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Using State Administrative Data to Measure Program Performance

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ABSTRACT

Using State Administrative Data to Measure Program Performance*

This paper uses administrative data from Missouri to examine the sensitivity of job training program impact estimates based on alternative nonexperimental methods. In addition to simple regression adjustment, we consider Mahalanobis distance matching and a variety of methods using propensity score matching. In each case, we consider estimates based on levels of post-program earnings as well as difference-in-difference estimates based on comparison of pre- and post-program earnings. Specification tests suggest that the difference-in-difference estimator may provide a better measure of program impact. We find that propensity score matching is generally most effective, but the detailed implementation of the method is not of critical importance. Our analyses demonstrate that existing data available at the state level can be used to obtain useful estimates of program impact.

JEL Classification: J2, J6, C1

Keywords: program evaluation, matching, job training

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I. Introduction

There has been growing interest on the part of governments in evaluating the efficacy of various programs designed to aid individuals and businesses. For example, state legislatures in California, Illinois, Massachusetts, Oregon, and Texas have all mandated that some type of evaluation of new state welfare programs be undertaken. In addition, the federal government has required that federally funded training and employment programs administered at the state and local level meet standards based on participant employment outcomes.

However, the best way for states to conduct evaluations remains an unanswered question. Early efforts to evaluate the effect of government-sponsored training program such as the Manpower Development Training Act (MDTA) or the Comprehensive Employment Training Act (CETA) focused on choosing the appropriate specification of the model in the presence of nonrandom selection on unobservables by participants in the program (Ashenfelter, 1978; Bassi, 1984; Ashenfelter and Card, 1985; Barnow, 1987; Card and Sullivan, 1988). This research culminated in the papers by LaLonde (1986) and Fraker and Maynard (1987), which concluded that nonexperimental evaluations had the potential for severe specification error and that the only way to choose the correct specification for the model is through the use of experimental control groups. This led both researchers and policy makers to argue that the only appropriate way to evaluate government sponsored training and education programs is through the use of randomized social experiments.

However, recent critiques of social experiments (Heckman and Smith, 1995; Heckman, LaLonde and Smith, 1999) argue that randomized experiments have important shortcomings that limit their usefulness in policy making. They point out that social experiments are seldom

implemented appropriately, raising serious questions about whether control groups are truly random samples. In addition, if one wants to evaluate the long-term impact of a program, randomized social experiments can be costly to implement since they require evaluators to collect data from both program participants and nonparticipants over an extended period of time. Finally, even when properly implemented, estimates of impact based on social experiments may not be directly relevant for policy makers in deciding whether to create new programs or to expand existing ones (see also Manski, 1996).

Based in part on these concerns, recent research has begun examining alternative, nonexperimental methods for evaluating government programs (Rosenbaum and Rubin, 1983; Heckman and Hotz, 1989; Friedlander and Robins, 1995; Heckman, Ichimura, and Todd, 1997, 1998; Heckman, Ichimura, Smith, and Todd, 1998; Dehejia and Wahba, 1999, 2002; Smith and Todd, forthcoming). The results from these papers suggest that there is no magic methodology that will always produce unbiased and useful estimates of program impacts. Instead program evaluation requires researchers to first adopt a methodology that is suitable for the question they want to address, second, to perform appropriate specification tests, and finally, to use data that is appropriate for estimating the parameters of interest. The results from these papers also suggest that, conditional on having the appropriate set of observable characteristics for both participants and nonparticipants and the use of appropriate statistical methods, it may be possible to evaluate government-sponsored training programs using existing data sources. If this is the case, there are tremendous opportunities for evaluating programs because most states already possess rich data sets on participants in various state programs that are used to administer these programs, as well as data on earnings for almost all workers in the state. Thus, it may be

possible to evaluate government training programs without resorting to expensive experimental evaluations, while simultaneously producing estimates of program impacts that are useful for policy makers.

The goal of this paper is to use administrative data from one state, Missouri, to examine the sensitivity of estimates of program impacts across alternative evaluation methods and alternative outcome variables. We also examine the sensitivity of our results to the quality of the data available for analysis. We assess the estimates from different methods by comparing them to each other, and also by comparing them to estimates of program impacts based on experimental methods that have been reported in the literature. In addition, we conduct a number of specification checks of our evaluation methods. The methods we consider are: simple difference, regression analysis, matching based on the Mahalanobis distance, and matching based on propensity score. For propensity score matching we also consider a number of alternative ways to match participants with nonparticipants such as pairwise matching, pairwise matching with various calipers, matching with and without replacement, matching using propensity score categories, and kernel density matching. Finally, for each method we also construct estimates based on the level of the outcome variable as well as a difference-in-difference estimate.

The program we examine is Missouri's implementation of job training programs under the federal Job Training Partnership Act (JTPA). Our data on participants come from information collected by the state of Missouri to administer this program. Our control group consists of individuals registered with the state's Division of Employment Security (ES) for job exchange services. Our data on earnings and employment history come from the Unemployment Insurance (UI) program in the state. These data have a number of features that make them ideal

for use in evaluating government programs. First, they contain very detailed location information allowing us to compare individuals in the same local labor market. Second, they allow us to identify individuals in our comparison group who are currently participating or who have recently participated in the JTPA program. Thus, we can avoid the problem of contamination bias, which occurs when individuals in the comparison group are either current or recent participants in the program being evaluated.¹ Finally, the data on wages and employment history are being generated by the same process for both participants and nonparticipants. Results in Heckman, Ichimura, Smith, and Todd (1998) indicate that these factors are critical in constructing an appropriate nonrandom comparison group. In addition, the data we have from Missouri is similar to administrative data collected by other states in implementing various workforce development and UI programs, so it should be possible to use the results from our study when conducting evaluations of other states' programs.

Our specification tests suggest that, when we use the difference-in-difference estimator, we are constructing comparison groups that are very similar to our participant group in terms of earnings growth, meaning we are comparing individuals who are comparable on relevant dimensions. In addition, we find that our estimates are insensitive to the method used for constructing comparison groups, which is exactly what one would expect conditional on having the appropriate set of observable characteristics for both participants and nonparticipants. Finally, we find that our estimates of the impact of the JTPA program on earnings are similar to previous estimates of the effect of JTPA based on data from randomized experiments (Orr, et al.,

¹ We do not know whether individuals in our comparison sample are participating in other government-sponsored training programs or private training programs. Therefore, there could be other sources of contamination bias.

1996). While certainly not definitive, these results do suggest that it is possible to evaluate government programs such as JTPA using administrative data that are currently being collected by most state governments.

The remainder of the paper is as follows. In the next section we discuss the various methods we use to construct our nonexperimental comparison groups, and section III contains a discussion of our data. Section IV presents our main results. In Section V we examine the sensitivity of our results to the quality of the data used in the analysis. Section VI concludes.

II. Methods for Creating Nonexperimental Comparison Groups

Our goal is to estimate the effect of participating in the JTPA program on program participants. In order to make the discussion concrete, we will focus on a single outcome measure. In the case of our study, we examine earnings following the treatment. Let us specify that Y_1 is earnings for an individual following participation in the program while Y_0 is earnings for that individual in the absence of participation. Let these be functions of measured individual characteristics, listed in the vector Z ,

$$Y_0 = \mu_0(Z) + U_0, \quad Y_1 = \mu_1(Z) + U_1. \quad (1)$$

We take

$$\mu_0(Z) = E(Y_0|Z), \quad \mu_1(Z) = E(Y_1|Z),$$

so that U_0 and U_1 are deviations from expected values, reflecting unmeasured factors.

If it were possible to observe Y_0 and Y_1 for each individual, a measure of the distribution of gains due to participation in the program as a function of Z could then be calculated.

However, it is impossible to observe both measures for a single individual. If we define $D=1$ for those who participate, and $D=0$ for those who do not participate, the outcome we observe for an individual is

$$Y = (1 - D)Y_0 + DY_1.$$

Experimental evaluations employ random assignment to the program, assuring that the treatment is independent of Y_0 and Y_1 and the factors influencing them. If we let $R=1$ for those participants who are randomly assigned to receive treatment and $R=0$ for those participants who are assigned to the control group then,

$$E(Y_1 - Y_0 | D=1) = E(\Delta Y | D=1) = E(Y | R=1, D=1) - E(Y | R=0, D=1). \quad (2)$$

Of course, in practice, such measures of program impact pertain not to all individuals but to the population who face randomized assignment.

Where D is not independent of factors influencing Y , participants may differ from nonparticipants in many ways, including the effect of the program. In many cases, we are interested in the effect of the program on the participants, $E(\Delta Y | D=1)$. However, with nonrandom assignment, the simple difference in the outcome between participants and nonparticipants does not identify program impact,

$$E(Y | D=1) - E(Y | D=0) = E(\Delta Y | D=1) + [E(Y_0 | D=1) - E(Y_0 | D=0)]. \quad (3)$$

The term in brackets identifies bias due to the fact that, even if they had not participated in the program, those who do participate would have faced different outcomes than nonparticipants.

Matching and adjustment methods estimate $E(\Delta Y|D=1)$ under the assumption that, conditional on measured characteristics, participation is independent of outcomes,

$$(Y_0, Y_1) \perp\!\!\!\perp D|X. \quad (4)$$

where X is another vector of individual characteristics that determine participation.²

If this condition holds, we know that

$$E(\Delta Y|X) = \mu_1(X) - \mu_0(X).$$

Under this assumption, since $\mu_1(X)=E(Y_1|D=1,X)$ and $\mu_0(X)=E(Y_0|D=0,X)$ and both μ_1 and μ_0 are observable, then the estimated impact of the program on participation is simply the difference in mean earnings, conditional on X , between participants and nonparticipants. Since participation is determined by X , once we condition on X , the difference in earnings between the two groups is an unbiased estimate of the effect of a program on participants.

Based on (3), it can be seen that a weaker assumption than (4) suffices to allow estimation of the impact of the program on participants. If nonparticipant outcomes are independent of participation, conditional on X ,

$$Y_0 \perp\!\!\!\perp D|X, \quad (5)$$

the program effect can be written as

$$E(\Delta Y|D=1,X) = \mu_1(X) - \mu_0(X) + E(U_1|D=1) = g(X) - \mu_0(X).$$

Since $g(X) = E(Y_1|D=1,X)$, and since $g(x)$ is observable, the impact of the program on participants is again straightforward to estimate. We should emphasize that all matching

² In general, X and Z do not have to contain the same characteristics.

techniques assume some version of (5). They differ in how they condition participation on X .

Although it is convenient to explicate estimation techniques in terms of a single population from which a subgroup receives the treatment, in practice treatment and comparison groups are often separately selected. The combined sample is therefore “choice based,” and conditional probabilities calculated from the combined sample do not reflect the actual probabilities that individuals with given characteristics face the treatment in the original universe. However, the methods used here require only that condition (5) apply in the choice-based sample. Furthermore, if (5) applies in the population from which the treatment and comparison groups are drawn, (5) will also apply (in the probability limit) in the choice-based sample where probability of inclusion differs for treated and untreated individuals, as long as selection criteria for the two groups do not depend on unmeasured factors.³

Simple Regression Adjustment

Given (4) the most common approach (e.g., Barnow, Cain and Goldberger, 1980) to estimating program impact is to assume that the earnings function is the same for participants and the comparison groups, except for a shifter δ ,

$$\mu_1(X) = \mu_0(X) + \delta.$$

Further assuming a linear functional form, δ is estimated, along with the vector of parameters of the earnings function, β , by fitting the equation

³As Smith and Todd (forthcoming) note, because choice-based sampling imposes a nonlinear transformation on the probability of treatment, estimates using the methods outlined below are not necessarily invariant under alternative choice-based sampling schemes. However, theory does not suggest that estimates based on one sampling scheme dominate another, and in the limit estimates should converge.

$$Y = X\beta + \delta D + e,$$

where e is an error term independent of X and D . Although this approach can be pursued using more flexible functional forms, even with modifications, estimates of program impact rely on a parametric structure in order to compare participants and nonparticipants. Where the support of X differs for participants and the comparison group, these methods extrapolate outside the sample range, and, in effect, compare individuals who are not comparable.

Matching Methods

Methods that focus more explicitly on matching by X are designed to ensure that estimates are based on outcome differences between comparable individuals. Where the set of relevant X variables is small and each has a very limited number of discrete values, it may be possible to calculate sample means that are direct estimates of $\mu_0(X)$ and $\mu_1(X)$. The estimated impact of the program is then,

$$E(\Delta T \mid D = 1) = \frac{1}{N} \sum [\bar{\mu}_1(X) - \bar{\mu}_0(X)]N(X),$$

where “ $\bar{\cdot}$ ” identifies sample means for particular values of X , $N(X)$ is the number of participants with values X , N is the total number of participants, and the summation is across all values of X .

In most cases, there are too many observed values of X to make such an approach feasible.

A natural alternative is to compare cases that are “close” in terms of X . Several matching approaches are possible. In the analysis here, we will first consider nearest neighbor pair matching, in which each participant is matched with one individual in the comparison group, and where no comparison case is used for more than one match. We also consider variations on this

basic matching technique. We then turn to methods based on grouping cases with similar measured characteristics.

Mahalanobus Distance Matching

We first undertake pair matching according to Mahalanobis distance. If we specify X' as the vector of observed values for a participant and X'' for a comparison individual, the distance between them is calculated as,

$$M(X', X'') = (X' - X'')^T V^{-1} (X' - X'')$$

where V is the covariance matrix for X . Mahalanobis distance has the advantage that matching will reduce differences between groups by an equal percentage for each variable in X , assuming that V is the same for the two groups.⁴ This ensures that the difference between the two groups in any linear function will be reduced (Rosenbaum and Rubin, 1985). Friedlander and Robins (1995) illustrate the use of Mahalanobis distance in program evaluation.

The simplest and most common pair matching approach begins by ordering participants and the comparison group members randomly. The first participant is matched to the comparison group member that minimizes $M(X', X'')$. The matched comparison group member is then eliminated from the set, and the second participant is matched to the remaining comparison group member that minimizes $M(X', X'')$. The process continues through all participants until the participant or comparison group is exhausted.

⁴ In practice one must estimate V using either the sample of participants or nonparticipants or using a weighted average of the covariance matrices from the two groups. We follow most of the previous literature in estimating V as a weighted average of the covariance matrices from participants and nonparticipants with the weights being the proportion of each group in the data. Calculating V in this manner minimizes sampling error.

One problem with the simple matching procedure is that the resulting matches are not invariant to the order in which the data are sorted prior to matching. Therefore, we also considered a modified matching procedure in which we not only compare the distance between the participant and all comparison group members but also compare the distance for all members of the comparison group that were previously matched to participants. Here, a prior match is broken and a new match formed if $M(X', X'')$ from the new match is smaller than that of the previous match. The participant in the broken match is then rematched, in accord with the same procedure. The advantage of the second procedure is that the results will be invariant to the ordering of the data.

Of course, if the comparison group contains sufficient numbers of cases with very similar values on all X , the matching procedure will produce directly comparable groups. In most cases, however, there remain substantial differences between matched pairs. We try to account for this in two ways. First, we examine the impact of additional regression adjustment on estimates of program impact. Second, we drop the one percent of the matches with the largest distance.

Propensity Score Matching

In the combined sample of participants and comparison group members, let $P(X)$ be the probability that an individual with characteristics X is a participant. Rosenbaum and Rubin (1983) show that

$$(Y_0, Y_1) \perp\!\!\!\perp D \mid X \Rightarrow (Y_0, Y_1) \perp\!\!\!\perp D \mid P(X).$$

This means that if we consider participant and comparison group members with the same $P(X)$, the distribution of X across these groups will be the same. Based on this “propensity score,” the matching problem is reduced to a single dimension. Rather than attempting to match on all

values of X , we can compare cases on the basis of propensity scores alone. In particular,

$$E(\Delta Y|P) = E_p(E(\Delta Y|X)),$$

where E_p indicates the expectation across values of X for which $P(X)=P$ in the combined sample.

This implies that

$$E(\Delta Y|D=1) = E_x(\Delta Y|P(X)),$$

where E_x is the expectation across all values of X for participants.⁵ We estimate $P(X)$ using a logit specification with a highly flexible functional form allowing for nonlinear effects and interactions. We first undertake one-to-one matching based on the propensity score using the methods described in the previous subsection. We also use a refinement of simple matching where we remove matches for which the difference in propensity scores between matched pairs exceeds some threshold or caliper. This is referred to as “caliper matching.” In the analysis we use calipers ranging from 0.05 to 0.2.

We also consider two alternative matching or weighting functions. The first is what we call matching by propensity score category or strata. Let the k^{th} category or strata be defined to include all cases with values of X such that $P(X) \in [P_1^k, P_2^k]$. Let N_k' be the number of participants within the k^{th} strata, N_k'' the number of individuals in the comparison group within the k^{th} strata, and N the total number of participants in our sample. Our estimate of the treatment effect within strata k is given by:

$$E_k(\Delta Y) = E(\Delta Y | P(X) \in [P_1^k, P_2^k]) = \sum_{i=1}^{N_k'} \frac{1}{N_k'} Y_{i1} - \sum_{j=1}^{N_k''} \frac{1}{N_k''} Y_{j0} \quad (6)$$

⁵ See Angrist and Hahn (1999) for a discussion of whether propensity score matching is efficient relative to matching on all the X s.

Our estimated average treatment effect across all strata is then given by:

$$E(\Delta Y) = \sum_k \frac{N'_k}{N} * E_k(\Delta Y). \quad (7)$$

In choosing P_1^k and P_2^k we follow the algorithm outlined in Dehejia and Wahba (2002). In particular, we choose P_1^k and P_2^k such that remaining differences in X between participants and nonparticipants within the strata are likely due to chance.⁶

Our second approach is the kernel matching procedure described in Heckman, Ichimura and Todd (1997), and Heckman, Ichimura, Smith and Todd (1998). The kernel matching estimator is given by:

$$E_k(\Delta Y) = \frac{1}{N^T} \sum_{i \in T} \left[Y_{i,1} - \frac{\sum_{j=1}^{N_i^C} Y_{j,0} K\left(\frac{p_j - p_i}{b_w}\right)}{\sum_{j=1}^{N_i^C} K\left(\frac{p_j - p_i}{b_w}\right)} \right] \quad (8)$$

where T is the set of cases receiving the treatment and N^T is the number of treated cases; $Y_{i,1}$ and $X_{i,1}$ are dependent and independent variables for the i^{th} treatment case; $Y_{j,0}$ and $X_{j,0}$ are dependent and independent variables for the j^{th} comparison cases that is within the neighborhood of

⁶Although we have chosen to present (6) in such a way as to highlight the symmetrical contribution of treatment and comparison cases in the estimation, the average treatment effect specified in (7) is numerically identical to that where the mean for all comparison cases within the specified stratum is taken as the comparison outcome for each treatment case in that stratum. It therefore corresponds to the approach used by Dehejia and Wahba (2002).

treatment case i , i.e., for which $|P(X_j^t) - P(X_j^c)| < b_w/2$; N_i^C is the number of comparison cases within the neighborhood of i ; $K(\bullet)$ is a kernel function; and b_w is a bandwidth parameter. In general, a kernel is simply some density function, such as the normal. In practice, the choice of $K(\bullet)$ and b_w is somewhat arbitrary. In our analysis we experiment with alternative choices of both and, as we indicate below, our results appear insensitive to our choice.

Additional Issues in Implementing Matching

There are a number of additional choices about how one actually forms a matched sample, such as the choice of whether to match with or without replacement, the choice of the number of nearest neighbors, the use of a caliper when matching, and the size of the strata or bandwidth, that warrant further discussion. The choice among these various options often involves a trade-off between bias and efficiency. For example, matching with replacement will, in general, produce closer matches than matching without replacement and therefore will result in estimates with less bias. However, matching with replacement can also increase sampling error because an individual in the comparison group can be matched to more than one individual in the treatment group. Similar tradeoffs exist in deciding how many comparison cases to match with a given treatment case. Using a single comparison case minimizes bias, while using multiple comparison cases can improve precision. Similarly, a caliper has the effect of omitting comparison cases that are poorly matched, reducing bias at the possible cost of precision.⁷

Which methods are appropriate depends on the overlap in the matching variables for the treatment and the comparison samples; the appropriate matching methodology also is a function of the quality of the data. To assess the effects these choices have on our estimates, we present

⁷See Dehejia and Wahba (2002).

results based on a variety of matching methods. In particular, we present results based on matching with and without replacement, matching to one, five and ten nearest neighbors, and matching using several different calipers. We also try a number of alternative bandwidths when conducting kernel matching. In addition, as we mentioned previously, we present estimates based on standard matching without replacement, where the match is dependent on the order of the data, as well as estimates based on modified matching, where the matches are independent of the order of the data.

For each of the alternative methods for estimating the effect of the program, we construct two different estimators, a levels estimator and a difference-in-difference estimator. The levels estimator is based on the difference in post-program earnings between the treatment and comparison sample. The difference-in-difference estimator is based on the difference for the comparison and treatment samples in the difference between pre- and post-program earnings. The advantage of the difference-in-difference estimator is that it allows one to control for any unobserved fixed individual factors that may affect program participation and earnings. Therefore, the difference-in-difference estimator is more likely to meet the assumption underlying matching that the determinants of program participation are independent of the outcome measure once observable characteristics are accounted for. The disadvantage of the difference-in-difference estimator, particularly in this setting, is that if there are any transitory shocks to pre-program earnings that affect program participation, this could bias the difference-in-difference estimator. Problems of estimating program effect in the presence of the now famous “Ashenfelter dip,” where earnings for participants fall shortly before participation, illustrates the potential bias. Since the Ashenfelter dip is a transitory decline in earnings, later

earnings are expected to increase even in the absence of intervention. If pre-program earnings are measured during the Ashenfelter dip, the difference-in-difference estimator will produce an upward biased estimate of the program's impact. Therefore, when measuring pre-program earnings we will try to do so prior to the onset of the Ashenfelter dip.

Matching Variables

The assumption that outcomes are independent of the treatment once we control for measured characteristics depends critically on the particular measured characteristics available. Any characteristic that is associated both with program participation and the outcome measure for nonparticipants, after conditioning on measured characteristics, can induce bias. It has long been recognized that controls for the standard demographic characteristics such as age, education and race are critical. Labor market experience of the individual is also clearly relevant. Where program eligibility is limited, factors influencing eligibility have usually been included as well. LaLonde (1986) includes controls for age, education, race, employment status, prior earnings, residency in a large metropolitan area, as well as measures associated with eligibility in the program, which were prior year AFDC receipt and marital status.

Several recent analyses (Friedlander and Robins, 1995; Heckman and Smith, 1999) have stressed the importance of choosing a comparison group in the same labor market. Since it is almost impossible to choose comparison groups in the same labor market as participants when drawing comparison groups from national samples, approaches that use these data are unlikely to produce good estimates, even if they are well matched on other individual characteristics. There is also a growing recognition that the details of the labor market experiences of individuals in the period immediately prior to program participation are critical. In particular, movements into and

out of the labor force and between employment and unemployment in the 18 months prior to program participation are strongly associated with both program participation and expected labor market outcomes (Heckman, Ichimura and Todd, 1997; Heckman, Ichimura, Smith and Todd, 1998; Heckman, LaLonde and Smith, 1999).

Finally, Heckman, Ichimura and Todd (1997) have argued that differences in data sources, resulting from different data collection methods, are an important source of bias in attempts to estimate program impact using comparison groups.

III. The Data

The data for this project are administrative data deriving from three sources. The first is Missouri's JTPA program, from which we draw our sample of program participants. The second is Missouri's Division of Employment Security (ES), from which we draw our comparison group sample. The final source is wage record data from the Unemployment Insurance programs in Missouri and Kansas. Using these data we obtain both pre- and post-enrollment earnings and information on labor force status prior to enrollment for both participants and nonparticipants.

The JTPA data comprise all individuals who apply to and then enroll in the JTPA program. The data include basic demographic and income information collected at the time of application that is used to assess eligibility, as well as information about any subsequent services received. Our initial sample consists of all applicants in program years 1994 (July 1994 through June 1995) and 1995 (July 1995 through June 1996) who are at least 22 years old and less than 65 and who subsequently enroll in the Title IIa program. We focus on participants 22 years old and older because younger individuals are eligible for the youth program, which is governed by a

different set of rules. Participants in Title IIa are eligible to participate in the JTPA program because they are judged to be economically disadvantaged.⁸ We focus on these participants because they are a fairly homogeneous group and because they have been the focus of previous evaluations of JTPA using experimental data (e.g., Orr, et al., 1996). Finally, we eliminate records with invalid values for our demographic variables (race, sex, veteran status, education, and labor force status).⁹ Our final sample consists of 2802 males and 6393 females.

Our Employment Security (ES) data include all individuals who applied to the ES employment exchange service in program years 1994 and 1995. With some exceptions, individuals who receive Unemployment Insurance payments in Missouri were required to register with ES during this period, although it is not clear how strictly this requirement was enforced. In general, ES employment services were not very intensive. Assistance could take a variety of forms such as providing access to a list of job openings in an area, helping individuals prepare resumes, referring individuals to jobs, or referring individuals to other agencies for more extensive training programs. All residents of Missouri were eligible to receive the basic ES services such as access to the list of job openings or assistance in preparing a resume. During the time of our sample almost every individual who wanted to obtain services from ES applied at one of the Employment Security offices located around the state.¹⁰ The ES data contain basic

⁸ JTPA also serves Title III participants, who are eligible for the program because they were displaced from their previous job.

⁹ We eliminate around 10 percent of the original sample because of invalid or missing demographic variables.

¹⁰ Subsequently many of these services became available on-line so individuals no longer needed to go into an ES office and register before obtaining the services.

demographic and income information obtained on the initial application, as well as information about subsequent services received.

When selecting our ES sample we chose individuals who were between 22 and 65 years old and were deemed economically disadvantaged. Since the ES program used the same criteria to determine whether someone was economically disadvantaged as the JTPA program, all of our ES participants should be eligible to participate in the JTPA program. In addition to these criteria we also chose ES participants who were not enrolled in JTPA in the program year.¹¹ We further eliminated records with missing or invalid demographic variables.¹² Our final sample consists of 45,339 males and 52,895 females.

The pre-enrollment and post-enrollment earnings for both our JTPA and ES samples come from the Unemployment Insurance (UI) data. These data consist of quarterly files containing earnings for all individuals in Missouri and Kansas employed in jobs covered by the UI system.¹³ Both the JTPA and ES data are matched to the UI data using Social Security Number (SSN). If we are unable to match an SSN to earnings data in a quarter, we considered the individual not employed in that quarter and set earnings equal to zero.

Using these earnings data, we determined total quarterly earnings from all employers for individuals in the eight quarters prior to participation, in the quarter they begin participation, and

¹¹ Although we eliminate individuals in the ES sample enrolled in the JTPA program we do not eliminate individuals receiving training through other private or public training programs.

¹² Approximately 10 percent of the original ES sample was eliminated due to missing or invalid demographic variables.

¹³ Inclusion of Kansas wage record data is valuable since a substantial number of Missouri residents in Kansas City and surrounding areas work in Kansas. The number of Missouri residents commuting across state lines is not significant elsewhere in the state.

in the subsequent eight quarters. For our levels estimator we use post-program earnings measured as the sum of earnings in the fifth through the eighth quarters after the initial quarter of participation. For our difference-in-difference estimator, we measure the difference between the sum of earnings in the fifth through the eighth quarters after the initial participation quarter and the fifth through the eighth quarters prior to the initial quarter of participation. As Ashenfelter and Card (1985) note, taking differences for periods symmetric around the enrollment quarter assures that the difference-in-difference estimator is valid in the case where there is autocorrelation in the transitory component of earnings. In order to capture the dynamics of earnings immediately prior to participation, we also control for earnings in the first through the fourth quarters prior to the initial quarter of participation.

Previous research (Heckman and Smith, 1999) found that the dynamics of an individual's prior labor market status is an important determinant of both program participation and subsequent earnings. We capture these dynamics using a series of four dummy variables. From both the JTPA and ES data we know whether an individual is employed at the time of enrollment. From the UI data we know whether an individual is employed in each of the eight quarters prior to enrollment. For an individual employed at the time of enrollment, we coded the transition as not employed/employed if earnings were zero in any of the eight quarters prior to enrollment and coded it as employed/employed if earnings in every quarter were positive. An individual not employed at the time of enrollment was coded employed/not employed if earnings were positive in any of the prior eight quarters and not employed/not employed otherwise.

Previous research has also found local labor market conditions to be an important determinant of program participation (Heckman, Ichimura, Smith and Todd, 1998). We capture

this effect by including a dummy variable for the Service Delivery Area (SDA) where an individual lives.¹⁴

Our measure of labor market experience is defined as:

$$\text{Experience} = \text{Age} - \text{Years of Education} - 6.$$

We also include dummy variables indicating whether someone was employed in each of the four quarters prior to participation, to capture labor market experience immediately prior to participation. Someone is considered employed in a quarter if earnings are greater than zero.

Table 1 presents summary statistics for our JTPA and ES samples separately for males and females. For most of the demographic variables the two samples are similar.¹⁵ However, looking at the labor market transition variables we see that JTPA participants are much more likely to be not employed over the entire eight quarters prior to beginning participation. Looking at earnings we see that mean post-enrollment earnings are similar for the two samples but that mean pre-enrollment earnings are lower for JTPA participants, particularly for female participants. The numbers in Table 1 demonstrate that there are differences in the JTPA and ES samples, particularly in earnings and employment dynamics prior to participation. This suggests that we need to account for these differences when estimating the impact of program participation on JTPA participants.

One of the conclusions reached by Heckman, LaLonde, and Smith in their chapter on program evaluation in the *Handbook of Labor Economics* (Heckman, LaLonde, and Smith, 1999)

¹⁴ There are 15 SDAs in Missouri.

¹⁵ We have modified both the occupation and education variables to ensure that they are comparable across the two files. The details of the modifications we made are provided in the Data Appendix.

is that "better data help a lot" (pg. 1868) when evaluating government-sponsored training programs. The most important criteria they mention are that outcome variables should be measured in the same way for both participants and non-participants, that members of the treatment and comparison groups should be drawn from the same local labor market, and that the data should allow one to control for the dynamics of an individual's labor force status prior to enrollment. Since our data meet all of these criteria, we feel they are ideal for examining the impact of government-sponsored training programs.¹⁶ An additional advantage that we should mention is that Missouri is not unique. Almost every state in the union collects similar administrative data. Therefore, the type of analysis we perform could be conducted for other states as well. We next turn to examining the effects of alternative methods for constructing comparison groups on the estimated impact of treatment.

IV. Estimates of Program Effects Using Alternative Methods to Form Comparison Groups

Specification Analysis

Before presenting our estimates of the effect of the JTPA program on participants, we first want to compare our various treatment and control samples and present the results from specification tests in order to assess whether our matching methods produce valid comparison samples. In this analysis we will focus on two variables, the sum of individual earnings in the eighth through the fifth quarters prior to beginning participation—what we call pre-program

¹⁶ Our data are somewhat limited in this latter criterion in that we are only able to identify the transition between working and not working as opposed to identifying the transition between employed, unemployed, and out of the labor force.

earnings—and the difference in earnings between the fifth and eighth quarters prior to beginning participation—what we call the growth in pre-program earnings. Our matching and adjustment procedures are based on individual demographic characteristics, and employment and earnings in the year prior to program enrollment, but our measure of pre-program earnings is not explicitly controlled in any of these approaches. Analysis of these earnings can therefore provide a specification test for our models. In particular, testing for differences between pre-program earnings for our treatment and comparison samples represents a formal test of our level estimator since our level estimator is based on post-program earnings (Heckman and Hotz, 1989). Testing for differences in the growth in pre-program earnings, although not a formal specification test, does provide some evidence on whether we have formed appropriate comparison samples for our difference-in-difference estimator.

Figure 1 plots quarterly earnings for our entire sample of JTPA and ES participants for the eight quarters prior to enrollment, the quarter of enrollment, and the eight quarters after enrollment. Earnings are plotted separately for men and women. Similar to Table 1, Figure 1 shows that JTPA and ES participants have very different earnings dynamics both prior to and after beginning participation. Prior to participation, the ES sample has much higher earnings levels and earnings growth than the JTPA sample. In addition, the Ashenfelter dip is present in both samples, although at somewhat different times. For the JTPA sample, quarterly earnings begin to decline four quarters prior to participation, whereas for ES participants earnings begin to decline one quarter prior to participation. The fact that earnings begin to decline four quarters prior to participation for JTPA participants is primarily why we measure pre-program earnings in the eighth through the fifth quarters when constructing the difference-in-difference estimator.

Figure 2 presents the same information as Figure 1 for our treatment and comparison samples created by matching on the Mahalanobis distance. For each participant in the JTPA sample, we choose a case from the comparison file for which the Mahalanobis distance is at its minimum, yielding a paired file. This pair matching method ensures that if there is at least one individual in the comparison sample that is similar on all values to each participant, the resulting matched comparison group will display the same variable distribution.¹⁷ In calculating the Mahalanobis distance, the characteristics in X' and X'' include education, race, prior experience, occupation (nine categories), our measures of employment status dynamics prior to enrollment (three dummy variables), dummy variables for whether an individual lived in either the St. Louis or Kansas City SDA, earnings for the four quarters prior to enrollment, and dummy variables indicating whether an individual was employed in each of the four quarters prior to enrollment.

The figure shows that while matching has produced a comparison sample with mean earnings prior to participation that are closer to the mean earnings of the treatment sample, they are still not identical. However, it does appear that the treatment and comparison samples experience similar growth in earnings prior to participation. Also, the timing of the Ashenfelter dip corresponds more closely for these two groups, although earnings begin falling one or two quarters earlier for JTPA participants and the decline in earnings is smaller for ES participants. The fact that earnings in the four quarters prior to participation are higher for ES participants is surprising since these earnings are included in the X vector used for matching. This suggests that Mahalanobis distance matching may fail to select a comparison group that corresponds closely

¹⁷ The matching method used here is that described by Rosenbaum and Rubin (1985). We describe it in detail above.

even on the variables that are used in the matching process. We will see below that propensity score matching is generally more effective.

One of the advantages of propensity-score matching is that the propensity score provides a simple measure to compare the overlap between the treatment and comparison samples. Properly estimating the effect of a program requires one to compare comparable individuals, which will occur only when the two samples have common support. In addition, the amount of overlap between the treatment and control sample determines the appropriate method to use when matching the treatment and comparison sample and will affect the quality of the resulting match. Figure 3 presents the distribution of propensity scores for both the JTPA and ES participants, separately for males and females. To estimate the propensity score we use a logit function to predict participation in the sample combining the JTPA and ES samples. In addition to the variables used when matching based on Mahalanobis distance, we tested nearly 300 interactions between these variables, using a stepwise procedure to enter all interactions that were statistically significant at the five percent level.¹⁸

While Figure 3 shows that a larger percentage of ES participants have a propensity score between 0.0 and 0.1, the overlap between ES and JTPA participants spans the entire range from 0.0-1.0. This suggests that, conditional on the assumptions of propensity score matching, it will be possible to form samples of comparable individuals. The large overlap between the ES and JTPA samples also suggests that it will be possible to produce close matches using matching without replacement. This is why we have chosen to focus on matching without replacement in our subsequent analysis.

¹⁸ The results from these logit estimations are available from the authors upon request.

Figure 4 provides evidence on the comparability of our samples matched using the propensity score. This figure plots quarterly earnings both prior to and after the initial quarter of participation for our treatment and comparison samples.¹⁹ Comparing Figure 4 with Figure 1 shows that matching using propensity score produces samples of JTPA and ES participants with similar pre-program earnings dynamics. Comparing Figures 2 and 4 shows that, relative to matching using Mahalanobis distance, matching using propensity score produces samples that match more closely on earnings in the four quarters immediately prior to participation. Since these variables are used in both matching procedures, this suggests that propensity score matching is more effective in practice. However, it is still the case that there are differences in pre-program earnings (earnings in the fifth through eighth quarters prior to participation), particularly for males.

Figure 5 plots earnings for a sample of JTPA and ES participants matched using the propensity score and applying a caliper of 0.1. In caliper matching we form optimal matches but then break any matches that are larger than the caliper value. Figure 5 shows that caliper matching produces samples that are very closely matched on earnings in the four quarters prior to the treatment, although there is still a difference in the level of pre-program earnings (the fifth through eighth quarters prior to participation) between the treatment and comparison samples. Of course, pre-program earnings are not used in the matching procedure, so this difference is not due to a technical shortcoming in the matching method.

Table 2 presents results from a more formal analysis of the difference in pre-program

¹⁹ These samples are formed using standard matching without a caliper. Further details on alternative matching procedures are provided below.

earnings levels and the difference in the growth in pre-program earnings between our treatment and comparison samples. The rows labeled “No regression adjustment” present the mean and standard error of the difference in either pre-program earnings level or the growth in preprogram earnings between our treatment and comparison samples. The rows labeled “Regression adjustment” present the coefficients and standard errors from a linear regression model where we include a dummy variable which equals one if an individual participated in JTPA. Controls in the model include the standard demographic variables (race, sex, experience, experience squared, and veteran status), years of education, along with a dummy variable identifying high school graduates and an additional term capturing years of schooling beyond high school, earnings in each of the four quarters prior to participation, dummy variables indicating whether the person worked in each of the four quarters prior to participation, our employment transition variables, nine occupation dummy variables, 15 dummy variables for each of the SDAs, and eight dummy variables indicating which calendar quarter an individual entered either the JTPA or ES program.²⁰

The results in Table 2 summarize what we have seen in Figures 1-2 and 4-5. We see that without controls, JTPA participants have appreciably lower earnings in the prior year (fifth through eighth quarters prior to enrollment), but that using regression adjustment or any of the matching procedures causes the difference to reverse. The differences in pre-program earnings for treatment and comparison samples are statistically significant (in the range of \$800 for males and \$400 for females) for all methods used. This suggests that there are differences between

²⁰ These are the control variables we use throughout the paper when we are estimating any linear regression.

treatment and control groups that are not captured through matching or the controls in our regressions. This implies that estimates based on post-program earnings will be based on a misspecified model since we will not be comparing comparable individuals. However, the results in Table 2 also show that there are much smaller, and in some cases insignificant, differences between some of our treatment and comparison samples in the growth of pre-program earnings. While not a formal specification test, these results do suggest that the unobserved difference between individuals in the treatment and comparison samples may be fixed over time and will be captured in our difference-in-difference specification. This, in addition to the fact that we measure pre-program earnings before the onset of the Ashenfelter dip, makes us optimistic that our difference-in-difference estimator may produce unbiased estimates of the effect of the program on participants.

Estimates of Program Effects without Matching

We now turn to presenting our estimates of program effects using comparison groups formed by matching based on alternative distance metrics and weighting schemes. We start by comparing the mean difference in earnings between our sample of JTPA and ES participants. Table 1 shows that the post-enrollment earnings differ between these two groups. The mean difference in post-enrollment earnings between these two samples, as well as the mean difference in the difference between pre- and post-enrollment earnings of the two samples are presented in line 1 of Table 3. Adult males in JTPA earn \$113 less than those in the ES sample while for adult females those in JTPA earn \$151 more than females in the ES sample. In contrast, males in the JTPA sample have almost a \$1100 greater increase in earnings relative to ES participants, while females in JTPA experience a \$1500 greater increase in earnings. Given the results in

Table 2, as well as the difference across groups in the mean values for other characteristics seen in Table 1, these earnings differences could easily be due to differences in pre-program characteristics.

Line 2 of Table 3 presents adjusted estimates of program effects based on the simple linear regression model. The structure of these regressions and the control variables included are described in the previous section. The coefficient estimates for the control variables are reported in Table A1 in the appendix. These coefficients generally correspond to expectations.

There are substantial differences in our level and difference-in-difference estimates of program impacts. For men the estimate based on post-program earnings is nearly \$1500, while the difference-in-difference estimate is only about \$630. For females, the levels estimate is just under \$1100, while the difference-in-difference estimate is about \$700. The results in Table 2 show that, even after regression adjustment, the levels estimate is based on a misspecified model. The differences in the two estimates could well be the result of unobserved differences between the two groups.

The critical question for regression adjustment is whether the functional form properly predicts what post-program wages would be for participants if they had not participated. As noted above, even under the maintained assumption that there are no unmeasured factors that distinguish participants from the comparison group, if differences in measured variables are great enough, regression adjustment may be predicting outcomes for participants by extrapolation. The large size of our comparison group has important advantages but it also entails substantial risks of misspecification. In particular, if most of the comparison sample has characteristics that are quite distinct from those of the participants, coefficients will be estimated based largely on

relationships for individuals with very different characteristics from participants. If the functional relationships differ by values of X , the regression function may be poorly estimated. There are no assurances regarding the direction of the bias for such regression adjustment.

Mahalanobis Distance Matching

One natural approach is to choose a selection of cases from the comparison group that have similar values to those of participants. One measure of similarity is the Mahalanobis distance metric. Line 3 of Table 3 shows our estimates of the program effects using the comparison sample formed by matching using the Mahalanobis distance. Comparing the estimates based on post-program earnings with the difference-in-difference estimates we again see that the difference-in-difference estimate is much smaller.

Line 4 of Table 3 presents our estimates of the effect of the program on participants using the matched samples and our basic linear regression model. To the extent that matching eliminates differences in the X s between the two samples, the estimates in lines 3 and 4 should be the same. While the estimates are similar for females, the regression produces a different estimate for males, suggesting that matching based on the Mahalanobis distance is not producing a sample with the same distribution of X s as the treatment sample. This is consistent with Figure 2, where we saw differences in the level and growth of earnings immediately prior to participation.

Line 5 in Table 3 presents our estimates based on our comparison sample matched using the Mahalanobis distance and our modified matching method. As we discussed above, when using the standard matching algorithm, the resulting matched sample depends on the order of the original data, whereas this is not the case with our modified matching algorithm. Line 6 presents

results based on a comparison sample created by using the standard matching algorithm but then dropping one percent of the sample with the largest Mahalanobis distance. Comparing the estimates in lines 3, 5 and 6 shows that all of these techniques produce similar estimates.

Propensity Score Matching

Matching cases on the basis of propensity score promises substantial simplification as compared with any general distance metric. The Mahalanobis distance between any pair of cases will approach zero only if all values of X are the same for both cases. In contrast, if cases are matched by propensity score, two cases with same propensity score will be matched perfectly even if values of X differ. The theory assures us that the distribution of independent variables will be the same across cases with a given propensity score, even when values differ for a particular matched pair.

As we indicate above, our estimate of $P(X)$ is based on a logit model. For each case the predicted value from our estimated logit function provides an estimate of $P(X)$. Table 4 presents our estimates of the program effects using a variety of methods for creating comparison samples based on $P(X)$. Since standard formulas for estimating standard errors do not reflect the fact that our samples are matched using $P(X)$, which is measured with error, all estimates of the standard errors are estimated using bootstrapping.²¹ Lines 1 and 2 of Table 4 present estimates based on comparison samples created using standard pair matching without replacement and without a caliper. Line 1 presents estimates without regression adjustment, while line 2 presents our estimates that correct for any difference between treatment and matched samples based on our

²¹ We estimate standard errors using a bootstrap procedure (100 replications) whenever our estimates are based on propensity score matching.

linear regression model. Comparing the estimates in line 1 based on post-program earnings with the difference-in-difference estimates again shows that these estimates are significantly different. Since our previous analysis suggests that the levels estimates are based on a misspecified model, we focus on the difference-in-difference estimates.

Comparing the regression adjustment estimates to the estimates in line 1 shows that regression adjustment has very little impact on the estimates. This is what one would expect if the matching was successful. These results show that the propensity score matching method is more successful than the Mahalanobis distance matching in creating an appropriate comparison sample.

Caliper matching differs from standard matching in that only matches within a specified distance are permitted, so not all treatment participants may be matched. Lines 3-6 show how our estimates differ when the caliper is set to 0.05, 0.1, and 0.2, respectively. The numbers in brackets show the number of cases in the treatment sample that are matched. In the full JTPA sample (after deletions of cases with missing data) there are 2802 males and 6395 females. Looking at the size of the sample when we impose the 0.05 sample, which is the most stringent caliper, shows that the sample size does not drop by very much. Even without imposing a caliper we are matching individuals with similar values of $P(X)$. Comparing our estimates in lines 3-6 with our estimates in line 1 shows that imposing a caliper has very little effect on our estimates of the program effect.

Comparing Pair Matching Algorithms

The matching algorithm used in the above analysis is the standard pair matching procedure. As we discussed in the previous section, we also consider a modified matching

procedure that produces matched samples that are insensitive to sample ordering and should increase the quality of the final matches. In searching the comparison sample to find a match, this alternative procedure not only compares unmatched cases but also previously matched cases, breaking previous matches if the new match distance is smaller.

Table 4 lines 7 and 8 present results using this alternative matching technique. The average difference in propensity scores between matched pairs was often appreciably smaller when this alternative was used. Nonetheless, it is clear that the effect of this alternative matching algorithm is small relative to estimated standard errors. This reflects the fact that although this method often selects a different comparison case to be matched with a particular treatment case, there is little impact on the overall comparison sample.

Matching with Replacement

Matching without replacement works well when there is sufficient overlap in the distribution of $P(X)$ between the treatment and comparison sample to ensure close matches. In cases where there is not sufficient overlap researchers often use matching with replacement, where an individual in the comparison sample can be matched to more than one person in the treatment sample. In order to examine the sensitivity of our estimates to this alternative matching strategy we have constructed matched samples using matching with replacement. We have also matched each person in the treatment sample to one, five, and ten nearest neighbors in the comparison sample. Our estimates based on these samples are presented in lines 9-11 in Table 4. Comparing these estimates with the estimates reported in line 1 again shows that this alternative matching method produces estimates that are quite similar to our original estimates. Equally important, standard errors are not substantially different across methods.

Matching by Propensity Score Category

All of the pair matching approaches described above have the important disadvantage that they require that we discard comparison group members who are not matched. In one-to-one matching, only one case from the larger sample can be used for each case in the smaller sample, resulting in an immediate loss of information. Where the distribution of participants and the comparison groups differ dramatically, either the matches will be poor, or, if a caliper is applied, additional cases will be lost.

Group matching relaxes the requirement that the two groups be matched on a one-to-one (or one-to-N) basis. In those regions of the data where there are some participants and some comparison group members, group matching allows us to use all the data. The only cases that must be discarded are those for which there are no similar cases in the other group. The approach we use is closely modeled on that recommended by Dehejia and Wahba (2002) and is described in section II.

In order to ensure that the propensity ranges were sufficiently small, we calculated the mean differences on our primary independent variables between participant and comparison groups within a propensity category. We first considered uniform propensity categories of size 0.1. However, given the large number of cases with propensity values less than 0.1, we found that differences in our basic variables within this the lowest group were often statistically significant. We ultimately created much smaller category widths at the lower end of the propensity distribution, corresponding approximately to deciles in the distribution of the combined sample.

The estimated program effects based on this approach are listed in Line 12 of Table 4.

The estimates are quite similar to our initial estimates although the standard errors are smaller.

Kernel Density Matching

The estimates based on propensity score categories use estimates of $E(\Delta Y|P)$ that are simple sample averages that combine cases with similar values of P . Following an approach outlined in Heckman, Ichimura and Todd (1997, 1998) and Heckman, Ichimura, Smith and Todd (1998), we employ a kernel density estimator to calculate the density of the propensity score and the means for post-program earnings by propensity score for participants and the comparison group.²² In forming our estimates we experimented with a variety of kernels and we considered bandwidths from 0.01 to 0.11. We found that the choice of kernel and bandwidth made little difference in our estimates. Therefore, we report estimates based on a Epanechnikov kernel using a bandwidth of 0.06. The results are reported in Line 13 in Table 4. These estimates are again similar to the other estimates reported in Table 4.

Summary of Estimated Program Effects

Table 5 presents selected estimates from Tables 3 and 4. We see that, in each case, the estimate based on Mahalanobis distance is the smallest one reported in Table 5, and usually the difference between this estimate and others is appreciable. Recall that results presented in Figure 2 suggested that Mahalanobis distance matching was not successful in producing samples that were comparable on the measures used for matching. Looking at the other methods that

²² Heckman, Ichimura and Todd (1997) and Heckman, Ichimura, Smith and Todd (1998) recommend using local linear regression matching, which is similar to kernel matching but, given the distribution of their data, has preferable properties. We tried local linear regression matching but, given the size of our samples, it was extremely time consuming to implement. In addition, the results we obtained with this approach were similar to the results obtained using kernel density matching, so we choose to focus on the results from the kernel density matching.

control for independent variables, we see that the range of estimates is moderate. Estimates differ by a maximum of about 30 percent, and in no case is the difference as great as two standard errors. Overall, the results in Table 5 show that, with the exception of Mahalanobis distance matching, which we have found does not effectively control independent variables, estimates of program effect on participants are relatively insensitive to the methods used to form comparison groups and weight the data.

As we have noted previously, our specification tests in Table 2 show that any estimates based on the level of post-program earnings are likely to be biased, as they depend on comparison between individuals whose pre-program earnings differ. However there is evidence in Table 2 suggesting that once individual fixed effects are removed, earnings patterns are similar, so that difference-in-difference estimates may be valid. Among the difference-in-difference estimates (omitting lines 1 and 3), we see that, for men, our estimates range from a low of \$628 to a high of \$856. For women the estimates range from \$693 to \$892.

Comparison with Previous Estimates of Treatment Effects Based on Randomized Control Groups

Table 6 compares our estimated program effects for enrollees with those reported in Orr et al. (1996, p. 107, Exhibit 4.6), which are based on an experimental evaluation of the JTPA program.²³ The Orr et al. estimates are for individuals who entered JTPA from November 1987 through September 1989, so we have adjusted their estimates for inflation so that they are comparable to our estimates. Since our estimates are for months 13-24 after assignment, we present the Orr et al. estimates for months 7-18 and months 19-30 after assignment. Comparing

²³ The estimates reported by Orr et al. include an adjustment for the fact that some of those assigned to treatment never enrolled. Since our data pertain to enrollees, this is the appropriate estimate for comparison.

our estimates for men with the Orr et al. estimates shows that our estimates lie between theirs. For women our estimates are below those reported by Orr et al. but the difference is not generally statistically significant. Our estimates based on nonexperimental data appear similar to the estimates produced using experimental data.

V. Robustness of Results to Limitations in Data Quality

The results reported in the previous section—especially those based on a difference-in-difference specification—suggest that program effect estimates are robust to alternative methods of matching and weighting the data. In this section we examine the sensitivity of our results to the quality of the data used to perform the analysis. We will focus on two key aspects of data quality, the observable characteristics for participants, and the size of the treatment and comparison samples.

Sensitivity of Results to Observable Characteristics

We begin by examining the robustness of our results to changes in the characteristics available for individuals. We will examine this by dropping variables from our analysis that previous researchers have found to be important when estimating program effects (see Heckman, LaLonde and Smith, 1999). The variables we will drop are our variables measuring employment transitions prior to entering the program, the SDA dummy variables, which capture an individual's local labor market, and the variables measuring employment status and earnings in the four quarters prior to participation. We will focus on estimates produced from treatment and comparison samples that are matched using propensity score matching with a 0.1 caliper.²⁴ For

²⁴ We use the standard pair matching algorithm without replacement.

this analysis, when we drop a set of variables, we reestimate the propensity score without those variables in the logit regression. Next we match the treatment and comparison sample using the new $P(X)$. We then compute the estimates using the new matched sample. We also drop the variables from any subsequent regression adjustment.

The results from this analysis are presented in Table 7. The first five lines of the table present estimates with no regression adjustment while lines 6-10 present results based on our linear regression model. The estimates in lines 1 and 6 are identical to the estimates found in lines 4 and 5 in Table 4 and are repeated here for ease in comparison.

Estimates of impact for males based on levels of post-program earnings appear to decline appreciably when controls for labor market experience are omitted, with wages in the four quarters prior to participation appearing most important. The impact of these measures is less important for females. The difference-in-difference estimates are not generally as sensitive to dropping any of these variables, but there are exceptions. For men, dropping the employment transition variables alone has a fairly large effect on the estimate, but dropping all three sets of variables results in estimates that are not substantially different from estimates produced controlling all of the variables. Effects for women display a different pattern, since dropping all three classes of variables has the largest impact, increasing estimated effects by about 25 percent. In addition, the regression adjustment has very little impact on our estimates. Overall, estimated impacts—especially the difference-in-difference estimates—appear remarkably robust to dropping these variables.

Sensitivity to Changes in Sample Size

In order to examine the sensitivity of our estimates to the size of the sample we vary our

sample in three ways. First, we set the size of the comparison sample equal to the size of the treatment sample. Second we reduce the size of the treatment sample by 90 percent, holding constant the size of the comparison sample. Finally we reduced the size of both the treatment and control sample by 90 percent. To form the smaller samples, we draw a sample of a given size with replacement from the original treatment and comparison samples. We then performed the analysis with this new sample. We repeated the process 100 times. For each repetition, we calculated program effects for regression adjustment, propensity score pairwise matching with a 0.1 caliper, and estimation based on propensity score category.²⁵ In each case, we present estimates based on levels of post-program earnings and difference-in-differences. Table 8 reports the mean and standard deviation of these 100 estimates of the program effect.

Comparing the mean estimates reported in lines 2-4 with the estimate based on the full sample reported in line 1 shows that changing the sample size does not usually have an effect on the expected estimate value, since most differences could easily be due to sampling error.²⁶ However, there are some exceptions. Four of the six mean values for the difference-in-difference estimator are substantially higher when the comparison sample is reduced to equal the treatment sample (line 2), and it is clear that this difference could not be due to chance. This suggests that having a sufficiently large comparison sample may be of importance in assuring that bias does not occur. Interestingly, the bias appears smaller in lines 3 and 4, as the treatment sample is

²⁵ Because we are using a caliper for pairwise propensity score matching, not all records in the treatment sample are matched. The number of matched records varies for the repetitions and depends on the actual sample drawn.

²⁶ With 100 replications, the standard error of the expected value of the estimate reported in the table is estimated as one-tenth of the standard deviation.

reduced. Of course, while there is clearly bias, it is modest relative to the standard deviation of the estimate, which is the appropriate measure of the standard error of the estimate in the reduced sample.

Comparing the standard deviation of our estimates in rows 2-4 with the standard error of our estimates reported in row 1 shows that reducing the sample size results in a substantially less precise estimate of the program effect. Focusing on the difference-in-difference estimates shows that the standard deviation of our estimates is sometimes as much as three times the standard error of the original estimate. The greatest increase occurs when we reduce the treatment sample size (in rows 3 and 4); estimated values would not generally be statistically significant, especially for men, in these cases.

In short, the primary effect of reducing the sample size is not an increase in the bias of the estimate but is instead a fall in the precision of the estimates, making it more difficult to find significant effects. Nonetheless, we find some evidence that a large comparison sample relative to the treatment sample may tend to reduce bias.

VI. Conclusion

Our results suggest that a variety of matching methods produce estimates of program effect that are quite similar if they are based on the same control variables. The most important exception is that we find Mahalanobis distance matching is less successful than the other methods in producing a comparison sample that is comparable. Regression adjustment, based on a simple linear model, seems to perform surprisingly well. We expect, however, that regression estimates may extrapolate beyond the range of the comparison data, so that they are not in fact

comparing comparable individuals (Heckman, Ichimura, and Todd, 1997; Heckman, Ichimura, Smith and Todd, 1998; Heckman, LaLonde and Smith, 1999). The slightly lower estimates produced by the simple regression methods as compared to those based on matching methods suggest that these estimates may suffer at least modest systematic bias.

Our specification tests suggest that program impact estimates based on post-program earnings levels are likely to suffer bias. In contrast, difference-in-difference estimators appear less likely to exhibit bias. Remarkably, difference-in-difference estimators are not only quite robust to the particular matching method that is used, but they also remain relatively stable in the face of changes in the available control variables.

Our results lead us to two conclusions. First, since matching by propensity group is the simplest method, and because it produces standard errors that are smaller than related methods, we suggest that those with nonexperimental data use this method in estimating program effects. However, other methods based on propensity score produce similar estimates, so any of these methods is likely to be acceptable. Second, since our data are typical of administrative data maintained by state governments, our results show that it is possible to apply these methods at the state level to obtain program impact estimates based on existing data sources.

Data Appendix

Occupational Codes

There are two major differences in the occupational variables in the JTPA and ES files. The first is the JTPA file contains many more records that have missing occupation codes than the ES file. Both programs ask applicants to report occupational information for their current or their most recent job. However, for applicants who have not been recently employed, this information was not considered relevant and is frequently left blank. As can be seen in Table 1, JTPA applicants are much more likely to have been unemployed for all eight quarters prior to beginning participating in JTPA, and this appears to account for why JTPA participants are more likely to have missing occupation data. In order to use occupational information for matching individuals, we felt it was important to ensure that the probability of having a missing occupation code was similar for comparable ES and JTPA participants. To accomplish this we first estimate the probability that a record in the JTPA file has a missing occupation code using a logit model. In this model we control for whether or not individuals were employed in the quarter of enrollment, whether they were employed in each of the four quarters prior to enrollment, their earnings in each of the four quarters prior to enrollment (with earnings set to zero for individuals who were not employed in the quarter), along with a complete set of interactions between these variables. We estimate this model separately for men and women. We use the results from these regressions to compute the estimated probability that someone in either the JTPA or ES file has a missing occupation code. For men we set the occupation variable equal to missing when the estimated probability is greater than 0.5. We do the same for women when the estimated probability is greater than 0.55.

The second difference in the occupation variable is that in the JTPA data occupation is coded using the Occupational Employment Statistics (OES) codes, while in the ES data occupation is coded using the Dictionary of Occupational Title (DOT) codes. To create similar codes in both files we first used a crosswalk obtained from the Bureau of Labor Statistics to convert the DOT codes into OES codes. We then used the OES codes to create nine occupation groups: Managers/Supervisors (OES codes 13-19, 41, 51, 61, 71, 81); Professionals (OES codes 21-39); Sales (43-49); Clerical (53-59); Precision Production, Craft and Construction (85, 87, 89, 95); Machine Operators, Inspectors, Transportation (83, 91-93, 97); Agricultural Workers/Laborers (73-79, 98); and missing.

Education

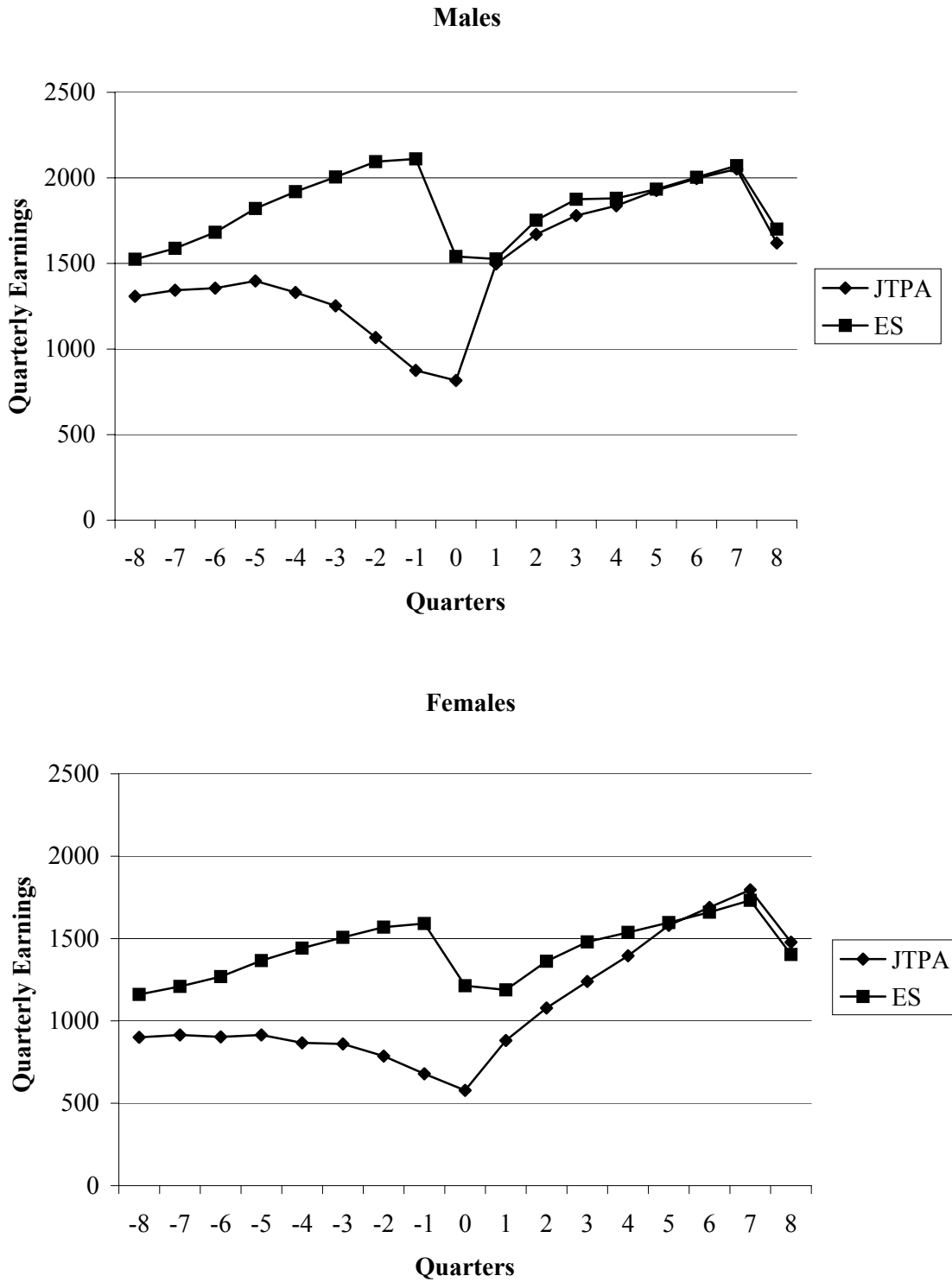
In both programs, applicants are asked about the highest grade they completed. Up through a high school diploma this information is coded as the number of years of schooling, so someone whose education stopped with a high school diploma will have the value 12. For individuals who complete more than 12 years of schooling but do not obtain a degree, the highest grade completed is again coded as the number of years of schooling. However, for individuals who complete a post-high school degree, different codes are entered into this field indicating what degree they completed. This is also true for individuals who obtained a high school equivalency certificate (GED) prior to entering the program. We convert this information into years of schooling as follows: GED=12 years; associate of arts degree=14 years; BA/BS degree=16 years; masters degree=17 years; Ph.D.= 20 years.

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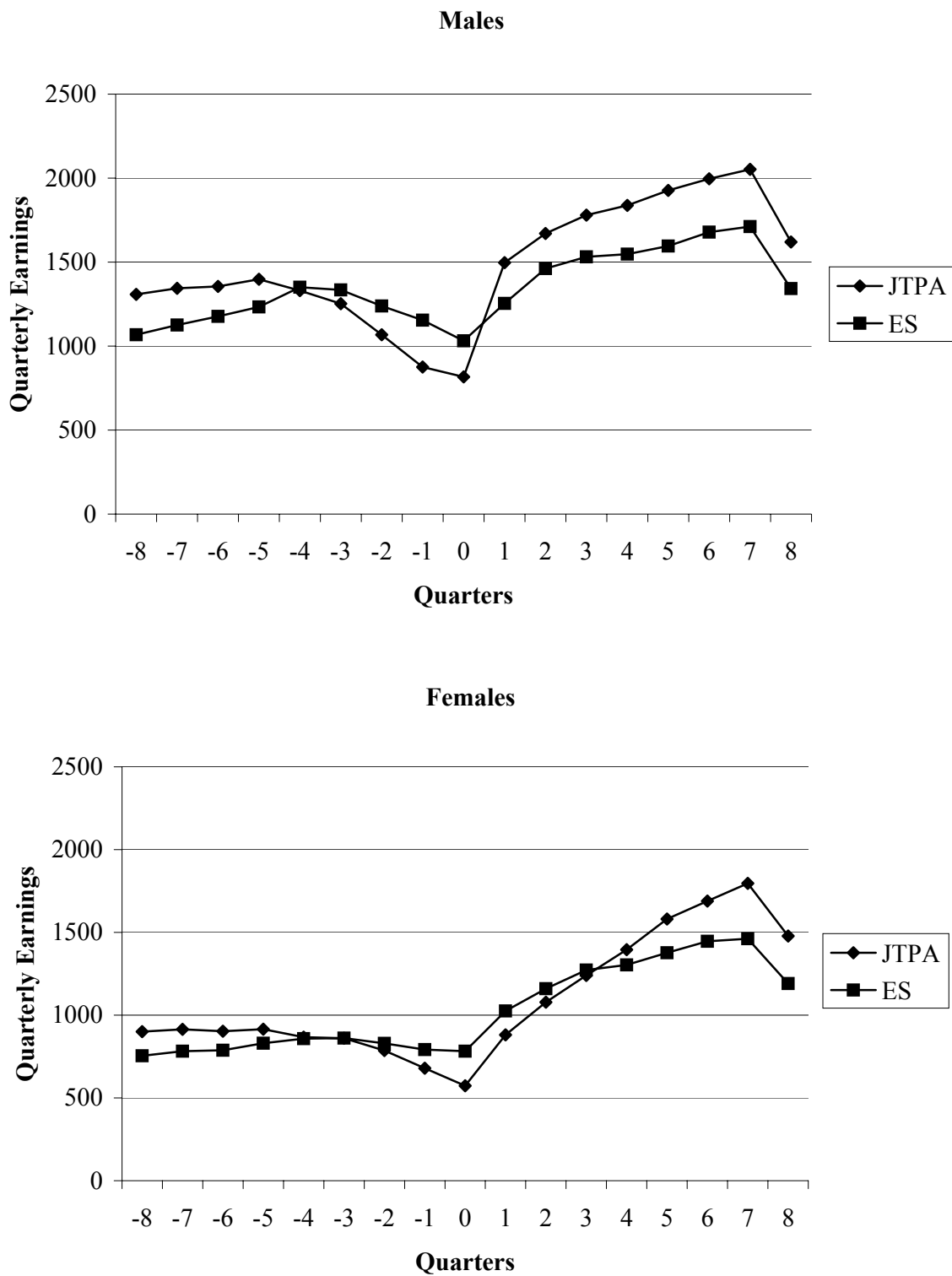
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Figure 1: Quarterly Earnings of JTPA and ES Participants



Note: Quarters are measured relative to the quarter of entry into the program. Quarter of entry is designated as quarter 0.

Figure 2: Quarterly Earnings of Matched JTPA and ES Participants--Matched Using Mahalanobis Distance



Note: Quarters are measured relative to the quarter of entry into the program. Quarter of entry is designated as quarter 0.

Figure 3: Propensity Score Distribution For JTPA and ES Samples

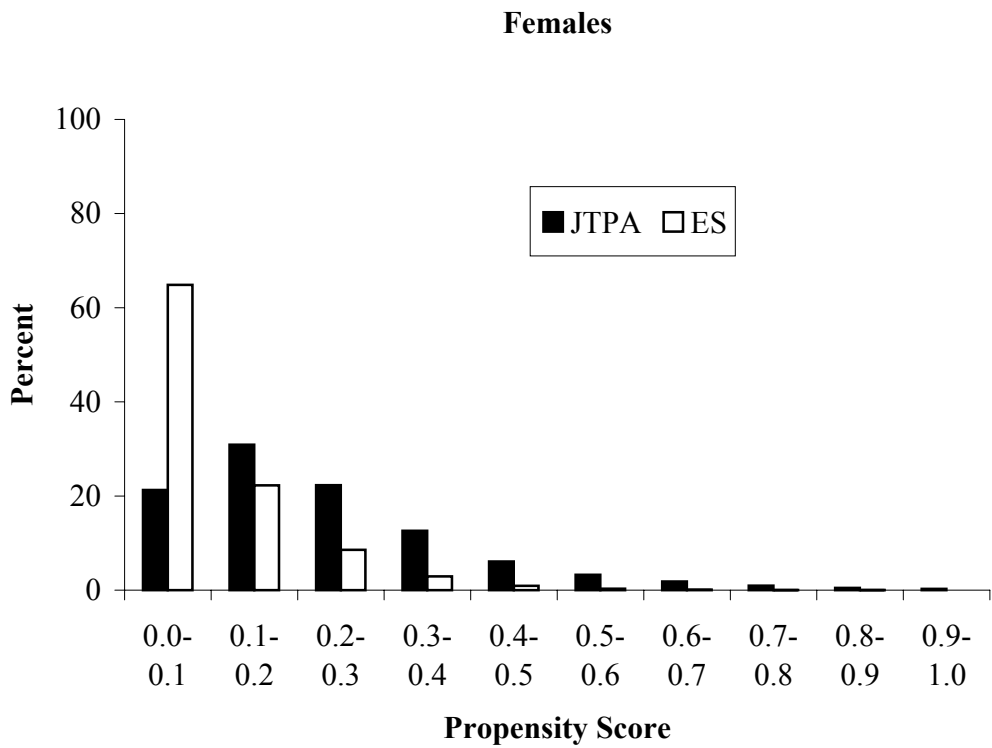
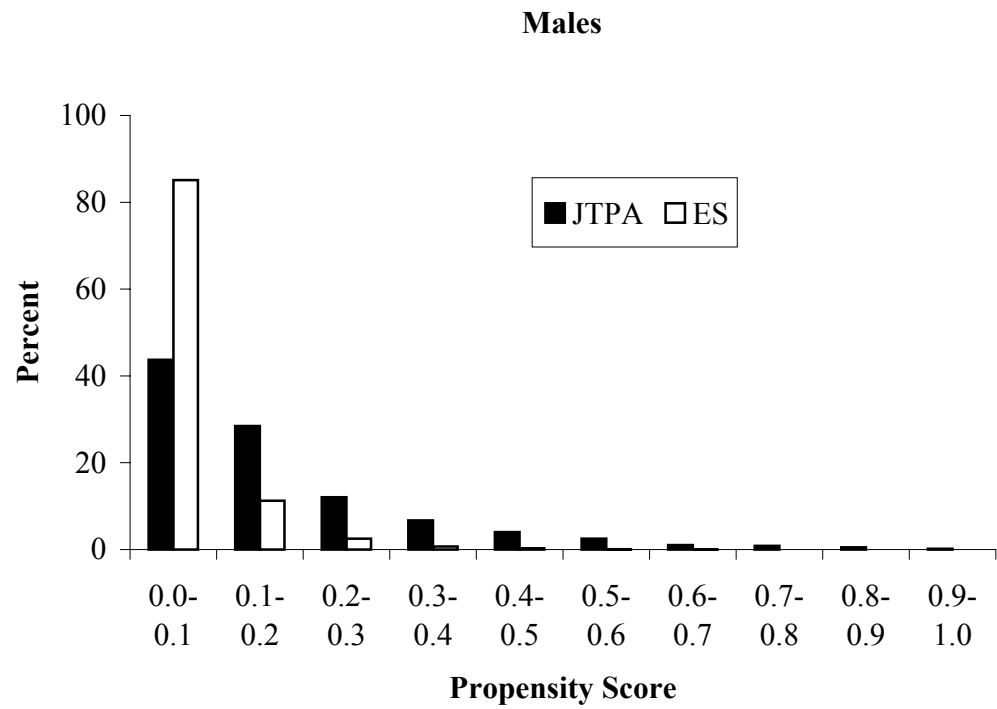
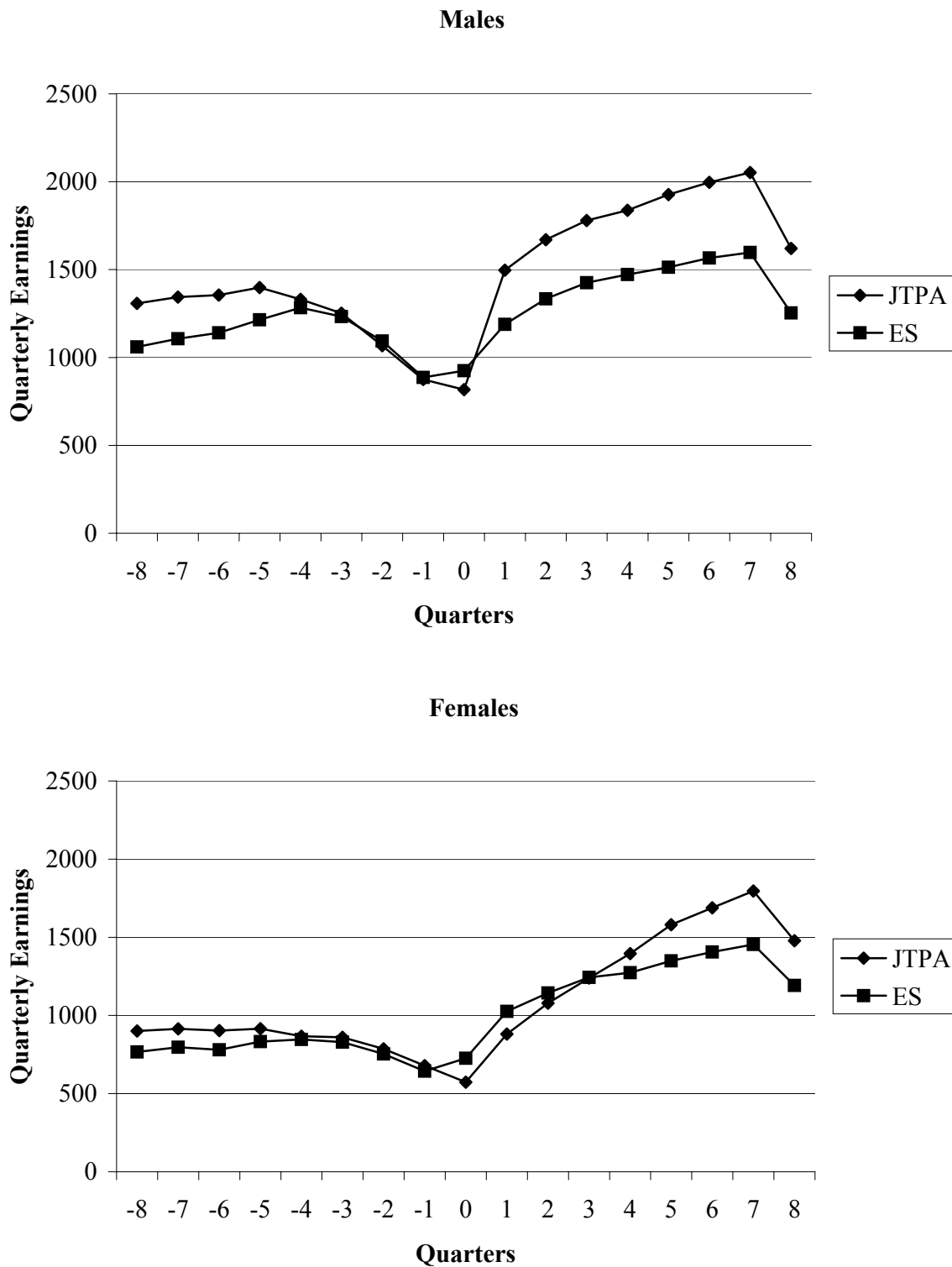
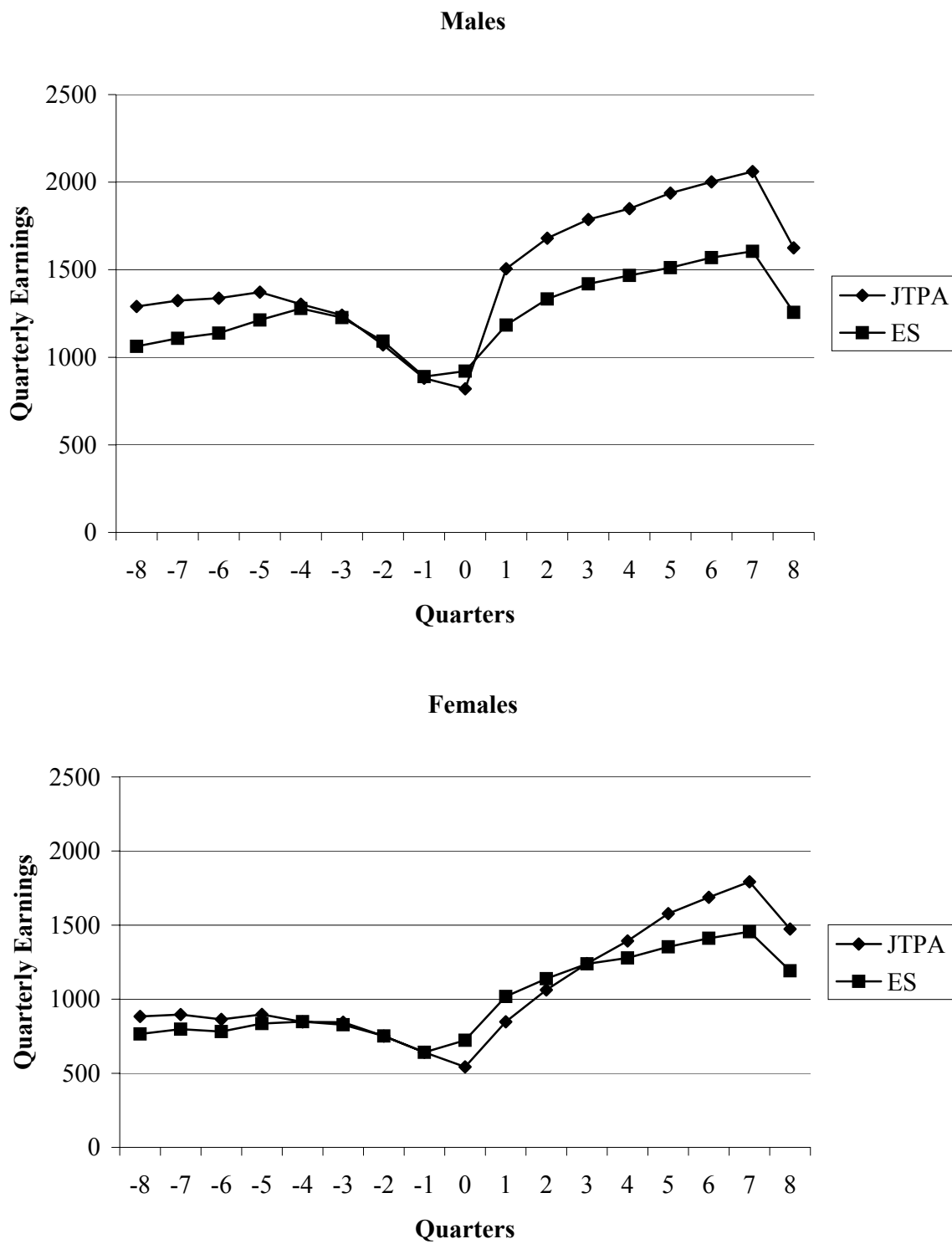


Figure 4: Quarterly Earnings of Matched JTPA and ES Participants--Matched Using Propensity Score



Note: Quarters are measured relative to the quarter of entry into the program. Quarter of entry is designated as quarter 0.

Figure 5: Quarterly Earnings of Matched JTPA and ES Participants--Matched Using Propensity Score with a 0.1 Caliper



Note: Quarters are measured relative to the quarter of entry into the program. Quarter of entry is designated as quarter 0.

Table 1: Summary Statistics

	Males		Females	
	JTPA	ES	JTPA	ES
Average years of education	11.84	11.77	12.02	11.91
Average years of experience	17.98	16.43	15.47	15.38
Percent white non-Hispanic	63.0	68.0	66.9	63.7
Percent veteran	29.1	15.5	2.3	1.4
Labor market transitions (percent)				
Not empl./empl	8.4	6.7	8.0	7.0
Empl./empl.	7.1	9.6	9.4	10.3
Empl./not empl.	23.7	36.7	15.6	34.5
Not empl./not empl.	60.8	47.0	67.0	48.2
Occupation (percent)				
Missing	54.6	35.6	65.7	40.0
Managers/supervisors	1.7	3.7	1.5	3.9
Professionals	1.7	3.1	2.6	4.9
Sales	2.8	2.5	4.7	6.7
Clerical	3.1	3.4	6.4	14.4
Service	8.9	7.9	11.9	12.9
Precision production, craft, construction	4.1	12.1	0.4	0.6
Machine operators, inspectors/transportation	9.6	19.1	4.5	11.8
Agricultural workers/laborers	13.3	12.1	2.2	4.8
Percent in Kansas City SDA	17.6	13.0	13.2	14.2
Percent in St. Louis SDA	15.3	14.7	9.0	14.7
Mean post-enrollment earnings (quarters 5 to 8)	7595	7708	6543	6392
Mean earnings in quarter of assignment	817	1541	573	1213
Mean earnings one quarter prior to enrollment	875	2111	679	1591
Mean earnings two quarters prior to enrollment	1067	2095	787	1570
Mean earnings three quarters prior to enrollment	1331	2005	860	1507
Mean earnings four quarters prior to enrollment	1398	1920	867	1442
Mean pre-enrollment earnings (quarters -8 to -5)	5405	6616	3633	5004
Growth in pre-enrollment earnings	90	297	14	205
Difference between pre- and post enrollment earnings	2190	1092	2911	1388
Mean estimated probability of participation	0.17	0.05	0.23	0.09
Number	2802	45339	6395	52895

Table 2: Estimates of Program Participation on Pre-Program Earnings and Earnings Growth

		Males		Females	
		Pre-Program Earnings Level	Growth in Pre- Program Earnings	Pre-Program Earnings Level	Growth in Pre- Program Earnings
Simple differences					
(1)	No regression adjustment	-1211 (162)	-207 (34)	-1371 (84)	-192 (18)
(2)	Regression adjustment	854 (96)	-105 (34)	393 (51)	-62 (18)
Mahalanobis distance matching					
(3)	No regression adjustment	803 (185)	-76 (44)	478 (92)	-62 (23)
(4)	Regression adjustment	937 (122)	-74 (47)	448 (59)	-43 (23)
P-Score matching					
No caliper					
(5)	No regression adjustment	823 (177)	-53 (45)	422 (88)	-62 (24)
(6)	Regression adjustment	811 (130)	-55 (44)	360 (64)	-63 (24)
0.10 caliper					
(7)	No regression adjustment	733 (167) [N=2748]	-61 (45) [N=2748]	327 (83) [N=6257]	-64 (24) [N=6257]
(8)	Regression adjustment	777 (128) [N=2748]	-57 (45) [N=2748]	330 (65) [N=6257]	-61 (24) [N=6257]

Note: Standard errors are in parentheses. There are 2802 male participants and 6395 female participants, except where numbers of participants are specified in brackets.

Table 3: Estimates of Program Effect Based on Simple Differences, Regression Analysis and Mahalanobis Distance Matching

	Males		Females	
	Post-Program Earnings	Difference-in-Difference	Post-Program Earnings	Difference-in-Difference
(1) Simple differences	-113 (173)	1098 (190)	151 (93)	1522 (104)
(2) Regression adjustment	1481 (157)	628 (177)	1087 (86)	693 (98)
Mahalanobis distance matching				
Standard pair matching				
(3) No regression adjustment	1267 (194)	465 (216)	1067 (108)	589 (122)
(4) Regression adjustment	656 (197)	719 (220)	1054 (110)	606 (121)
(5) Modified pair matching	1285 (194)	482 (216)	1132 (108)	620 (122)
(6) Standard pair matching-trimming tail	1227 (191)	513 (212)	1066 (108)	630 (119)
	[N=2770]	[N=2770]	[N=6331]	[N=6331]

Note: Standard errors are in parentheses. There are 2802 male participants and 6395 female participants, except where numbers of participants are specified in brackets.

Table 4: Estimates of Program Effect Based on Propensity Score Matching

		Males		Females	
		Post-Program Earnings	Difference-in- Difference	Post-Program Earnings	Difference-in- Difference
Matching without replacement					
Standard pair matching					
(1)	No regression adjustment	1532 (199)	709 (206)	1179 (97)	757 (128)
(2)	Regression adjustment	1562 (196)	751 (194)	1173 (94)	814 (110)
Standard pair matching with caliper					
(3)	0.05 caliper	1480 (198) [N=2740]	723 (201) [N=2740]	1173 (99) [N=6228]	845 (124) [N=6228]
	0.10 caliper				
(4)	No regression adjustment	1496 (201) [N=2748]	764 (204) [N=2748]	1177 (100) [N=6257]	850 (125) [N=6257]
(5)	Regression adjustment	1522 (199) [N=2748]	746 (198) [N=2748]	1187 (96) [N=6257]	857 (112) [N=6257]
(6)	0.20 caliper	1525 (200) [N=2765]	727 (205) [N=2765]	1184 (105) [N=6318]	847 (117) [N=6318]
Modified pair matching					
(7)	No regression adjustment	1731 (199)	822 (212)	1165 (97)	722 (124)
(8)	Regression adjustment	1707 (196)	947 (202)	1136 (92)	776 (103)
Matching with replacement					
(9)	One nearest neighbor	1682 (249)	701 (306)	1253 (154)	892 (163)
(10)	Five nearest neighbors	1661 (203)	705 (221)	1204 (104)	754 (130)
(11)	Ten nearest neighbors	1681 (153)	758 (214)	1234 (98)	777 (130)
(12)	Matching by propensity score category	1608 (135)	782 (164)	1209 (87)	787 (106)
(13)	Kernel density matching	1291 (164)	856 (165)	1141 (87)	838 (115)

Note: Standard errors are in parentheses. All of the standard errors have been estimated using bootstrapping to reflect the fact that $P(X)$ is measured with error. There are 2802 male participants and 6395 female participants, except where numbers of participants are specified in brackets.

Table 5: Summary of Estimates of Program Effect

	Males		Females	
	Post-Program Earnings	Difference-in-Difference	Post-Program Earnings	Difference-in-Difference
(1) Simple difference	-113 (173)	1098 (190)	151 (93)	1522 (104)
(2) Regression adjustment	1481 (157)	628 (177)	1087 (86)	693 (98)
(3) Mahalanobis distance matching	1267 (194)	465 (216)	1067 (108)	589 (122)
P-score matching without replacement				
(4) No caliper	1532 (199)	709 (206)	1179 (97)	757 (128)
(5) 0.10 caliper	1496 (201)	764 (204)	1177 (100)	850 (125)
P-score matching with replacement				
(6) One nearest neighbor	1682 (249)	701 (306)	1253 (154)	892 (163)
(7) Matching by P-score category	1608 (135)	782 (164)	1209 (87)	787 (106)
(8) Kernel density matching	1291 (164)	856 (165)	1141 (87)	838 (115)

Note: Standard errors are in parentheses. There are 2802 male participants and 6395 female participants, except where numbers of participants are specified in brackets.

Table 6: Comparison of Estimated Program Effects Using Difference-in-Difference Estimator with Effects Based on Randomized Control Groups

	Orr, et al. (1996)		Current Analysis			
	Months 7-18	Months 19-30	Propensity Score, No Caliper	Propensity Score, 0.10 Caliper	Propensity Score Categories	Kernel Density Matching
Men	666 (478)	1001 (511)	709 (206)	764 (204)	780 (190)	856 (165)
Women	1015 (288)	990 (319)	757 (128)	850 (125)	787 (111)	838 (115)

Note: Standard errors in parentheses. The Orr et. al. (1996) estimates are taken from Exhibit 4.6, page 107. They have been adjusted for inflation so that they are comparable to the estimates from the current analysis.

Table 7: Estimates of Program Effect Dropping Certain Variables--Propensity Score Matching with 0.1 Caliper

	Males		Females	
	Post-Program Earnings	Difference-in-Difference	Post-Program Earnings	Difference-in-Difference
No regression adjustment				
(1) Including all variables	1496 (201)	764 (204)	1177 (100)	850 (125)
(2) Dropping employment transitions	1498 (203)	462 (256)	1421 (99)	981 (117)
(3) Dropping SDA	1552 (210)	899 (230)	1157 (120)	790 (124)
(4) Dropping earnings 4 quarters prior	957 (235)	643 (279)	1025 (129)	803 (136)
(5) Dropping all 3	924 (217)	838 (244)	980 (134)	1077 (139)
Regression adjustment				
(6) Including all variables	1522 (199)	746 (198)	1187 (96)	857 (112)
(7) Dropping employment transitions	1409 (202)	627 (246)	1410 (102)	998 (115)
(8) Dropping SDA	1614 (206)	843 (228)	1198 (118)	779 (121)
(9) Dropping earnings 4 quarters prior	752 (222)	910 (262)	944 (122)	899 (132)
(10) Dropping all 3	994 (211)	829 (240)	1021 (128)	1089 (133)

Note: Standard errors are in parentheses.

Table 8: Sensitivity of Estimates to Changes in the Size of the Samples

	Males						Females					
	Regression Adjustment			Propensity Score, 0.1			Regression Adjustment			Propensity Score, 0.1		
	Post-Program Earnings	Diff-in-Diff	Caliper	Post-Program Earnings	Diff-in-Diff	Caliper	Post-Program Earnings	Diff-in-Diff	Caliper	Post-Program Earnings	Diff-in-Diff	Caliper
(1) Full sample	1481 (157)	628 (177)	1496 (201)	764 (204)	1608 (135)	782 (164)	1087 (86)	693 (98)	1177 (100)	850 (125)	1209 (87)	787 (106)
(2) Comparison sample = treatment sample	1508 (224)	848 (246)	1635 (280)	966 (327)	1635 (273)	849 (317)	1096 (147)	684 (203)	1290 (139)	918 (149)	1284 (147)	877 (161)
(3) Reduce treatment sample by 90%	1493 (448)	646 (514)	1662 (633)	850 (728)	1518 (488)	905 (556)	1126 (263)	751 (288)	1274 (424)	791 (444)	1116 (277)	845 (278)
(4) Reduce both samples by 90%	1428 (448)	618 (533)	1516 (625)	756 (679)	1524 (517)	758 (625)	1119 (268)	692 (324)	1224 (333)	862 (358)	1194 (325)	778 (374)

Note: Mean estimates based on 100 repetitions. Standard deviations of estimates in parentheses.

Table A1: Regression Predicting Post-Program Earnings

	Males		Females	
	Post-Program Earnings	Difference in Earnings	Post-Program Earnings	Difference in Earnings
Participation in JTPA	1481.37 (156.84)	627.76 (176.84)	1086.99 (86.11)	693.71 (98.16)
Years of education	33.51 (48.83)	-40.76 (55.06)	-14.19 (40.16)	-120.34 (45.79)
High school graduation	935.09 (136.75)	797.95 (154.20)	1038.20 (106.40)	721.74 (121.30)
Years of higher education	325.97 (62.68)	399.34 (70.68)	735.94 (48.88)	787.25 (55.73)
Experience	-50.03 (14.50)	-122.75 (16.36)	49.24 (10.26)	-0.59 (11.70)
Experience ²	-0.90 (0.34)	0.04 (0.39)	-2.13 (0.25)	-1.87 (0.28)
Not employed/employed	2294.88 (161.66)	2263.94 (182.28)	1641.44 (111.96)	1617.58 (127.63)
Employed/employed	3483.03 (177.50)	4312.43 (200.14)	2350.91 (126.56)	2975.95 (144.28)
Employed/ not employed	-67.30 (144.55)	1255.52 (162.99)	-288.00 (107.95)	525.62 (123.06)
White	946.47 (97.98)	504.32 (110.48)	-61.74 (71.23)	-295.22 (81.20)
Veteran	580.98 (101.22)	1105.02 (114.13)	316.86 (211.40)	1297.11 (240.99)
Earnings 1 quarter prior	0.73 (0.03)	0.48 (0.03)	0.56 (0.03)	0.38 (0.03)
Earnings 2 quarters prior	0.32 (0.04)	0.05 (0.04)	-0.12 (0.03)	-0.60 (0.03)
Earnings 3 quarter prior	0.27 (0.04)	-0.29 (0.04)	0.37 (0.03)	-0.04 (0.04)
Earnings 4 quarters prior	0.36 (0.03)	-1.70 (0.03)	0.57 (0.03)	-1.22 (0.03)
Employed 1 quarter prior	-482.82 (147.51)	-660.51 (166.32)	56.86 (103.69)	77.79 (118.20)
Employed 2 quarters prior	-352.56 (137.63)	-144.29 (155.19)	421.98 (94322)	576.58 (107.40)
Employed 3 quarter prior	-310.55 (132.63)	245.08 (152.93)	-219.78 (95.62)	96.53 (109.00)
Employed 4 quarters prior	160.79 (133.00)	-531.79 (149.96)	-319.49 (93.93)	-1460.64 (107.08)
7 Quarter of enrollment dummies	Yes	Yes	Yes	Yes
8 occupation dummies	Yes	Yes	Yes	Yes
13 service delivery area dummies	Yes	Yes	Yes	Yes
Adjusted R ²	0.23	0.18	0.19	0.17
N	48141	48141	59290	59290

Note: Standard errors are in parentheses.

IZA Discussion Papers

No.	Author(s)	Title	Area	Date
771	A. Calvó-Armengol Y. Zenou	Job Matching, Social Network and Word-of-Mouth Communication	5	05/03
772	E. Patacchini Y. Zenou	Search Intensity, Cost of Living and Local Labor Markets in Britain	3	05/03
773	A. Heitmueller	Job Mobility in Britain: Are the Scots Different? Evidence from the BHPS	1	05/03
774	A. Constant D. S. Massey	Labor Market Segmentation and the Earnings of German Guestworkers	1	05/03
775	J. J. Heckman L. J. Lochner P. E. Todd	Fifty Years of Mincer Earnings Regressions	5	05/03
776	L. Arranz-Aperte A. Heshmati	Determinants of Profit Sharing in the Finnish Corporate Sector	2	05/03
777	A. Falk M. Kosfeld	It's all about Connections: Evidence on Network Formation	6	05/03
778	F. Galindo-Rueda	Employer Learning and Schooling-Related Statistical Discrimination in Britain	5	05/03
779	M. Biewen	Who Are the Chronic Poor? Evidence on the Extent and the Composition of Chronic Poverty in Germany	1	05/03
780	A. Engellandt R. T. Riphahn	Temporary Contracts and Employee Effort	1	05/03
781	J. H. Abbring J. R. Campbell	A Structural Empirical Model of Firm Growth, Learning, and Survival	5	05/03
782	M. Güell B. Petrongolo	How Binding Are Legal Limits? Transitions from Temporary to Permanent Work in Spain	1	05/03
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