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## Does "Work First" Work? The Long-Term Consequences of Temporary Agency and Direct-Hire Job Placements

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## Does "Work First" Work? The Long-Term Consequences of Temporary Agency and Direct-Hire Job Placements

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**PRELIMINARY****Does “Work First” Work? The Long-Term Consequences  
of Temporary Agency and Direct-Hire Job Placements**

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**Abstract**

A principal objective of the welfare reform act of 1996 (PRWORA) was to encourage welfare recipients to obtain jobs rapidly, a strategy termed “Work First.” Much analysis shows that Work First raises the incidence of direct-hire and – in a sizable minority of cases – temporary-help agency jobs among welfare clients. But the effect of these jobs on longer term labor market outcomes, such as labor force participation, earnings, and welfare recidivism, is unknown. Because welfare recipients who obtain jobs rapidly are positively selected from the pool of all Work First participants, a simple comparison of long-term outcomes among job takers and non-takers is potentially misleading.

We evaluate the effects of Work First job placements on program recidivism and earnings of welfare recipients over a two-year period following Work First program assignment. We draw upon administrative data from an unusual policy experiment in the state of Michigan. Welfare recipients in a major metropolitan were randomly assigned to a large number of Work First contractors that had widely varying placement rates in direct-hire and temporary-help jobs but provided otherwise similar services. These assignments significantly impacted job-finding rates among ex ante comparable clients living in the same neighborhoods. We find that Work First placements in direct-hire jobs substantially raised earnings over the two years following program assignment, but that temporary help agency placements yielded no lasting gains in welfare recipients’ employment and earnings.

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A principal objective of U.S. welfare reform legislation in 1996 (PRWORA) was to encourage welfare recipients to obtain jobs rapidly. Indeed, Federal regulations essentially require that individuals work as a condition for welfare receipt. Much analysis shows that welfare-to-work programs, implemented to help place welfare recipients in jobs, raise the incidence of direct-hire and – in a substantial minority of cases – temporary-help agency jobs among welfare participants. But whether these programs, often called “Work First,” improve longer term labor market outcomes and reduce welfare dependency is unknown. Because Work First participants who obtain jobs are positively selected from the pool of all participants, a simple comparison of long-term outcomes among those who take direct-hire jobs, those who take temporary agency jobs, and those who find no employment while in the Work First program is potentially misleading.

We evaluate whether Work First job placements in direct hire and temporary help jobs improve employment and earnings and reduce program recidivism over a two-year period following Work First program assignment. Our analysis draws upon administrative data from an unusual policy experiment in a major metropolitan area in Michigan. In this metropolitan area welfare recipients were, in effect, randomly assigned among Work First contractors servicing their districts. These contractors had significantly different placement rates in direct-hire and temporary-help jobs, but provided otherwise similar services. We exploit the fact that contractor assignments significantly affected the rates of direct-hire and temporary job placements among ex-ante comparable participants living in the same neighborhoods to identify the effects of direct-hire and temporary agency job placements on Work First participants’ earnings and program recidivism.

Our analysis utilizes administrative data from the Work First program linked with Unemployment Insurance (UI) wage records data for over 50,000 Work First spells initiated from 1997 to 2003. The Work First data include demographic information on participants and detailed information on jobs found during the program. UI wage records enable us to track earnings of all participants over time. Among Work First participants who found employment, about 20 percent held temporary help jobs.

Exploiting the randomization of Work First participants to Work First program contractors with differing job placement policies to draw causal inferences, we find that only Work First placements in direct-hire jobs substantially raise earnings and reduce program recidivism over the subsequent two-year period. Placement of Work First participants in temporary agency jobs does not improve outcomes relative to not placing individuals in jobs. Numerous robustness and consistency tests confirm that these results are comparable across randomization clusters within our metropolitan area and are stable across time periods.

The organization of our paper is as follows. Section 1 elaborates on the questions we address in our paper, reviews prior research methodologies and findings on these issues, and outlines our methodological approach. Section 2 details our econometric methodology. Section 3 describes the Work First program and our data. It also presents tests confirming that assignment of Work First participants among contractors operating in the same district is consistent with random assignment, but that job placement outcomes among contractors operating in the same district are significantly different. Sections 4 and 5 present our main findings on the consequences of job placement and job type on program recidivism and long-term earnings. Section 6 concludes and discusses implications for policy.

## **1. Hypotheses and Methodologies**

### **a. Hypotheses**

The underlying premise of welfare reform legislation was that welfare recipients could find stable employment and escape poverty and welfare dependency, given proper incentives and assistance in finding suitable jobs. Pursuant to federal regulation, states generally require some work as a condition of welfare receipt. Most states, including Michigan, have implemented a “Work First” strategy, in which applicants for TANF assistance who do not meet mandatory work requirements must participate in programs that help them find employment. These Work First programs emphasize job search and placement services, and provide few resources for other services such as assessment of participants’ needs or job training. In our paper, we analyze 1) whether job placements through our Michigan Work First program improve long-term outcomes for program participants, and 2) whether long-term outcomes differ between placements with temporary help agencies and those directly with employers.

Whether the Work First approach improves long-term outcomes for welfare participants has been at the heart of the intense debate over the merits of welfare reform. Supporting the Work First approach is a substantial body of research on government-sponsored job programs suggesting that programs emphasizing job search and readiness services are more effective than more expensive classroom training programs in placing individuals in improving individuals’ earnings (Bloom et al. 1997; Riccio et al. 1994; Martinson and Freedlander 1994). On the other hand, some researchers have stressed that many on welfare face multiple barriers to employment, including drug abuse, learning disabilities, mental health problems, and domestic violence (Danziger and Seefeldt 2002, Ramey and Keltner 2002). Skeptics of welfare reform argue that the Work First emphasis on rapid job placement fails to address the underlying problems that hamper many welfare recipients from obtaining stable employment and escaping poverty. Thus,

on the margin, job placements through Work First programs may fail to improve long-term outcomes because these program participants require more intense interventions.

In addition, we should note that even if welfare reform *policies* improve long-term outcomes, it is possible that the Work First *programs* designed to help individuals find jobs have little value-added. The main effect of work requirements may be to discourage individuals from applying for welfare benefits and instead to encourage them to find employment on their own. Similarly, mandatory Work First programs for welfare applicants may not lead to better jobs for these individuals than the jobs they would find on their own.

A separate but related controversy concerns the high incidence of temporary help employment among Work First participants. In the Work First data reported below, about 20 percent of those finding employment while in the Work First program obtain a job with a temporary help agency. These high rates of temporary agency placements are not unusual. Recent analyses of state administrative welfare data reveal that 15 to 40 percent of former welfare recipients who obtained employment in the years following welfare reform took jobs in the temporary help sector.<sup>1</sup> In spite of the high incidence of temporary help employment among welfare participants, there is little consensus as to whether these jobs benefit or harm the long-term employment outcomes among this population. Two plausible, but opposing, hypotheses of how temporary agency jobs may impact the career trajectories of welfare recipients and other low-skilled workers have been articulated.

One hypothesis is that temporary help agencies may face lower screening, termination, wage, and other non-wage costs compared to client employers, and hence they may hire individuals who otherwise would have been unemployed (Autor, 2003; Houseman, Kalleberg,

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<sup>1</sup> See Autor and Houseman, 2002, on Georgia and Washington state; Cancian et al., 1999, on Wisconsin; Heinrich, Mueser and Troske, 2002, on North Carolina and Missouri; and Pawasarat, 1997, on Wisconsin.

and Erickcek, 2003; Autor and Houseman, 2002). Accordingly, spells in temporary help may reduce the time workers spend in unproductive, potentially discouraging job search, and facilitate rapid entry into employment. Temporary assignments may further permit workers to develop human capital and labor market contacts that lead, directly or indirectly, to longer-term jobs. Several pieces of evidence support the plausibility of this hypothesis. The vast majority of temporary help workers leave the sector within one year, indicating high mobility (Segal and Sullivan, 1997). A panel study of Michigan TANF recipients shows three-fourths of those taking temporary agency employment reported learning new skills on the job and one-fourth reported that temping led to a permanent job at the firm (Corcoran and Chen, 2004). A relatively high fraction of low-wage workers in the temporary help sector eventually transition to higher wage jobs in other sectors (Andersson, Holzer, and Lane, 2005). Consistent with this evidence, a large and growing number of employers use temporary help assignments as a means to screen workers for direct-hire jobs (Abraham, 1988; Autor, 2001; Houseman 2001, Kalleberg et al. 2003), a practice that appears particularly prevalent in the manufacturing sector (Ballantine and Ferguson, 1999).

A contrasting but coherent alternative hypothesis is that the unstable and primarily low-skilled placements offered by temporary help agencies provide little opportunity or incentive for workers to invest in human capital or develop productive job search networks (Jorgenson and Riemer, 2000). In support of this view, several researchers document that workers in temporary agency jobs receive on average lower pay and fewer benefits than would be expected in direct-hire jobs (DiNatalie, 2000; General Accounting Office, 2000; Segal and Sullivan, 1998). And while mobility out of the temporary help sector is high, a disproportionate share of leavers enters unemployment or exits the labor force (Segal and Sullivan, 1997). These facts would not be of



great concern if temporary help jobs exclusively substituted for spells of unemployment. But to the degree that spells in temporary help crowd out productive direct-hire job search, they may inhibit longer-term labor advancement. Under this scenario, the short term gains accruing from nearer-term employment in temporary help jobs may be offset by employment instability and poor earnings growth.

**b. Previous Research: Approaches and Evidence**

Distinguishing among these competing hypotheses is an empirical challenge. The average characteristics of the non-employed are likely to be substantially different from the employed (as is true in our data). And among the employed, those taking temporary agency jobs likely differ from those in direct-hire jobs. Although some characteristics, such as education and labor force history, can be directly controlled in a regression, others like motivation and job-preparedness will impact earnings but are unobservable in conventional data sources. Consequently, analyses of the effects of temporary help and direct-hire jobs on subsequent labor market progression that do not take adequate account of these confounding factors are likely to be unconvincing and potentially biased.

Although a large literature examines the effects of various aspects of welfare policy reform on participant outcomes,<sup>2</sup> to our knowledge, no previous study addresses whether job placements in Work First programs improve long-term participant outcomes.<sup>3</sup> Several recent studies have tried to identify the effects of temporary agency employment vis-à-vis direct-hire and non-employment on subsequent labor market outcomes among welfare or low-income

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<sup>2</sup> Blank (2003) provides an excellent summary of the methodologies and findings in the voluminous literature on the effects of welfare reform.

<sup>3</sup> In practice, the performance of local Work First service providers typically is evaluated based on several criteria, such as the fraction of participants placed in jobs and, among those placed, the fraction remaining employed after 90 days. No follow-up data are collected for those who leave the program without finding a job, and thus not even crude comparisons of outcomes between those finding jobs and those who do not are conducted. Hence, these evaluation criteria reveal nothing about the effects of job placements on participant outcomes—short or long term.

populations. Lane et al. (2003) use matched propensity score techniques to study the effects of temporary agency employment on the labor market outcomes of low-income workers and those at risk of being on public assistance. To adequately control for selection bias through propensity score techniques, it must be the case that differences among those in temporary, direct-hire, and non-employment are fully captured by variables observable to researchers. In addition, researchers must be able to construct comparable groups of individuals in non-employment, temporary agency jobs, and direct-hire jobs using these observable variables. In the SIPP data Lane et al. utilized, there was insufficient overlap in a key variable—earnings histories—among those in temporary, direct-hire, and non-employed to construct well-matched groups, a fact that they acknowledge potentially biases their results. With this caveat, they cautiously conclude from their analysis that temporary employment improves labor market outcomes among those who might otherwise have been unemployed, and suggest the use of temporary agencies by welfare agencies may be beneficial.

Heinrich, Mueser and Troske (2002) study the effects of temporary agency employment on subsequent earnings among welfare recipients in two states. To control for possible selection bias in the decision to take a temporary agency job, they estimate a selection model that is identified through the exclusion of various county-specific measures. Interestingly, their correction for selection bias has little effect on their regression estimates, suggesting either that the selection problem is unimportant or that their instruments do not adequately control for selection on unobservable variables.<sup>4</sup> Like Lane et al., they find that the earnings trajectories of

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<sup>4</sup> Their empirical strategy assumes that the county-level variables used to identify the selection model influence earnings only through their impact on employment and job type, an assumption they acknowledge is likely violated.

those taking temporary help jobs are somewhat worse than of those taking direct-hire jobs, but are significantly better than of those who are not employed.

Several recent studies have addressed the role of temporary employment in facilitating labor market transitions in Europe. Using propensity score matching methods, Ichino et al. (2004) conclude that jobs with temporary help agencies significantly increase the probability of finding permanent employment within 18 months relative to unemployment. Booth, Francesconi and Frank (2002) and Garcia-Perez and Munoz-Bullon (2002) study the effects on subsequent employment outcomes of temporary (agency and fixed-term) employment in Britain and temporary agency employment in Spain, respectively. Their empirical strategies are similar to those used in Heinrich, Muser, and Troske (2002) and they also find generally positive effects of temporary employment.

In sum, recent studies generally conclude that temporary agency employment serves as a stepping stone for the non-employed welfare or low-income workers into employment. What these studies have in common is that they draw exclusively on observational data to ascertain causal relationships. Hence, their findings will depend critically on the methods used to correct the non-experimental data for self-selection into temporary help, direct hire, and non-employment.

**c. Our Methodological Approach**

To potentially overcome these confounds, we exploit a unique, multi-year policy experiment in a large Michigan metropolitan area. For the administration of Work First and related welfare programs, this metropolitan area was divided into multiple geographic districts, with typically two or three independent organizations or “contractors” providing Work First services to participants in each district in any given program year. As we demonstrate below,

within each district, the division of Work First participants among contractors was observationally equivalent to random assignment. Within many of the districts, however, there were large and persistent differences across contractors in the fraction of participants who were placed in temporary agency jobs, who were placed in direct-hire jobs, and who were not placed in any job. These differences, we argue, likely reflected differences in policies and practices among Work First contractors and constitute natural experiments.

We exploit these differences in the probability of temporary agency, direct-hire, or non-employment among statistically identical populations to identify the effects of Work First employment and job type on long-term earnings and program recidivism. In our econometric specification, we use contractor assignment as an instrument for whether an individual obtains employment while in the program, and, conditional on employment, the job type. Our methodology assumes that contractors only affect participant outcomes through their effects on job placements. We provide some support for this assumption below.

## **2. Using Randomization to Study the Impact of Temporary Help Employment on Participant Outcomes**

A formidable obstacle to determining how spells in temporary help and direct-hire employment affect Work First participants' earnings and employment is, as noted, that key confounding characteristics such as job preparedness and motivation are unobservable in standard data sources (including our own). To purge this bias, we require a source of variation that affects the probability that participants obtain temporary help or direct hire employment versus non-employment but does not otherwise impact their outcomes. In this analysis, we use the random assignment of participants to Work First programs as this source of variation.

Formally, define each Work First participant's potential outcome set as the triple

$Y_i \equiv \{Y_i^t, Y_i^d, Y_i^n\}$ , where the superscripts  $t, d, n$  correspond to participants' potential outcomes after

obtaining temporary help employment, direct-hire employment, or no employment during the Work First treatment period. There are two parameters of interest for our analysis,  $\delta^{tn} = E[Y^t - Y^n]$  and  $\delta^{dn} = E[Y^d - Y^n]$ . The first parameter is the effect of temporary help relative to non-employment on the outcome variable, and the latter is the effect of direct hire employment relative to non employment on the outcome (with  $\delta^{td} = E[Y^t - Y^d] = \delta^{tn} - \delta^{dn}$  by transitivity).

Because we observe only one employment status for each participant, we must estimate the parameters  $\delta^{tn}$  and  $\delta^{dn}$  by contrasting outcomes among participants who obtained different employment statuses. Define the job status attained by each participant as  $J \equiv \{t, d, n\}$ . A comparison of outcomes among Work First participants with different employment status  $J$  will generally not recover the parameters of interest. Contrasting for example the outcomes of participants with temporary employment versus no employment yields the following expression (cf. Angrist and Imbens, 1995):

$$(1) \quad \begin{aligned} E[Y | J = t] - E[Y | J = n] &= E[Y^t | J = t] - E[Y^n | J = n] \\ &= E[Y^t - Y^n | J = t] + \{E[Y^n | J = t] - E[Y^n | J = n]\}, \end{aligned}$$

In this equation, the first term in the bottom row is the effect of temporary employment relative to non-employment among participants who attained temporary help employment,  $\delta^{tn}$ , which is a parameter of interest. The second term is the average difference in potential outcomes conditional on non-employment for the participant group that obtained temporary help employment relative to the participant group that did not attain employment. We would generally presume that this latter term is positive; participants who obtained temporary employment while in the Work First program would have fared on average better than those who remained

unemployed regardless. Consequently, estimates of equation (1) will typically be upward biased. A similar bias holds when using  $E[Y | J = d] - E[Y | J = n]$  to estimate  $\delta^{dn}$ .

To purge this bias requires a source of variation that influences participants' employment status but is independent of potential outcomes. The candidate instrument  $Z$  that we use is participants' assignment to Work First sites within districts. For simplicity, but without loss of generality, let us assume that there are three sites to which participants may be randomly assigned (in a district), with  $Z$  denoting a participant's site assignment:  $Z \in \{1, 2, 3\}$ . Define  $J_z \in \{t, d, n\}$  as the employment status that the participant would attain during her Work First spell if assigned to site  $Z$ . The potential employment status  $J_z$  is assumed to exist for every person in the sample, although of course each person is only assigned to one site. For each sample member, we observe the triple  $\{Z, J, Y\}$  where  $Z$  is site assignment,  $Y = Y_j$  is the outcome variable,  $J = J_z = 1\{Z = 1\} \cdot J_1 + 1\{Z = 2\} \cdot J_2 + 1\{Z = 3\} \cdot J_3$  is employment status attained during the spell, and  $1\{\cdot\}$  is the indicator function.

To use site assignment as an instrumental variable for studying the impact of temporary employment on participant outcomes, we make two key identifying assumptions. First, given random assignment of participants across sites, we assume that the random variables  $\{J_1, J_2, J_3, Y_1, Y_2, Y_3\}$  are jointly independent of  $Z$ . This independence assumption is supported by the evidence below that the demographics of participants randomly assigned to the two programs are in general statistically indistinguishable. Second, we assume that site assignment only affects participant outcomes through its effect on employment status (temporary, direct-hire, or no employment) attained during the Work First spell. This exclusion restriction amounts to assuming that the minimal variation among sites in participant counseling services has negligible effects on participant outcomes beyond its impacts on job placement.

Under the independence and exclusion assumptions, the differences in outcomes across sites 1, 2 and 3 are caused by site-level level differences in the distribution of employment outcomes (direct-hire, temporary, and non-employment) among participants assigned to those sites. For example, the contrast in outcomes between participants assigned to sites 1 and 2 may be written as follows:

$$\begin{aligned}
 & E[Y | Z = 1] - E[Y | Z = 2] \\
 (2) \quad & = E[Y_1 - Y_2 | J_1 = d, J_2 = n] \cdot \Pr[J_1 = d, J_2 = n] + E[Y_1 - Y_2 | J_1 = n, J_2 = d] \cdot \Pr[J_1 = n, J_2 = d] \\
 & + E[Y_1 - Y_2 | J_1 = t, J_2 = n] \cdot \Pr[J_1 = t, J_2 = n] + E[Y_1 - Y_2 | J_1 = n, J_2 = t] \cdot \Pr[J_1 = n, J_2 = t] \\
 & + E[Y_1 - Y_2 | J_1 = d, J_2 = t] \cdot \Pr[J_1 = d, J_2 = t] + E[Y_1 - Y_2 | J_1 = t, J_2 = d] \cdot \Pr[J_1 = t, J_2 = d].
 \end{aligned}$$

This equation indicates that the expected difference in mean outcomes between site 1 and site 2 participants is composed of six contrasts between potential outcomes for participants whose employment status is changed by the random assignment: the contrast for those whose employment is switched from temporary employment to non-employment and visa versa; the contrast for those switched from direct-hire employment to non-employment and visa versa; and the contrast for those switched from direct-hire employment to temporary help employment and visa versa. Each term in equation (2) represents the average effect on the outcome variable for participants who are induced to switch employment status among  $\{d, t, n\}$  due to the randomization weighted by the share of participants whose employment statuses are changed. We do not assume that the contrast in outcomes for participants switching from one employment status to another (say from temporary help employment to non-employment) is equal to the contrast in outcomes for participants switching in the opposite direction (i.e., from non-employment to temporary help employment) since the sets of individuals switching in opposing directions are, by definition, non-overlapping.<sup>5</sup> Additionally, participants whose employment

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<sup>5</sup> Hence we do not assume, for example, that  $E[Y_1 - Y_2 | J_1 = t, J_2 = n] = E[Y_1 - Y_2 | J_1 = n, J_2 = t]$ .

status is not altered by their program assignment do not contribute to the contrast of outcomes in equation (2). We can similarly write set of six contrasts for  $E[Y | Z = 1] - E[Y | Z = 3]$ , with  $J_3$  replacing  $J_2$  and  $Y_3$  replacing  $Y_2$  in equation (2).<sup>6</sup>

With three observed outcomes  $\{\bar{Y}_1, \bar{Y}_2, \bar{Y}_3\}$  and twelve contrasts among potential outcomes, we must place additional structure on the problem to draw inferences about the parameters of interest.

One set of assumptions sufficient to identify  $\delta^{in}$  and  $\delta^{dn}$  in our experimental setup is that there is no heterogeneity in treatment effects among the treated— that is the effect of direct-hire versus non-employment,  $\delta^{dn}$ , and the effect of temporary help versus non-employment,  $\delta^{in}$  are the same for all of those induced to change program outcome by contractor assignment:

$$(3) \quad Y'_i - Y_i^n = \delta^{in}, \quad Y_i^d - Y_i^n = \delta^{dn} \quad \forall i.$$

Under this constant treatment effects assumption, equation (2) reduces to:

$$(4) \quad \begin{aligned} & E[Y | Z = 1] - E[Y | Z = 2] \\ &= (\Pr[J_1 = d] - \Pr[J_2 = d])\delta^{dn} + (\Pr[J_1 = t] - \Pr[J_2 = t])\delta^{in} \\ &= \gamma^1 \delta^{dn} + \gamma^2 \delta^{in}, \end{aligned}$$

where  $\gamma^1 = \Pr[J_1 = d] - \Pr[J_2 = d]$  and  $\gamma^2 = \Pr[J_1 = t] - \Pr[J_2 = t]$ . Similarly, we can write the contrast between mean outcomes at sites 1 and 3 as:

$$(5) \quad E[Y | Z = 1] - E[Y | Z = 3] = \gamma^3 \delta^{dn} + \gamma^4 \delta^{in},$$

where  $\gamma^3 = \Pr[J_1 = d] - \Pr[J_3 = d]$  and  $\gamma^4 = \Pr[J_1 = t] - \Pr[J_3 = t]$ .

Equations (4) and (5) yield a system of two equations and two unknowns:

$$(6) \quad \begin{pmatrix} E[Y | Z = 1] - E[Y | Z = 2] \\ E[Y | Z = 1] - E[Y | Z = 3] \end{pmatrix} = \begin{pmatrix} \gamma^1 & \gamma^2 \\ \gamma^3 & \gamma^4 \end{pmatrix} \begin{pmatrix} \delta^{dn} \\ \delta^{in} \end{pmatrix},$$

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<sup>6</sup> The contrast  $E[Y | Z = 2] - E[Y | Z = 3]$  is simply a linear combination of the prior two contrasts.



This system we can readily solved for  $\hat{\delta}^{\delta^n}$  and  $\hat{\delta}^m$  using the observed contrasts in mean outcomes across sites  $\bar{Y}_1, \bar{Y}_2, \bar{Y}_3$  and the relative frequencies of direct-hire, temporary-help and non-employment among participants randomly assigned to the three treatment sites:<sup>7</sup>

$$(7) \quad \begin{pmatrix} \hat{\delta}^{\delta^n} \\ \hat{\delta}^m \end{pmatrix} = \begin{pmatrix} \hat{\gamma}^1 & \hat{\gamma}^2 \\ \hat{\gamma}^3 & \hat{\gamma}^4 \end{pmatrix}^{-1} \begin{pmatrix} \bar{Y}_1 - \bar{Y}_2 \\ \bar{Y}_1 - \bar{Y}_3 \end{pmatrix}.$$

This demonstrates that in a constant treatment effects model, the contrast in outcomes across participant sites is sufficient for identification of the causal parameters of interest.

Although this assumption may appear quite restrictive at first blush, our sample of welfare recipients is fairly homogeneous and, presumably, the subsample whose employment or job status is impacted by program assignment is more homogeneous. Below, we utilize over-identification of our parameters to present evidence suggesting that an assumption of a constant treatment effect among those affected by program assignment may be a reasonable approximation.

Even if the constant treatment effects assumption does not hold (but the assumption that program assignment affects outcomes only through its effect on employment status continues to hold), we may still ascribe a causal interpretation to differences in outcomes across programs. These differences would represent the marginal effect on outcomes of a treatment that induces higher rates of temporary agency or direct-hire employment. Thus, our policy experiment directly addresses the policy-relevant question of whether an increase in temporary agency or direct-hire job placements confers, on balance, long-term benefits among the program's clientele. However, in this case, the mixture of employment status switches as shown in (2) would be

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<sup>7</sup> Because randomization insures the independence of  $J$  and  $Z$ , parameters  $\{\gamma^1, \dots, \gamma^4\}$  may be estimated consistently as  $\hat{\gamma}^1 = \{\Pr(J = d \mid Z = 1) - \Pr(J = d \mid Z = 2)\}$ , and similarly for  $\gamma^2$ , etc.

unknown, and hence estimates would not have a direct interpretation as the effect of temporary help or direct-hire employment for a specific population.

### **3. The Michigan Work First Program and Data**

#### **a. Program Context**

In the metropolitan area we study, individuals applying for Temporary Assistance for Needy Families (TANF) must report the Family Independence Agency (FIA) office servicing their district. FIA determines their eligibility for cash assistance and refers them to a Work First contractor. Eligible applicants not meeting mandatory work requirements must begin the Work First program within approximately two weeks of applying for assistance. In districts with more than one Work First contractor, contractors alternate taking new participants. Thus, assignment to a Work First contractor depends on the timing of when an individual applies for TANF benefits.

The focus of the Work First program, as the name implies, is to place participants quickly into jobs. All contractors operating in our metropolitan area offer a fairly standardized one-week orientation that teaches participants basic job-search and life skills. Services such as child-care and transportation are provided by outside agencies and are available on an equal basis to participants at all contractors.

By the second week of the program, participants are expected to search intensively for and find employment. While participants may find jobs on their own, job developers at each contractor play an integral role in placing participants. Job developers encourage participants to apply for—or discourage participants from applying for—jobs with certain employers, including temporary agencies. Job developers also provide more targeted services, helping to screen participants and referring them to employers for specific job openings. In addition, they often arrange visits by employers, who screen and recruit participants for jobs directly at the

contractors' Work First office. Thus, the jobs that Work First participants hold depend, in part, on the employer contacts that their contractor's job developers have. Of particular interest for our study, the probability of holding a temporary agency job depends partly on job developers' beliefs about the suitability of temporary agency employment for their participants, which, in turn affects referrals and other contacts they provide with agencies.

Critical to our empirical approach is the assumption that contractors only affect participant outcomes by affecting the probability of placing individuals in jobs and the types of jobs in which they are placed. The fact that very few resources are spent on anything besides job development, that general or life skills development occurring in the first week of the program are very similar across contractors, and that support services (e.g. childcare and transportation) that would aid in job retention are provided outside this program and are equally available to participants in all contractors supports this assumption.

**b. Data and Randomization**

Our data set comprises administrative records data from the Work First program linked with quarterly earnings from the state of Michigan's unemployment insurance wage records data base. For our metropolitan area, we have Work First administrative data on all Work First spells initiated from the fourth quarter of 1999 through the first quarter of 2004. The data include information on the FIA district and Work First contractor to which the participant was assigned, and basic demographic information on age, race, gender, and education level. Individuals may have multiple spells in our data. In addition, one of our outcome measures is program recidivism anywhere in the state. To construct this measure, we link Work First data for our metropolitan area to state level Work First program data to identify individuals who reentered the Work First program elsewhere in the state.

An important and unique feature of our Work First data is that they include detailed information on jobs obtained by Work First participants while in the program. For each spell, we have information on the hourly wages, weekly hours, employer name, and job title for up to six jobs obtained while the individual was in the Work First program. We construct the implied weekly earnings for each Work First job by multiplying the reported hourly wage rate by weekly hours. We used the employer name in conjunction with several lists of temporary help agencies in the metropolitan area to code each employer as either a temporary agency or a direct-hire employer.<sup>8</sup> In a small number of cases where the appropriate coding of an employer was unclear, we collected additional information on the nature of the business through an internet search or telephone contact. We coded jobs into broad occupational groups based on job title.

Work First administrative data are linked to quarterly state-level unemployment insurance earnings records.<sup>9</sup> These data include total earnings in the quarter and the industry in which the individual had the most earnings in the quarter. UI wage records data are available from the third quarter of 1997 through the fourth quarter of 2004. We constructed earnings for the eight quarters prior to the quarter of program entry for each participant spell. We were able to construct quarterly earnings for four quarters following the quarter of program entry for each participant spell in our sample; where possible, we constructed quarterly earnings out to eight quarters following the quarter of program entry.

In thirteen of the FIA districts in our metropolitan area, more than one Work First contractor serviced the district over the time period studied.<sup>10</sup> Contracts to provide Work First

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<sup>8</sup> Particularly helpful was a comprehensive list of temporary agencies developed operating in our metropolitan area as of 2000, developed by David Fasenfest and Heidi Gottfried.

<sup>9</sup> The UI wage records exclude earnings of federal and state employees and the self-employed.

<sup>10</sup> We dropped two districts from our sample because each included a contractor servicing primarily ethnic populations. For these contractors only, Work First participants throughout the metropolitan area were allowed to select one of these two providers if

services ran for one year, from the fourth quarter of a calendar year through the third quarter of the following year. Although the contractors providing services within a district could change across program years, there was considerable stability in the assignment of contractors to districts, particularly in the last two and a half program years covered by our data.

From the thirteen districts represented in the three and a half program years covered by our data, we selected Work First spells initiated in the last two and a half program years in six districts to form the “primary” sample for which we report results below. Although the discussion below focuses on results from our primary sample, we also report results over the entire time period and for all thirteen districts to show that our conclusions are not sensitive to the selection of this sample. We selected this primary, six-district sample because: 1) contractor assignments within these districts were stable over this time period (a number of changes in contractor-district assignments occurred between the first and second program years covered by our data); and 2) within each of the six districts, there were large and persistent differences across contractors in the fraction of Work First participants placed in jobs and/or the type of job placement (temporary versus direct hire).

Table 1 presents the means of variables on demographics, work history, and earnings following program entry for all Work First participants in our primary sample as well as by program outcome: direct-hire job, temporary agency job, or no job. The sample as a whole is predominantly female (94 percent) and black (96 percent). Slightly under half of Work First spells resulted in job placements. Among spells resulting in jobs, 20 percent have at least one job with a temporary agency.

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they were unable to speak English. Thus, the participant pool serviced by these contractors was quite different from that of the other contractor operating in the district.

The average earnings over the four quarters following program entry are comparable for those obtaining temporary agency and direct-hire jobs, while earnings for those who do not obtain employment during the Work First spell are 40 to 50 percent lower. These simple comparisons have no causal interpretation. As is evident from the other descriptive statistics reported in Table 1, the average characteristics of Work First participants vary considerably according to Work First job outcome. Those who do not find jobs in Work First are more likely to have dropped out of high school and to have worked fewer quarters and have lower prior earnings than those who find jobs. Among those placed in jobs, those taking temporary agency jobs actually have somewhat higher average prior earnings and quarters worked than those taking direct-hire jobs. Not surprisingly, those who take temporary jobs in Work First have higher prior earnings and more quarters worked in the temp sector than those who take direct-hire jobs. Data used in previous studies show that blacks are much more likely than whites to work in temporary agency jobs (Autor and Houseman 2002; Heinrich, Muser, and Troske 2002). Even in our predominantly African-American sample, we also find this relationship.

The descriptive statistics in Table 1 show that there are large average differences in the observed characteristics of Work First participants with direct-hire, temporary agency, and no jobs. These differences in demographic characteristics and work histories likely affected job placements in Work First and subsequent labor market outcomes. The large differences we observe suggest that there may also be important differences we do not observe in the characteristics of Work First participants that affect job placements and subsequent labor market outcomes. Thus, simple comparisons of outcomes among Work First participants in direct-hire, temporary agency, or no employment—or even simple regressions that only control for

differences in observed characteristics—are unlikely to provide a reliable estimate of the effect of direct-hire or temporary agency employment on subsequent earnings.

Our approach to addressing the potential selection problem exploits the natural experiments within the six Work First districts comprising our primary sample. If assignment of Work First participants across contractors operating within each of these six districts is functionally equivalent to random assignment, then the large and persistent differences among contractors in job placement patterns arguably are attributable to differences in contractor practices—not to differences in the Work First populations serviced by the contractors. Therefore, it is important to establish that these Work First assignments are consistent with random assignment. Accordingly, for each district and year, we test for non-random assignment of the following participants characteristics to contractors in the district: gender, race, age, high-school drop-out status, number of quarters worked in the eight quarters prior to program entry, number of quarters primarily employed with a temporary agency in these prior eight quarters, total earnings in these prior eight quarters, and total earnings from quarters where a temporary agency was the primary employer in the prior eight quarters.

To implement this test, we estimate a Seemingly Unrelated Regression (SUR) system for each district and year and use it to calculate the probability that the observed distribution of participant covariates across contractors within districts and years is consistent with chance.<sup>11</sup>

Let  $X_{it}^k$  be a  $k \times 1$  vector of covariates containing individual characteristics for Work First participant  $i$  assigned to one Work First contractor in district  $d$  during year  $t$ . Let  $Z_{it}$  be a vector of indicator variables designating the contractor assignment for participant  $i$ , where the

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<sup>11</sup> This method for testing randomization across multiple outcomes is proposed by Kling et al. 2004 and Kling and Liebman 2004.

number of columns in  $Z$  is equal to the number of contractors in district  $d$ . Let  $I_k$  be a  $k$  by  $k$  identity matrix. We estimate the following SUR model:

$$(8) \quad X_{dt} = (I_k \otimes (Z_{dt} \ 1))\theta + \nu \quad X_{dt} = (X_{dt}^1, \dots, X_{dt}^k)' ,$$

Here,  $X_{dt}$  is a stacked set of the participant covariates, the set of control variables include contractor assignment dummies and a constant, and  $\nu$  is a matrix of error terms that allows for cross-equation correlations among participant characteristics within district-contractor cells.<sup>12</sup> The p-value for the joint significance of the elements of  $Z$  in this regression system provides an omnibus test for the null hypothesis that participant covariates do not differ among Work First participants assigned to different contractors within a district and year, with a high p-value corresponding to an acceptance of this null.

The p-values for the significance of  $Z$  in estimates of equation (8) for each district and year (6 districts  $\times$  4 years) are provided in Table 2 in the row labeled “Randomization.”

Consistent with the hypothesis that assignment of Work First participants across contractors operating within each district is functionally equivalent to random assignment, we find that 22 of 24 comparisons accept the null hypothesis at the 10 percent level or better.

These 24 individual tests are not entirely germane to our subsequent analysis, however. Our main analyses in the outcomes sections pool variation across districts and years to identify the effect of Work First job status on labor market outcomes. Hence, it is critical to perform grouped statistical tests to evaluate the validity of the randomization for the entire experiment. With 24 independent p-values, a simple comparison of each p-value to a conventional significance threshold (such as  $\alpha = 0.05$ ) is likely to lead to one or more false rejections of the null. To maintain the overall probability of Type I error at a target level, we implement Holm’s

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<sup>12</sup> Since the contractor assignment dummies in  $Z$  are mutually exclusively, one is dropped.



Sequentially Selective Bonferroni Method for multiple-comparisons (Holm, 1979). The Holm version of the widely-used Bonferroni multiple-comparison test provides a more conservative test of our null hypothesis that these eight covariates are balanced across contractors operating within each district in each program year—i.e. the test is more likely to reject our hypothesis of random assignment. The Holm-Bonferonni method is explained in some detail in the Appendix. We discuss the results here.

The p-values for the Holm-Bonferroni tests for the Work First randomization are given in Table 2. The right-hand column of the table provides p-values for the multiple comparison test of randomization of participant characteristics in all six districts in each assignment year. The bottom row of the table provides p-values for the multiple comparison test of randomization of participant characteristics in all four assignment years in each district. The bottom right-hand cell of the table provides the p-values for the multiple comparison test for all districts and years simultaneously.

Consider first the top row in the right-hand column. The p-value of 0.10 indicates that for all six districts considered simultaneously in assignment years 1999-2000, the null hypothesis of random assignment is accepted at the 10 percent level. Subsequent rows show this null is accepted at or above the 10 percent level in each year of the randomization. Similarly, the bottom rows of each column show that the null of random assignment is accepted at the 7 percent level or better for each of the six districts considering all four years of data simultaneously. Finally, the bottom-right cell of Table 2 reveals that the omnibus test for all 24 comparisons – that is., the entire experiment – is consistent with the null of random assignment with a p-value of 0.84 level. In net, the distribution of Work First participants across contractors within districts appears functionally equivalent to what would be achieved by random assignment.

#### **4. First Stage Results: The Impact of Contractor Assignment on Employment Outcomes**

##### **a. Do Contractor Assignments Affect Job Placements?**

Having established that the randomization of participants across contractors within districts appears valid, we now test whether the random assignment of participants to contractors had significant effects on the job outcomes – direct-hire employment, temporary help employment, no employment – that participants obtained during their Work-First spells. We evaluate this hypothesis in manner identical to our prior test for participant randomization. Specifically, we estimate a set of SUR models akin to equation (8) where in this case the dependent variables are participants Work First job outcomes following program assignment rather than their demographic characteristics and prior earnings history. Whereas in the prior test of randomization, we expected that characteristics of incoming participants would be balanced among Work-First contractors within a district, here we anticipate that job outcomes of participants will differ significantly depending on the Work First contractors to which they are assigned.

Results for this set of tests are also provided in Table 2. For each district and year, we tabulate two p-values, one corresponding to the null that overall employment/non-employment rates did not differ across sites in a district-year (a two-way comparison), and the second corresponding to the null hypothesis that temporary-help employment, direct hire employment and non-employment rates did not differ across sites in a district-year (a three-way comparison). Since overall employment may be identical across sites even while direct-hire and temporary employment levels differ substantially, the two and three-way hypotheses tests are not nested.

Consistent with expectations, most comparisons soundly reject the null hypothesis of no effect of contractor assignments on participants' job placement outcomes. Of 24 three-way

comparisons of job placement outcomes across contractors within each district-year shown in Table 2, only two have a p-value higher than 10 percent, and 19 of 24 have a p-value at or under 1 percent. This stands in sharp contrast to the p-values for the tests of random assignment, where only 2 of 24 comparisons are significant at the 5-percent level. These results are indicative of highly significant contractor effects on job placements among randomly assigned participants.

We next perform Holms-Bonferroni tests for multiple comparisons to evaluate whether we can reject the null of no contractor effects on job outcomes across multiple sites and years. Paralleling the randomization tests in the prior section, the right-hand column of Table 2 provides p-values for the multiple comparison test across all six districts in each assignment year and the bottom row provides p-values for the multiple comparison test across all four assignment years in each district. These tests provide quite strong support for the efficacy of the research design: all of the tests of contractor-assignment effects on participant job placements – either across contractors within a year or within contractors across years – reject the null at the 2-percent level or better. Moreover, the omnibus test for all 24 comparisons (bottom-right cell of the table) rejects the null of no contractor effects on participants’ job outcomes at the 0.02 level for the two-way comparison and at less than the 0.01 level for the three-way comparison. Given the evidence above that participants were effectively randomized across contractor sites, we conclude that contractor assignments had highly significant effect on participant’s subsequent job placements.

**b. The Effect of Job Placements on In-Program Earnings**

We now present initial evidence on the relationship between participants’ program assignments and their “in-program” earnings – that is, wage and hours for jobs obtained during

Work First spells (recorded by Work First contractors).<sup>13</sup> These short-term earnings measures do not provide a complete account of the earnings of Work First participants. In-program earnings are collected only for Work First participants who successfully find a job placement during their Work First spell. They do not enumerate short-term earnings of Work First participants terminated from their programs due to rules violations or inadequate job search – even if these participants find outside employment immediately. Our primary purpose in this section is therefore to summarize the earnings and occupational characteristics of jobs obtained by Work First participants during program spells. As it turns out, these in-program outcome data will also prove useful in interpreting our later results for long-term employment and earnings consequences of Work First job placements.

We begin by estimating the following model by Ordinary Least Squares (OLS):

$$(9) \quad y_{icdt} = \alpha + \delta_1 D_i + \delta_2 T_i + X_i' \beta + \gamma_d + \theta_t + \gamma_d \theta_t + \varepsilon_{icdt}.$$

The dependent variable in this equation,  $y_{icdt}$ , is the in-program earnings variable (real hourly or weekly wages, average weekly hours) of individual  $i$  assigned to contractor  $c$  in randomization district  $d$  in year  $t$ . In this model,  $D_i$  and  $T_i$  are indicator variables equal to one if the participant obtained a direct-hire or temporary-agency job during the Work First spell,  $X$  is a vector of covariates including gender, race (white, black or other), age, education (primary school only, high school dropout, high school graduate, greater than high school), and UI earnings (in real dollars) for the 4 quarters prior to random assignment, and  $\gamma$  and  $\theta$  are vectors of district and year dummies respectively. In estimating equation (9), we calculate Huber-White robust standard errors clustered at the contractor  $\times$  year of assignment level to account for the

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<sup>13</sup> Hours and earnings data used for estimates of in-program earnings models correspond to the first direct-hire or temporary help agency job a participant took during her Work First spell. By definition, hours and earnings are equal to zero for participants who

grouping of participants within Work First service programs. The coefficients of interest in this model are  $\delta_1$  and  $\delta_2$ , which estimate the contrast in hours and earnings for participants who obtained direct-hire or temporary-agency jobs during their Work First spells relative to participants who did not obtain any employment. Since in-program earnings and hours are by definition zero for participants who do not find employment in Work First, OLS estimates of  $\delta_1$  and  $\delta_2$  will necessarily be positive.

The first column of Table 3 presents an estimate of equation (9) for our primary six-district sample using Work First participant cohorts entering 2000 through 2003. Participants who obtained any employment during their Work First spell received, on average, \$7.37 per hour, worked 34.5 hours per week, and earned \$257 per week. Columns (4) and (5) of Table 3 re-estimate this OLS model for wages and hours, now differentiating between temporary-help and direct-hire job placements. The three-way contrast reveals that hourly wages, weekly hours, and weekly earnings are uniformly higher for Work First participants in temporary help jobs than in direct-hire jobs. These differences are both economically and statistically significant. The mean hourly wage in temporary agency job placements is 7 percent higher, weekly hours are 9 percent higher and weekly earnings are 16 percent higher than in direct-hire jobs.

While this pattern stands in contrast to the widely reported finding of lower wages in temporary help positions (cf. Segal and Sullivan, 1998), it appears consistent with the substantial differences in the occupational distribution of temporary help and direct-hire jobs observed in our data. As shown in Figure 1, production and health care positions (primarily nursing aids) account for almost 50 percent of temporary help jobs but less than 20 percent of direct-hire jobs held by Work First participants. Production and healthcare jobs also have among the highest

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did not obtain any employment during their Work First spells. Since Work First contractors face powerful incentives to dutifully

hourly and weekly wages in our sample, as is shown in Appendix Table 1 (clerical work is the highest overall, however). By contrast, direct-hire workers are dispersed across a variety of predominantly low-paying service occupations including cashier, janitor, sales worker, and childcare occupations. Fewer than 1 in 20 direct-hire workers hold production positions, as compared to 1 in 3 temporary help workers.

As with prior earnings comparisons, these relationships do not have a causal interpretation. Participants who find employment during their Work First spell differ systematically from those who do not (Table 1); consequently, wages for these workers are unlikely to provide a reliable guide to the potential earnings of non-employed Work First participants.

We next estimate two-stage least squares (2SLS) models for Work First participants' in-program earnings, where contractor random assignments are used as instrumental variables for participants' employment status attained during their Work First spells. The 2SLS estimates of equation (9) do have a causal interpretation. In the LATE framework above, these estimates will recover the effect of Work First job placements on in-program earnings for participants whose employment status is changed by their contractor random assignment. We refer to these participants as 'marginal' employees.

Columns (2) and (3) of Table 3 provide 2SLS estimates of the effect of any Work First employment on in-program hours and earnings for marginal employees. In the first specification (column 2), Work First participants' employment status is instrumented by contractor assignment dummies; in the subsequent specification, employment status is instrumented by contractor  $\times$  year of assignment dummies. Both models yield a striking pattern of results: hourly wages for

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track the hours and earnings of their program participants, these administrative earnings measures are generally quite accurate.

marginal workers are distinctly lower – by about 40 cents, or 5 percent – than the corresponding OLS estimates. These differences, which are highly significant, suggest that Work First participants whose employment outcomes are changed by their contractor assignment (those at the margin of employment versus non-employment) have weaker skills than infra-marginal Work First workers. This finding is a robust result of our analysis, as demonstrated below.

Columns (5) and (6) of the Table repeat these 2SLS estimates, now differentiating between direct-hire and temporary-help job placements (both instrumented by contractor assignment or contractor assignment  $\times$  year dummies). In both columns, we find that ‘marginal’ temporary help workers and ‘marginal’ direct -hire workers command lower wages (on the order of 5 to 7 percent lower) than do inframarginal workers in the same job categories. This is again consistent with our underlying econometric assumption that Work First participants ‘pushed’ into temporary-help and direct-hire jobs by their contractor assignments are primarily drawn from the ranks of participants who would otherwise be non-employed.

Notably, the 2SLS estimates also bear out the finding that Work First participants placed in temporary help jobs earn more per hour than participants placed in direct hire jobs. In fact, the gap between the estimated direct-hire and temporary-help hourly earnings in 2SLS models is quite comparable to that for the OLS model.

The lower panels of Table 3 present comparable analyses for weekly hours and weekly earnings. In contrast to the findings for hourly wages, 2SLS estimates for the effect of employment on weekly earnings are generally quite comparable to the corresponding OLS estimates. The difference between OLS and 2SLS estimates for weekly hours helps to explain why. The 2SLS models find that marginal workers typically work 2 to 3 more hours per week more than do inframarginal workers, a difference that is highly significant. Since marginal

workers have lower hourly earnings but higher weekly hours than inframarginal workers, the net result is that 2SLS estimates for weekly earnings are comparable to the corresponding OLS models.<sup>14</sup>

To reiterate, Table 3 reveals two key results. First, the short-term earnings gains are generally greater for temporary help than direct-hire placements, particularly on a per-hour basis. Second, Work First participants whose outcomes are changed by the experiment appear to have lower earnings power than inframarginal workers – that is, workers who find jobs regardless of their contractor placements. These results suggest that our identification strategy of using contractor random assignments as instrumental variables has the potential to identify the effects of Work First job placements on participants' post-assignment labor market outcomes.

To evaluate the robustness of these main findings, we present several further sets of estimates performed on expanded data samples. In Panel B of Table 3, we expand the sample to encompass the 1999 entering Work First cohort (in addition to the 2000 through 2003 cohorts in Panel A). Although the Michigan economy was at a business cycle peak in 1999 that subsequently became a prolonged contraction starting in 2000, we find very similar patterns for the more inclusive sample. In the expanded sample, temporary help workers earn higher hourly wages than do direct hire workers; and marginal workers (both temp and direct-hire) earn less than their inframarginal counterparts. We expand the sample further in Table 4 to include all 13 random assignment districts for both 2000 to 2003 and 1999 to 2003 (Panel B). The qualitative pattern of results is again closely comparable.<sup>15</sup>

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<sup>14</sup> One puzzling finding in Table 3 is that the estimated weekly hours of marginal temporary-agency workers are typically lower than those of inframarginal temporary workers while the estimated weekly hours of marginal direct-hire workers are typically greater than those of inframarginal direct-hire workers. These differences are not generally significant (particularly in later tables but are nevertheless unexpected).

<sup>15</sup> The one exception is that it is less clear in the expanded sample that marginal-direct hire workers earn less than inframarginal direct-hire workers (though this pattern remains clearly visible for temporary help workers).



## **5. The Longer-Term Consequences of Work First Job Placements: Evidence on Work First Recidivism and Payroll Earnings**

Although the Tables 3 and 4 results appear to provide encouraging initial evidence that Work First placements into direct-hire and temporary help jobs raise the earnings of Work First clients, they may be potentially misleading for two reasons. The first is that the in-program measures above only reflect earnings accrued during the Work First employment spell, which is typically brief. If these initial job placements are transitory and they do not foster subsequent employment opportunities, the short-term earnings gains may be rapidly eroded by spells of non-employment. Secondly, because the in-program earnings measures exclude any wage earnings for participants terminated from Work First – even those that find work immediately – the estimated wage effects of Work First job placements on subsequent earnings may be upward biased, though in practice this bias may be small.

To provide a more comprehensive assessment of the effect of Work First job placements on participants' employment and earnings, we must look to other outcome measures. One such measure available in the administrative data is Work First recidivism.

### **a. The Effect of Job Placements on Work First Recidivism**

Recidivism in Work First in the Michigan metro area that we study is high. Thirty-six percent of Work First participants in our sample reenter the Michigan Work First program within 360 days of the commencement of their prior spell, and 53 percent reenter within 720 days.<sup>16</sup> recidivism is substantially lower for participants who find employment during their Work First spell, however. As is shown in Table 5, participants who obtain any employment during their Work First spell are 8 to 13 percentage points less likely to reenter the program within one to two

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<sup>16</sup> Our data do not allow us to determine whether participants reenter Work First in other states.

years following Work First entry than are participants who do not obtain employment. These differentials, obtained from OLS estimates of equation (9), are highly significant.

When we subdivide jobs obtained during Work First into temporary-help and direct-hire positions, we find notable differences. Participants who obtain direct-hire jobs are 10 to 14 percentage points less likely to reenter the program in one to two years than are participants who do not find a job. Participants who take temporary-help jobs are also less likely to reenter than non-job-takers, but this differential is much smaller than that for direct-hire jobs: 4 to 8 percentage points versus 10 to 14. Both point estimates are significantly different from zero. They are also significantly different from one another.

These pronounced recidivism differentials are not surprising in light of the earlier comparisons of participant characteristics by Work First employment status (see Table 1). Participants who obtained any employment during their Work First spells had significantly higher earnings and more quarters in paid employment in the year prior to Work First placement than did participants who did not obtain jobs. This suggests that there may be other factors not measured in our data that affect both in-program employment and long-term outcomes. In this case, some or all of the negative correlation between Work First job placement and subsequent recidivism would be explained by heterogeneity among Work First participants rather than true effects of job-taking on recidivism.

To purge this source of bias, we estimate 2SLS models for program recidivism where participants' Work First employment outcomes are again instrumented using randomized program assignments. These instrumental variables estimates differ substantially from their OLS counterparts. In fact, 2SLS models for the two-way comparison between employment and non-employment in Table 5 never detect a significant effect of Work First job placements on

subsequent recidivism. Moreover, the 2SLS estimates are typically close to zero or weakly positive, suggesting that Work First job placements may slightly increase recidivism among participants.

When we disaggregate Work First placements into temporary-help and direct-hire positions – again, using program assignments as instrumental variables – an equally striking pattern emerges. These estimates reveal that the small and statistically insignificant effect of any employment on Work First recidivism appears to mask large, countervailing impacts of temporary-help and direct-hire job placements. Columns (5) and (6) find that direct-hire placements reduce participants' Work First recidivism by 15 to 30 percentage points over the one to two years following placement. Conversely, temporary help placements are found to increase program recidivism by 20 percentage points (more in some estimates) during the one to two years following program entry.

It must be stressed that most estimates obtained using the 2SLS model in the primary 6-district sample are not statistically significant. In addition, the estimated magnitudes are typically smaller in magnitude (though still sizable) when contractor  $\times$  year assignments are used as instruments rather than simply contractor assignments. However, the pattern of results suggesting that direct-hire job placements reduce program recidivism and that temporary-help placements raise recidivism is remarkably consistent and stable across all of the many samples and time periods found in Table 5.

These results provide some initial evidence that the positive, short-term effects of job placements on in-program earnings documented in Tables 3 and 4 may not culminate in net gains in Work First participants' labor market outcomes. At a minimum, the 2SLS recidivism estimates provide no evidence that Work First job placements reduce welfare dependency in the

one to two years following Work First assignment. More speculatively, the results suggest that temporary-help and direct-hire job placements have opposing impacts on Work First participants' post-program outcomes, with direct-hire placements reducing welfare dependency and temporary-help placements increasing it.

**b. The Effect of Job Placements on Longer-Term Labor Market Outcomes: Evidence from Matched Unemployment Insurance Records**

We now employ the quarterly earnings records from the state of Michigan's unemployment insurance (UI) database linked to administrative Work First records to assess how Work First job placements affect earnings over one to two years (at quarterly intervals) following random assignment.<sup>17</sup> We again estimate OLS and 2SLS variants of equation (9), where the outcome variable is the sum of real UI payroll earnings over a designated time interval following Work First placement.

As an initial test of the reliability of the UI data, we estimate the relationship between participants' in-program job placements and their reported UI earnings during the actual calendar quarter of their Work First program assignment, which we refer to as 'quarter zero.' The UI records should accurately capture all payroll earnings received in the state of Michigan excluding those of federal and state employees and the self-employed. We therefore expect to find that participants who obtain employment during their Work First spell will have higher UI earnings during their actual quarter of assignment than participants who do not find jobs.<sup>18</sup>

The first set of rows in Table 6 provides earnings estimates for quarter zero for the primary, six-district sample. The OLS estimate in column (1) shows that participants who find

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<sup>17</sup> Although it is potentially feasible to track post-assignment earnings for more than 8 quarters, this is not currently practical because only 1 to two years have elapsed since assignment for the bulk of the Work-First spells in our sample.

<sup>18</sup> We do not generally use quarter zero earnings as a primary outcome measure, however. Since participants' Work First spells may commence at any point during a calendar quarter, UI earnings observed in quarter zero potentially combines earnings accrued both before and after the Work First placement.

employment during their Work First spell have significantly higher UI earnings than those who do not. The point estimate for this contrast is 306 dollars, with a standard error of 17. When we distinguish between temporary help and direct-hire jobs, we also find highly significant relationships. Both job types are associated with approximately 300 additional dollars of quarter zero UI earnings, and the estimates for temporary-help and direct-hire jobs do not differ significantly.

The next columns of Table 6 provide 2SLS estimates of the effect of job placements on quarter zero earnings. These estimates also confirm the quality of the match between the Work First and UI databases. Consistent with the administrative wage records for in-program earnings, the UI data confirm that marginal job-placements induced by the randomization lead to significant gains in payroll earnings in quarter zero, the calendar quarter of the Work First placement.<sup>19</sup> These results provide assurance that the UI data is suited to measuring the effects of Work First job placements on subsequent payroll earnings.

We now perform estimates of the effect of Work First job placements on payroll earnings for quarters 1 through 4 following Work First assignment. These estimates have several advantages over the prior administrative wage results. First, all UI earnings during these quarters reflects post-assignment earnings, providing a longer-term follow-up on the earlier in-program earnings results. Second, because both in-program and out-of-program earnings are captured by the UI data, the analysis is not biased towards under-counting payroll earnings of participants who exit Work First and find employment immediately. Consequently, 2SLS estimates of the relationship between Work First placements and post-assignment UI earnings analysis should

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<sup>19</sup> We have no reason to expect a correlation between the point in a calendar quarter when participants enter a Work First spell and their subsequent employment outcomes during that spell. Hence, 2SLS estimates of the effect of in-program job placements on quarter zero earnings should not be biased by the possible inclusion of pre-assignment UI earnings in the quarter zero measure.

provide a fairly clean and powerful test of whether Work First job placements raise participants' total payroll earnings.

Descriptive OLS estimates in columns (1) and (4) show that direct-hire and temporary-help Work First placements are associated with substantially higher payroll earnings in the year following the commencement of a Work First spell. Participants who obtain any employment during their spell earn approximately 1,700 dollars more in quarters 2 through 4 than do participants who do not find employment. The point estimates for direct-hire and temporary help jobs are also quite comparable to one another (column 4). Taken at face value, these results suggest that both types of job placements are equally effective for raising participants' payroll earnings.

The 2SLS estimates for these models suggest otherwise. This may initially be seen by comparing the OLS and 2SLS estimates for the effect of any in-program employment on UI earnings in quarters two through four (columns 2 and 3). As noted, OLS estimates indicate that participants who obtain any job during their Work First spell earn about 1,700 dollars more in quarters two through four than participants who do not take a job. When Work First employment is instrumented with contractor assignment, however, this earnings effect falls to 851 dollars, which is statistically significantly different from zero and from the OLS estimate. This comparison suggests that the OLS comparisons may substantially overstate any effect of Work First job placements on post-placement earnings.

Subsequent columns of Table 6 reveal why OLS and 2SLS estimates diverge. In the 2SLS models in columns (5) and (6), we find that temporary-help and direct-hire placements have strongly countervailing effects on participants' payroll earnings. Direct-hire job placements induced by randomized contractor assignments have large, positive and highly significant effects

on payroll earnings, typically in the range of 1,700 to 3,500 dollars over quarters two through four following contractor assignment. The 2SLS estimates of the effects of temporary-help employment on subsequent payroll earnings, by contrast, are primarily negative and occasionally large, though generally not significant.

In Table 7, we extend the analysis over a longer time period to evaluate the effect of job placements on payroll earnings over the eight calendar quarters following Work First contractor assignment. For this analysis, we limit the sample to participants placed prior to 2003, i.e., those for whom we have sufficient post-placement earnings data. We find that the effects of Work First job placements on payroll earnings strongly persist into the second earnings year following placement. For the sub-sample used for this analysis, we confirm that direct hire Work First placements induced by contractor assignment also yield an earnings gain of approximately 1,600 to 4,000 during quarters two through four following the start of the spell (all estimates are significant). In the second year following placement, we find that assignment-induced direct-hire placements also raise earnings in quarters five through eight by an additional 1,600 to 5,900 dollars (all significant). In net, Table 7 shows that direct hire placements increase Work First participants' payroll earnings by 4,000 to 11,000 dollars in the two years following placement.

In contrast, estimates for the effect of temporary-help placements on post-program earnings are uniformly negative for quarters two through eight following the commencement of the Work First spell. These earnings losses average two to three thousand dollars in quarters two through four and an additional one to three thousand dollars in quarters five through eight. Although most of point estimates are not significant at below the 10 percent level, the consistency of the signs and magnitudes of the results strongly suggests that temporary help placements do retard subsequent payroll earnings.

One notable pattern shown in the final rows of Table 7 is that 2SLS estimates of the effect of any Work First placement on payroll earnings over the two years following assignment are generally positive but insignificant. From this pattern, one might conclude that Work First job placements have generally modest and non-robust effects on clients' later earnings. But the breakdown of Work First placements into temporary-help and direct-hire placements clearly shows that the relatively weak pooled effect masks a strong positive effect of direct-hire placements on earnings and an imprecise but decidedly negative effect of temporary-help placements on Work First participants' earnings.

UI earnings are a function of individuals' hourly wages and of the hours spent in employment. Unfortunately, we cannot decompose the effects of job placement on earnings into the part attributable to variation in wage levels and the part due to variation in hours worked with our data. However, the data do allow us to tell whether an individual had any earnings, and hence any employment, in a quarter, which is a crude measure of the amount the individual worked. Paralleling our analysis of participant earnings, we examine the effects of job placement on quarters of employment up to two years following program entry. Tables 8 and 9 report these results for our six primary randomization districts over the 2000-2003 period and the 1999-2003 period.

Placement in any job significantly increases the quarters of employment in both OLS and 2SLS models. According to our 2SLS estimates, placement of Work First clients in any job increases quarters of employment 0.3 quarters in the initial quarter following program entry, by 0.3 to 0.4 quarters in the second through fourth quarter following entry, and by 0.7 to 0.9 quarters over the eight quarters following entry.



However, these estimates for any job placement mask large differences in the impacts of placements in direct-hire versus temporary agency jobs on subsequent employment. In the OLS models, the point estimates of the effects of temp agency and direct-hire job placements are similar to each other and to the estimates for any job placement. Mirroring the patterns we observe for earnings, the 2SLS squares estimates indicate that the positive effects of marginal job placements on quarters employed derive entirely from the positive effects of placements in direct-hire jobs. The 2SLS estimates of temp agency placements on subsequent quarters worked generally are close to zero or, in several models, large and negative, though the latter are imprecisely measured and generally not statistically significant. Thus, among these marginal temps, temp agency jobs do not increase, and may even reduce, subsequent quarters worked.

These results are consistent with our findings that placements in temporary agency jobs do not reduce, and may even increase, program recidivism. Moreover, coupled with our findings that temp agency jobs are associated with higher hourly wages than direct-hire jobs, these results indicate that the effects of temporary jobs on subsequent hours worked, not on hourly wages, likely explains why marginal temp jobs fail to raise subsequent earnings relative to no job placement and lower earnings relative to direct-hire placements.

**c. Evaluating the Robustness of the Long-Term Earnings Effects**

One may be legitimately concerned that our identification might be driven by outliers. We have performed a large number of robustness tests to verify the basic pattern of results in Tables 6 and 7. In Table 10, we re-estimate the UI wage models using the broadest feasible sample of all thirteen randomization districts and all years of data (a total of 50,298 Work First spells). In all cases, the 2SLS estimates indicate a short-term positive effect of temporary-help placement on payroll earnings in quarters zero and one of assignment, and a negative but

insignificant effect of temporary-help placement on payroll earnings in quarters two through four, five through eight, and one through eight summed. As with prior tables, the 2SLS estimates for the impacts of direct-hire placements on payroll earnings are consistently positive and significant, with stable magnitudes that are comparable to those above.

To provide a more specific check that our results are not driven by one or two districts, we separately estimate the earnings impacts of Work First job placements separately for each of the six primary random assignment districts in our sample. If this is the case, it should be apparent in our by-district results. Because there are only two Work First contractors per district in two of the six districts, we only provide two-way (employment/non-employment) earnings contrasts for these districts in the 2SLS models. For the remaining four districts, we provide both two and three-way (direct-hire, temporary help, non-employment) contrasts.

The overall pattern of results by district for the six districts in our primary sample, found in Table 11, appears (to us) surprisingly consistent. Focusing on the 2SLS models, the majority of estimates of the effect of any job placement on payroll earnings over one to eight quarters following Work First placement – and all of the significant estimates – are positive. When we form instrumental variables estimates of the three-earnings contrast between direct-hire, temporary help, and non-placement earnings (now limiting the analysis to the four districts having three-plus contractors), we also find that the majority of direct-hire earnings effects – and all of the significant effects – are positive. As strikingly, more than 75 percent of the 2SLS point estimates of the effect of temporary help placements on payroll earnings are negative (though none are significant in these sub-samples). The discussion in the econometric framework in Section 2 raises the concern that the possible heterogeneity of treatment effects across randomization sites potentially complicates interpretation of the 2SLS results. The consistency of

the by-district results provides some direct evidence that the assumption of homogeneous treatment effects among those whose job outcome is affected by contractor assignment is a reasonable approximation in our data.

One might also be concerned that a contractor's placement rate with temporary agencies is correlated with some other contractor behavior that results in weaker labor market outcomes for its clients. Above, we cast doubt on this possibility, noting that contractors provide little beyond job placement services and that support services, such as transportation or childcare, which might improve job retention, are provided outside the Work First program and are available on an equal basis to clients serviced by all contractors. Nevertheless, it is still possible that contractors with higher temp placement rates are generally poorer at matching clients in good jobs. Under this scenario, our estimates, which instrument temp agency and direct-hire placements by contractor assignment, would be capturing the effect of "bad contractors," not the effect of temporary agency placements. If this were the case, then we would expect that clients placed in direct-hire jobs would also do relatively poorly in high-temp-use contractors. To test this possibility, for the sample of Work First clients placed in direct-hire jobs, we regressed earnings over the four quarters following program entry on the fraction placed in temporary jobs and the fraction not placed in jobs by that individual's contractor in that program along with controls for demographic characteristics, the quarter and year of program entry, and earnings during the four quarters prior to program entry. In results not reported here, we find that higher temporary agency placements at a contractor are associated with modestly higher—not lower—earnings outcomes for those placed in direct-hire jobs. Thus, although this test is not definitive, we find no support for the contention that higher temp agency placement rates are associated with contractors that, overall, provide poor placement services.

Finally, one might be concerned that our results are driven by our sample period, which was dominated by a weak economy. One might hypothesize that the primarily negative effects of temporary help placements found in earlier tables may reflect the poor state of the economy, and that these placements might have had more positive impacts on earnings in an expansionary period. To test this possibility, we also performed separate estimates of the impact of Work First job placements on payroll earnings for Work First cohorts entering exclusively in the year 1999. As noted earlier, 1999 is an exceptional year for our study in that it is the only non-recessionary year covered by our data. As displayed in Table 12, the 1999-cohort estimates do not appear to support this hypothesis. We consistently find for the 1999 cohort – as with later cohorts – direct-hire placements raise earnings by several thousand dollars over the next eight calendar quarters (always significant) while temporary help placements appear to reduce earnings by 1,500 to 3,000 dollars, though these results are never significant in the single cohort sample.<sup>20</sup>

The consistency of the estimates for payroll earnings effects of temporary-help and direct-hire placements across entry cohorts, outcome periods, and randomization districts substantially increases our confidence in the reliability of these findings.

## **6. Conclusions**

The primary finding of our analysis is that direct-hire Work First placements induced by the random assignment of participants to Work First contractors significantly increase quarters employed and raise marginal participants' payroll earnings by several thousand dollars over the subsequent two years – a relationship that is significant, consistent across samples, and economically large. By contrast, we find that temporary-help placements do not raise – and quite

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<sup>20</sup> Notably, 2SLS estimates of the effects of Work First job placements on recidivism for the 1999 cohort (not reported) are again quite comparable to those for the pooled 2000 through 2003 cohorts.

possibly lower – quarters employed and the payroll earnings of Work First clients over the one to two years following placement.

We had anticipated finding, consistent with the studies cited in Section 1, that temporary-help placements yield small but potentially positive improvements in labor market outcomes for Work First participants. This is not what we conclude. Both administrative and UI data are consistent in indicating that temporary help placements do not appear to improve labor market outcomes for Work First participants. As shown in Tables 3 and 4, temporary-help placements do raise participants' short-term earnings during their Work First spells. This pattern is initially found in the administrative earnings data collected by Work First contractors and confirmed by the analysis of UI wage records in Tables 6 through 8. Yet, the recidivism results found in the administrative data (Table 5) hinted that these gains are fleeting. All instrumental variables estimates of the effects of job-placement on Work First recidivism suggest that temporary-help placements increase Work First recidivism, even while direct-hire placements reduce it. Most dispositive for our conclusions are the results for payroll earnings drawn from the universe of Michigan Unemployment Insurance records. For all permutations of sampled districts, entry cohorts, and post-assignment time interval, our estimates uniformly demonstrate that temporary help placements have zero or negative effects on payroll earnings while direct-hire placements have significant, durable earnings benefits.

Our data do not permit a detailed exploration of why temporary help placements appear to provide (at best) no long-term benefits to Work First participants. Our leading hypothesis, however, is that temporary-help assignments may displace other productive job-search and employment opportunities. Consequently, the short-term earnings benefits of temporary help jobs – including, as shown in Table 3, comparatively high wages, weekly hours and weekly

earnings during the initial placement – are more than offset by other negatives of temporary help positions, most likely the short-term and sporadic nature of job assignments. This hypothesis is supported by our findings that temporary agency employment does not increase, and may even reduce, quarters worked over the subsequent eight quarters, and by our findings that temporary agency jobs do not reduce program recidivism and may even increase it. The termination of temporary help job spells may lead to either non-employment or a repeat spell in the Work First program while participants seek to reenter the labor market. Either outcome may result in earnings losses over the longer term that more than offset the short-term earnings benefits of temporary help placements.

We should emphasize that some individuals may well reap long-term benefits from a temporary agency assignment. Our results showing no long-term benefit of temporary agency jobs only pertain to the marginal temp job placements; our identification strategy estimates the effects for individuals whose job status or type was impacted by contractor assignment. Nevertheless, the findings from our quasi-experiment are particularly germane for policy. The relevant policy question is whether job programs assisting welfare and other low-wage workers could improve clients' labor market outcomes by placing more clients with temporary agency positions. Our quasi-experiment provides direct evidence on this question.

Based on observational data used in the studies cited above, several researchers recently have advocated greater use of temporary help agencies in job placement programs to help welfare and low-wage workers transition to employment (Lane et al., 2003; Andersson et al., 2005; Holzer 2004). The evidence from our quasi-experimental study indicates that such a policy prescription is certainly premature and likely misguided.

## Appendix: The Holm-Bonferroni test

The canonical Bonferroni test is based on the ‘Bonferroni inequality:’  $\text{pr}(A \text{ or } B) \leq \text{pr}(A) + \text{pr}(B)$ . This inequality is useful because it holds regardless of whether  $A$  and  $B$  are independent. Consequently, if we want to test whether  $(\text{pr}(A) \leq \alpha \text{ or } \text{pr}(B) \leq \alpha)$ , it is sufficient to test that  $\text{pr}(A) \leq \alpha/2$  and  $\text{pr}(B) \leq \alpha/2$ . Using this logic, the Bonferroni test compares each individual p-value in a multiple comparison to the critical value  $\alpha$  divided by the number of comparisons,  $N$ . The Bonferroni rejects the null if any of the  $N$  comparisons falls below the critical value  $(\alpha/N)$ .

As is well known, the Bonferroni method is extremely conservative and hence has limited power to reject the null if two or more of the null hypotheses are in fact false. The reason for this low power is that the Bonferroni applies the same critical value to each null; yet, after each null that is accepted, fewer tests remain and hence a higher (less conservative) critical threshold is appropriate.

Holm’s variant of the Bonferroni accounts for this fact by applying a different critical value for each hypothesis. With  $N$  tests  $\{A_1, A_2, \dots, A_N\}$  and critical value  $\alpha$ , the Holm-Bonferroni orders the p-values from lowest to highest and compares each p-value to the critical value of  $\alpha/(N-i+1)$ , where  $i$  is the ranking of the p-value. The procedure is sequential: the lowest p-value is compared to the most conservative critical value  $(\alpha/N)$ ; conditional on acceptance of the null, the next p-value is compared to  $\alpha/(N-1)$ , etc. If any comparison rejects, the multiple-comparison is said to reject the null. Because each sequential test uses the appropriate Bonferroni threshold for the number of hypotheses remaining (e.g., the critical value for the final hypothesis is  $\alpha/(N-N+1) = \alpha$ ), the Holm-Bonferroni maintains an expected Type I

error level of no greater than  $\alpha$  while providing more power against Type II errors than the simple Bonferroni.



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Table 1. Characteristics of Work First Participants, by Job Type Sample: Six Primary Randomization Districts, Clients Assigned in Program Years, 2000-2003

Variable	All	Direct-Hire Job	Temp Agency	No Job
<b>Demographics</b>				
Age	29.5 (0.06)	29.7 (0.09)	30.5 (0.17)	29.2 (0.08)
% Female	93.9 (0.17)	93.7 (0.28)	93.2 (0.57)	94.1 (0.24)
% Black	96.4 (0.13)	96.2 (0.22)	97.6 (0.35)	96.3 (0.19)
% White	2.7 (0.12)	2.9 (0.19)	1.8 (0.30)	2.7 (0.16)
% Other race	0.9 (0.07)	0.9 (0.11)	0.6 (0.18)	1.0 (0.10)
% High school dropout	34.2 (0.34)	31.4 (0.53)	32.2 (1.06)	36.7 (0.48)
% High school	36.1 (0.34)	37.4 (0.55)	39.5 (1.11)	34.4 (0.48)
% High school plus	8.4 (0.2)	9.2 (0.3)	9.2 (0.7)	7.6 (0.3)
% Education unknown	21.3 (0.3)	22.0 (0.5)	19.1 (0.9)	21.3 (0.4)
<b>Work History, previous 4 quarters</b>				
Earnings	4,606 (46)	4,973 (75)	5,266 (147)	4,199 (63)
Quarters employed	2.03 (0.01)	2.16 (0.02)	2.24 (0.03)	1.89 (0.02)
Temp earnings	258 (11)	234 (16)	538 (50)	222 (16)
Temp quarters employed	0.12 (0.00)	0.11 (0.01)	0.22 (0.02)	0.11 (0.01)
<b>Labor Market Outcomes, subsequent 4 quarters</b>				
Earnings	4,355 (44)	5,839 (76)	5,753 (153)	2,954 (52)
Quarters employed	1.95 (0.01)	2.45 (0.02)	2.44 (0.03)	1.47 (0.01)
Temp earnings	278 (13)	247 (19)	1130 (82)	137 (12)
Temp quarters employed	0.11 (0.00)	0.10 (0.01)	0.41 (0.02)	0.06 (0.00)
Recidivism	0.34 (0.00)	0.28 (0.01)	0.32 (0.01)	0.39 (0.00)
N	19,554	7,612	1,940	10,002

Mean (standard error) of variable reported.

Table 2. P-Values of Holm-Bonferroni Tests of Random Assignment across Work First Contractors and of First Stage Effects of Contractor Assignment on Employment Outcomes during Work First Spells: Primary Six-District Sample, Assignment Years 1999 - 2003.

	Random Assignment District						
Assignment Year	District A	District B	District C	District D	District E	District F	All
1999 - 2000							
Randomization	0.78	0.02	0.46	0.17	0.59	0.42	0.10
Any employment	0.00	0.00	0.06	0.00	0.74	0.49	0.00
Temp v. direct v. none	0.00	0.00	0.07	0.00	0.00	0.00	0.00
N	1,951	1,216	900	963	844	720	6,594
2000 - 2001							
Randomization	0.10	0.10	0.03	0.12	0.57	0.22	0.18
Any employment	0.02	0.00	0.01	0.00	0.16	0.00	0.00
Temp v. direct v. none	0.01	0.00	0.00	0.00	0.11	0.00	0.00
N	2,026	1,474	887	858	900	1,590	7,735
2001 - 2002							
Randomization	0.34	0.18	0.21	0.61	0.80	0.59	0.99
Any employment	0.00	0.00	0.44	0.21	0.00	0.00	0.00
Temp v. direct v. none	0.00	0.00	0.00	0.08	0.00	0.00	0.00
N	2,093	1,651	1,051	970	822	1,693	8,280
2002 - 2003							
Randomization	0.46	0.96	0.37	0.25	0.63	0.28	0.99
Any employment	0.03	0.00	0.03	0.74	0.00	0.00	0.00
Temp v. direct v. none	0.01	0.00	0.05	0.27	0.00	0.00	0.00
N	775	649	337	334	431	1,013	3,539
All Years							
Randomization	0.39	0.07	0.12	0.47	0.99	0.90	0.84
Any employment	0.00	0.00	0.02	0.00	0.00	0.00	0.02
Temp v. direct v. none	0.00	0.00	0.00	0.00	0.00	0.00	0.00
N	6,845	4,990	3,175	3,125	2,997	5,016	26,148

The first row of each panel provides the p-value for the null hypothesis that the 8 main sample covariates are balanced across clients assigned to Work First contractors within a randomization district. These covariates are: gender, race, age, high-school dropout status, total quarters employed and total employment earnings in eight quarters prior to Work First assignment, total quarters employed in temporary help agencies and total temporary help agency earnings in eight quarters prior to Work First assignment. The second row in each panel provides the p-value for the null-hypothesis that the share obtaining any employment during the Work First spell is balanced across contractors in a region. The third row in each panel provides the p-value for the null-hypothesis that the share obtaining direct-hire employment, temporary help agency employment, and no employment during the Work First spell is balanced across contractors in a region.

Table 3. The Effect of Work-First Job Placements on In-Program Earnings  
Sample: Six Primary Randomization Districts

	A. Clients Assigned 2000 - 2003						B. Clients Assigned 1999 - 2003					
	OLS (1)	2SLS (2)	2SLS (3)	OLS (4)	2SLS (5)	2SLS (6)	OLS (1)	2SLS (2)	2SLS (3)	OLS (4)	2SLS (5)	2SLS (6)
<i>Hourly Wages</i>												
Any job	7.37 (0.04)	6.96 (0.11)	7.01 (0.10)				7.31 (0.04)	6.98 (0.14)	7.05 (0.10)			
Temp agency Job				7.79 (0.09)	7.47 (0.43)	7.27 (0.36)				7.67 (0.07)	7.31 (0.38)	7.17 (0.29)
Direct-hire job				7.27 (0.04)	6.60 (0.30)	6.87 (0.18)				7.22 (0.04)	6.75 (0.29)	6.98 (0.16)
R-Squared	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.90	0.89	0.89
<i>Weekly Hours</i>												
Any job	34.5 (0.3)	37.1 (1.2)	37.4 (0.9)				34.2 (0.3)	35.6 (1.5)	36.5 (1.1)			
Temp agency Job				37.1 (0.3)	34.1 (2.7)	33.6 (2.0)				37.0 (0.3)	31.5 (3.0)	33.4 (2.1)
Direct-hire job				33.9 (0.3)	39.2 (2.7)	39.5 (1.5)				33.5 (0.3)	38.5 (2.9)	38.3 (1.4)
R-Squared	0.93	0.92	0.92	0.93	0.90	0.90	0.93	0.92	0.92	0.93	0.90	0.91
<i>Weekly Earnings</i>												
Any job	257 (3)	258 (9)	262 (7)				253 (3)	249 (10)	257 (7)			
Temp agency Job				289 (4)	261 (26)	246 (20)				284 (3)	235 (24)	241 (16)
Direct-hire job				249 (3)	256 (23)	270 (14)				245 (3)	258 (22)	266 (12)
R-Squared	0.81	0.81	0.81	0.81	0.81	0.80	0.81	0.81	0.81	0.81	0.80	0.80
Contractor x yr dummies		No	Yes		No	Yes		No	Yes		No	Yes
N				19,554						26,149		

Robust standard errors clustered on Work First contractor assignment x year are in parentheses.

Table 4. The Effect of Work-First Job Placements on In-Program Earnings  
Sample: Thirteen Randomization Districts

	A. Assigned 2000 - 2003				B. Assigned 1999 - 2003			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS
<i>Hourly Wages</i>								
Any job	7.23 (0.03)	7.16 (0.14)			7.27 (0.03)	7.22 (0.10)		
Temp agency Job			7.56 (0.07)	6.73 (0.20)			7.61 (0.06)	7.05 (0.22)
Direct-hire job			7.15 (0.03)	7.37 (0.17)			7.19 (0.03)	7.28 (0.13)
R-Squared	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90
<i>Weekly Hours</i>								
Any job	33.7 (0.2)	35.0 (1.0)			34.2 (0.2)	35.5 (0.8)		
Temp agency Job			36.6 (0.2)	37.0 (2.1)			36.9 (0.2)	36.5 (1.7)
Direct-hire job			33.0 (0.2)	34.0 (0.9)			33.6 (0.2)	35.1 (1.0)
R-Squared	0.92	0.92	0.93	0.93	0.93	0.93	0.93	0.93
<i>Weekly Earnings</i>								
Any job	246 (2)	252 (7)			251 (2)	257 (6)		
Temp agency Job			276 (3)	249 (14)			280 (2)	257 (13)
Direct-hire job			239 (2)	253 (9)			245 (2)	258 (9)
R-Squared	0.81	0.81	0.81	0.81	0.81	0.81	0.82	0.81
N		36,121				50,298		

Robust standard errors clustered on Work First contractor assignment x year are in parentheses. Instrumental variables: contractor x year of assignment.

Table 5. The Effect of Work-First Job Placements on Program Recidivism

	A. 6 Districts, Clients Assigned 2000 - 2003						B. 6 Districts, Clients Assigned 1999 - 2003						C. 13 Districts, 1999 - 2003			
	OLS	2SLS	2SLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)
<i>360 Day Recidivism: 2000 - 2003 Entrants</i>							<i>360 Day Recidivism: 1999 - 2003 Entrants</i>									
Any job	-0.09 (0.01)	0.01 (0.05)	0.03 (0.04)				-0.09 (0.01)	-0.01 (0.05)	0.02 (0.04)				-0.11 (0.01)	-0.03 (0.04)		
Temp agency Job				-0.05 (0.01)	0.21 (0.17)	0.07 (0.14)				-0.05 (0.01)	0.30 (0.15)	0.10 (0.10)			-0.08 (0.01)	0.06 (0.08)
Direct-hire job				-0.10 (0.01)	-0.15 (0.11)	0.01 (0.08)				-0.10 (0.01)	-0.26 (0.11)	-0.03 (0.06)			-0.12 (0.01)	-0.07 (0.05)
R-Squared	0.03	0.02	0.01	0.03	.	0.02	0.03	0.02	0.02	0.03	.	0.02	0.04	0.03	0.04	0.03
N				19,544						26,194					50,298	
<i>360 Day Recidivism: 2000 - 2002 Entrants</i>							<i>360 Day Recidivism: 1999 - 2002 Entrants</i>									
Any job	-0.10 (0.01)	0.03 (0.07)	0.04 (0.05)				-0.11 (0.01)	0.00 (0.08)	0.03 (0.05)				-0.13 (0.01)	-0.06 (0.05)		
Temp agency Job				-0.07 (0.02)	0.38 (0.26)	0.24 (0.21)				-0.06 (0.01)	0.49 (0.23)	0.20 (0.11)			-0.08 (0.01)	0.19 (0.09)
Direct-hire job				-0.11 (0.01)	-0.27 (0.16)	-0.09 (0.12)				-0.12 (0.01)	-0.42 (0.14)	-0.11 (0.07)			-0.14 (0.01)	-0.19 (0.06)
R-Squared	0.03	0.01	0.01	0.03	.		0.03	0.02	0.02	0.04	.	0.01	0.04	0.04	0.04	0.01
N				11,912						18,507					36,321	
<i>720 Day Recidivism: 2000 - 2002 Entrants</i>							<i>720 Day Recidivism: 1999 - 2002 Entrants</i>									
Any job	-0.08 (0.01)	-0.02 (0.08)	0.00 (0.07)				-0.08 (0.01)	-0.01 (0.08)	-0.04 (0.06)				-0.09 (0.01)	-0.07 (0.05)		
Temp agency Job				-0.04 (0.01)	0.32 (0.22)	0.22 (0.17)				-0.04 (0.01)	0.39 (0.19)	0.19 (0.09)			-0.05 (0.01)	0.07 (0.08)
Direct-hire job				-0.08 (0.01)	-0.30 (0.16)	-0.15 (0.13)				-0.10 (0.01)	-0.36 (0.12)	-0.21 (0.09)			-0.10 (0.01)	-0.14 (0.06)
R-Squared	0.04	0.04	0.03	0.04	.	0.01	0.04	0.04	0.04	0.04	.	0.00	0.05	0.05	0.05	0.04
N				11,912						18,507					36,321	
Contractor-yr dummies		No	Yes		No	Yes		No	Yes		No	Yes		Yes		Yes

Robust standard errors clustered on Work First contractor assignment x year are in parentheses. Recidivism is measured from date of



Table 6. The Effect of Work-First Job Placements on Wage and Salary Earnings  
Sample: Six Primary Randomization Districts

	A. Clients Assigned 2000 - 2003						B. Clients Assigned 1999 - 2003					
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
<i>Earnings During Quarter of Work First Assignment</i>												
Any job	306 (17)	300 (71)	274 (66)				312 (16)	310 (69)	290 (64)			
Temp agency Job				281 (30)	9 (161)	439 (161)				298 (30)	-8 (163)	386 (127)
Direct-hire job				312 (19)	527 (217)	173 (109)				315 (16)	567 (194)	225 (96)
R-Squared	0.26	0.26	0.26	0.26	0.25	0.26	0.26	0.26	0.26	0.26	0.24	0.26
<i>Earnings in First Quarter Following Work First Assignment</i>												
Any job	812 (24)	740 (174)	635 (136)				783 (22)	767 (156)	646 (120)			
Temp agency Job				720 (53)	-421 (317)	213 (322)				690 (49)	-256 (298)	274 (236)
Direct-hire job				835 (24)	1,641 (421)	893 (179)				808 (23)	1,595 (364)	896 (149)
R-Squared	0.18	0.18	0.18	0.19	0.04	0.17	0.18	0.18	0.18	0.19	0.07	0.18
<i>Earnings in Quarters 2 - 4 Following Work First Assignment</i>												
Any job	1,707 (78)	909 (541)	851 (378)				1,637 (63)	858 (535)	869 (326)			
Temp agency Job				1,635 (146)	-1,793 (1,156)	-605 (1,066)				1,478 (122)	-2,430 (1,078)	-941 (802)
Direct-hire job				1,725 (87)	3,009 (1,174)	1,741 (585)				1,680 (71)	3,519 (1,077)	2,082 (505)
R-Squared	0.19	0.19	0.19	0.19	0.12	0.17	0.20	0.19	0.19	0.20	0.09	0.17
Contractor x yr dummies N		No	Yes		No	Yes		No	Yes		No	Yes
			19,554						26,149			

Robust standard errors clustered on Work First contractor assignment x year are in parentheses.

Table 7. The Effect of Work-First Job Placements on Wage and Salary Earnings  
Sample: Six Primary Randomization Districts

	A. Clients Assigned 2000 - 2002						B. Clients Assigned 1999 - 2002					
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
<i>Earnings During Quarter of Work First Assignment</i>												
Any job	298 (21)	360 (107)	393 (80)				309 (19)	313 (105)	389 (75)			
Temp agency Job				274 (43)	33 (183)	308 (207)				302 (39)	5 (177)	306 (127)
Direct-hire job				303 (21)	637 (219)	453 (131)				311 (17)	577 (220)	455 (100)
R-Squared	0.26	0.26	0.25	0.26	0.23	0.25	0.26	0.26	0.25	0.26	0.23	0.25
<i>Earnings in First Quarter Following Work First Assignment</i>												
Any job	772 (25)	659 (232)	600 (196)				745 (24)	596 (193)	625 (160)			
Temp agency Job				667 (53)	-190 (385)	254 (493)				645 (53)	-42 (330)	307 (299)
Direct-hire job				798 (26)	1,379 (401)	845 (247)				772 (24)	1,143 (441)	876 (190)
R-Squared	0.18	0.18	0.18	0.18	0.10	0.18	0.18	0.18	0.18	0.18	0.14	0.18
<i>Earnings in Quarters 2 - 4 Following Work First Assignment</i>												
Any job	1,585 (102)	816 (745)	678 (629)				1,529 (71)	587 (708)	747 (490)			
Temp agency Job				1,532 (188)	-2,677 (1,713)	-1,450 (1,735)				1,346 (141)	-3,271 (1,350)	-1,636 (995)
Direct-hire job				1,598 (111)	3,776 (1,589)	2,186 (992)				1,578 (80)	3,895 (1,335)	2,632 (710)
R-Squared	0.19	0.19	0.18	0.19	0.06	0.16	0.20	0.19	0.19	0.20	0.04	0.15
<i>Earnings in Quarters 5 - 8 Following Work First Assignment</i>												
Any job	1,676 (117)	1,064 (829)	1,238 (667)				1,628 (88)	1,570 (830)	1,421 (591)			
Temp agency Job				1,613 (232)	-1,371 (1,707)	-580 (1,884)				1,421 (174)	-3,438 (1,598)	-1,934 (1,308)
Direct-hire job				1,692 (143)	3,127 (1,522)	2,527 (1,084)				1,684 (104)	5,862 (1,537)	4,074 (952)
R-Squared	0.18	0.18	0.18	0.15	0.18	0.17	0.18	0.18	0.18	0.18	0.05	0.13
<i>Earnings in Quarters 1 - 8 Following Work First Assignment</i>												
Any job	4,033 (207)	2,539 (1,737)	2,517 (1,428)				3,902 (152)	2,753 (1,627)	2,793 (1,145)			
Temp agency Job				3,812 (399)	-4,237 (3,508)	-1,777 (3,952)				3,412 (313)	-6,751 (2,925)	-3,264 (2,449)
Direct-hire job				4,087 (240)	8,282 (3,269)	5,558 (2,164)				4,034 (172)	10,899 (3,044)	7,582 (1,711)
R-Squared	0.23	0.22	0.22	0.23	0.15	0.20	0.23	0.23	0.23	0.23	0.07	0.18
Contractor x yr dummies		No	Yes		No	Yes		No	Yes		No	Yes
N			11,912						18,507			

Robust standard errors clustered on Work First contractor assignment x year are in parentheses.

Table 8: The Effect of Work-First Job Placements on Quarters of Employment  
Sample: Six Primary Randomization Districts

	A. Clients Assigned 2000 - 2003						B. Clients Assigned 1999 - 2003					
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
<i>Worked During Quarter of Work First Assignment</i>												
Any job	0.251 (0.012)	0.234 (0.053)	0.191 (0.039)				0.243 (0.011)	0.237 (0.049)	0.196 (0.036)			
Temp agency Job				0.280 (0.022)	0.054 (0.111)	0.225 (0.105)				0.277 (0.018)	0.131 (0.094)	0.258 (0.076)
Direct-hire job				0.243 (0.012)	0.373 (0.110)	0.170 (0.053)				0.234 (0.010)	0.324 (0.091)	0.155 (0.046)
R-Squared	0.17	0.17	0.17	0.17	0.13	0.17	0.17	0.16	0.17	0.17	0.15	0.16
H0: Temp=DH				0.057	0.106	0.688				0.006	0.289	0.224
<i>Worked in First Quarter Following Work First Assignment</i>												
Any job	0.371 (0.011)	0.325 (0.065)	0.312 (0.057)				0.350 (0.011)	0.314 (0.060)	0.290 (0.050)			
Temp agency Job				0.377 (0.020)	-0.057 (0.135)	0.064 (0.102)				0.359 (0.017)	-0.089 (0.101)	0.023 (0.076)
Direct-hire job				0.370 (0.011)	0.622 (0.108)	0.463 (0.063)				0.348 (0.011)	0.640 (0.093)	0.470 (0.055)
R-Squared	0.18	0.18	0.18	0.18	0.02	0.13	0.17	0.17	0.17	0.17		0.11
H0: Temp=DH				0.716	0.003	0.005				0.449	0.000	0.000
<i># Quarters Worked in Quarters 2 - 4 Following Work First Assignment</i>												
Any job	0.564 (0.020)	0.424 (0.138)	0.389 (0.115)				0.532 (0.020)	0.350 (0.140)	0.365 (0.100)			
Temp agency Job				0.538 (0.038)	-0.296 (0.303)	0.052 (0.231)				0.503 (0.031)	-0.509 (0.276)	-0.077 (0.182)
Direct-hire job				0.570 (0.021)	0.984 (0.272)	0.595 (0.135)				0.539 (0.021)	1.046 (0.261)	0.662 (0.116)
R-Squared	0.13	0.13	0.13	0.13	0.05	0.12	0.13	0.13	0.13	0.13	0.01	0.11
H0: Temp=DH				0.434	0.016	0.071				0.255	0.003	0.002
Contractor x yr N		No 19,554	Yes 19,554		No 19,554	Yes 19,554		No 26,149	Yes 26,149		No 26,149	Yes 26,149

Robust standard errors clustered on Work First contractor assignment x year are in parentheses.

Table 9: The Effect of Work-First Job Placements on Quarters of Employment  
Sample: Six Primary Randomization Districts

	A. Clients Assigned 2000 - 2002						B. Clients Assigned 1999 - 2002					
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
<i># Quarters Worked Quarters 2 - 4 Following Work First Assignment</i>												
Any job	0.521 (0.024)	0.297 (0.151)	0.242 (0.137)				0.490 (0.022)	0.206 (0.155)	0.238 (0.108)			
Temp agency Job				0.514 (0.042)	-0.149 (0.374)	0.215 (0.339)				0.473 (0.031)	-0.571 (0.318)	-0.082 (0.243)
Direct-hire job				0.523 (0.027)	0.675 (0.291)	0.261 (0.157)				0.495 (0.024)	0.872 (0.320)	0.491 (0.148)
R-Squared	0.13	0.12	0.12	0.13	0.10	0.12	0.13	0.12	0.12	0.13	0.02	0.11
H0: Temp=DH				0.847	0.159	0.915				0.499	0.079	0.011
<i># Quarters Worked Quarters 5 - 8 Following Work First Assignment</i>												
Any job	0.445 (0.028)	0.329 (0.198)	0.310 (0.163)				0.434 (0.020)	0.214 (0.192)	0.281 (0.130)			
Temp agency Job				0.415 (0.055)	0.075 (0.316)	0.274 (0.367)				0.387 (0.040)	-0.794 (0.351)	-0.237 (0.275)
Direct-hire job				0.452 (0.029)	0.544 (0.346)	0.336 (0.176)				0.447 (0.021)	1.078 (0.454)	0.690 (0.179)
R-Squared	0.12	0.12	0.12	0.12	0.11	0.12	0.12	0.12	0.11	0.12	0.01	0.10
H0: Temp=DH				0.492	0.379	0.890				0.154	0.012	0.012
<i># Quarters Worked Quarters 1 - 8 Following Work First Assignment</i>												
Any job	1.318 (0.057)	0.919 (0.384)	0.817 (0.338)				1.253 (0.044)	0.680 (0.377)	0.762 (0.267)			
Temp agency Job				1.285 (0.098)	-0.104 (0.706)	0.553 (0.763)				1.199 (0.069)	-1.505 (0.675)	-0.318 (0.569)
Direct-hire job				1.326 (0.059)	1.785 (0.661)	1.005 (0.347)				1.268 (0.047)	2.553 (0.849)	1.616 (0.350)
R-Squared	0.18	0.17	0.17	0.18	0.14	0.17	0.18	0.17	0.17	0.18	0.01	0.14
H0: Temp=DH				0.674	0.091	0.617				0.332	0.011	0.004
Contractor x yr N		No 11,912	Yes		No 11,912	Yes		Yes 18,507	No		Yes 18,507	No

Robust standard errors clustered on Work First contractor assignment x year are in parentheses.

Table 10. The Effect of Work-First Job Placements on Wage and Salary Earnings.  
Sample: Thirteen Randomization Districts

	Assigned 1999 - 2003				Assigned 1999 - 2002			
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
<i>Earnings During Quarter of Assignment</i>								
Any job	295 (11)	228 (52)			286 (13)	260 (71)		
Temp agency Job			291 (21)	396 (106)			288 (27)	334 (126)
Direct-hire job			296 (11)	156 (55)			286 (12)	220 (80)
R-Squared	0.26	0.26	0.26	0.25	0.25	0.25	0.26	0.25
<i>Earnings in First Quarter Following Assignment</i>								
Any job	790 (15)	538 (97)			756 (16)	419 (134)		
Temp agency Job			721 (34)	573 (214)			681 (40)	583 (303)
Direct-hire job			806 (16)	523 (121)			774 (16)	330 (164)
R-Squared	0.19	0.18	0.19	0.18	0.19	0.17	0.19	0.17
<i>Earnings in Quarters 2 - 4 Following Assignment</i>								
Any job	1,674 (46)	641 (254)			1,598 (53)	508 (381)		
Temp agency Job			1,487 (94)	-436 (647)			1,361 (108)	-1,066 (852)
Direct-hire job			1,719 (50)	1,101 (325)			1,655 (58)	1,360 (499)
R-Squared	0.20	0.19	0.20	0.19	0.20	0.19	0.20	0.18
<i>Earnings in Quarters 5 - 8 Following Assignment</i>								
Any job					1,642 (66)	782 (527)		
Temp agency Job							1,437 (127)	-938 (1,319)
Direct-hire job							1,692 (80)	1,713 (792)
R-Squared					0.18	0.18	0.18	0.17
<i>Earnings in Quarters 1 - 8 Following Assignment</i>								
Any job					3,996 (115)	1,708 (965)		
Temp agency Job							3,479 (236)	-1,421 (2,330)
Direct-hire job							4,121 (132)	3,403 (1,382)
R-Squared					0.24	0.23	0.24	0.22
N		50,298				35,321		

Robust standard errors clustered on Work First contractor assignment x year are in parentheses. Instrumental variables: contractor x year of assignment.

Table 11: The Effect of Work-First Job Placements on Non-Program Earnings  
Sample: Six Primary Randomization Districts and All Districts,  
Clients Assigned in Program Years 1999 - 2000

	6 districts			All districts			6 districts			All districts	
	OLS	2SLS		OLS	2SLS		OLS	2SLS		OLS	2SLS
<u>A. Earnings During Quarter of Work First Assignment</u>											
Any employment	327.58 (37.14)	376.96 (200.51)		332.59 (20.76)	235.19 (144.75)						
Temp agency job							345.57 (73.32)	298.28 (182.76)	0.00	359.50 (45.74)	213.96 (182.75)
Direct-hire job							321.89 (30.48)	466.78 (247.96)	0.00	324.79 (17.83)	248.92 (142.24)
R-Squared	0.25	0.25		0.26	0.25		0.25	0.25		0.26	0.25
<u>B. Earnings in First Quarter Following Work First Assignment</u>											
Any employment	693.00 (48.18)	717.29 (254.01)	0.00	757.71 (30.98)	375.03 (239.79)						
Temp agency job							610.40 (110.52)	392.31 (313.84)	0.00	711.72 (71.07)	448.80 (377.96)
Direct-hire job							719.13 (45.96)	1088.28 (332.52)	0.00	771.04 (30.28)	327.30 (301.36)
R-Squared	0.18	0.18		0.19	0.17		0.19	0.17		0.19	0.17
<u>C. Earnings in Quarters 2 - 4 Following Work First Assignment</u>											
Any employment	1,416.34 (81.49)	916.21 (415.65)	0.00 20.76	1,501.24 (70.95)	522.76 (340.29)						
Temp agency job							1,075.48 (191.26)	-1,701.47 (566.23)	0.00 20.76	1,148.78 (157.82)	-1,460.10 (783.14)
Direct-hire job							1,524.19 (101.72)	3,904.50 (648.59)	0.00 20.76	1,603.38 (70.81)	1,805.80 (765.89)
R-Squared	0.22	0.21		0.21	0.20		0.22	0.12		0.21	0.18
<u>D. Earnings in Quarters 5 - 8 Following Work First Assignment</u>											
Any employment	1,528.83 (145.20)	1,909.25 (1,316.07)	0.00 20.76	1,562.33 (87.73)	807.98 (1,051.33)						
Temp agency job							1,148.59 (261.76)	-2,894.95 (1,341.70)	0.00 20.76	1,229.68 (176.28)	-1,354.72 (2,027.07)
Direct-hire job							1,649.14 (144.80)	7,393.65 (1,426.38)	0.00 20.76	1,658.72 (92.35)	2,207.39 (1,589.79)
R-Squared	0.18	0.18		0.18	0.18		0.18			0.18	0.17
<u>E. Earnings in Quarters 1 - 8 Following Work First Assignment</u>											
Any employment	3,638.16 (222.89)	3,542.74 (1,552.20)	0.00 20.76	3,821.28 (160.01)	1,705.77 (1,454.32)						
Temp agency job							2,834.47 (493.82)	-4,204.10 (1,806.80)	0.00 20.76	3,090.18 (356.23)	-2,366.02 (2,993.29)
Direct-hire job							3,892.46 (226.90)	12,386.43 (1,984.61)	0.00 20.76	4,033.14 (158.66)	4,340.49 (2,568.97)
Observations	6,595			13,945			6,595			13,945	

Robust standard errors, clustered on Work First contractor assignment x year, are in parentheses.

Table 12: The Effect of Work-First Job Placements on Non-Program Earnings  
Estimates by Randomization District, Clients Assigned Years 2000 - 2003

	<i>District A</i>		<i>District B</i>		<i>District C</i>		<i>District D</i>		<i>District E</i>		<i>District F</i>	
	<u>OLS</u>	<u>2SLS</u>	<u>OLS</u>	<u>2SLS</u>	<u>OLS</u>	<u>2SLS</u>	<u>OLS</u>	<u>2SLS</u>	<u>OLS</u>	<u>2SLS</u>	<u>OLS</u>	<u>2SLS</u>
<i>A. Any Employment versus Non-Employment</i>												
<i>Earnings During Quarter of Work First Assignment</i>												
Any employment	286 (32)	2 (331)	309 (33)	326 (151)	411 (36)	1,314 (855)	305 (28)	461 (150)	262 (39)	419 (481)	276 (42)	-272 (310)
R-Squared	0.26		0.26		0.29		0.26		0.28		0.29	
<i>Earnings in First Quarter Following Work First Assignment</i>												
Any employment	801 (45)	-52 (450)	800 (46)	252 (219)	903 (52)	916 (1115)	861 (43)	1,468 (232)	722 (51)	592 (644)	749 (60)	295 (411)
R-Squared	0.19		0.17		0.19		0.19		0.22		0.21	
<i>Earnings in Quarters 2 - 4 Following Work First Assignment</i>												
Any employment	1,769 (149)	-1,724 (1535)	1,791 (154)	309 (714)	2,044 (180)	-2,086 (4056)	1,500 (138)	2,805 (733)	1,702 (178)	1,482 (2,233)	1,512 (187)	-550 (1,341)
R-Squared	0.19		0.20		0.19		0.18		0.19		0.22	
N	4,894		3,774		2,275		4,296		2,162		2,153	
<i>B. Temporary-Help versus Direct-Hire versus Non-Employment</i>												
<i>Earnings During Quarter of Work First Assignment</i>												
Temp-agency job	329 (59)	-62 (656)	193 (56)	-149 (576)	303 (66)	-3,197 (4,137)	361 (56)	256 (583)				
Direct-hire job	276 (34)	127 (1,253)	348 (36)	884 (681)	437 (39)	-697 (2,129)	294 (30)	554 (309)				
R-Squared	0.26		0.26		0.29		0.26					
<i>Earnings in First Quarter Following Work First Assignment</i>												
Temp-agency job	776 (86)	-2,186 (1,566)	567 (77)	-1,464 (952)	636 (87)	-5,859 (7,667)	1,039 (100)	908 (953)				
Direct-hire job	807 (48)	4,161 (2,708)	878 (51)	2,271 (1,081)	967 (58)	-2,105 (3,900)	826 (44)	1,723 (506)				
R-Squared	0.19		0.17		0.20		0.19					
<i>Earnings in Quarters 2 - 4 Following Work First Assignment</i>												
Temp-agency job	1,498 (294)	-5,209 (3,581)	1,231 (255)	-4,571 (3,100)	2,081 (352)	-9,647 (19,191)	2,019 (289)	-486 (3,064)				
Direct-hire job	1,833 (158)	5,158 (6,294)	1,977 (169)	6,048 (3,549)	2,035 (191)	-5,457 (9,838)	1,396 (143)	4,304 (1,633)				
R-Squared	0.19		0.20		0.19		0.18					
N	4,894		3,774		2,275		4,296					

Robust standard errors are in parentheses. Instrumental variables: Contractor.