Is the Threat of Training More Effective Than Training Itself? Experimental Evidence from the UI System*

by

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Is the Threat of Training More Effective Than Training Itself?
Experimental Evidence from the UI System

Abstract

This paper examines the effect of the Worker Profiling and Reemployment Services (WPRS) system. This program "profiles" UI claimants to determine their probability of benefit exhaustion (or expected spell duration) and then provides mandatory employment and training services to claimants with high predicted probabilities (or long expected spells). Using a unique experimental design, we estimate that the WPRS 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. Much (but not all) of the effect results from a sharp increase in early exits from UI in the experimental treatment group compared to the experimental control group. These exits coincide with claimants finding out about their mandatory program obligations rather than with actual receipt of employment and training services. While the program targets those with the highest expected durations of UI benefit receipt, we find no evidence that these claimants benefit disproportionately from the program. In addition, we find strong evidence against the "common effect" assumption, as the estimated treatment effect differs dramatically across quantiles of the untreated outcome distribution. Overall, the profiling program appears to successfully reduce the moral hazard associated with the UI program without increasing the take-up rate.

1 Introduction

This paper examines the behavioral effects of a new program that "profiles" Unemployment Insurance (UI) claimants using econometric models of expected spell duration or of the probability of benefit exhaustion. 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.[1] We consider the effects of this program on claimant behavior using unique experimental data from Kentucky.

It is well known that the UI system provides incentives for workers to lengthen their spells of unemployment by providing a subsidy to job search. A large empirical literature provides evidence of behavior consistent with these incentives. For example, Meyer (1990) documents spikes in the empirical hazard function as workers approach the exhaustion of their UI benefits, and Card and Levine (1998) and Noel (1998) document that increasing the length of time that claimants may receive benefits causes the empirical hazard function to fall substantially.[2] Looking at search behavior directly, Barron and Mellow (1979) find that those workers receiving UI searched 1.6 fewer hours per week than unemployed workers not receiving payments. St. Louis, Burgess, and Kingston (1986) offer compelling evidence that claimants systematically violate the search requirements that UI imposes. This evidence

[1] See U.S. Department of Labor (1999) for a more detailed description of the program and of how it varies across states.

[2] See also Ehrenberg and Oaxaca (1976), Moffitt (1985), Katz and Meyer (1990) and many others.

has led to a search for policies that would modify the system by reducing the incentives for excess benefit receipt while at the same time not punishing workers for whom a longer search is optimal.

The recent reemployment bonus experiments surveyed in Meyer (1995) tested one such policy. In these studies, claimants who find a job quickly and keep it receive a cash payment.[3] These experiments indicate that the unemployment spells of UI claimants can be shortened without loss of post-program earnings.[4] Though reemployment bonuses reduce the length of UI spells, they prove expensive because many claimants receive bonuses who would have exited quickly without them. Moreover, Meyer argues convincingly that 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.[5]

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. Ashenfelter, Ashmore and Deschênes (1999) present experimental evidence on work search

[3] In the Illinois experiment, for a subset of the treatment group it was the employer rather than the employee who received the bonus. This form of the bonus had a much lower utilization rate and was omitted from subsequent experiments.

[4] See Anderson (1992), Decker (1994), Decker and O'Leary (1995) and Woodbury and Spiegelman (1987) for analyses of the individual bonus experiments.

[5] O'Leary, Decker and Wandner (1998) propose using profiling to allocate eligibility for reemployment bonuses to get around the problem of increases in the take-up rate and of subsidizing current claimants who would have short spells even without the bonuses.

enforcement programs in four states that suggests at most a small deterrent effect. 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.

The profiling program we examine in this paper combines aspects of both types of UI reforms. For some claimants, the services they must receive may represent a "stick" that raises the cost of staying on UI and thereby induces early exit. In essence, the services operate as a leisure tax for these claimants. For others, the services may represent a "carrot" that augments their human capital and job search skills.

In this paper, we make five main points. First, we use a unique experimental design. The randomization in our experiment occurs only to satisfy capacity constraints. In Kentucky, UI claimants are assigned "profiling scores" that take on integer values from one to 20, with higher scores indicating claimants with longer expected spell 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 requirement. Thus, if there are 10 claimants with a profiling score of 11 but only seven remaining slots, seven claimants are randomly assigned to the treatment group and three are assigned to the control group. Campbell (1969) terms this experimental design a "tie-breaking experiment." It was apparently first advocated by Thistlethwaite and Campbell (1960) as a means of evaluating the impact of receiving a college scholarship.[6] To our knowledge, our experiment is the first to use this design. In general, the "tie-breaking experiment" does not directly identify many commonly estimated parameters, such as the impact of treatment on the treated. Instead, further assumptions are required. We discuss these assumptions in Section 4.

Second, using our unique experimental data, we evaluate the WPRS system for persons at the profiling score margin in the prototype program in Kentucky. 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 over $1,000.

Third, we argue that the dynamics of the treatment effect provide important evidence about *how* the program works. The experimental treatment group has significantly higher earnings and employment in the first and second 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 earnings gains result primarily from earlier return to work in the treatment group. Thus, the program acts to remove job-ready claimants who have little trouble finding employment from UI, thereby reducing the extent of moral hazard in the UI program.

Fourth, using the framework in Heckman, Smith, and Clements (1997) we estimate the distribution of impacts from the WPRS treatment. We find strong evidence against the "common effect" assumption in our data, as the

[6] 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 Heckman, LaLonde and Smith (1999) and Angrist and Krueger (1999) for discussions of the regression discontinuity design.

estimated impact of treatment varies widely across quantiles of the untreated outcome distribution. The pattern of impacts suggests that the treatment has its largest effect on persons whose spells without treatment would be of moderate duration.

Finally, we evaluate the use of profiling scores based on expected UI duration as a method 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 experimentally estimated impact of treatment and the profiling score. This suggests that profiling in this way 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.[7]

The remainder of the paper proceeds as follows: In the next section, we describe the Kentucky WPRS system and the design of the experiment. Section 3 presents a theoretical framework for our investigation. Section 4 discusses the parameters of interest in evaluating the WPRS program and indicates what assumptions are required to obtain estimates of these parameters using our data. The fifth section analyzes the experimental data and the final section concludes.

[7] See Berger, Black, and Smith (1999) for a more detailed discussion of this issue.

2 How the WPRS System Works

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

Under their contract from DES, 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.[8] US Department of Justice regulations prevent states from using sex, age, ethnicity, and veteran status in their profiling models. While the econometric profiling model provides a continuous measure of the expected number of weeks that a claimant will receive benefits, CBER provides DES 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 20, claimants predicted to exhaust between 90 and 95 percent of their unemployment benefits receive a 19, and so on.[9] The WPRS system was implemented in October of 1994; we make use of UI spells starting between that date and June 30, 1996.

[8] See Berger, Black, Chandra, and Allen (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.

[9] The distribution of scores is unimodal. The mean score is 15.3, the median is 16, the mode is 18, and the interquartile range is 14 to 18.

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.[10] CBER then provides local DES offices with the profiling scores of claimants in their area. Finally, the DES contacts those claimants selected to receive reemployment services through the mail to inform them of their rights and responsibilities under the program. A copy of the letter sent by DES appears in Exhibit 1.

Because of capacity constraints, local offices at some times during the year are not able to serve the entire population of claimants. At these times, it is 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 an experimental treatment group required to participate in reemployment services as a condition of continued UI receipt or an experimental 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 all happen to 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 experiment but fortunately for the claimants, the Kentucky economy was extremely strong from October 1994 to June 1996, the

[10] Individuals who have a definite recall-to-work date or who receive reemployment services through a union hall are exempt from profiling.

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.[11] Combining all of the PTGs yields a treatment group of 1236 claimants and a control group of 745. Thus, the experimental design uses only about 2.6 percent of the treated population and 7.6 percent of the untreated population.

Table 1 compares the demographic characteristics of the treatment and control groups as well as the population of treated claimants. Not surprisingly, the groups are quite similar. Table 1 also reveals that Kentucky has relatively little racial diversity: whites comprise over 90 percent of both the treatment and control groups. On average, the claimants in the experiment are about 37 years old and have just over 12 years of schooling.

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 assess the claimants and then refer them to specific services such as assisted job search, employment counseling, job search workshops, and retraining programs. The vast majority of profiled

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

claimants are referred to less expensive job search and job preparation activities rather than formal education and training programs.[12] With the exception of the retraining programs, which can be quite extensive, most of the available services require only modest effort on the part of claimants.

3 Theoretical Framework

In order to get a better feel for the potential impact of the program, in this section we outline a simple model of an unemployed worker's search with unemployment insurance. We begin by considering the utility of being unemployed in the absence of the WPRS system. In the US unemployment insurance system, benefits are paid for a limited duration, usually 26 weeks. As a result, the utility of being unemployed decreases over time up to the point of benefit exhaustion. As depicted in Figure 1, after benefit exhaustion the utility of being unemployed remains a constant, reflecting the assumed stationarity of the distribution of wage offers. A worker will maximize his or her discounted, expected utility by setting the reservation wage so that the value of employment at the reservation wage just equals the value of being unemployed, as in standard search models. Therefore, the declining utility of being unemployed implies that the worker's reservation wage declines until the worker exhausts benefits.

There are at least two ways in which the WPRS system may influence workers' valuation of unemployment. First, if the reemployment services are

[12] According to U.S. Department of Labor (1999), Figure 5, 9.34 percent of claimants in Kentucky who were referred to at least one service were referred to education and training as of the fourth quarter of 1997.

effective they will improve the distribution of wage offers during and after receipt of the services. This has the further effect of increasing the value of being unemployed prior to the start of services, as claimants anticipate receiving them. There are a couple of reasons to believe, however, that any impact of reemployment services on wage offers will be small. First, past experience with government training programs suggests that they are often ineffective (Heckman, LaLonde and Smith, 1999). Second, most of the treatments offered to the unemployed were of modest duration and cost relatively little to provide. Assuming reasonable rates of return on investment, their modest cost suggests at best a modest effect. Moreover, if such services were highly effective at increasing wages, we would expect the private sector to offer them, which, in general, it does not.

Second, because the reemployment services take time, participation in them reduces the quantity of leisure that unemployed workers may consume. As shown in Figure 1, this "leisure tax" lowers the value of remaining unemployed during the time the services are received.[13] Figure 1 assumes that claimants do not anticipate the profiling treatment. If they do, then the expectation of the leisure tax also reduces the utility of being unemployed prior to the start of services.

The net effect of the profiling system on the value of unemployment, and hence on the probability of leaving UI in each week, depends on the signs and magnitudes of these two effects. If the services are effective, then the two

[13] If the requirements of the WPRS system are sufficiently onerous, it may lead eligible unemployed persons to avoid UI entirely. Interestingly, even in the absence of the profiling system, empirical studies find UI take-up rates of substantially less than one. For instance, Blank and Card (1991) report an average rate of 71 percent. See Blank and Card (1991) and Anderson and Meyer (1997) for recent estimates and extended discussion of the measurement issues involved.

factors work in opposite directions prior to and during the services. Once done with services, only the wage offer effect persists and the employment hazard is increased. If the services are ineffective, then the net effect prior to and during receipt of services is to lower the value of unemployment and thereby speed exit from UI. In this case, the program has no effect after receipt of services. If the program works for some claimants but not for others, then we might observe a mixture of effects, including a situation where the hazard rate out of UI increases prior to service receipt due to claimants for whom the services represent a cost, and after service receipt for those claimants to whom they represent a benefit.

4 Experimental Design and Identification

4.1 Parameters of Interest

Let Y_1 denote the outcome of interest if a claimant receives treatment and Y_0 denote the outcome if a claimant does not receive treatment. The impact of treatment on some person i is then

$$Y_{1i} - Y_{0i} = \Delta_i.$$ [14]

Because many members of the experimental treatment group do not re-

[14] We assume throughout that the impact of the program on a particular person is independent of the size of the profiling program or the particular set of persons participating in it. That is, we assume that there are no general equilibrium effects. In the statistics literature, this is called the Stable Unit Treatment Value Assumption (SUTVA).

ceive employment and training services, it is important to distinguish the effect of actually receiving services from the effect of being assigned to the treatment group per se. We use the word "treatment" to mean being assigned to the program – i.e., receiving the letter in Exhibit 1 and being subject to the requirement to receive services in order to continue receiving UI – rather than actual receipt of employment and training services. Everyone in the experimental treatment group receives this treatment.

The first parameter of interest is the effect of treatment on a randomly selected UI claimant. Letting $C = 1$ if a person is a UI claimant and letting $C = 0$ otherwise, we have

$$\Delta_1 = E(\Delta_i | C = 1) = E(Y_{1i} - Y_{0i} | C = 1).$$

This parameter is of interest if the policy under consideration is the extension of the treatment to the entire claimant population, keeping in mind that doing so might change that population if eligible persons respond to the policy change by changing their UI participation decision.

The second parameter conditions on both UI receipt and on being selected for treatment. This parameter gives the mean impact of the treatment the treated. Letting $S = 1$ if a person is selected for treatment and $S = 0$ otherwise, we have

$$\Delta_2 = E(\Delta_i | C = 1, S = 1) = E(Y_{1i} - Y_{0i} | C = 1, S = 1).$$

The conditioning set in this case includes not just those in the PTGs but all inframarginal treated persons as well.

The third parameter conditions on membership in a PTG and so gives the mean impact for persons who had the marginal profiling score for the week and local office of their claim. Letting $G = 1$ for persons in a PTG and $G = 0$ otherwise, the parameter is given by:

$$\Delta_3 = E(\Delta_i | C = 1, S = 1, G = 1) = E(Y_{1i} - Y_{0i} | C = 1, S = 1, G = 1).$$

This parameter defines a Local Average Treatment Effect (LATE) in the sense of Imbens and Angrist (1994). This parameter is of policy interest if the choice under consideration is a modest increase or decrease in the budget for treatment, which would imply a modest decrease or increase in the marginal profiling score.

4.2 Identification

We now consider identification of the various parameters of interest given the data at our disposal. Heckman, Smith and Clements (1997) emphasize that even with experimental data, many parameters of interest require additional assumptions to secure identification. Because of the unusual nature of random assignment in our experiment, identification is a particularly important issue.

Conditional on $C = 1, S = 1$, and $G = 1$ we have random assignment of claimants into treatment and control groups. Under the standard assumptions that justify interpreting data from social experiments as providing estimates of the mean effect of treatment on those randomly assigned – see, e.g., Heckman and Smith (1995) or Heckman, LaLonde and Smith (1999) – our data identify Δ_3, the LATE associated with the PTGs.

To identify other parameters requires additional assumptions regarding the variation in the impact of treatment, Δ_i, across persons. The simplest assumption to make is that of the common effect model, in which $\Delta_i = \Delta$ for all persons i. Under the common effect assumption,

$$\Delta_1 = \Delta_2 = \Delta_3 = \Delta,$$

and our data identify all three parameters defined in Section 4.1. A similar result holds if the treatment effect varies across persons, so that $\Delta_i = \Delta + \varepsilon_i$, but the idiosyncratic component of the treatment effect is either unknown or known but not acted upon.

Another case in which our data can identify parameters other than just the local average treatment effect, Δ_3, is when the variation in the treatment effect across persons depends on observable characteristics. Thus, if

$$\Delta_i = \Delta(X_i), \tag{1}$$

where the X_i are observable characteristics of the claimant, then providing a support condition on the X_i is met, we can estimate the $\Delta(X)$ function using the experimental data for the PTGs and then re-weight according to the distribution of the X in other conditioning sets to obtain the other parameters. For example, suppose that the support of X (the set of X values with positive density) is the same for claimants in the PTGs as it is for all claimants. Then, having estimated $\Delta(X)$ using our data, under assumption (1) we can consistently estimate Δ_2 by constructing the sample analog to

$$\Delta_2 = \int \Delta(X) f(X|C=1, S=1) dX,$$

where $f(X|C=1, S=1)$ is the density of X in the population of claimants, which will likely differ from the density of X in the population of persons in a PTG. A similar analysis holds if $\Delta_i = \Delta(X_i) + \varepsilon_i$, so long as the ε_i is either unknown or known but not acted upon.

The problem of a sufficient support for X within the population of persons in a PTG is not of merely theoretical interest. Suppose that the impact of the program is a function of the profiling score, P, so that $\Delta_i = \Delta(P)$. In this case, if we wished to estimate Δ_2, the support condition would prevent us from doing so, because the population of treated claimants includes values of P from 1 to 20 while the population of claimants in PTGs includes only values of P from 6 to 19.[15]

4.3 Estimation

To produce our experimental impact estimates, we estimate

[15] In the most general case, where $\Delta_i = \Delta(X_i) + \varepsilon_i$ and the individual knows and acts on ε_i, further assumptions are required to secure identification of more than just the LATE directly identified by the experimental data. We do not explore this case in detail here. See Heckman, Smith and Clements (1997) and Heckman and Smith (1998) for extended discussions of this case.

$$y_{ij} = \beta_0 + \beta_1 T_{ij} + \mu_j + \nu_{ij} \tag{2}$$

where y_{ij} is the outcome for the ith individual in the jth PTG, T_{ij} is a binary indicator for whether or not the ith individual in the jth PTG received treatment, μ_j is a fixed effect to control for differences in expected earnings in the absence of treatment across PTGs, ν_{ij} is a random disturbance term, and β_0 and β_1 (along with the μ_j) are parameters to be estimated. In some cases, the treatment indicator T_{ij} is also interacted with various observable characteristics of the claimants. As just discussed, the interpretation of β_1, the coefficient on the treatment indicator, and of any interaction terms, depends critically on the assumptions that are maintained about the presence or absence of variation in the impact of treatment, Δ_i, across persons and, if it is present, the particular form that variation takes.

Conditioning on the PTG has two important consequences for the estimates. First, because the proportion of claimants in the treatment group varies across PTGs, failure to control for PTGs would result in biased estimates of the impact of the program if expected earnings in the absence of treatment differ across PTGs. Second, because each PTG consists of individuals with a specific profiling score at a particular location on a particular week, including the μ_i 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.

5 Empirical Analyses

5.1 Aggregate Impacts

We focus on three outcomes of interest: the number of weeks that a claimant receives benefits, the amount of benefits that the claimant receives, 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 (DES).

The measure of earnings after the unemployment event is less than ideal for three reasons. First, because DES 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 (MSAs) in Kentucky, only Lexington is not located on the border of an adjoining state. While this does not interject any bias into the experiment per se, it is important to keep in mind that we are measuring the earnings of claimants in Kentucky, not their total earnings.

Second, earnings are not observed for claimants who work in a non-covered sector. This is a standard problem in all analyses that use earnings variables constructed from unemployment insurance system records. Third, DES records do not include any "informal" activities. To the extent that claimants work "off the books," the DES 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 gain from treatment.

If WPRS is effective, we expect that treatment should reduce the number of weeks that benefits are received, lower the amount of benefits received, and increase earnings. Table 2 presents the basic impact estimates from the experiment, obtained by estimating equation (2) above. We find that the treatment group collects payments for about 2.2 fewer weeks than the control group; this difference is highly statistically significant. The treatment group receives about $143 less in benefits than the control group. This difference is statistically significant at the ten percent level, and becomes significant at the five percent level if we reduce the residual variance by adding additional baseline characteristics when estimating equation (2). Finally, the treatment group earned $1,054 more than the control group in the year following initiation of the UI claim; this difference is statistically significant at the five percent level. Thus, in terms of mean impacts, the WPRS treatment does what it is intended to do; it shortens UI claims, reduces total benefits paid and raises earnings.[16]

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 $370. In contrast, a savings of $143 suggests only a 0.85 week reduction in weeks paid. We examine this apparent discrepancy in detail in Appendix A. In sum, 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 value

[16]We wondered if the impact of treatment might diminish over calendar time as later cohorts of claimants learned about the relatively modest time commitment that the program usually requires. We found, however, no systematic pattern over time.

was updated in the administrative records to reflect the second spell but the weeks paid variable was not. This suggests an upward bias in our impact estimate. We also construct estimates of the impact on weeks paid using the earnings impact estimates and an estimate of average earnings. This procedure yields an estimate very similar to that in Table 2. Taken together, the evidence presented in Appendix A suggests that the weeks paid impact estimate presented in Table 2 may have a modest upward bias.

To put these estimated impacts into perspective, consider the estimates from the UI bonus experiments presented in Woodbury and Spiegelman (1987). 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 the 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. The WPRS program has the further advantage that it is unlikely to increase the UI take-up rate.

In addition, the WPRS impacts reported here tend to be larger than those from experimental evaluations of job search assistance programs for UI claimants summarized in Meyer (1995).[17] Most of these programs (see his Tables 5A and 5B) have estimated impacts equal to or less than one week of benefit receipt. Decker, Freeman, and Klepinger (1998) 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

[17] See Corson, Long, and Nicholson (1985), Anderson, Corson, and Decker (1991) and Johnson and Klepinger (1993) for analyses of the individual job search experiments.

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 generally 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 benefit from them for those who do.

It is interesting to consider the costs and benefits of the profiling program from the point of view of the UI system. Our estimates from the PTGs indicate that treated claimants receive $143 less in benefits than untreated claimants. We can compare these average benefits with the average costs per treated claimant in the Kentucky UI system. To construct the average costs 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 (DES), 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.[18] These costs sum to $11.93 per treated claimant. Even if one adds approximately $0.5 million in start-up 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.[19]

[18] These data were provided by Ted Pilcher of the Kentucky DES.

[19] The costs shown 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 from the standpoint of society (rather than of the UI system) would include those costs, along with the increased earnings of the treated claimants and some measure of the

5.2 The Effect of Treatment Over Time

Figure 2 displays hazard rates for leaving UI for the experimental treatment and control groups.[20] It documents a large impact of treatment after receipt of the first UI payment (i.e., in weeks 1 and 2): about 13 percent of the treatment group exits after the first payment but only about 4 percent of the control group does so. Subsequently, the hazard rate of the treatment group is almost always (always with bi-weekly hazards) higher than that of the control group until weeks 25 and 26, although the difference is statistically significant only once. We may use the hazard function 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.

Experimental evaluations of mandatory job search assistance in other contexts report similar results. Corson and Decker's (1989) analysis of the New Jersey search experiments and Johnson and Klepinger's (1994) analysis of the Washington search experiment both find evidence of early return to work. Decker, Freeman, and Klepinger's (1999) 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. Dolton and O'Neill's (1996) experimental examination of the Restart component of Britain's UI system

value of their foregone leisure.

[20]Parameter estimates are presented in Appendix Table B1. Most benefits are paid bi-weekly. 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 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.

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 case worker. 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. Finally, Dickinson, Kreutzer, and Decker (1997) use nonexperimental econometric methods to evaluate the WPRS in three states: Delaware, Kentucky, and New Jersey. For Kentucky, they find that the program reduced the number of weeks receiving benefits by 0.72 weeks – an estimate that is very similar to our estimates using the imputed weeks paid described in Appendix A – and reduced the amount paid by $96, which is again similar to our estimate. They find no evidence that the WPRS program increased earnings.[21]

In Figures 3 and 4, we graph mean earnings and employment by quarter after the start of the UI spell for the treatment and control groups.[22] 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

[21] The absence of location from their regression model may explain the discrepancy. Heckman, Ichimura, Smith, and Todd (1998) find that the failure to draw comparison groups from the same location as the treated population results in significant bias in econometric evaluations of the JTPA program.

[22] Appendix Table B2 provides the estimates.

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.[23]

These 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 (2) 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% increase in the number of interrupted spells.

If the WPRS program lowers claimants' reservation wages early in their unemployment spells, then treatment group members who exited early should have lower earnings than control group members who exit early. In Table 3, we interact the treatment indicator with an indicator for whether or not the claimant exited early – that is, within four weeks of the start of the UI claim. We find strong evidence of lower earnings among treatment group members

[23] We also consider whether a claimant returned to a previous employer. We find that 23.6 percent of the treatment group have earnings at the same firm in the quarter before and the quarter after their UI spell compared to 14.3 percent of the control group. A one-sided Fisher's exact test rejects the hypothesis that these rates are the same with a p-value of 0.028, which is consistent with the profiling program reducing moral hazard in the treatment group.

exiting early compared to control group members who do so.[24] The lack of an earnings impact more than three quarters after the start of the claim indicates that the effect of these poor initial matches washes out over time.

Finally, we consider the period after the first four weeks of the UI claim. That the exit hazard in the treatment group continues to lie above that for the control group for most of the standard 26 week benefit eligibility period is consistent with either selection of low-hazard rate persons out of UI in the first four weeks, or with a positive impact of employment and training services on those who receive them. The latter explanation is consistent with the evidence on the modest but detectable positive effects of these programs in previous UI experiments and in the AFDC work/welfare experiments documented in Gueron and Pauly (1991).

In sum, we have reasonably strong evidence that the earnings gains we document result from more early exits from UI in the experimental treatment group. Most of these exits take place prior to possible receipt of reemployment services. Instead, they coincide with receipt of the letter indicating the claimant's obligation to receive services. This evidence suggests that the WPRS treatment is an effective tool for reducing the extent of moral hazard in the UI program.

[24] To insure that these results do not occur because of potential error in the weeks paid measure, we reestimated the model using only observations in which the weeks paid measure and the imputed weeks paid measure defined in Appendix A agree. While this exclusion drops numerous claimants with multiple unemployment spells in the year, the results continue to indicate that members of the treatment group who exit UI early have lower earnings than early exiters in the control group.

5.3 Are the Treatment Effects "Common Effects?"

Recent work by Heckman, Smith, and Clements (1997) and others emphasizes variation in the impact of treatment across persons as an important aspect of the evaluation problem. In this section, we construct person-specific impact estimates for the treatment group in a simple way and examine their distribution.

The assumption underlying the person-specific impacts we construct is a generalization of the common effect model. This generalization preserves the property that the ranks of persons in the treated and untreated distributions are the same, which arises in the common effect model from the fact that in the population the treatment group outcome distribution is just the control outcome distribution shifted by a constant. At the same time, it relaxes the assumption that the size of the impact is the same for each person. Under this generalization, impact estimates are constructed by taking differences across quantiles of the treated and untreated outcome distributions. The difference between the analysis here and that in Heckman, Smith and Clements (1997) is that we first remove the PTG fixed effects from the outcomes prior to differencing across quantiles.

Formally, we construct

$$\widetilde{Y}_{1ij} = Y_{1ij} - \widehat{\mu}_j \tag{3}$$

$$\widetilde{Y}_{0ij} = Y_{0ij} - \widehat{\mu}_j \tag{4}$$

where $\widetilde{Y}_{1ij}$ ($\widetilde{Y}_{0ij}$) is the outcome for the ith member of the treatment (control) group in the jth PTG with the PTG fixed-effect removed, Y_{1ij} (Y_{0ij}) is the unadjusted outcome for the ith member of the treatment (control) group in the jth PTG, and $\widehat{\mu}_j$ is the estimated fixed effect for the jth PTG. We calculate impacts by running quantile regressions of $\widetilde{Y}$ on an intercept and a treatment indicator. The impact estimate for a given quantile of the $\widetilde{Y}_{0ij}$ distribution is just the coefficient on the treatment indicator from the corresponding quantile regression. Figure 5 plots the estimates.[25]

In their analysis of the JTPA program, Heckman, Smith, and Clements (1997) find reasonably strong support for the common effect model; that is, in their data the estimated impact is relatively constant across much of the control group outcome distribution. In contrast, we find a great deal of heterogeneity in the impact of treatment. For weeks of benefits paid the impact estimates range from 1.1 to 3.7 weeks, for the amount paid the impacts range from about -$400 to $130, and for earnings they range from about $280 to over $1300. Indeed, there are statistically significant positive and negative estimates of the impact of treatment on the amount of benefits paid. Thus, we find little evidence for the common effects model. The estimated impacts depend strongly on the untreated outcome for all of the outcome variables we examine.[26]

[25] Appendix Table B2 reports the estimates. Like Heckman, Smith and Clements (1997), we use quantiles of the outcome distributions, rather than simply matching individuals across distributions, because the treatment and control groups are of unequal size.

[26] A simple formal test for the common effect model is a test of equal variances of the treatment and control group outcome equation residuals. We use a test due to Glesjer (1969). Note that the variances can be the same even if the common effect model is false if the outcome equation residual and the person-specific impacts have the right correlation. To perform this test, we estimated the models for earnings, amount of benefits paid, and weeks paid without the PTG fixed effects using weighted least squares. The weights for the treatment group equal one and the weights for the control group are the relative frequency of the treatment group in the PTG. Using weighted least squares here saves on degrees of freedom because it frees us from having to estimate the 286 PTG fixed effects. We then transformed the residuals by taking their absolute value and the logarithm of their absolute

The estimated impacts also tell an interesting story. For all three outcome variables, the impacts are not monotonic in $\widetilde{Y}_{0ij}$. For the weeks of benefits paid and amount of benefits paid variables, Figure 5 shows that the impacts of the program are concentrated in the middle of the control group outcome distribution. The treatment appears to have very little impact on persons who would otherwise exhaust or come close to exhausting their benefits, but a very large impact on persons who would otherwise be between the 25th and 75th percentiles in terms of either outcome. For those expected to have short spells and receive few benefits, there appears to be a modest impact on weeks of benefits paid but little or no impact on amount of benefits paid. These findings represent further evidence that allocating the treatment on the basis of the expected duration of UI benefit receipt may not represent an optimal strategy.

5.4 Impacts by Group

In this section, we present estimates of the impact of the WPRS treatment for various subgroups of the participant population. A finding that the impact of the treatment varies substantially across subgroups among participants in the PTGs suggests the importance of re-weighting the subgroup impact estimates when constructing overall impact estimates for broader populations of interest, such as Δ_2 and Δ_1.

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value. We regressed the transformed residuals against a treatment indicator, again using weighted least squares. If the residuals are homoskedastic, the treatment indicator should not be statistically significant. The absolute value of the residuals from the earnings and weeks paid regressions failed the test at the 5 percent confidence level. The logarithm of the absolute value of the residuals from the weeks paid and amount paid equations failed the test at the 5 percent confidence level.

Because of the relatively modest size of the Kentucky WPRS experiment, it is only possible to analyze the impact of treatment across relatively coarsely defined groups. To construct the subgroup impact estimates, we re-estimate equation (2) allowing both the treatment and control group means to vary by the demographic (or other) variable of interest. We explore differences by sex, race (white and nonwhite), age (under 35, 35 to 49, and 50 and over), education (less than high school graduate, high school graduate, and more than high school), location, and profiling score in the experimental impact estimate. In addition, we test for differences in the impact of treatment across PTGs. Table 4 presents selected results.

Among the demographic variables, we found statistically significant subgroup impacts only for age. Those under the age of 35 appear to benefit less from the program than older claimants. Younger claimants have smaller reductions in the duration of their UI receipt and in the amount of benefits they receive, although we cannot reject the hypothesis that the program's impacts are the same for these two outcomes. The earnings impact of treatment is positive and statistically significant for older claimants, but negative and statistically insignificant for younger claimants. We can reject the hypothesis that the earnings impacts are the same for these age categories.

The fifth row of Table 4 presents the results of F-tests of the null hypothesis that the impact of treatment is the same across all 286 PTGs. We fail to reject this null hypothesis for any of the outcome measures, but note that the sample sizes for the individual PTGs are quite small.

We also explore whether or not impacts differ by profiling score. To do so, we divide claimants into four groups: those with profiling scores between 6 and 13 (approximately 26% of the treatment group), those with profiling scores of 14 or 15 (approximately 20% of the treatment group), those with profiling scores of 16 (approximately 21% of the treatment group), and those with profiling scores between 17 and 19 (approximately 33% of the treatment group). The results suggest that the treatment effect is a highly nonlinear function of the profiling score, but we can reject the null of equal impacts across profiling score subgroups only for earnings. For earnings, the estimated impact is about \$940 for persons with profiling scores between 6 and 13. Treatment group members with profiling scores between 14 and 15, however, have lower earnings than the corresponding control group members, although the estimated standard error is quite large. For those with a profiling score of 16, the impact of the program is positive, extremely large (over \$4,175), and highly significant. Finally, the impact for those with the highest profiling scores is only about \$690. An F-test rejects the null hypothesis of equal earnings impacts across profiling score subgroups.

The underlying assumption of the WPRS program is that those with the longest expected UI spells benefit the most from the profiling treatment. The estimates in Table 6 provide little justification for this assumption. There does not appear to be a monotonic relationship between the profiling score and the impact of treatment. This calls into question the wisdom of using expected UI spell duration as a means allocating treatment.

5.5 Estimates of Δ_2 When $\Delta = \Delta(X)$

In Section 4, we discussed how to construct estimates of the mean impact for populations broader than that for which we actually have experimental data in the cases where $\Delta_i = \Delta(X)$ or where $\Delta_i = \Delta(X) + \varepsilon_i$ and the

ε_i is not known to the agent (or is known but not acted upon). We now present estimates of Δ_2, the mean impact of treatment on everyone selected for treatment. To construct these estimates we first estimate $\Delta(X)$ using the experimental data for a number of choices of X, including (one at a time) the profiling score, sex, race, age, education and location within Kentucky. We then construct the estimates of Δ_2 by re-weighting the conditional (on X) experimental impact estimates using the distribution of each X in the sample of all persons profiled into treatment. For example, among all treated claimants ($C = 1$ and $S = 1$ in the notation of Section 4), only 2.73 percent have profiling scores of 11. In contrast, 6.36 percent of the claimants in a PTG (and therefore subject to random assignment) have profiling scores of 11.[27] Table 5 presents estimates Δ_2 based on re-weighting.

Not surprisingly, our estimates of Δ_2 depend on the particular X variable used to do the re-weighting. For example, the mean impact estimates for earnings range from a high of \$1,362 to a low of \$828. In general, however, the estimates of Δ_2 tell the same substantive story as our estimates of Δ_3. In no case do the estimates of Δ_2 based on reweighting fall outside of the 95 percent confidence bounds of the estimates of Δ_3.

[27] The profiling score is the one variable for which the support condition discussed in Section 4 comes into play. As a result, we perform the re-weighting using the conditional density of the profiling score in the treated population over the set of profiling scores in the common support.

6 Conclusions

In this paper, we use unique experimental 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, called a tie-breaking experiment by Thistlethwaite and Campbell (1960), allows the introduction of random assignment without major program disruption and without denying services to those most in need. In so doing, it may reduce both line worker resistance to random assignment and the negative publicity sometimes associated with random assignment experiments in the social services.

For this group, 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.

The reduction in the length of recipiency in the treatment group is largely accomplished by early exits from the program. 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 prospective of the UI system, and likely from that of society as well, it produces a wide excess of benefits over costs.

We find strong evidence against the "common effect" assumption. For the WPRS program, the estimated treatment effect appears to differ dramatically across quantiles of the untreated outcome distribution. In particular, for both the weeks of benefits paid and the amount of benefits paid outcomes, the impact of the program is concentrated in the middle of the untreated outcome distribution. That is, the program reduces weeks paid and benefits paid for persons who would otherwise have had moderate values of those variables. It has little effect on persons who would otherwise exit very early and receive few benefits and on those who would otherwise exhaust or come close to exhausting their benefits.

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. Our results provide little justification for this assumption as we do not find a monotonic 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, then our findings call into question the wisdom of using the expected benefit duration as a means allocating treatment. They also suggest the value of further thought and study before extending profiling to other programs.

Table 1: Demographic Characteristics of Treatment and Control Groups: Kentucky WPRS Experiment, October 1994 to June 1996

	Control Group	Treatment Group	P-values for tests of differences in means	Treated Population
Age	37.0 (10.9)	37.1 (11.1)	0.717	37.4 (11.2)
Years of schooling	12.3 (2.10)	12.6 (2.14)	0.221	12.4 (2.06)
White male	0.564	0.518	0.095	0.517
White female	0.352	0.372	0.060	0.398
Nonwhite male	0.040	0.055	0.433	0.042
Nonwhite female	0.044	0.055	0.691	0.043
Earnings in year before claim	$19,759 (13,678)	$19,047 (13,636)	0.666	$19,168 (14,588)
N	745	1,236	---	48,002

Source: Authors' calculations from the Kentucky WPRS Experiment. 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. In the notation in the text, this group has $C=1$ and $S=1$.

Table 2: Impact of Treatment on Duration of Benefits and Earnings: Kentucky WPRS Experiment, October 1994 to June 1996

Outcome Measures	Impact of Treatment
Number of weeks receiving UI benefits	-2.241
	(0.509)
	[0.000]
UI benefits received	-$143.18
	(100.3)
	[0.077]
Earnings in the year after the start of the UI claim	$1,054.32
	(588.0)
	[0.037]
N	1981

Source: Authors' calculations from the Kentucky WPRS Experiment. 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 there are 286 PTGs. Standard errors are in parentheses and p-values from one-tailed tests are in brackets.

Table 3: Impact of Early Exit and Treatment on Earnings: Kentucky WPRS Experiment, October 1994 to June 1996

Dependent Variable: Earnings in the year after the start of the UI claim.	(1)
Treatment group	$1,434.62 (636.62) [0.012]
Claimant exited early (paid benefits less than 5 weeks)	$7,380.99 (1,217.73) [0.000]
Claimant exited early and in the treatment group	-$4,575.47 (1445.60) [0.001]
N	1981

Source: Authors' calculations from the Kentucky WPRS Experiment. 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 there are 286 PTGs. T-statistics are given in parentheses and p-values from one-tailed tests appear in brackets. An early exit is a person for whom the weeks of benefits paid variable equals less than five.

Table 4: Impact of Treatment by Age, PTG, and Profiling Score: Kentucky WPRS Experiment, October 1994 to June 1996

	Weeks of UI benefits received	UI benefits received	Earnings in year after claim	N
Age				
Age under 35	-1.476	-$80.27	-$144.72	779
	(0.720)	(140.9)	(840.0)	
	[0.020]	[0.285]	[0.569]	
Age between 35 and 49	-2.994	-$198.62	$1,957.42	851
	(0.770)	(150.6)	(898.2)	
	[0.000]	[0.094]	[0.015]	
Age 50 and over	-2.469	-$145.82	$2,479.54	351
	(1.265)	(247.5)	(1475.0)	
	[0.026]	[0.278]	[0.047]	
P-value for test of equality of impacts	0.335	0.843	0.013	
Profiling Tie Groups				
P-Value for test of equality of impacts (286 groups)	0.842	0.706	0.823	1981
Profiling Scores				
Profiling score between 6 and 13	-2.238	-$270.08	$939.51	515
	(0.913)	(179.74)	(1052.05)	
	[0.007]	[0.067]	[0.186]	
Profiling score between 14 and 15	-1.891	-$14.42	-$1,257.14	390
	(1.050)	(206.77)	(1,210.25)	
	[0.036]	[0.472]	[0.851]	
Profiling score of 16	-3.057	-$465.73	$4,175.83	424
	(1.102)	(216.94)	(1,269.76)	
	[0.003]	[0.016]	[0.001]	
Profiling score between 17 and 19	-1.861	$182.09	$689.71	652
	(1.039)	(204.60)	(1,197.55)	
	[0.037]	[0.813]	[0.283]	
P-value for test of equality of impacts	0.851	0.132	0.021	

Source: Authors' calculations from the Kentucky WPRS Experiment. Each of the regressions controls for the Profiling Tie Group (PTG) of the recipients. Standard errors are given in parentheses and p-values from one-tailed tests are given in brackets.

Table 5: Estimates of the Impact of Treatment on the Treated (Δ_2) Based on the Assumption that $\Delta = \Delta(X)$: Kentucky WPRS Experiment, October 1994 to June 1996

Weights	Number of weeks receiving UI benefits	UI benefits received	Earnings in year after claim
Unweighted	-2.241	-$143.18	$1,054.32
	(.509)	(100.3)	(588.0)
	[0.000]	[0.077]	[0.037]
Profiling score weights	-2.239	-$86.67	$1,362.19
	(0.547)	(109.55)	(656.87)
	[0.000]	[0.215]	[0.019]
Local office weights	-2.424	-$128.91	$1,250.81
	(0.560)	(110.44)	(644.75)
	[0.000]	[0.122]	[0.027]
Time weights	-2.327	-$150.48	$1,139.57
	(0.506)	(99.46)	(591.82)
	[0.000]	[0.065]	[0.027]
Age category weights	-2.316	-$154.11	$828.21
	(0.502)	(100.12)	(586.82)
	[0.000]	[0.062]	[0.079]
Education category weights	-2.276	-$151.20	$1,070.49
	(0.500)	(98.52)	(563.82)
	[0.000]	[0.063]	[0.029]
Sex weights	-2.284	-$154.95	$1,036.86
	(0.509)	(100.66)	(596.27)
	[0.000]	[0.062]	[0.041]
Race category weights	-1.979	-$140.81	$989.40
	(0.508)	(101.71)	(597.99)
	[0.000]	[0.083]	[0.049]
N	1981	1981	1981

Source: Authors' calculations from the Kentucky WPRS Experiment. Each of the regressions controls for the Profiling Tie Group (PTG) of the recipients. Standard errors are given in parentheses and p-values from one-tailed tests are given in brackets.

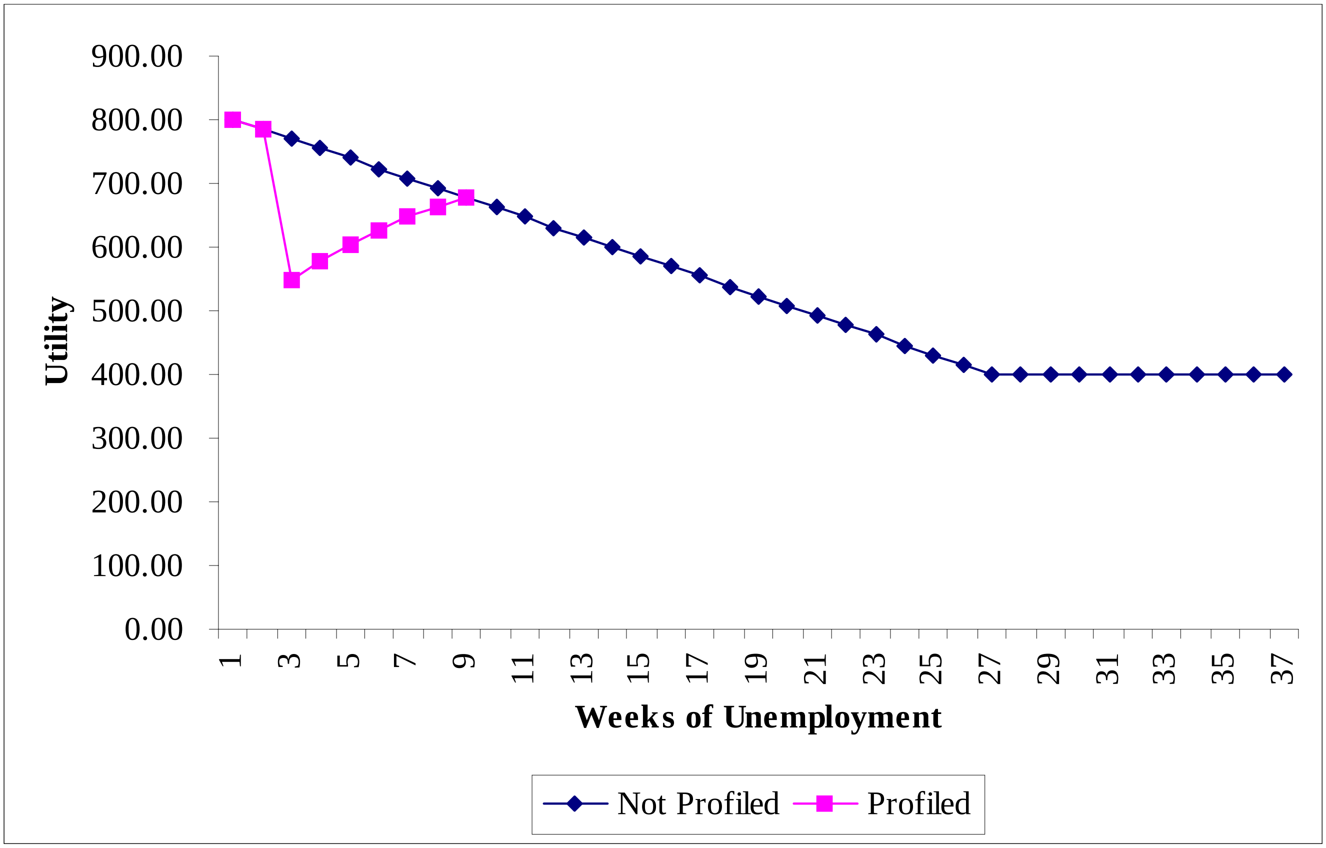

Figure 1: The Impact of Profiling on the Utility of Unemployment

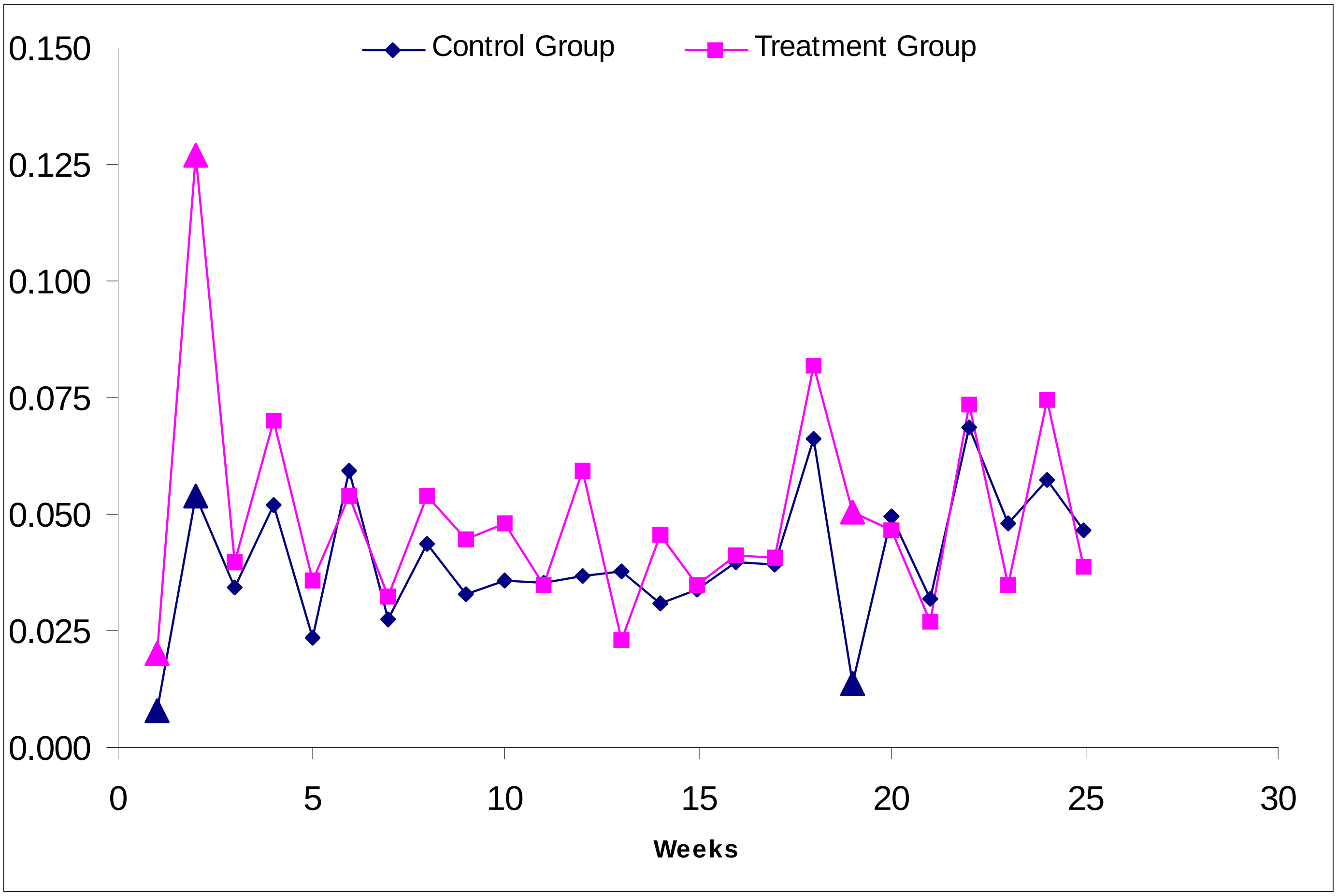

Figure 2: Hazard Functions of the Treatment and Control Groups, Kentucky WPRS Experiment, October 1994 to June 1996

Notes: Authors' calculation from Kentucky WPRS Experiment. Triangles denote significant differences at the five-percent level. The parameter estimates used to construct the graph appear in Table B1.

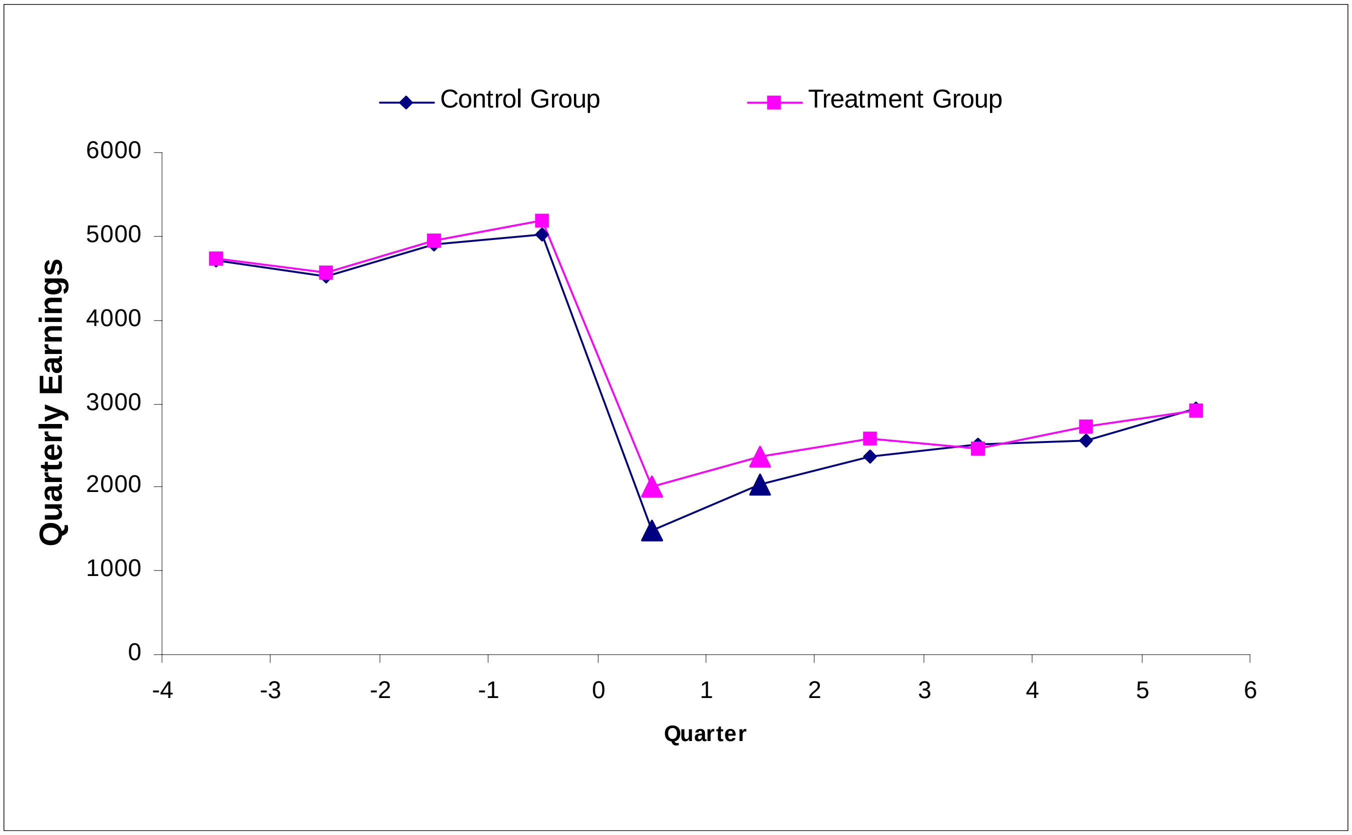

Figure 3: Earnings of the Treatment and Control Groups, Kentucky WPRS Experiment, October 1994 to June 1996

Notes: Authors' calculation from Kentucky WPRS Experiment. Triangles denote significant differences at the five-percent level. The parameter estimates used to construct the graph appear in Table B2.

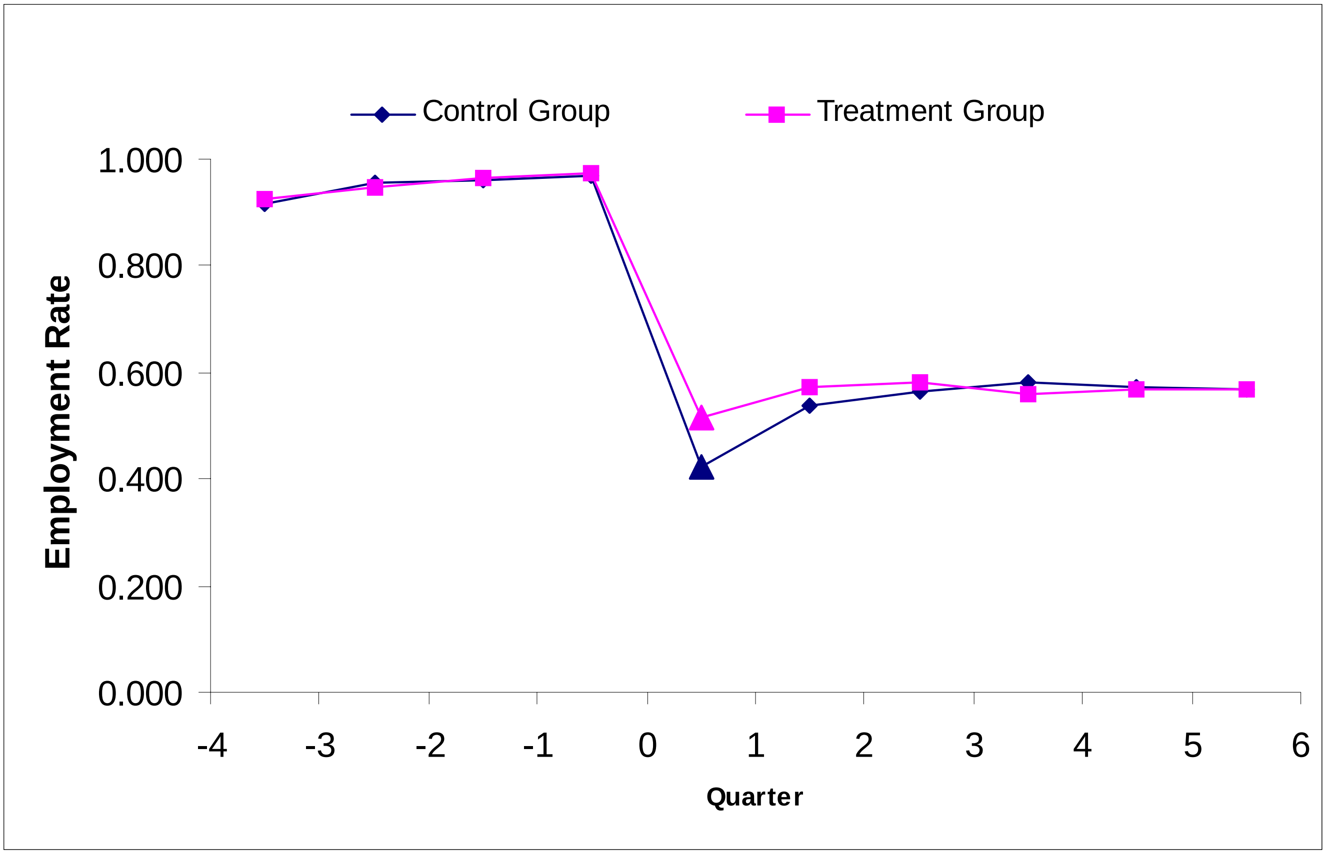

Figure 4: Employment of the Treatment and Control Groups, Kentucky WPRS Experiment, October 1994 to June 1996

Notes: Authors' calculation from Kentucky WPRS Experiment. Triangles denote significant differences at the five-percent level. The parameter estimates used to construct the graph appear in Table B2.

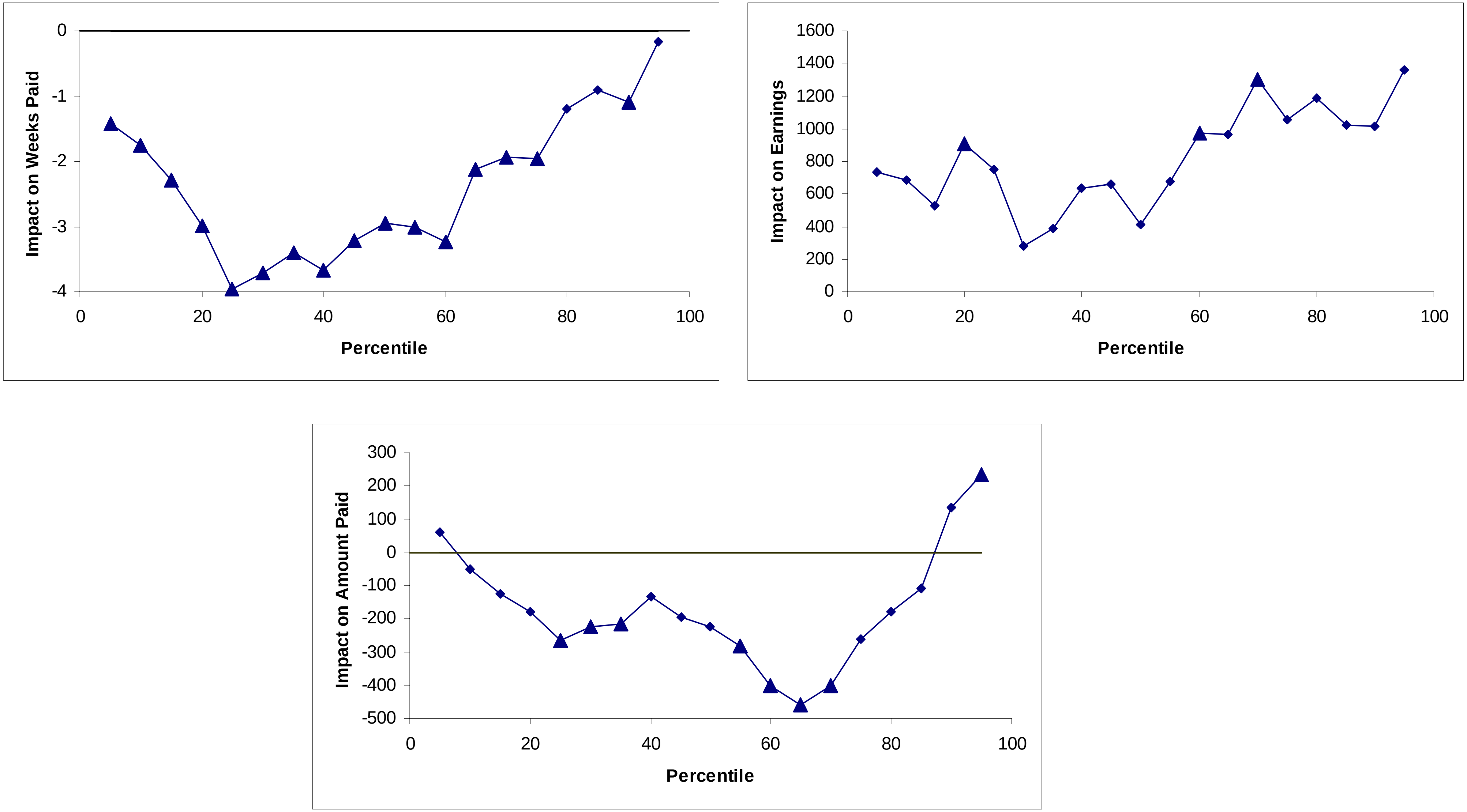

Figure 5: Impact of Treatment on Weeks Paid, Benefits Paid, and Earnings at Quantiles of the Untreated Outcome Distribution, Kentucky WPRS Experiment, October 1994 to June 1996

Notes: Authors' calculation from Kentucky WPRS Experiment. Triangles denote significant differences at the five-percent level. The parameter estimates used to construct the graph appear in Table B3.

CABINET FOR HUMAN RESOURCES
COMMONWEALTH OF KENTUCKY
FRANKFORT 40621

DEPARTMENT FOR EMPLOYMENT SERVICES

DATE:
SS #:
LO #:

Dear Claimant:

You have been identified 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 employment. 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.

If you are presently enrolled in training, have recently received job search services, or are engaged in any job search services that you believe may exempt you from participation in this program, bring all documents or relevant information concerning your participation with you when you report to the local office.

You are REQUIRED BY LAW, KRS 341 .350(2)(b), to attend the Orientation Session at the place, date and time specified below:

PLACE:

DATE:

TIME:

You may be determined ineligible to receive unemployment insurance benefits for failure to report to your local office as instructed or failure to participate in required services.

If you are UNABLE TO ATTEND,

Your participation in orientation may be postponed if you have a compelling reason to prevent you from attending on the date and time stated above, BUT it must be for circumstances beyond your control. Any postponement will be reported to UI for review of your availability.

BRING THIS LETTER WITH YOU WHEN YOU COME IN.

UI-P-100

(Rev. 09/94) JOB SERVICE

Exhibit 1

7 Appendix A

We noted in Section 5.1 that the estimated impacts on weeks of benefits paid and on the amount of benefits paid presented in Table 2 seem inconsistent. The impact on weeks of benefits paid in Table 2 is constructed using the weeks paid variable recorded in the UI administrative data. In this appendix, we compare impact estimates obtained using the weeks paid variable with alternative estimates based on a measure of weeks paid constructed indirectly from other elements of the administrative data. We construct this alternative measure, which we call "imputed weeks of benefits paid" by dividing the total benefits paid variable by the weekly benefit level variable. Because the weekly benefit amount may change over a spell, this measure may contain some error, but it provides a useful check on the weeks paid variable.

The weeks paid variable and the imputed weeks paid variable are highly correlated (0.884), but the correlation is higher for the control group (0.947) than for the treatment group (0.849). On closer examination, we found that most of the disagreements occurred for treatment group members with four or fewer weeks paid (using the weeks paid variable), where a disagreement occurs when imputed weeks paid exceed the weeks paid variable by at least 1.5 weeks. In Table A1, we provide a breakdown of whether or not the weeks paid and imputed weeks paid variables disagree by whether or not the claimant exited early. Among those who did not exit early, we find only three disagreements, two in the treatment group and one in the control group. Among those who exit early, the two measures of weeks paid differ for only 2 of the 105 claimants in the control group but for 116 of the 290 claimants in the treatment group.

Using the information in the administrative data, we can calculate the elapsed calendar time from the first week each claimant received benefits to the last week. If the elapsed calendar time exceeds the imputed weeks paid by two weeks or more, we assume that there was an interruption in the spell. That is, we assume the claimant had a spell of UI receipt, followed by a spell of non-receipt (presumably due to employment) followed by a second spell of UI receipt within the benefit year. For the sample of all early exits, the median difference between elapsed calendar time and imputed weeks paid is 17 weeks. Among the early exits, 66 claimants – 2 in the control group and 64 in the treatment group – appear to have an interrupted spell. In these cases, we believe that the weeks of benefits paid variable was not updated to reflect the second spell of UI receipt while the amount of benefits paid variable was. This is consistent with the fact that the latter, but not the former, plays an important role in the UI administrative system. This interpretation helps to account for the apparent inconsistency in the estimates in Table 2. It also leads to a somewhat different interpretation of the spike in the hazard at week 2 in Figure 1, as it suggests that many of those who leave early in response to the "threat" of reemployment services end up returning to UI after a spell of employment.

Of course, our measure of interrupted spells depends on correct reporting of the first and last weeks paid variables. For 16 recipients, we have doubts about the accuracy of these data elements. Although they have no interruptions according to our measure, each has either a value of weeks paid in excess of 13 weeks combined with positive earnings in the first quarter or imputed earnings (calculated by dividing quarterly earnings by 13 minus the value of the imputed weeks paid variable) in the first quarter in excess of

$2,000 a week. We believe that these observations probably have interrupted spells combined with coding errors in the first or last week paid variables. For the remaining 35 observations where the weeks paid and imputed weeks paid variables disagree we find no evidence of an interrupted spell and have no explanation for the disagreement.

The impact of treatment on earnings provides an indirect means of assessing the impact of treatment on the number weeks of employment and thereby on the number of weeks of benefits paid. With an estimate of weekly earnings, we can use the estimated earnings impacts to estimate the impact of treatment on the number of weeks of employment. To estimate mean weekly earnings, we use earnings in the quarter before the start of the UI spell, which is the quarter with the highest earnings. To be conservative, we exclude observations with less than $2,500 in quarterly earnings and estimate weekly earnings by dividing mean quarterly earnings by 12 rather than 13 weeks, which represents an implicit unemployment rate during the quarter of about 7.7 percent. Even with these restrictions, the mean weekly earnings of claimants is only $523 ($6,277/12). This suggests that, if the impact of treatment is independent of earnings (a strong assumption), then the WPRS treatment increases weeks of employment by about 2.02 weeks. This estimate almost certainly understates the true effect because we have very likely overestimated the weekly earnings of claimants. Nevertheless, this estimate is surprisingly similar to the estimate for weeks of benefits paid in Table 2.

In addition to measurement problems, the seeming inconsistency in the estimated impacts in Table 2 could be due to a correlation between the impact of treatment and the benefit level.[28] If the treatment has a stronger effect, in terms of reductions in weeks of benefits paid, on persons with low benefit levels, this would account for the seeming inconsistency. We examined this relationship and found that it exhibited a U-shaped pattern. Thus, while impacts and benefits do not appear to be independent, neither does this relationship account for the seeming inconsistency.

Taking all of the evidence into account suggests that there may be some upward bias in the estimate of the impact of the WPRS treatment on weeks of benefits paid reported in Table 2. The evidence presented in this appendix constitutes an important reminder that administrative data are not a panacea and must be used with care. While they avoid some problems associated with survey data, such as non-response, they often have unique problems of their own.

[28]It could also result from a correlation between benefit levels and early exit. However, the difference in mean benefit levels between early and later exiters in the treatment group is only $17, which suggests that this factor is not of major importance.

Table A1: A Comparison of Weeks Paid and Imputed Weeks Paid: Kentucky WPRS Experiment, October 1994 to June 1996

	Control Group	Treatment Group
Weeks of benefits paid is greater than 4 weeks		
Weeks paid and imputed weeks paid agree	639 (99.84%)	943 (99.79%)
Weeks paid and imputed weeks paid disagree	1 (0.16%)	2 (0.21%)
N	640 (100.0%)	945 (100.0%)
Weeks of benefits paid is less than 5 weeks		
Weeks paid and imputed weeks paid agree	103 (98.10%)	174 (60.21%)
Weeks paid and imputed weeks paid disagree		
Interrupted spell of benefit recipiency	2 (1.90%)	64 (22.15%)
Apparent interruption of benefit recipiency, imputed earnings in excess of $2,000 per week	0	16 (5.54%)
No apparent interruption of benefit recipiency	0	35 (12.11%)
N	105 (100.0%)	290 (100.0%)

Source: Authors' calculations from the Kentucky WPRS Experiment.

8 Appendix B

In this appendix, we provide point estimates for results depicted graphically. Table B1 displays hazard rates for leaving UI and the corresponding survivor functions for each group by week. These estimates are used to construct Figure 2. In Table B2, we provide estimates of the impact on earnings and employment by quarter after the start of the UI spell. These estimates are used to construct Figures 3 and 4. Table B3 reports the estimates depicted in Figure 5.

Table B1: Nonparametric Estimates of Hazard Rates and Survivor Functions for Benefit Duration: Kentucky WPRS Experiment, October 1994 to June 1996

	Hazard Rate			Survivor Function	
	Control Group	Treatment Group	P-value on difference	Control Group	Treatment Group
Week 1	0.008	0.020	**0.038**	0.992	0.980
Week 2	0.054	0.127	**0.000**	0.938	0.855
Week 3	0.034	0.040	0.536	0.906	0.821
Week 4	0.052	0.070	0.127	0.859	0.764
Week 5	0.023	0.036	0.122	0.839	0.736
Week 6	0.059	0.054	0.670	0.789	0.697
Week 7	0.027	0.033	0.534	0.768	0.674
Week 8	0.044	0.054	0.393	0.734	0.638
Week 9	0.033	0.044	0.323	0.710	0.609
Week 10	0.036	0.048	0.324	0.685	0.580
Week 11	0.035	0.035	0.967	0.660	0.560
Week 12	0.037	0.059	0.075	0.636	0.527
Week 13	0.038	0.023	0.131	0.612	0.515
Week 14	0.031	0.046	0.185	0.593	0.491
Week 15	0.034	0.035	0.955	0.573	0.474
Week 16	0.040	0.041	0.931	0.550	0.455
Week 17	0.039	0.041	0.886	0.529	0.436
Week 18	0.066	0.082	0.493	0.494	0.400
Week 19	0.014	0.051	**0.005**	0.487	0.380
Week 20	0.050	0.047	0.855	0.463	0.362
Week 21	0.032	0.027	0.664	0.448	0.353
Week 22	0.069	0.073	0.823	0.417	0.327
Week 23	0.048	0.035	0.409	0.397	0.316
Week 24	0.057	0.074	0.464	0.374	0.292
Week 25	0.047	0.039	0.651	0.357	0.281
Week 26	0.812	0.729	**0.029**	0.067	0.076

Source: Authors' calculations from the Kentucky WPRS Experiment. There are 745 claimants in the control group, 1,236 claimants in the treatment group, and there are 286 PTGs. We estimate the hazard rates using linear probability models. The survivor functions are generated from the estimated hazard functions. P-values are from two-tailed tests using Huber-White standard errors, where we switch to two-tailed tests here because we have no definite prediction of the effect of treatment on the time to exit.

Table B2: Dynamic Impact of Treatment on Earnings: Kentucky WPRS Experiment, October 1994 to June 1996

Quarter	Earnings	Employment
First quarter after unemployment spell begins	$525.58	0.092
	(192.82)	(0.027)
	[0.003]	[0.001]
Second quarter after unemployment spell begins	$344.05	0.036
	(161.38)	(0.027)
	[0.017]	[0.088]
Third quarter after unemployment spell begins	$220.67	0.021
	(181.55)	(0.027)
	[0.112]	[0.214]
Fourth quarter after unemployment spell begins	-$35.99	-0.020
	(176.10)	(0.027)
	[0.581]	[0.776]
Fifth quarter after unemployment spell begins	$181.78	-0.006
	(187.42)	(0.027)
	[0.166]	[0.588]
Sixth quarter after unemployment spell begins	-$5.59	0.001
	(199.54)	(0.027)
	[0.511]	[0.478]
N	1981	1981

Source: Authors' calculations from the Kentucky WPRS Experiment. 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 there are 286 PTGs. T-statistics are given in parentheses and p-values from one-tailed tests appear in brackets.

Table B3: Impact of Treatment at Deciles: Kentucky WPRS Experiment, October 1994 to June 1996

Percentile	Number of weeks receiving UI benefits	UI benefits received	Earnings in year after claim
5th percentile	-1.422	$61.79	$730.52
	(0.669)	(131.34)	(567.25)
	[0.034]	[0.638]	[0.198]
10th percentile	-1.746	-$49.41	$680.66
	(0.665)	(99.69)	(491.29)
	[0.009]	[0.620]	[0.166]
15th percentile	-2.290	-$125.97	$531.53
	(0.442)	(105.12)	(380.74)
	[0.000]	[0.231]	[0.163]
20th percentile	-2.980	-$178.11	$909.02
	(0.756)	(118.53)	(417.22)
	[0.000]	[0.133]	[0.029]
25th percentile	-3.961	-$264.21	$754.00
	(0.537)	(106.93)	(567.12)
	[0.000]	[0.014]	[0.184]
30th percentile	-3.704	-$224.59	$283.50
	(0.531)	(103.57]	(469.86)
	[0.000]	[0.030]	[0.546]
35th percentile	-3.404	-$214.95	$388.23
	(0.682)	(110.07)	(413.45)
	[0.000]	[0.051]	[0.348]
40th percentile	-3.662	-$133.87	$632.01
	(0.686)	(97.72)	(474.76)
	[0.000]	[0.171]	[0.183]
45th percentile	-3.207	-$196.82	$661.67
	(0.669)	(110.07)	(423.89)
	[0.000]	[0.074]	[0.119]
50th percentile	-2.954	-$223.92	$416.39
	(0.667)	(134.91)	(465.21)
	[0.000]	[0.097]	[0.371]
55th percentile	-3.015	-$280.08	$678.26
	(0.714)	(115.18)	(455.23)
	[0.000]	[0.015]	[0.136]
60th percentile	-3.237	-$400.28	$974.84
	(0.495)	(126.30)	(452.15)
	[0.000]	[0.002]	[0.031]

Table B3 Continued

Percentile	Number of weeks receiving UI benefits	UI benefits received	Earnings in year after claim
65th percentile	-2.118	-$459.34	$968.23
	(0.369)	(122.85)	(525.62)
	[0.000]	[0.000]	[0.066]
70th percentile	-1.937	-$399.21	$1305.96
	(0.546)	(80.88)	(583.01)
	[0.000]	[0.000]	[0.025]
75th percentile	-1.966	-$260.13	$1056.10
	(0.601)	(171.12)	(734.73)
	[0.001]	[0.129]	[0.151]
80th percentile	-1.187	-$179.08	$1190.35
	(0.682)	(119.52)	(965.07)
	[0.082]	[0.134]	[0.218]
85th percentile	-0.937	-$109.18	$1023.27
	(0.404)	(189.65)	(1012.81)
	[0.020]	[0.565]	[0.312]
90th percentile	-1.087	$133.81	$1014.05
	(0.416)	(132.73)	(1247.37)
	[0.009]	[0.313]	[0.416]
95th percentile	-0.157	$233.04	$1363.45
	(0.378)	(120.51)	(1666.32)
	[0.678]	[0.053]	[0.413]
N	1981	1981	1981

Source: Authors' calculations from the Kentucky WPRS Experiment. We removed the profiling tie group fixed effect from each dependent variable before calculating quantile regressions using the transformed data. Bootstrapped standard errors are reported in parentheses (500 replications), and two-tailed p-values are given in brackets.

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