

continuing eligibility in order to receive the second check. Thus, if the letters, and the mandatory reemployment services they imply, are to have a deterrent effect, we would expect to observe it between weeks two and four. Within ten working days following notification of the program, claimants selected for treatment report to a local office for an orientation where they learn about the program and complete a questionnaire. Using this information, Employment Services staff assesses the claimants and then

There are, however, at least two different offices for each profiling score among the PTGs.

refers them to specific services, such as assisted job search, employment counseling, job search workshops, and retraining programs.

Among those claimants who attended the orientation, 76.7 percent were referred to less expensive job search and job preparation activities. These less expensive services are also less intensive, typically consuming from four to six hours of claimant time. In contrast, only 13.8 percent were referred to (relatively) more expensive, and intensive, education and training programs.<sup>6</sup> The average number of services received following orientation was 1.02. Conditional on completing at least one service, the average number of additional services received was 2.10. Of those referred to services, only 61.3 percent completed at least one. Another 5.7 percent started at least one service but returned to employment before completing any. Overall, 31.8 percent of those referred received no services because they had returned to employment, chose not to claim benefits, or were exempted because their previous employer provided similar services.<sup>7</sup>

## II. Estimation

In this section, we present experimental estimates of the mean impact of treatment for claimants in a PTG. In particular, we estimate versions of:

$$(1) \quad y_{ij} = \mu_j + \beta^*T_{ij} + v_{ij},$$

where  $y_{ij}$  is the outcome for the  $i$ th individual in the  $j$ th PTG,  $T_{ij}$  is a binary indicator for whether or not the  $i$ th individual in the  $j$ th PTG received treatment,  $\mu_j$  is a vector of PTG fixed effects to control for differences in expected earnings in the absence of treatment across PTGs, and  $v_{ij}$  is

a random disturbance term.  $\beta^*$  and  $\mu_j$  are parameters to be estimated.

Conditioning on the PTG fixed effects has two important consequences for the estimates. First, because the proportion of claimants in the treatment group varies among PTGs, failure to include PTGs fixed effects would bias the estimated impacts if expected earnings in the absence of treatment vary among PTGs in a way that is correlated with the random assignment ratio. Second, because each PTG consists of individuals with a specific profiling score at a particular location on a particular week, including the  $\mu_j$  implicitly conditions on the profiling score, location, and time period. Conditioning on these factors substantially reduces the residual variation in these data and thereby increases the precision of our estimated treatment effects.

When the impact of treatment is the same for each PTG, unweighted estimation of equation (1) provides efficient estimates of the impact of treatment for claimants in a PTG. When the impact of treatment varies across PTGs, however, the situation is more complex. Let  $\hat{\Delta}_j = \bar{Y}_{1j} - \bar{Y}_{0j}$  denote the estimated impact for the  $j$ th PTG. Following Angrist (1998), it can be shown that the ordinary least-squares (OLS) estimate of  $\beta^*$  from unweighted estimation of equation (1) is:

$$(2) \quad \beta^* = \sum_j w_j \hat{\Delta}_j$$

where  $w_j = \frac{r_j(1-r_j)N_j}{\sum_{k=1}^{286} r_k(1-r_k)N_k}$ ,  $N_j$  is the

number of claimants in the  $j$ th PTG, and  $r_j$  is the probability that a member of  $j$ th PTG receives treatment. In this case, estimating equation (1) produces a weighted average of the PTG-specific treatment effects, where the weights correspond to the conditional variance of treatment in each PTG.

Two features of the implicit weights on the  $\hat{\Delta}_j$  are of interest in this context. First, for a given random assignment ratio within a PTG, increases in the number of claimants in the PTG increases the implicit weight on that PTG in the estimation of  $\beta^*$ . Second, for a given size of PTG the weight is larger the closer the random assignment ratio is to 0.5. As Angrist (1998)

<sup>6</sup> Individuals could be referred to more than one service and some persons were referred to miscellaneous other services. See Noel (1998) for a detailed description of the available services.

<sup>7</sup> The fraction exempted due to receiving similar services from their previous employer is not precisely known, but program staff indicate that it is small. The remaining 1.2 percent was referred in error or had incomplete data on service completion.

emphasizes, this weighting tends to reduce the variance of the estimates because  $\hat{\Delta}_j$  is more precisely estimated when  $N_j$  is larger and the assignment rate  $r_j$  is closer to 0.5. If the assignment rate is the same for all PTGs (whether or not it equals 0.5) as in a classic experimental design, then the weighting is simply based on the size of the PTG.

To examine the importance of this issue, we present estimates based on an alternate weighting scheme that provides consistent estimates of the impact of treatment for claimants in a PTG even if the impact of treatment varies by PTG. This estimator treats each PTG as a separate experiment. It consists of a weighted average of the mean differences in outcomes between the treatment group and control group members in each PTG, with the weights proportional to the number of treated individuals ( $r_j N_j$ ) in each PTG. We refer to this estimator as the “matching” estimator, because it has the same structure as a nonexperimental cell matching estimator, with the crucial difference that in this case we know that the conditional independence assumption that justifies the matching holds because of the random assignment within each PTG. In a world where the impacts do not vary among PTGs, the matching estimator remains consistent, but is inefficient relative to estimating equation (1) by OLS.

### III. Empirical Analyses

#### A. Aggregate Estimates

We focus on four outcomes of interest: the number of weeks that a claimant receives benefits, the amount of benefits that the claimant receives, the fraction of claimants exhausting benefits, and the claimant’s earnings in the quarters following initiation of the UI claim. All data elements are taken from administrative records of the Kentucky Department of Employment Services.

The measure of earnings after the start of the unemployment insurance claim is less than ideal for three reasons.<sup>8</sup> First, because UI

records are only for the Commonwealth of Kentucky, no earnings are recorded for claimants who crossed state lines to begin employment. This is likely to be particularly problematic in the urban areas of Kentucky. Of the seven Metropolitan Statistical Areas in Kentucky, only Lexington is not located on the border of an adjoining state. Thus, if the WPRS treatment affects the probability of taking a job outside Kentucky, this will bias our results.

Second, earnings are only observed for claimants who work in jobs covered by the UI system.<sup>9</sup> Third, UI records do not include any “informal” activities. To the extent that claimants work “off the books,” the UI records understate total earnings. If the treatment increases participation in the formal labor market and reduces participation in the informal labor market, then our measure of earnings will tend to overstate the earnings impact of treatment. These problems are standard in all analyses that use earnings variables constructed from UI records.

Table 2 presents the basic impact estimates. In column (1) we report the results from estimating the fixed-effects regression in equation (1) above. We find that the treatment group collects payments for about 2.2 fewer weeks than the control group. The treatment group receives about \$143 less in benefits than the control group, but this difference is statistically significant only at the 10-percent level.<sup>10</sup> We

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Bloom (1999) for a comparison of UI data and survey data in an evaluation context.

<sup>9</sup> Certain wages and salaries are exempt from the UI system, although the U.S. Department of Commerce (1994) estimates that about 98 percent of wages and salaries are included in the system for all but eight industries. The industries that are not well covered are railroads, farms, farm contractors, private household, private elementary and secondary educational institutions, religious organizations, the military, and “other,” which is comprised of U.S. citizens working for exempt international agencies and foreign consulates and embassies. Combined, these industries appear to account for less than 3 percent of wage and salary earnings. Thus, the coverage rate appears to be in excess of 95 percent of wage and salary earnings.

<sup>10</sup> The reductions in weeks paid and amount of benefits paid, however, give conflicting estimates of the magnitude of the treatment effect. The mean weekly benefit payment is approximately \$168, which suggests that a 2.2-week reduction in weeks paid should reduce the amount paid by about

<sup>8</sup> See V. Joseph Hotz and Karl Scholz (2000) for a general discussion of the advantages and disadvantages of administrative data and Robert Kornfeld and Howard

TABLE 2—IMPACT OF TREATMENT ON DURATION OF BENEFITS AND EARNINGS: KENTUCKY WPRS EXPERIMENT, OCTOBER 1994 TO JUNE 1996

| Outcome measures  | (1)<br>Fixed-effect<br>regression<br>estimates | (2)<br>Matching<br>estimates   |
|---|--|--------------------------------|
| Number of weeks receiving<br>UI benefits                | -2.241<br>(0.509)<br>[0.000]                   | -2.045<br>(0.411)<br>[0.000]   |
| UI benefits received                                    | -143.18<br>(100.3)<br>[0.077]                  | -81.44<br>(81.6)<br>[0.159]    |
| Fraction exhausting benefits                            | -0.024<br>(0.023)<br>[0.152]                   | -0.030<br>(0.0019)<br>[0.0055] |
| Earnings in the year after<br>the start of the UI claim | 1,054.32<br>(588.0)<br>[0.037]                 | 1,599.99<br>(475.2)<br>[0.001] |
| N   | 1,981  | 1,981                          |

*Notes:* Each of the regressions controls for the Profiling Tie Group (PTG) of the recipients. There are 745 claimants in the control group, 1,236 claimants in the treatment group, and 286 PTGs. Standard errors are in parentheses and *p*-values from one-tailed tests are in brackets. The “Fixed-effect regression” estimates result from OLS estimation of equation (1) in the text. The “Matching” estimates represent weighted averages of the mean differences in treatment and control group outcomes within each PTG, with the weight for each PTG proportional to the number of treatment group members it contains.

*Source:* Authors’ calculations are from the Kentucky WPRS Experiment.

estimate that 2.4-percent fewer claimants in the treatment group exhaust their benefits, but this difference is statistically insignificant. Finally, the treatment group earned, on average, \$1,054 more than the control group in the year follow-

\$370. In contrast, a savings of \$143 suggests a reduction of only 0.85 in weeks paid. This latter estimate is similar to estimates from other programs in the existing literature; see Bruce D. Meyer (1995). We examine this discrepancy in detail in the Appendix to Black et al. (2002). We find evidence of more repeat UI spells in the treatment group. Our evidence suggests that for some of these repeat spells, the benefits paid variable was updated in the administrative records to reflect the second spell but the weeks paid variable was not. As a result, the analysis suggests that the weeks paid impact estimates in Table 2 may have a modest upward bias.

ing initiation of the UI claim.<sup>11</sup> Thus, in terms of mean impacts, the WPRS treatment shortens the duration of UI claims, reduces benefits paid, and raises earnings.<sup>12</sup> Column (2) presents the matching estimates described at the end of Section II. They tell the same substantive story as the estimates in column (1), with slightly smaller estimated impacts on the number of weeks of benefits received and the dollar value of benefits received, but larger estimates of the impact of treatment on the fraction exhausting benefits and earnings in the year after the start of the claim.

As discussed in Section II, our unique data directly identify only the impact of treatment for claimants in a PTG. Given the similarity in characteristics we show in Table 1 among claimants in PTGs, all claimants with profiling scores between 6 and 19, inclusive, and all claimants receiving the WPRS treatment, our estimates may generalize to these larger populations, and so also provide guidance on broader policy questions regarding retaining or scrapping the entire WPRS system.<sup>13</sup>

### B. Putting the Aggregate Estimates in Perspective

Economists have studied several policies that modify the U.S. unemployment insurance system by reducing the incentives for excess benefit receipt while at the same time not punishing claimants for whom a longer search is optimal. The reemployment bonus experiments surveyed in Meyer (1995) tested one such policy. In these studies, claimants who find a job quickly and

<sup>11</sup> We wondered if the impact of treatment might diminish over time as later cohorts of claimants learned about the modest time commitment that the program usually requires. We found, however, no systematic pattern over time.

<sup>12</sup> Interestingly, Katherine P. Dickinson et al. (1997) evaluate the WPRS using nonexperimental methods for three states: Delaware, Kentucky, and New Jersey. For Kentucky, they find that the program reduced weeks of benefit receipt by 0.72, reduced benefits paid by \$96, and had no impact on earnings. Overall, their estimates suggest that simple nonexperimental estimators have trouble replicating the experimental impact estimates, which is consistent with the usual findings in the literature.

<sup>13</sup> Black et al. (2002) provide additional evidence on this point.



keep it receive a cash payment.<sup>14</sup> These experiments indicate that the unemployment spells of UI claimants can be shortened without loss of post-program earnings. Though reemployment bonuses reduce the length of UI spells, many of the claimants who receive bonuses would have exited quickly without them. Moreover, Meyer argues that the permanent adoption of reemployment bonuses would substantially increase the UI take-up rate as eligible persons who expect short spells and who do not at present file for benefits would do so in order to collect the bonus. This response would further increase the cost of the program without increasing its benefits.<sup>15</sup>

While the UI bonus schemes represent a “carrot” designed to lure claimants back into employment, other experiments used “sticks,” such as greater enforcement of UI job search requirements, to push claimants who could find work back into employment by raising the costs of staying on UI. Orley Ashenfelter et al. (1999) present experimental evidence on small “stick” programs in four states. These programs include detailed eligibility reviews at the start of the claim, more information about the work search requirement and, for a random subsample of the treatment groups, random work search verification very early in the spell. The findings suggest that the treatment, particularly the eligibility reviews, had a small but noticeable effect on qualification rates (see the top of their Table 6). Meyer (1995) reviews other experiments that examined programs that combined stricter enforcement with job search assistance. These programs had stronger effects and passed standard cost-benefit tests. Such “stick” policies have the potential to shorten UI spells without causing the increases in the take-up rate generated by reemployment bonuses.

To compare our estimated impacts of the WPRS program with those of other programs, consider the estimates from the Illinois UI bo-

nus experiment that Woodbury and Spiegelman (1987) present. They estimate that a \$500 bonus to UI claimants who found a job within 11 weeks resulted in a reduction in the duration of UI spells of about 1.1 weeks. The earnings of those offered a bonus were comparable to the earnings of those not offered a bonus. Thus, relative to the Illinois bonus experiment, the Kentucky WPRS appears to have had a substantial impact on claimants. This may reflect the fact that claimants have until week 11 to find alternative employment under the Illinois bonus, but to avoid reemployment services under WPRS claimants must find a job within the first few weeks of their unemployment spell. The WPRS program has the further advantage that it is unlikely to increase the UI take-up rate.

The WPRS impacts reported here also tend to be larger than those from experimental evaluations of job search assistance programs for UI claimants summarized in Meyer (1995).<sup>16</sup> Most of these programs (see his Tables 5A and 5B) have estimated impacts equal to or less than one week of benefit receipt. Decker et al. (2000) analyze the recent Job Search Assistance (JSA) experiment, which used profiling to assign workers to job search assistance in Washington, DC and Florida. They find that structured job search assistance in Washington lowered the number of weeks receiving benefits by 1.13 weeks and reduced payments by \$182, while the impacts in Florida were -0.41 weeks and \$39, respectively. The larger impacts we find here are consistent with the somewhat more intensive employment and training services being offered, which presumably raise the cost of continued UI receipt for those who do not value them and raise the benefits of service receipt for those who do.

Finally, we consider the costs and benefits of the profiling program from the point of view of the UI system. Our estimates from Table 2 indicate that treated claimants receive, on average, \$143 less in benefits than untreated claimants. We can compare these average benefits with the average cost per treated claimant

<sup>14</sup> See Stephen Woodbury and Robert Spiegelman (1987), Patricia Anderson (1992), Paul T. Decker (1994), and Decker and Christopher J. O’Leary (1995) for analyses of the individual bonus experiments.

<sup>15</sup> O’Leary et al. (2002) propose using profiling to allocate eligibility for reemployment bonuses to avoid these problems.

<sup>16</sup> See Walter Corson et al. (1985), Anderson et al. (1991) and Terry R. Johnson and Daniel H. Klepinger (1994) for analyses of the individual job search experiments.

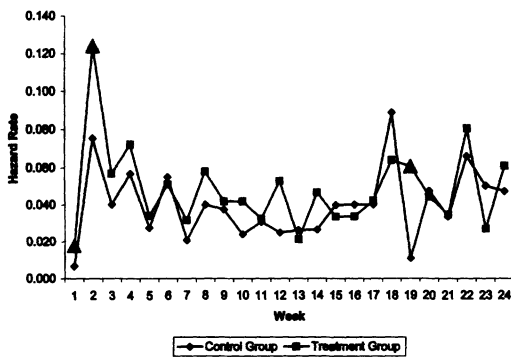


FIGURE 2. HAZARD FUNCTIONS OF THE TREATMENT AND CONTROL GROUPS, KENTUCKY WPRS EXPERIMENT, OCTOBER 1994 TO JUNE 1996

Note: Triangles denote significant differences at the 5-percent level.

Sources: Authors' calculations are from Kentucky WPRS Experiment. The parameter estimates used to construct the graph appear in Table B1 of Black et al. (1999).

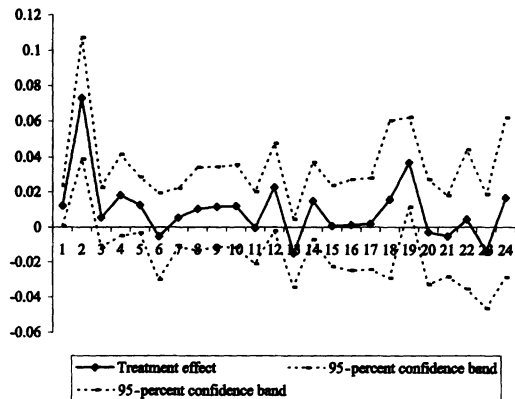


FIGURE 3. IMPACT OF TREATMENT ON PROBABILITY OF EXITING UI PROGRAM, KENTUCKY WPRS EXPERIMENT, OCTOBER 1994 TO JUNE 1996

Sources: Authors' calculations are from Kentucky WPRS Experiment. The parameter estimates used to construct the graph appear in Table B1 of Black et al. (1999).

in the Kentucky UI system. To construct the average cost per treated claimant, we use data on the average hours spent per week on profiling in each of the 28 local offices and the state UI office, the average compensation per hour for employees of the Kentucky Department for Employment Services, the annual cost of the contract with CBER at the University of Kentucky to maintain the profiling model and data system, and the number of treated claimants in the first 86 weeks of profiling.<sup>17</sup> These costs sum to \$11.93 per treated claimant. Even if one adds approximately \$0.5 million in startup costs and initial model development and spreads them over the treated claimants from the first 86 weeks of profiling, the costs are still only \$22.35 per recipient. Thus, the profiling system appears to save the UI program a substantial amount of money.<sup>18</sup>

<sup>17</sup> These data were provided by Ted Pilcher of the Kentucky DES.

<sup>18</sup> The costs included here do include short-term training provided by UI staff but do not include the cost of long-term training referrals to outside providers. A full cost-benefit analysis would include these additional costs. A cost-benefit analysis from the standpoint of society (rather than of the UI system) would also include the increased earnings of the treated claimants and some measure of the value of their forgone leisure.

### C. The Effect of Treatment Over Time

Figure 2 displays hazard rates for leaving UI for the experimental treatment and control groups, and Figure 3 displays the difference between the two with the corresponding 95-percent confidence bands. They document a large impact of treatment after claimants receive the letter notifying them of their obligation to receive reemployment services. About 13 percent of the treatment group exits after the first two weeks but only about 4 percent of the control group exits. Subsequently, the hazard rate of the treatment group is almost always higher than that of the control group, although the difference is statistically significant only a couple of times. We may use these estimates to calculate the survivor function. The maximum difference between the treatment and control group survivor functions is 0.11, which is achieved in week 12. The difference after just two weeks is 0.083 or about 75 percent of the maximum difference.<sup>19</sup>

<sup>19</sup> Parameter estimates are presented in Appendix Table B1 of Black et al. (1999). Most benefits are paid biweekly (every other week). Technically, these data are not true hazards because we do not observe whether the weeks of benefit receipt are consecutive. Rather, they represent counts of the number of weeks within the benefit year that

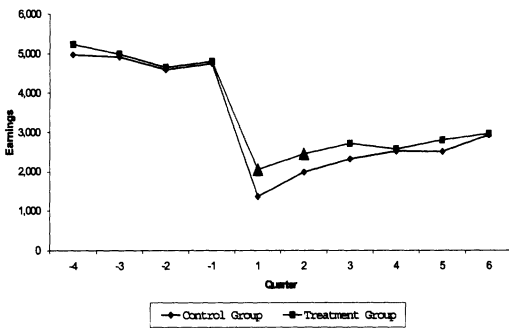


FIGURE 4. EARNINGS OF THE TREATMENT AND CONTROL GROUPS, KENTUCKY WPRS EXPERIMENT, OCTOBER 1994 TO JUNE 1996

Note: Triangles denote significant differences at the 5-percent level.

Sources: Authors' calculations are from Kentucky WPRS Experiment. The parameter estimates used to construct the graph appear in Table B2 of Black et al. (1999).

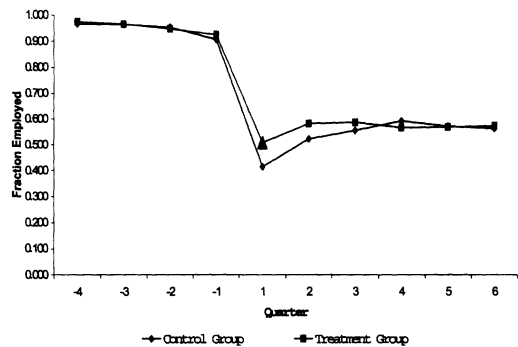


FIGURE 6. EMPLOYMENT OF THE TREATMENT AND CONTROL GROUPS, KENTUCKY WPRS EXPERIMENT, OCTOBER 1994 TO JUNE 1996

Note: Triangles denote significant differences at the 5-percent level.

Sources: Authors' calculations are from Kentucky WPRS Experiment. The parameter estimates used to construct the graph appear in Table B2 of Black et al. (1999).

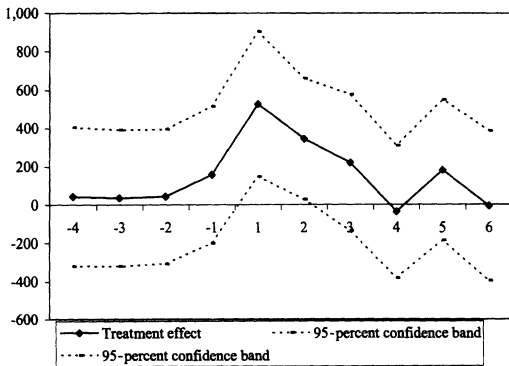


FIGURE 5. IMPACT OF TREATMENT ON EARNINGS, KENTUCKY WPRS EXPERIMENT, OCTOBER 1994 TO JUNE 1996

Sources: Authors' calculations are from Kentucky WPRS Experiment. The parameter estimates used to construct the graph appear in Table B2 of Black et al. (1999).

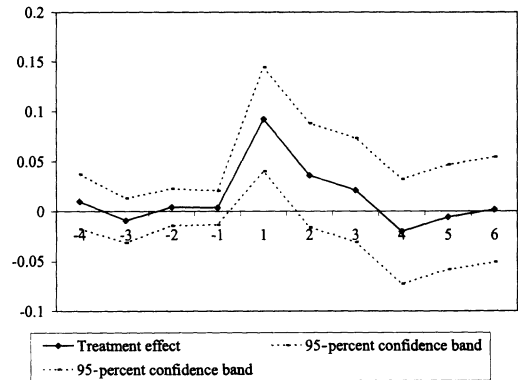


FIGURE 7. IMPACT OF TREATMENT EMPLOYMENT PROBABILITIES, KENTUCKY WPRS EXPERIMENT, OCTOBER 1994 TO JUNE 1996

Sources: Authors' calculations are from Kentucky WPRS Experiment. The parameter estimates used to construct the graph appear in Table B2 of Black et al. (1999).

The exit hazard in the treatment group continues to lie above that for the control group for most of the eligibility period. This could result from a positive impact of employment and training services on those who receive them.

This explanation is consistent with the evidence of modest but detectable impacts in the AFDC work/welfare experiments documented in Judith Gueron and Edward Pauly (1991). Alternatively, it is possible that persons with low hazard rates in the treatment group exit UI in the first few weeks at a higher rate than similar persons in the control group.

In Figures 4 and 6, we graph mean earnings

a claimant receives payments. Over 80 percent of claimants in PTGs, of treated claimants, and of all claimants had either no interruption or one of two weeks or less.

and employment by quarter after the start of the UI spell for the treatment and control groups, and in Figures 5 and 7, we graph the differences between the treatment and control groups along with 95-percent confidence bands. The earnings estimates illustrate the impact of early exit from unemployment in the treatment group. In the first quarter, treatment group members average \$525 more in earnings than control group members, indicating that about half of the earnings gain occurs in the first quarter. In the second quarter the earnings impact is about \$344. By the third quarter, the difference, while positive, is no longer statistically significant, and for subsequent quarters there is virtually no difference in mean earnings. The impact of treatment on employment—where employment is defined as positive earnings during a quarter—indicates a substantial increase in the probability of employment in the first quarter, a modest increase in the second quarter and little effect after that. Only the first quarter effect is statistically significant.

Experimental evaluations of mandatory job search assistance in other contexts report similar results. Corson and Decker's (1989) analysis of the New Jersey search experiment and Johnson and Klepinger's (1994) analysis of the Washington search experiment both find evidence of early return to work. Decker et al.'s (2000) analysis of JSA experiments in Washington, DC and Florida also find sharp increases in the hazard rate in the second and third weeks of the JSA program. Peter Dolton and Donald O'Neill's (1996) experimental examination of the Restart component of Britain's UI system parallels our findings on a different dimension. After receiving benefits for six consecutive months, the Restart program requires recipients to participate in an interview with a caseworker. Dolton and O'Neill (1996) document a sharp spike in the hazard rate of the treatment group relative to the control group when claimants receive notice of the interview. Johnson and Klepinger (1991, Table 4) find a similar spike in the UI exit hazard in response to a letter notifying the recipient of an eligibility review interview in the Washington Alternative Work Search Experiment.

Our evidence that WPRS reduces moral hazard in the UI system by acting as a "leisure tax"

on some claimants comports with the findings in the literature that UI reduces the incentive to find a job quickly. For example, Meyer (1990) documents spikes in the hazard function as workers approach the exhaustion of their UI benefits, and David Card and Phillip B. Levine (2000) document that increasing the length of time that claimants may receive benefits causes the hazard function to fall substantially.<sup>20</sup> Looking at search behavior directly, John M. Barron and Wesley Mellow (1979) find that those workers receiving UI searched 1.6 fewer hours per week than unemployed workers not receiving payments. Robert D. St. Louis et al. (1986) offer evidence that claimants systematically violate the search requirements that UI imposes.

Our results are consistent with the idea that the WPRS system lowers the worker's reservation wage and increases search intensity early in the unemployment spell. A faster return to employment implies worse matches on average in the treatment group. This in turn implies that we should observe treatment group members having more interrupted spells of unemployment as more of their matches fail to result in stable employment. To test this prediction, we estimated a linear probability model based on equation (1) with an indicator for the presence of an interrupted spell as the dependent variable. The results indicate that the treatment group had a 0.06 higher probability of having an interrupted spell than the control group (with a *p*-value of 0.003), which corresponds to about a 36-percent increase in the number of interrupted spells.<sup>21</sup> The absence of significant earnings impacts in quarters three through six after the start of the claim, however,

<sup>20</sup> See also Ronald Ehrenberg and Ronald Oaxaca (1976), Robert Moffitt (1985), Lawrence Katz and Meyer (1990), and many others.

<sup>21</sup> If the WPRS program lowers claimants' reservation wages early in their unemployment spells, then treatment group members who exit early should have lower earnings than control group members who exit early. To test this, we interacted the treatment indicator with an indicator for whether or not the claimant exits early—that is, within four weeks of the start of the UI claim. We find strong evidence of lower earnings among treatment group members exiting early compared to control group members who do so. See Black et al. (1999) for these estimates.

TABLE 3—ESTIMATES OF THE IMPACT OF TREATMENT ON THE TREATED  
BY PROFILING SCORE CATEGORY:  
KENTUCKY WPRS EXPERIMENT, OCTOBER 1994 TO JUNE 1996

|  | Weeks<br>paid                | Amount<br>paid                   | Fraction<br>exhausting<br>benefits | Annual<br>earnings                   |
|--|------------------------------|----------------------------------|------------------------------------|--------------------------------------|
| Profiling score between 6 and 13   | -2.238<br>(0.913)<br>[0.007] | -\$270.08<br>(179.74)<br>[0.067] | -0.055<br>(0.041)<br>[0.090]       | \$939.51<br>(1052.05)<br>[0.186]     |
| Profiling score between 14 and 15  | -1.891<br>(1.050)<br>[0.036] | -\$14.42<br>(206.77)<br>[0.472]  | 0.030<br>(0.0049)<br>[0.771]       | -\$1,257.14<br>(1,210.25)<br>[0.851] |
| Profiling score of 16  | -3.057<br>(1.102)<br>[0.003] | -\$465.73<br>(216.94)<br>[0.016] | -0.095<br>(0.051)<br>[0.032]       | \$4,175.83<br>(1,269.76)<br>[0.001]  |
| Profiling score between 17 and 19  | -1.861<br>(1.039)<br>[0.037] | \$182.09<br>(204.60)<br>[0.813]  | 0.027<br>(0.047)<br>[0.784]        | \$689.71<br>(1,197.55)<br>[0.283]    |
| <i>p</i> -value for test of equal impacts<br>across approximate profiling<br>score quartiles | 0.851                        | 0.132                            | 0.174                              | 0.021                                |

*Notes:* Each of the regressions controls for the Profiling Tie Group (PTG) of the recipients. There are 745 claimants in the control group, 1,236 claimants in the treatment group and 286 PTGs. The approximate quartiles for the profiling scores are scores 6 to 13 (515 members), scores 14 and 15 (390 members), score 16 (424 members), and scores 17 to 19 (652 members).

*Source:* Authors' calculations are from the Kentucky WPRS Experiment.

indicates that there is no long-term harm from the treatment.

In sum, we have strong evidence that the earnings gains we document result from more early exits from UI in the treatment group. Most of these exits take place prior to the receipt of reemployment services. Earnings are significantly higher in the first and second quarters after claimants' file their claims. We find no evidence that claimants ever suffer reduced earnings through the first six quarters after their claims. This evidence suggests that the "leisure tax" implicit in the WPRS treatment represents an effective tool for reducing the moral hazard in the UI program.

#### D. Evaluating Profiling as an Allocation Mechanism

In addition to evaluating the impact of the profiling treatment on those assigned to it, we also briefly consider a different evaluation question: How well does the profiling mechanism

allocate the treatment?<sup>22</sup> If the goal of profiling is to increase the efficiency of treatment allocation then, assuming that the costs of the treatment do not vary across persons, it should allocate the treatment to those for whom it has the largest impact. To address this question, we examine impact estimates for profiling score subgroups. If the profiling mechanism enhances the efficiency of treatment allocation, then we should find larger impacts for claimants with higher scores, as claimants with higher scores are much more likely to get treated.

Table 3 presents impact estimates for our four outcome variables, with the claimants divided into four subgroups based on their profiling scores: 6–13 (about 26 percent of the treatment group), 14 or 15 (about 20 percent of the treatment group), 16 (about 21 percent of the treatment group) and 17 to 19 (about 33 percent of

<sup>22</sup> See Berger et al. (2000) for an extended discussion of these issues.

the treatment group). The results suggest that the impact varies nonlinearly with the profiling score, but we can reject the null of equal impacts across profiling score subgroups only for earnings.

The assumption underlying the WPRS is that those with the longest expected UI spells benefit the most from the profiling treatment. The estimates in Table 3 provide little justification for this assumption, as there does not appear to be a monotonic relationship between the profiling score and the impact of treatment. Thus, the evidence in Table 3 calls into question the wisdom of using expected UI spell duration (rather than, say, predicted impacts) as a means of allocating treatment.

#### IV. Conclusion

We use unique data to examine the impact of the Worker Profiling and Reemployment Services (WPRS) initiative. Our experimental data are for persons in marginal profiling groups—that is, persons whose expected UI spells are just long enough to put them in the group required to receive reemployment services in return for continued receipt of benefits. This design, which Thistlethwaite and Campbell (1960) call a tie-breaking experiment, allows the introduction of random assignment without disrupting the program and without denying services to those most in need. In so doing, it may reduce resistance to random assignment by line workers and program administrators (and politicians) and also reduce the negative publicity sometimes associated with random assignment in the social services.

For claimants in the profiling tie groups, we find that random assignment to the WPRS treatment results in a 2.2-week reduction in benefit receipt relative to the control group. This represents a reduction in mean benefits payments of slightly over \$143 per recipient. In addition, the experimental treatment group had significantly higher earnings in the year after the start of their UI claim. This earnings difference arises almost entirely from higher earnings in the first two quarters after the start of the claim. This suggests that earnings gains are due primarily to the earlier return to work of some treatment group members rather than due to

higher wages conditional on employment. We find no evidence that the earnings of the treatment group are lower through the first six quarters after the start of the unemployment spell, suggesting that there is no long-term harm from the treatment provided by the program.

The reduction in the length of reciprocity in the treatment group is largely accomplished by early exits from UI. Many of these early exits coincide in time with the letters sent out to treatment group members to notify them of their obligations under the program. These findings suggest that the gains from the program result in large part from removing claimants from the UI rolls who were job ready and had little trouble locating employment. Hence, the WPRS treatment appears to be successful at reducing the moral hazard associated with the UI program. Moreover, from the perspective of the UI system, and likely from that of society as well, it produces a wide excess of benefits over costs.

Finally, the underlying assumption of the WPRS program is that those with the longest expected UI spell durations would benefit the most from the requirement that they participate in reemployment services in order to continue receiving their UI benefits. It is also assumed that treating these claimants will result in the largest budgetary savings for state UI systems. Our results provide little justification for either assumption, as we do not find a monotone relationship between the profiling score and the impact of treatment. If the goal of profiling is to allocate the treatment to those claimants with the largest expected impact from it, or to save the state UI system the most money, then our findings call into question the wisdom of using the expected benefit duration as a means of allocating treatment. They also suggest the value of further thought and study before extending profiling to other programs.

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