Do Temporary Help Jobs Improve Labor Market Outcomes for Low-Skilled Workers? Evidence from Random Assignments*

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Abstract

A disproportionate share of low-skilled U.S. workers is employed by temporary help firms. These firms offer rapid entry into paid employment, but temporary help jobs are typically brief and it is unknown whether they foster longer-term employment. We draw upon an unusual, large-scale policy experiment in the state of Michigan to evaluate whether holding temporary help jobs facilitates labor market advancement for low-skilled workers. To identify these effects, we exploit the random assignment of welfare-to-work clients across numerous welfare service providers in a major metropolitan area. These providers feature substantially different placement rates at temporary help jobs but offer otherwise similar services. We find that moving welfare participants into temporary help jobs boosts their short-term earnings. But these gains are offset by lower earnings, less frequent employment and potentially higher welfare recidivism over the next one to two years. In contrast, placements in direct-hire jobs raise participants' earnings substantially and reduce recidivism both one and two years following placement. We conclude that encouraging low-skilled workers to take temporary help agency jobs is no more effective – and possibly less effective – than providing no job placements at all.

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A disproportionate share of low-skilled U.S. workers is employed by temporary help firms. In 1999, African American workers were overrepresented in temporary help agency jobs by 86 percent, Hispanics by 31 percent and high school dropouts by 59 percent; by contrast, college graduates were underrepresented by 47 percent (DiNatilie, 2002). Just two occupations, clerical work and operators, fabricators and laborers account for 65 percent of all temporary help jobs – and clerical work is primarily unavailable to temporary help workers without a high school degree. Nowhere is the concentration of low-skilled workers in temporary help jobs more pronounced than among welfare recipients. Recent analyses of state administrative welfare data reveal that 15 to 40 percent of former welfare recipients (almost all high school dropouts) who obtained employment in the years following the 1995 U.S. welfare reform took jobs in the temporary help sector. These numbers are especially striking in light of the fact that the temporary help industry accounts for less than 3 percent of average U.S. daily employment.

The concentration of low-skilled workers in the temporary help sector has catalyzed a research and policy debate about whether temporary help jobs foster labor market advancement. One hypothesis is that because temporary help firms face lower screening and termination costs than do conventional, direct-hire employers, they may choose to hire individuals who otherwise would have difficulty finding any employment (Katz and Krueger 1999; Autor and Houseman 2002b; Autor 2003; Houseman, Kalleberg, and Erickcek 2003). If so, temporary help jobs may reduce the time workers spend in unproductive, potentially discouraging job search, and facilitate rapid entry into employment. Moreover, temporary assignments may permit workers to develop human capital and labor market contacts that lead, directly or indirectly, to longer-term jobs. Indeed, 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. 2000).

In contrast to this view, numerous scholars and practitioners have argued 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 (Parker 1994; Pawasarat 1997; Jorgenson and Riemer 2000). In support of this hypothesis, much work documents that workers in temporary help jobs receive on average lower pay and fewer benefits than would be expected in direct-hire jobs (Segal and Sullivan 1998; General Accounting Office 2000; DiNatalie 2001). 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). If temporary help jobs exclusively substitute for spells of unemployment, these facts would be of little concern. But to the degree that spells in temporary help employment crowd out productive direct-hire job search, they may inhibit longer-term labor advancement. Hence, the short term gains accruing from nearer-term employment in temporary help jobs may be offset by employment instability and poor earnings growth.

Distinguishing among these competing hypotheses is an empirical challenge. The fundamental problem for empirical analysis is that there are economically large, but typically unmeasurable, differences in skills and motivation of workers taking temporary help and direct-hire jobs, as we show below. In the absence of a random assignment of low-skilled workers to job types that would overcome this confound, a statistical comparison of labor force outcomes among low-skilled workers in different types of employment arrangements is unlikely to be informative about the causal effects of holding temporary help or direct-hire jobs on subsequent labor force advancement.

Cognizant of these confounds, numerous recent studies, summarized below, attempt to identify the effects of temporary help employment on subsequent labor market outcomes among low-skill and low-income populations in the United States. In addition, a burgeoning parallel literature using data from Continental Europe and the United Kingdom evaluates whether temporary help agency employment, as well as other non-standard work arrangements such as fixed-term contracts, provides a ‘stepping stone’ into stable employment. Notably, both the recent U.S. and European literatures have consistently rejected the negative view of temporary help jobs articulated above.[1] Studies typically find that temporary help jobs provide a viable port of entry into the labor market for low-skilled workers and lead to longer-term labor market advancement.

In addition to their findings, something these studies have in common is that they draw exclusively on observational data to ascertain causal relationships. That is, the research designs depend upon regression control, matching, and selection-adjustment techniques to account for the likely non-random selection of workers with different earnings capacities into different job types. The veracity of their findings therefore depends critically on the efficacy of these methods for correcting the non-experimental data for self-selection.

In this study, we take an alternative approach to evaluating whether temporary help jobs improve labor market outcomes for low-skilled workers. We exploit a unique, multi-year policy experiment in a large Michigan metropolitan area which randomized welfare recipients participating in a return-to-work program ("Work First") across a large number of welfare service providers (contractors) featuring substantially different placement rates at temporary help jobs but offering otherwise similar services. As we demonstrate below, this quasi-experiment gave rise to significant differences in direct-hire and temporary-help job taking rates among Work First participants randomly assigned to different contractors. We analyze this randomization using an "intention to treat" framework where randomization alters the probabilities that ex-ante identical individuals are placed in different types of jobs (direct-hire, temporary-help, non-employment) during their Work First spells.

To assess the labor market consequences of these placements, we use administrative data from the Work First program linked with complete Unemployment Insurance (UI) wage records for the State of Michigan for over 36,000 Work First spells initiated from 1997 to 2003. The Work First data include demographic information on Work First participants and detailed information on jobs found

[1] Given the broad differences among labor market institutions in Anglo-Saxon and Continental European economies, there is no presumption that the cross-country findings should be comparable – which makes it all the more striking that the twelve existing

during the program. The 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.

Our primary finding is that direct-hire Work First placements induced by the random assignment of participants to Work First contractors substantially increase payroll earnings and quarters of employment for marginal Work First participants – by several thousand dollars over the subsequent two years. This relationship is significant, consistent across randomization districts, and economically large. By contrast, we find that temporary-help placements do not raise – and quite possibly lower – payroll earnings and quarters of employment of Work First clients over the one to two years following placement. This adverse finding for payroll earnings is corroborated by evidence from Work First administrative records that 'marginal' temporary help placements are found primarily in low paying occupations and appear to lead to increased welfare recidivism.

We present numerous robustness tests that verify the consistency of these findings across outcome measures (earnings, employment, recidivism), calendar years, randomization districts and post-placement time intervals. Most significantly, we consider and present strong evidence against two salient threats to validity. First, we show that that the adverse findings for the labor market consequences of temporary help jobs are not spuriously driven by a general association between 'bad contractor' practices and use of temporary help placements. In particular, even among contractors who make extensive use of temporary help placements, poor labor market outcomes are confined to the set of participants placed in temporary help jobs and not to those placed in direct-hire positions. Secondly, we demonstrate that 'marginal' workers placed in temporary help positions by the randomization have comparable demographic and pre-placement earnings histories to marginal workers placed in direct-hire positions. Hence, the contrast between the positive labor market

studies in this literature have developed such consistent results.

consequences of direct-hire placements and the generally negative consequences of temporary help placements appears to stem from differences in job quality rather than differences in worker quality.

We also use our detailed administrative data to estimate conventional OLS and fixed-effects models for the relationship between temporary help job-taking and subsequent labor market outcomes. Consistent with the U.S. and European literature above – but quite opposite to our main, quasi-experimental estimates – we find that workers who take temporary help jobs fare almost as well as those taking direct-hire positions. The contrast with our core findings suggests that non-experimental estimates are substantially biased by the endemic self-selection of workers into job types according to unmeasured skills and motivation. We suggest that the emerging consensus of the U.S. and European literatures that temporary help jobs foster labor market advancement – based wholly on non-experimental evaluation – should be reconsidered in light of the evidence from random assignments.[2]

1. Prior Non-Experimental Analyses and the Michigan Work-First Quasi-Experiment

a. Prior non-experimental estimates

The characteristics of workers who take direct-hire and temporary help jobs differ significantly. Even in our relatively homogenous sample, comprised almost entirely of black, female welfare recipients with less than a college education from one metropolitan area in Michigan, we find that Work First participants who take temporary help jobs are older, more likely to be black, and have higher prior earnings in the temporary help sector than do Work First recipients who take direct-hire jobs (see Table 1). Not surprisingly, the contrast with those who take no employment during their Work First spells is much more pronounced. These contrasts underscore the difficulty of

[2] Our microeconomic evidence answers the question of whether temporary help jobs benefit the individuals who take them but it does not address whether the activities of temporary help firms and other flexible labor market institutions (such as fixed-term contracts) improve or retard aggregate labor market performance by reducing search frictions or improving the quality of worker-firm matches. See Katz and Krueger 1999, Blanchard and Landier 2002, García-Pérez and Muñoz-Bullón 2002 and Neugart and Storrie 2002 and 2005.

disentangling the effects of job-taking on subsequent labor market outcomes from the causes that determine what jobs are taken initially.

A number of recent studies attempt to overcome this confound. 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 welfare. They cautiously conclude that temporary employment improves labor market outcomes among those who might otherwise have been unemployed, and suggest the use of temporary help jobs by welfare agencies as a means to improve labor market outcomes. For propensity score techniques to be effective for this problem, two conditions must be met. First, differences among those in temporary, direct-hire, and non-employment must be fully captured by variables available to the analyst. This ‘selection on observables’ assumption is not testable and its plausibility is difficult to judge. Second, it must be feasible to construct groups of individuals who are closely comparable on the matching covariates but who obtain different job types (non-employment, temporary agency jobs, and direct-hire jobs). Lane et al. report that in their Survey of Income and Program Participation data, it was infeasible to construct groups that were well-matched on earnings histories but differed on job types. As they acknowledge, this is a potential source of bias for their findings.

Using a research population and database closely comparable to the one used in this study, Heinrich, Mueser and Troske (2005) 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 from the models for earnings but not for employment. Interestingly, the 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.[3] Like Lane et al., they find that the earnings trajectories of 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 and converge over time.

An alternative approach, pursued by Ferber and Waldfogel (1998) and Corcoran and Chen (2005), is to use fixed-effects regressions to assess whether individuals who move into temporary-help and other non-traditional jobs generally experience in improvements in labor-market outcomes. A potential virtue of the fixed-effects model is that it will purge time-invariant unobserved heterogeneity in individual earnings levels that might otherwise be a source of bias. Consistent with other work, both studies find that temporary help and other non-standard work arrangements are generally associated with improvements in individuals' earnings and employment.[4]

Numerous 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, 2005) conclude that jobs with temporary help agencies significantly increase the probability of finding permanent employment within 18 months relative to unemployment. In a similar vein, Lecnher (2002) uses matching techniques to estimate the effect of subsidized temporary help placements on the labor market prospects of unemployed workers in Switzerland and finds significant benefits to these placements. 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, Mueser and Troske (2005) and they also find generally positive effects of temporary employment. Using matching and regression control

[3] 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.

[4] In section 4, we assess whether fixed-effects model adequately address the biases stemming from self-selection and conclude that they do not.

techniques, studies by Andersson and Wadensjö (2004), Amuedo-Dorantes, Malo and Munoz-Bullon (2005) and Kvasnicka (2005) also find positive effects of temporary help employment on labor market advancement for workers in Sweden, Spain, and Germany respectively,

In addition to the similarity of their findings, these studies are unified by their use of non-experimental techniques for analyzing how spells in temporary help jobs affect labor market outcomes for low-skilled workers. Our alternative approach is described below.[5]

b. The Michigan Work-First quasi-experiment

In the Michigan metropolitan area we study, individuals applying for welfare ('Temporary Assistance for Needy Families') report to the Family Independence Agency (FIA) office serving their district to apply for welfare benefits. The FIA office refers those eligible for cash assistance to a Work First contractor to whom participants must report within two weeks. For administrative purposes, welfare services in this metropolitan area are divided into fifteen geographic districts, which we refer to as randomization districts, each served by one to four independent Work First contractors in each program year. To ensure an even allocation of participants across multiple contractors serving a district, FIA offices are contractually obliged to alternate participant assignments among contractors. The contractor to which a participant is assigned depends on the date of his or her visit to the FIA office and, in some cases, whether the placement quota for specific contractors has already been filled.[6]

As the name implies, the Work First program focuses on placing participants into jobs quickly. 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

[5] The approach taken in this paper follows our earlier pilot study using a comparable research design, Autor and Houseman 2002a. The pilot study exploits a smaller quasi-experimental randomization of Work First participants in another metropolitan area of Michigan and analyzes only short-term labor market outcome measures. (Unemployment Insurance wage records were not available for that study.) The results of the earlier study and the current work for short-term outcome measures are consistent; both demonstrate positive short-term effects of temporary-help placements on earnings. The findings of the current study for long-term outcome measures reveal that these short-term benefits wash out rapidly.

[6] Participants reentering the system for additional Work First spell are randomly assigned to contractors on each occasion.

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 employment. While Work First participants may find jobs on their own, job developers at each contractor play an integral role in the process. This role includes encouraging and discouraging participants from applying for specific jobs and employers, referring participants directly to job sites for specific openings, and arranging on-site visits by employers – temporary help agencies in particular – that screen and recruit participants at the Work First office. The jobs that Work First participants take ultimately depend in part on contractors' employer contacts and, more generally, on contractor policies that foster or discourage temporary agency employment among their randomly assigned participants.

Given that all contractors in our study face the same performance incentives from the contracting agency (FIA), it is logical to ask why their placement practices appear to vary significantly. Two answers appear plausible. One is that contractors hold considerable uncertainty about which types of job placements are most effective and hence pursue different policies. We encountered this uncertainty frequently during in-person and phone interviews with Work First contractors conducted for this study. A second answer is that the performance of Work First service providers is not evaluated by the Family Independence Agency using the labor market criteria that we study here. Rather, FIA applies performance metrics such as the fraction of participants placed in jobs and the fraction remaining employed after 90 days. FIA does not collect follow-up data for participants who leave the program without a job and hence it is not feasible for FIA or its contractors to rigorously assess whether job placements improve participant outcomes or whether specific job placement types matter. This combination of uncertainty and (imperfect) incentives may explain why contractors working in close geographic proximity with identical client populations exhibit considerable heterogeneity in job placement practices.

We exploit these differences, which impact 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 instrumental variable affecting the probability that a participant obtains a temporary help job, direct-hire job, or no job during the program. Our methodology assumes that contractors only systematically affect participant outcomes through their effects on job placements. We underscore that we do not assume that contractors have no effect on participant outcomes other than through affecting job placements – only that these non-placement effects are not correlated with contractor placement rates.

One piece of prima facie evidence supporting the assumption that non-placement effects are likely to be relatively unimportant is that very few resources are spent on anything but job development. General or life skills training provided in the first week of the Work First program is very similar across contractors. And support services intended to aid job retention, such as childcare and transportation, are equally available to participants in all contractors and are provided outside the program. In Section 5, we provide econometric evidence supporting the validity of the identification assumption.

2. Testing the Research Design

a. Data and Sample

Our research data are comprised of Work First administrative records data linked to quarterly earnings from the state of Michigan's unemployment insurance wage records data base. We use administrative data on all Work First spells initiated from the fourth quarter of 1999 through the first quarter of 2004 in the metropolitan area. The administrative data contain detailed information on jobs obtained by participants while in the Work First program. We use these data to determine job placement types. To classify jobs into direct-hire and temporary help, we use the names of employers at which participants obtained jobs in conjunction with carefully compiled lists of temporary help agencies in the metropolitan area.[7] 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 also hand-coded jobs into broad occupational groups based on job title. We additionally use the Work First data to calculate the implied weekly earnings for each Work First job by multiplying the reported hourly wage rate by weekly hours.

We link the Work First administrative data to quarterly state-level unemployment insurance earnings records from the third quarter of 1997 through the fourth quarter of 2004. These UI data include total earnings in the quarter and the industry in which the individual had the most earnings in the quarter. We use them to construct pre- and post Work First UI earnings for each participant for the four to eight quarters prior to and subsequent to the Work First placement.[8]

In thirteen of the FIA randomization districts in the metropolitan area, two or more Work First contractors served the district over the time period studied.[9] From these thirteen districts and three and a half program years covered by our data, we developed a primary sample of the Work First spells initiated within nine districts. These districts were chosen both because contractor assignments within them were stable and because 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). We have also conducted our primary analyses using all thirteen districts and found that our conclusions are not sensitive to the sample selection criteria.[10]

Table 1 summarizes 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 help job, or no job. As noted earlier, the sample is predominantly

[7] 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.

[8] The UI wage records exclude earnings of federal and state employees and the self-employed.

[9] We dropped two districts from our sample that each included a contractor serving primarily ethnic populations. Participants in these districts were allowed to choose contractors based on language needs.

female (94 percent) and black (97 percent). Slightly under half of Work First spells resulted in job placements. Among spells resulting in jobs, 21 percent have at least one job with a temporary agency. The average earnings and total quarters of employment over the four quarters following program entry are comparable for those obtaining temporary agency and direct-hire jobs, while earnings and quarters of employment for those who do not obtain employment during the Work First spell are 40 to 50 percent lower.[11]

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 work 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 the Work First program have higher prior earnings and more quarters worked in the temporary help 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 2002b; Heinrich, Mueser and Troske 2005). Even in our predominantly African-American sample, we also find this relationship.

The table also reveals one further noteworthy pattern: hourly wages, weekly hours, and weekly earnings are uniformly <u>higher</u> for Work First participants in temporary help jobs than in direct-hire jobs. While this pattern stands in contrast to the widely reported finding of lower wages in temporary help positions (Segal and Sullivan 1998; General Accounting Office 2000; DiNatalie 2001), 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 (columns labeled “average

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[10] Tables are available on request.

[11] Because welfare benefits are terminated (for some time) for participants who do not find jobs during their Work First assignments, unsuccessful Work First participants continue to face strong work incentives after leaving Work First.

placements"), eighty-percent of temporary help jobs are found in just four occupations: production, general laborer, health care (primarily nursing aids) and clerical work. Clerical and health care positions are among the highest-paying of the ten occupations in our classification scheme while production and general laborer are below the median. As a consequence, temporary help workers in our data have high average wages but a substantial share holds low wage positions. Direct-hire workers by contrast are dispersed across a variety of predominantly low-paying service occupations including cashier, janitor and childcare occupations.[12]

b. Testing the efficacy of the random assignment

If Work First assignments are functionally equivalent to random assignment, there should not be significant differences in the observed characteristics of clients assigned to contractors within a randomization district other than those due to chance. We test the random assignment by comparing the following eight participants characteristics across Work First contractors within randomization district by year: 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.

With eight participant characteristics, we are likely to obtain many false rejections of the null (i.e., Type I errors), and this is exacerbated by the fact that not all participant characteristics are independent (i.e., less educated participants are more likely to be minorities). To obviate this confound, we use a Seemingly Unrelated Regression (SUR) system to estimate the probability that the observed distribution of participant covariates across contractors within each randomization

[12] Many studies that report lower earnings for temporary help agency jobs, including Segal and Sullivan 1998, rely on quarterly unemployment insurance records which report total earnings but not hours of work. Because temporary help agency are generally transitory, the absence of hours information in UI data may lead to the inference that temporary help jobs pay low hourly wages when in fact they simply provide few total hours.

district and year is consistent with chance.[13] The SUR accounts for both the multiple comparisons (eight) simultaneously in each district and the correlations among demographic characteristics across participants at each contractor.

Formally, let X_{idt}^{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_{idt} be a vector of indicator variables designating the contractor assignment for participant i, where the 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:

$$(1) \qquad X_{dt} = (I_k \otimes (Z_{dt} \quad 1))\theta + \psi \qquad\qquad X_{dt} = (X_{dt}^{1\prime}, ..., X_{dt}^{k\prime})' \ ,$$

Here, X_{dt} is a stacked set of the participant covariates, the set of control variables include contractor assignment dummies and a constant, and ψ is a matrix of error terms that allows for cross-equation correlations among participant characteristics within district-contractor cells.14 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.

Table 2 provides the p-values for the significance of Z in estimates of equation (1) for each district and year (8 districts × 4 years and 1 district × 2 years) 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 30 of 34 comparisons accept the null hypothesis at the 10 percent level and 32 of 34 at the 5 percent level.

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[13] This method for testing randomization across multiple outcomes is proposed by Kling et al. 2004 and Kling and Liebman 2004.

[14] The contractor assignment dummies in Z are mutually exclusively and one is dropped.

Because our main analysis pools variation <u>across</u> these 34 districts and years to identify the effect of Work First job placement on labor market outcomes, we next perform grouped statistical tests to evaluate the validity of the randomization for the entire experiment. To maintain the overall probability of Type I error at the target level of 0.05 with 34 independent p-values, we implement Holm's Sequentially Selective Bonferroni Method for multiple-comparisons (Holm, 1979). The Holm version of the widely-used Bonferroni multiple-comparison test provides a relatively sensitive test of the null hypothesis that these eight covariates are balanced across contractors operating within each district in each program year – where, by sensitive, we mean that the Holm-Bonferroni is more likely than a conventional Bonferonni to reject the null. We provide further detail on the Holm-Bonferonni in the Appendix.

The p-values for the Holm-Bonferroni tests for randomization are given in the outer rows and columns of Table 2. The right-hand column of the table provides p-values for the multiple comparison test of randomization of participant characteristics in all nine 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 provides the p-values for the multiple comparison test for <u>all</u> districts and years simultaneously.

Consider first the top row in the right-hand column. The p-value of 0.13 indicates that for all nine randomization districts considered simultaneously in assignment years 1999-2000, the null hypothesis of random assignment is accepted at the 13 percent level. Subsequent rows show this null is also accepted at or above the 24 percent level in each subsequent year of the randomization. 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 nine districts considering all four years of data simultaneously. Finally, the bottom-right cell of Table 2 reveals that the omnibus test for all 34 comparisons – that is, the entire experiment – is consistent with the null of random assignment with a p-value of 0.56. In net, the data appear to appear the efficacy of the random assignment.

c. Do contractor assignments affect job placements?

Our research design also requires that contractor random assignment have significant effects on participant job placement outcomes. To test whether this occurs, we estimate a set of SUR models akin to equation (1) where in this case the dependent variables are participant Work First job outcomes (direct-hire, temporary help, non-employment) following program assignment.

Results are also found 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 contractors 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 contractors 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.

Almost all comparisons soundly reject the null hypothesis of no effect of contractor assignments on participant job placement outcomes. Of 34 two-way comparisons of job placement outcomes across contractors within each district-year shown in Table 2, only three have a p-value higher than 10 percent, and 30 have a p-value under 5 percent. Similarly, for the three-way outcome comparison (no employment, temporary help employment, direct-hire employment), 31 of 34 comparisons have a p-value at or below 2 percent.

We again use the Holm- Bonferroni to test the null of no contractor effects on job outcomes across multiple sites and years. These tests provide quite strong support for the efficacy of the research design: all 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 1 percent level or better. Moreover, the omnibus test for all 34 comparisons (bottom-right cell of the table) rejects the null of no contractor effects on participant job outcomes at under the 1 percent level for both the two and three-way employment comparisons.

Did the randomization also have economically large impacts on Work First participant job placement outcomes (in addition to its statistical significance)? To answer this question, we first calculated partial R-squared values from a set of regressions of each job placement outcome on the random assignment dummy variables. These partial-R-squared values are: 0.023 for any employment; 0.016 for temporary help employment; and 0.014 for direct-hire employment. We benchmarked these values against the partial R-squareds from a set of regressions of the three job placement outcomes on all other pre-determined covariates in our estimates including eight demographic and earnings history variables and a complete set of district by year and calendar year by quarter of assignment dummies. The partial-R-squared values for these pre-determined covariates are: 0.031 for any employment; 0.014 for temporary help employment; and 0.020 for direct-hire employment. A comparison of the two sets of partial R-squared values shows that the random assignment explains 75 percent as much of the variation in job placement outcomes among Work First participants as do the combined effects of demographics, earnings history and district and time effects. In the case of temporary help job placements, the random assignment variables are more predictive than all other pre-determined covariates. Hence, the economic magnitude of the randomization on job-taking outcomes appears substantial.

3. The Effect of Job Placements on Earnings, Employment and Welfare Recidivism: Evidence from Random Assignments

We now use the linked quarterly earnings records from the state of Michigan's unemployment insurance (UI) system to assess how Work First job placements affect participants' earnings and employment over the subsequent eight calendar quarters following random assignment.[15] Our primary empirical model is:

$$(2) \qquad y_{icdt} = \alpha + \beta_1 T_i + \beta_2 D_i + X_i{}' \lambda + \gamma_d + \theta_t + (\gamma_d \times \theta_t) + \varepsilon_{idtc} \, ,$$

where the dependent variable is real UI earnings or quarters of UI employment following Work First assignment. Subscripts i refer to participants, d to randomization districts, c to contractors within randomization districts and t to assignment years. The variables D_i and T_i are indicators equal to one if participant i obtained a direct-hire or temporary-agency job during the Work First spell. The vector of covariates, X, includes 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. The vectors γ and θ contain dummies for randomization districts and year by quarter of random assignment.

The coefficients of interest in this model are β_1 and β_2, which provide the conditional mean difference 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. The estimation sample includes all 36,105 Work First participant spells initiated between 1999 and 2003 in the nine randomization districts in our sample. To account for the grouping of participants within Work First contractors, we use Huber-White robust standard errors clustered at the contractor × year of assignment level.[16] To facilitate comparisons with prior work, we begin with ordinary least squares (OLS) estimate of equation (2).

[15] It is not currently practical to track post-assignment earnings for more than eight quarters because many of the Work First assignments in our data occurred as recently as 2002 and 2003.

[16] These standard errors do not, however, account for the fact that there are repeat spells for some Work First participants in our data (23,746 unique individuals and 36,105 spells), which may induce serial correlation in employment outcomes across spells for the same individual. We demonstrate below that our results are qualitatively identical when the sample is limited to the first spell for each participant.

a. Ordinary least squares estimates

The first two columns in Table 3 presents OLS estimates of equation (2) for real earnings and quarters of employment for the first four calendar quarters following Work First placement for all 36,105 spells in our data. As show in column (1), participants who obtained any employment during their Work First spell earned $781 more in the calendar quarter following UI placement than did Work First clients who did not obtain employment. Interestingly, there is little difference between the post-placement earnings of Work First participants taking direct-hire and temporary help jobs. First quarter earnings are estimated at $795 and $723, respectively. Both are highly significantly different from zero but not significantly different from one another.

In addition to their descriptive value, these results confirm the quality of the match between the Work First administrative data and UI databases. Coding error in the employment records of the Work First administrative data or UI records, or in the matching of the two, would be expected to attenuate the link between Work First placements and UI earnings. The substantial precision of the Table 3 estimates suggest that the matched Work First and UI data are likely to be quite informative.

Additional rows of Table 3 repeat the OLS estimates for total UI earnings in the four quarters following Work First placement.[17] Work First participants who obtained any employment during their Work First assignment earned approximately $2,500 more over the subsequent calendar year than those who did not. In all post-assignment quarters, those who obtained direct hire placements earned about 15 percent more than those who obtained temporary help placements, but this difference is never statistically significant. Panel B, which presents comparable OLS models for quarters of employment following Work First assignment shows that participants who obtained

[17] To include UI outcomes for eight calendar quarters following assignment, we drop all Work First spells initiated after 2002. This step reduces the sample size from 36,105 to 25,118 spells but does not qualitatively affect our findings. In particular, earnings and employment results for the restricted sample for quarters one through four following assignment are closely comparable to those for the full sample.

direct-hire or temporary help employment jobs worked about 0.9 quarters more over the subsequent year than did participants who did not find work.

Table 4 extends the UI earnings and employment estimates to two full calendar years following Work First assignment. In the two years following assignment, Work First participants who obtained temporary and direct-hire placements earned $3,516 and $4,086 more than those who did not find a job, and worked 1.3 additional quarters. While the post-placement quarters worked by direct-hire and temporary-help job-takers are almost identical, the $500 lower earnings of temporary help takers is statistically significant ($p = 0.06$). The estimated earnings and employment gains associated with Work First job placements are substantially smaller (though still highly significant) in the second year following placement.

b. Instrumental variables estimates

The preceding OLS estimates are consistent with existing research, most notably with Heinrich et al., who find that welfare participants in the states of Missouri and North Carolina who leave welfare for temporary help jobs fare about as well over the subsequent two years (in terms of labor earnings) as those who take obtain direct-hire employment – and much better than non job-takers. Like Heinrich et al., our primary empirical models for earnings and employment contain relatively rich controls, including prior (pre-assignment) earnings and standard demographic variables.[18] Given the relative homogeneity of the Work First participant sample – almost all are black females with no college education from one metropolitan area in Michigan – one reading of the OLS estimates is that they provide a relatively clean measure of the causal effect of Work First job placements on post-placement earnings and employment. If so, instrumental variables estimates should yield comparable findings.

[18] Notably, these controls do affect the key point estimates. As is show in Appendix Tables 1a and 1b, controlling for demographic and earnings history covariates, in addition to time and district dummies (which are always included), reduces the estimated wage and employment gain to both direct-hire and temporary-help Work First placements by about 10 to 20 percent.

Instrumental estimates for the labor market consequences of Work First placements appear initially consistent with the OLS models. The 2SLS models in columns (3) and (4) of Table 3 and 4 confirm an economically large and statistically significant earnings gain accruing from Work First job placements during the first post-placement quarter. The estimated gain to Work First job placement, \$634 ($t = 5.8$), is smaller than, but statistically indistinguishable from the OLS estimate of \$781.

When job placements are disaggregated by employment types, however, discrepancies emerge. Temporary help and direct hire job placements are estimated to raise quarter one earnings by \$494 and \$705 respectively, both statistically significant. While available precision does not allow us to reject the null that these point estimates are drawn from the same distribution ($p = 0.44$), it is noteworthy that the IV estimate for the earnings gain to temporary help placements is fully one-third smaller than the wage gain for direct-hire jobs. Comparable 2SLS models for quarters of employment (rather than earnings) confirm important differences in the employment consequences of temporary help and direct-hire job placements. Work First placements in direct-hire jobs raise the probability of any employment in the first post-placement quarter by 35 percentage points ($t = 5.8$). By contrast, placements in temporary help jobs raise the probability of first quarter post-placement employment by only 12 percentage points. This point estimate is not distinguishable from zero but is significantly different from the point estimate for direct-hire placements.

When the wage and employment analysis is extended beyond the first post-placement quarter, a far more substantial disparity is evident. In the first four calendar quarters following random assignment, Work First clients placed in temporary help jobs earn \$2,216 less than those receiving a direct-hire placement, and \$132 less than those receiving no placement at all (though this latter contrast is not significant). Estimates for quarters of employment tell a comparable story. Direct-hire placements are found to raise total quarters employed by 0.87 over the subsequent four calendar quarters ($t = 5.8$) while temporary help placements have no significant effect on total quarters worked in the first year.

Examining outcomes over a two-year period following Work-First assignment adds to the strength of these conclusions. Losses associated with temporary help job placements are economically large, \$2,304 in earnings and 0.22 calendar quarters of employment, though generally not statistically significant. By contrast, direct-hire placements raise earnings by \$6,420 and total quarters of employment by 1.51 over two years. For both estimates, we can easily reject the null that the effects of direct-hire and temporary-help job placements are equal. Hence, the clear picture that emerges from these 2SLS models is that temporary help placements do not improve – and potentially hinder – labor market outcomes for the low-skilled Work First population.

c. The dynamics of temporary-help and direct-hire job placements

To explore the dynamics that lead to these divergent outcomes, we estimate a set of 2SLS models that distinguish between employment and earnings in temporary help versus direct-hire jobs. Specifically, we estimate a variant of equation (2) where the dependent variable is earnings or employment in temporary-help employment or direct-hire employment. Participants not receiving earnings or employment in the relevant sector are coded as zero for these outcome measures.[19]

Table 5 shows that participants placed in temporary help jobs by the random assigned earn an additional \$968, and work an additional 0.46 quarters, in temporary help jobs in the first calendar year following random assignment. Were temporary help employment the primary margin of earnings and employment adjustment, temporary help placements would clearly improve labor market outcomes for Work First participants over the first post-assignment year. Unfortunately, these gains in temporary help earnings and employment appear to come at the expense of earnings and

[19] For a small set of cases, we are unable to identify the industry of employment because the industry code is missing from the UI data (although we do measure total earnings and employment). These observations are not excluded from the Table 5 analysis but the outcome variables are coded as zero for both direct-hire and temporary-help earnings and employment. Due to this missing category, the Table 5 point estimates do not sum precisely to the totals in Tables 3 and 4. In the Michigan administrative data

employment in direct hire jobs. Specifically, we estimate that temporary help placements displace $1,223 in direct-hire earnings and 0.39 quarters in direct-hire employment. In net, the first-quarter benefits to temporary help placements, clearly apparent in Table 4, wash out entirely over the first post-placement year.

In the second post-placement year, the employment consequences of temporary help placements look even less favorable. While temporary help placements yield no improvement in outcomes in the temporary help sector in the second year following placement, the reductions in direct-hire outcomes detected in the first year persist into the second. Work First clients randomized into temporary help jobs earn $1,277 less and work 0.15 quarters less in direct-hire jobs in the second year following temporary placement than do clients receiving no temporary-help placement, though it must be emphasized that neither point estimate is statistically significant. The loss in direct-hire earnings is not compensated by additional earnings in the temporary help sector. In fact, participants randomized into temporary help jobs in year one have no greater earnings or employment in the temporary help sector during year two than do participants receiving no job placement.

Why do these adverse consequences continue to accrue into year two? Our data cannot directly answer this question but we can speculate. It seems likely that the reductions in direct-hire employment and earnings exposure in year caused by temporary help placements lead to fewer durable direct-hire job matches and lower human capital acquisition, potentially dampening employment outcomes in year two. Moreover, as shown in Table 3, temporary help placements appear highly transitory – leading to only a 12 percentage point gain in employment odds in the first post-placement quarter (relative to 35 percentage points for direct hire placements). When these placements end, it seems likely that a subset of participants experiences non-employment, possibly

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used to code the job placement obtained during the Work First spell, we are always able to identify type of job (temporary help or direct-hire) using employer names.

followed by Work First recidivism. The latter outcome will of course retard labor force participation further. We examine this possibility next.

d. Do job placements affect welfare recidivism?

As shown in Table 1, 36 percent of the Work First spells result in welfare program recidivism in Michigan within one year and 51 percent lead to reentry within two years. To test whether randomization of Work First clients to jobs affects recidivism rates, we estimate a variant of equation (2) where the dependent variable is an indicator variable equal to one if a Work First participant returns to welfare within 360 or 720 days of the commencement of the prior spell. An advantage of the recidivism measure is that, unlike our UI earnings and employment measures, it is constructed using the same Work First administrative data as our participant sample. There should therefore not be any slippage between the treatment variable and the outcome measure (except for participants who leave the state of Michigan).

As shown in Table 6, participants who obtained jobs during their work first spells were substantially less likely to recidivate in the one and two years thereafter. Those taking direct hire were 11 and 10 percentage points less likely to recidivate over one and two years, respectively (about 31 and 20 percent). Those who took temporary help jobs were 7 and 5 percentage points (19 and 10 percent) less likely to recidivate over one and two years. As before, these OLS estimates are unlikely to reflect causal relationships.

When we reestimate these models using Work First random assignments as instruments for job attainment, we find that direct-hire jobs reduce the probability of recidivism while temporary help jobs <u>increase</u> it. Specifically, direct-hire placements reduce two-year recidivism by 14 percentage points (27 percent) while temporary-help placements raise recidivism by 13 percentage points (25 percent). Although these point estimates are not statistically significant at conventional levels, we can reject the null that the causal effects of direct-hire and temporary-help job placements are equal for two-year recidivism. These estimates suggest that one way in which temporary help placements may retard Work First participants' labor market advancement is by increasing the frequency of repeat spells in Work First.

4. Robustness tests

a. Fixed effects estimates

We have performed a large number of robustness tests to validate these basic findings. One such test is to re-estimate the earnings and employment models using the sub-sample of participants in our data with multiple Work First spells.[20] In this repeat-spell sample, we can include individual fixed effects that absorb time invariant unmeasured individual attributes affecting the level of earnings or employment. The coefficients on job placements in these models are identified by participants whose placements (temporary help, direct hire, no employment) differed between spells. Because the randomization of clients should balance unmeasured heterogeneity across contractor sites, the instrumental variables (but not OLS) fixed effects estimates should not differ substantially from the pooled estimates above – unless the randomization is invalid. Hence, these estimates can also be viewed as a further validation of the experimental design. An unattractive feature of the fixed-effects approach is that we are forced to limit the sample to participants who have multiple spells in the data, which is a form of selection on the outcome variable. For this reason, we do not use fixed-effects models for our primary estimates.

Notably, fixed effects estimates for the causal effects of job placements on employment and earnings (Table 7) are closely comparable to 2SLS estimates that do not include fixed effects, except that fixed effects appear to increase precision.[21] Both pooled and fixed-effects 2SLS models yield no

[20] Our primary sample has 36,105 spells experienced by 23,746 participants. Our fixed-effects sample, limited to those with multiple spells, has 20,267 spells experienced by 7,908 participants, with a mean of 2.6 spells per participant. Of this sample, 5,580 participants (representing 15,030 spells) are randomly assigned to two or more distinct contractors across spells (either to a new contractor in the same randomization district or to a contractor in a different randomization district if the participant relocated).

[21] It was not feasible with our statistics package to estimate approximately 8,000 fixed-effects in these 2SLS models. We use the following procedure to circumvent this limitation: we perform a set of initial regressions to orthogonalize the outcome variables, endogenous variables (i.e., job type), and instrumental variables with respect to a complete set of participant dummies; we aggregate the orthogonalized data to randomization-district × year means; we perform the 2SLS analysis using this aggregated

evidence that placements in temporary help jobs raise earnings over the subsequent calendar year. The fixed-effects models do confirm that direct-hire placements raise earnings substantially.

For comparison, Table 7 also presents a set of comparable OLS models estimated both excluding and including fixed-effects. The contrast between these models demonstrates that even with detailed controls, pooled OLS models do not adequately account for participant heterogeneity. In fact, inclusion of fixed-effects reduces the OLS-estimated earnings and employment benefits to direct-hire and temporary-help placements by half. The further contrast between OLS fixed-effects and 2SLS estimates suggests that fixed-effects are <u>also</u> inadequate for obtaining unbiased estimates of the consequences of job placements for labor market outcomes.

Why is the fixed effects model unable to purge the bias in the OLS estimates? A likely explanation is that the fixed-effects estimator is only suited to a problem were successive earnings observations for each participant reflect simple deviations from a stable mean – i.e., a fixed, additive error component. But many low-skilled workers, and especially those receiving welfare, are likely to be undergoing significant shifts in labor force trajectory as they transition from non-employment to employment. This heterogeneity in slopes rather than intercepts will not be resolved by the fixed-effects model. Hence, the Table 7 estimates suggest that considerable caution should be applied in interpreting prior fixed-effects estimates of the impact of job types, particularly temporary help employment, on the earnings of low-skilled workers (e.g., Segal and Sullivan 1997 and 1998; Ferber and Waldfogel 1998; Corcoran and Chen 2004).

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data, weighting by cell size. Simulations demonstrate that this procedure produces 2SLS coefficients that are near-identical to microdata estimates, while the aggregation step yields appropriately conservative standard-errors, that is using degrees of freedom equal to the number of district × year means. To verify this procedure, we estimated 2SLS fixed-effects models using microdata demeaned at the individual participant level with standard errors clustered on district × year. These models produce near-identical coefficients to those in Table 7 with somewhat smaller (less conservative) standard errors, reflecting the fact that they do not account for additional degrees of freedom consumed by demeaning.

b. Accounting for the possibility of serial correlation: First-spell sample

A remaining confound in all of our prior estimates stems from the potential for serial correlation in the labor market outcomes of participants who experience multiple Work First spells. The standard errors that we estimate above cannot simultaneously account for the clustering of errors among participants assigned to a contractor and the clustering of errors across time within the same individual. Since the latter factor is likely to be much more important, we have so far clustered the standard-errors on contractor by year.

A simple means to evaluate the importance of serial correlation is to estimate the key models using only one single Work First spell per participant, specifically, the first spell in our sample. These first-spell estimates, shown in Appendix Table 2, are closely comparable to our main models for earnings and employment in Tables 3 and 4. Notably, we find little reduction in the precision of estimates, as would be expected if positive autocorrelation were biasing the standard errors of the main models, particularly given the one-third reduction in sample size. We conclude that our primary estimates are not substantially affected by serial correlation.

5. Bad Jobs or Bad Contractors?

A salient objection to the interpretation of our core results is that they may conflate the effects of contractor quality with the effects of job types. Imagine, for example, that low quality Work First contractors – that is, contractors who generally provide poor services – place a disproportionate share of their randomly assigned participants in temporary help jobs, perhaps because these jobs are easiest to locate. Also assume for argument that temporary help jobs have the same causal effect on employment and earnings as direct-hire jobs. Under these assumptions, our 2SLS estimates will misattribute the effect of receiving a bad contractor assignment to the effect of obtaining a temporary help job. Our causal model assumes that contractors systematically affect participant outcomes only through job placements, not through other quality differentials. The above scenario violates this assumption.

We view the 'bad contractor' scenario as somewhat improbable, primarily because it is hard to conceive of what services contractors provide other than job placements that might significantly affect participant labor market outcomes one to two years following random assignment. However, we can test this alternative hypothesis directly. If it is poor services, not high temporary help placement rates, that explains why 'bad contractors' produce poor participant outcomes, we would expect generally poor labor market outcomes among all participants assigned to these contractors – including participants who do not receive a temporary help placement.

We test this implication by estimating the following OLS model for post-random-assignment earnings of Work First participants:

(3) $$y_{icdt} = \alpha + b_1\bar{T}_{ct} + b_2\bar{D}_{ct} + X_i{}'\lambda + \gamma_d + \theta_t + (\gamma_d \times \theta_t) + \varepsilon_{idtc}\,.$$

This equation is similar to our main estimating equation above, with the key difference that we replace individual-level job outcomes dummies with contractor-by-year means ($\times$ 100) of job placement rates. Specifically, $\bar{D}_{ct}$ and $\bar{T}_{ct}$ are the percentage of all randomly assigned participants placed in direct-hire and temporary help jobs respectively at contractor c in year t. This equation is roughly akin to a reduced-form of our 2SLS model, where the contractor-by-year means correspond to the random assignment dummies in the first-stage equation.

Table 8 presents estimates of equation (3) for the post-random-assignment earnings of Work First participants grouped by job-placement outcome: all, temporary help and non-temporary help. For comparison with prior models, the first pair of estimates includes all randomly assigned participants, regardless of employment outcome. Our main 2SLS estimates in Table 3 imply that $b_1 \approx 0$ and $b_2 > 0$ in equation (3); that is, direct-hire placements raise participant earnings whereas temporary help placements have little earnings impact. Column (1) confirms this expectation. Participants assigned to contractors with 10 percentage point above average job placement rates earn approximately \$130 more over the next year than do participants assigned to contractors with average placement rates.

Column (2) shows distinct earnings effects for direct-hire and temporary help placements. A 10 percent higher placement rate in direct-hire jobs yields a \$190 gain in annual earnings. A 10 percent higher placement rate in temporary-help jobs yields a statistically insignificant \$43 gain in earnings.

To test the 'bad contractor' hypothesis, we reestimate equation (3) for the subsample of participants who did not receive a temporary help placement (columns (3) and (4)). If our 2SLS results are driven by the effects of 'bad contractors' rather than 'bad jobs,' we should find that $\hat{b}_1 < 0$ in the restricted sample, i.e., earnings should be relatively low for participants who did not receive a temporary help placement at contractors with high temporary help placement rates. This prediction is not affirmed. In fact, we find an insignificant positive relationship between the share of program participants placed in temporary jobs and the post-program earnings of participants who did not receive temporary help placements.[22] Apparently, only participants placed in temporary help jobs fare (relatively) poorly at contractors with high temporary help placement rates. This is strong evidence against the 'bad contractor' hypothesis.

For completeness, the final two columns of Table 8 present analogous models for the earnings of participants placed in temporary help jobs (the complement of the sample in columns (3) and (4)). We find no significant relationship between contractors' overall job placement rates and the average earnings of their participants placed in temporary help jobs. However, higher temporary-help placement rates are associated with lower earnings for participants placed in temporary help jobs, which may indicate marginal returns to temporary help placements (perhaps contractors dip deeper into the job quality queue to generate additional temporary help placements). Higher direct-hire placement rates are also associated with higher earnings for participants placed in temporary help

[22] Column (4) also shows that earnings among non-temporary-placed participants are higher at contractors with a greater direct-hire placement rate. This follows automatically from the earlier finding that direct-hire placements raise participant earnings.

positions.[23] In summary, these results provide no evidence that contractors with high temporary-help placement rates produce generally weak labor market outcomes among randomly assigned participants. Rather, poor labor market outcomes are confined to the set of participants placed in temporary help jobs.

As a further consistency check, we also reestimate our main models separately for each of the nine randomization districts in our sample. If our aggregate results are driven by the practices of a small number of 'bad contractors' or aberrant randomization districts, these models should reveal this fact. Appendix Table 3a contains OLS and 2SLS by-district models for the two-way contrast between employment and non-employment. Consistent with the pooled-district estimates in Table 3, seven of nine 2SLS point estimates for the effect of job placements on earnings are positive and five are statistically significant. Of the two negative point estimates, only one is significant. All nine 2SLS estimates for the effects of job placement on quarters of employment are positive and seven of nine are statistically significant.

In Appendix Table 3b, we provide analogous estimates for the three-way contrast between direct-hire employment, temporary help employment and non-employment. To identify the three-way contrast using within-district variation, these estimates are limited to the sub-sample of districts (four of nine) where participants are randomly assigned across three or more contractors. The three-way models also provide consistent support for the main inferences. The 2SLS estimated effect of direct-hire placements on earnings is positive and significant in three of four districts. The 2SLS estimated effect of temporary help placements on earnings is negative in three of four districts (and, unlike our primary estimates, significant in one case). In all four districts, the point estimate for temporary-help

[23] This correlation is difficult to interpret without additional structure. It may reflect composition – those least suitable for temporary help jobs are placed in direct-hire positions. Or it may reflect a complementarity between direct-hire and temporary help placements. When the Table 8 results are further disaggregated into post-assignment earnings by all job placement types (temporary help, direct-hire and non-employment), we find that contractors with higher direct-hire placement rates produce lower average participant earnings in direct-hire positions. This result is a complement to the point estimate for temporary-help employment in column (6).

placements is substantially below that of direct-hire placements (by at least $2,000). Models for quarters of employment provide equally compelling evidence that direct-hire placements increase post-assignment employment rates while temporary help placements do not appear to do so. In sum, we find a robust and consistent set of results across randomization districts in our sample.

6. Marginal Workers and Marginal Jobs

a. The marginal worker

Our estimates above demonstrate that direct-hire job placements, but not temporary help job placements, substantially raise earnings and employment of 'marginal workers,' by which we mean Work First participants whose employment outcomes are affected by the randomization.[24] Who are these 'marginal workers'? While it is not possible to individually identify marginal workers (since we cannot know who would have had a different job outcome if assigned to a different contractor), it is feasible to characterize key attributes of the affected population, including work history and demographics.

Consider the following regression model:

$$(4) \qquad 1[D_i + T_i > 0] \bullet X_{icdt} = \alpha + \pi_1 T_{ict} + \pi_2 D_{ict} + \gamma_d + \theta_t + (\gamma_d \times \theta_t) + \varepsilon_{idtc} .$$

Here, X is a demographic measure of interest, $1[\bullet]$ is the indicator function, and D and T are dummy variables indicating whether participant i obtained a direct-hire job or temporary help job during her work first spell. As before, subscripts c, d and t denote contractors, randomization districts and calendar quarters. By construction, the dependent variable is equal to X_i if participant i obtained obtain employment during the Work First spell and zero otherwise.

If equation (4) is fit using OLS, the parameters $\hat{\pi}_1$ and $\hat{\pi}_2$ estimate the (conditional) mean values of demographic variable X for Work First participants who obtained temporary help and direct-hire jobs respectively during their Work First spells. For example, OLS estimates of (4) in column (1) of

[24] 'Compliers' in the terminology of Imbens and Angrist 1994.

Table 9 show that participants who found any employment during their Work First spell earned an average of \$4,772 and worked 2.16 quarters in 4 calendar quarters prior to random assignment. Column (2) shows that the prior earnings and labor force participation of participants who took temporary help and direct hire jobs during their Work First spells are quite comparable to one another (see also Table 1). The only notable difference between the two groups is that participants who took temporary help jobs during their Work First spells had significantly higher earnings and employment in the temporary help sector over the prior four quarters (and a comparable amount less in direct-hire jobs).

Now consider 2SLS estimates of equation (4) where the variables T and D are instrumented by contractor and year of assignment dummies. In this case, the parameters $\hat{\pi}_1$ and $\hat{\pi}_2$ estimate the average characteristics (X's) of 'marginal workers,' that is participants whose employment status is changed by the random assignment (Abadie, 2003). To see this, consider a simplified case with only employment outcome, $J \in \{0,1\}$, and a single instrumental variable, $Z \in \{0,1\}$, that affects the odds that a randomly assigned participant obtains employment during her spell. Assume that the standard Local Average Treatment Effect assumptions are satisfied (Imbens and Angrist 1994), in particular that random assignment to treatment ($Z = 1$) weakly increases the odds that any participant obtains employment during her work first spell. In this case, a Wald estimate of equation (4) yields the following quantity:

$$(5) \qquad \hat{\beta}_{wald} = \frac{E[J \bullet X \mid D=1] - E[J \bullet X \mid D=0]}{E[J \mid D=1] - E[J \mid D=0]} .$$

The numerator of this expression is a scaled contrast between 'treated' and 'untreated' (i.e., $Z = 1$ or $Z = 0$) participants, reflecting both the effect of random assignment on employment odds ($E[J \mid Z=1] - E[J \mid Z=0]$) and the difference in the average X of employed participants in the treatment and control groups. The denominator rescales this contrast by the effect of random assignment on employment odds. Hence, the ratio of these two expressions provides an estimate of the average characteristics X of marginal workers – workers whose employment status was changed by the random assignment.[25]

Two-stage least squares estimates of equation (4), found in columns (3) and (4) of Table 9, establish two key results. First, the earnings histories of 'marginal workers' are substantially weaker than those of average workers. Specifically, prior-year earnings of marginal workers are about $500 (15 percent) below that of average workers while prior year labor force participation is lower by about 0.20 quarters (10 percent). Hausman tests for the equality of OLS and 2SLS coefficients (bottom row of each panel) confirm that most of these work history differences are statistically significant, although, interestingly, demographic differences are much less pronounced. Hence, contractor random assignments alter employment outcomes among Work First participants by moving those with relatively weak earnings histories into or out of the labor force. This appears eminently sensible.

The second result established in Table 10 is that there are no significant differences between the pre-placement work histories of marginal temporary workers and marginal direct-hires. Both groups have weaker prior earnings and employment histories than 'average' workers, but they do not differ from one another. This result is critical for the interpretation of our main findings because it indicates that the employment effects of direct-hire and temporary help jobs measured above are estimated on comparable populations. We can therefore conclude that the 'marginal temporary workers' in our

[25] A simple numerical example illustrates. Let X be a dummy variable equal to one if a participant is a high-school dropout and zero otherwise. Assume that 20 percent of treated participants and 10 percent of control participants find jobs during their spell. Also assume that 70 percent of treated participants who find jobs are high school dropouts versus 50 percent of untreated participants. Using equation (5), these numbers imply that 90 percent of marginal employed are high school dropouts. The intuition for this result is that the marginal 10 percent of employed participants must have been composed of 90 percent high school dropouts to raise the average high school dropout share among employed from 50 to 70 percent among the treated group relative to the control group.

sample would likely have fared significantly better had they instead been randomized into direct hire jobs, and vice versa for the 'marginal direct-hires.'[26]

b. The marginal job

Because the 'marginal' Work First participants placed in temporary and direct hire types appear similar, we are left with a puzzle as to why marginal temporary help and direct-hire job placements produce such dissimilar labor market outcomes. A likely possibility is that there are important differences in the quality of marginal temporary help and direct hire jobs.

To characterize earnings in marginal jobs we first present in Table 10 a set of OLS and 2SLS estimates for Work First participant earnings in the jobs obtained during Work First spells, i.e., under the supervision of Work First contractors. These earnings values are calculated using Work First administrative data. Consistent with the descriptive statistics in Table 1, the OLS models show that on average, participants who obtained temporary help placements earned higher initial wages, worked slightly more hours, and received higher weekly earnings than participants who obtained direct-hire jobs. But these higher earnings in average temporary help placements are not found in marginal temporary help placements. Rather, 2SLS estimates for in-program earnings show that marginal temporary help jobs pay significantly lower hourly and weekly wages than do average temporary help jobs: \$7.03 versus \$7.64 hourly and \$258 versus \$281 weekly.

While it is tempting to interpret this fact as further evidence that 'marginal' workers have weaker skills and experience than average workers, this interpretation cannot fully explain the pattern of results. As shown in column (4), wages in marginal and average direct-hire jobs are closely comparable: \$7.17 versus \$7.18 hourly and \$255 versus \$243 weekly. Given that Work First participants placed in each type of job appear similar, this suggests that marginal temporary help placements are of 'low quality' relative to marginal direct-hire placements.

[26] If instead the two marginal populations were disjoint, the direct-hire and temporary help estimates would still reflect causal estimates. But they would not necessarily inform the question of how 'marginal temps' would have fared if randomized into

A further means to measure the quality of these jobs is to examine the occupations in which marginal jobs are found. Using the administrative data, we estimate a series of OLS and 2SLS models for the occupational distributions of average and marginal direct-hire and temporary-help placements. These models, summarized in Figure 1, reveal an important contrast between marginal temporary help and marginal direct-hire placements.[27] Whereas the occupational distributions of marginal and average direct-hire placements appear closely comparable, those of marginal and average temporary help placements differ noticeably. Marginal temporary help jobs over-represent production and 'miscellaneous' occupations relative to average temporary help placements, and under-represent sales and health care occupations. This is significant because production positions are among the three lowest paying temporary-help occupation in our data (along with child care and general laborer) while sales and health care positions are two of the three highest paying (along with clerical).[28]

In summary, it appears that marginal temporary help placements are found in lower paying jobs than are average temporary help placements, while there is no obvious quality degradation in marginal versus average direct-hire placements. This may in part explain why temporary help placements induced by the randomization lead to relatively poor labor market outcomes – both relative to direct-hire placements and to no placement at all. Most critically, the estimates in Table 9 and 10 appear to demonstrate that the weak outcomes associated with temporary help placements stem in large part from the characteristics of marginal jobs rather than marginal workers.

7. Conclusion

The primary finding of our analysis is that <u>direct-hire</u> Work First placements induced by the random assignment of low-skilled workers to Work First contractors significantly increase payroll

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direct-hire jobs, and vice versa.

[27] Estimates are available from the authors.

[28] Marginal temporary help placements also slightly over-represent Miscellaneous and Clerical occupations (both occupations that have high average pay), but this is not entirely offsetting.

earnings and quarters of employment for marginal participants – by several thousand dollars over the subsequent two years. This relationship is significant, consistent across randomization districts, and economically large. We had also anticipated finding, consistent with the studies cited in Section 1, that temporary-help placements yield small but significant improvements in labor market outcomes for Work First participants. The data clearly indicate otherwise. While temporary-help placements increase participants' earnings over the near term, we find that temporary help placements do not raise – and quite possibly lower – payroll earnings and quarters of employment of Work First clients over the one to two years following placement. These adverse findings for payroll earnings are robust across all permutations of sampled districts, entry cohorts, and post-assignment time intervals in our data. They are corroborated by evidence from Work First administrative records that marginal temporary help placements are found in low paying jobs and appear to lead to increased Work First recidivism.

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 is that temporary help assignments displace other productive job-search and employment opportunities. The short-term earnings benefits of temporary help jobs – including, as shown above, comparatively high wages, weekly hours and weekly earnings during the initial placement – appear to be more than offset by other negatives that may lead to spells of non-employment and welfare recidivism. These considerations are augmented by the evidence that marginal temporary help jobs appear concentrated in low-paying occupations (relative to other temporary help jobs), suggesting that they may be particularly undesirable.

We emphasize that our results pertain to the marginal temporary help job placements induced by the randomization of Work First clients across contractors. Our analysis does not preclude the possibility that infra-marginal workers reap long-term benefits from temporary agency placements. Nevertheless, our findings are particularly germane for the design of welfare programs. The operative question for program design is whether job programs assisting welfare and other low-wage workers could improve participants' labor market outcomes by placing more clients with temporary agency positions. Our analysis suggests not. The simple reason is that marginal workers obtaining these placements do not appear to benefit. While several researchers 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; Holzer 2004; Andersson et al. 2005), we conclude that such a policy prescription is premature and potentially misguided.

Our research finally speaks to the growing European literature that finds that temporary help and other non-standard work arrangements serve as effective 'stepping stones' into the labor market. Although we do not presume that our results for low-skilled U.S. workers should generalize across disparate labor markets and worker populations, it is notable that comparable non-experimental methodologies applied to the same empirical question in the U.S. and Europe have produced comparable findings – namely, that temporary help jobs foster positive labor market outcomes. Our evidence strongly suggests that these non-experimental methods are inadequate to resolve the endemic self-selection of workers into job types according to unmeasured skills and motivation. We suggest that the emerging consensus of the U.S. and European literatures that temporary help jobs foster labor market advancement – based wholly on non-experimental evaluation – should be reconsidered in light of the evidence from random assignments.

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 α divided by the number of comparisons, N. The Bonferroni rejects the null if any of the N comparisons falls below the critical value (α / 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 $\{A_1, A_2, ..., A_N\}$ and critical value α, 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 (α / 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 α while providing more power against Type II errors than the simple Bonferroni.

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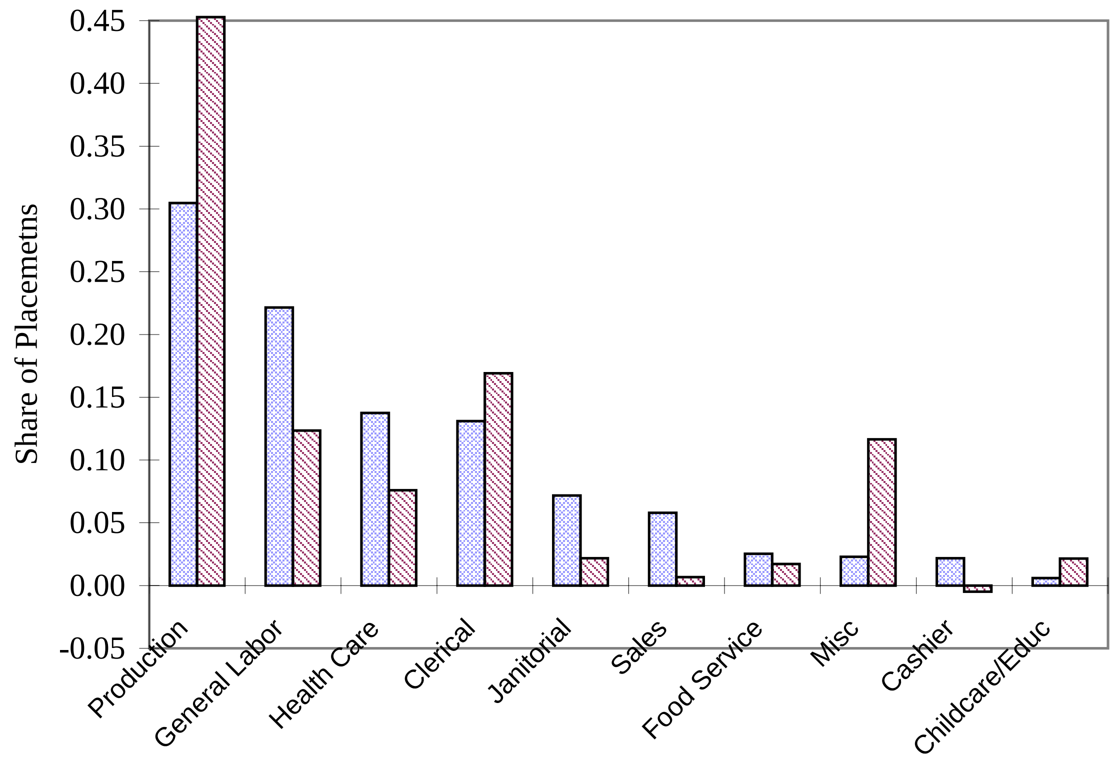
A. Temporary-Help Placements



B. Direct-Hire Placements

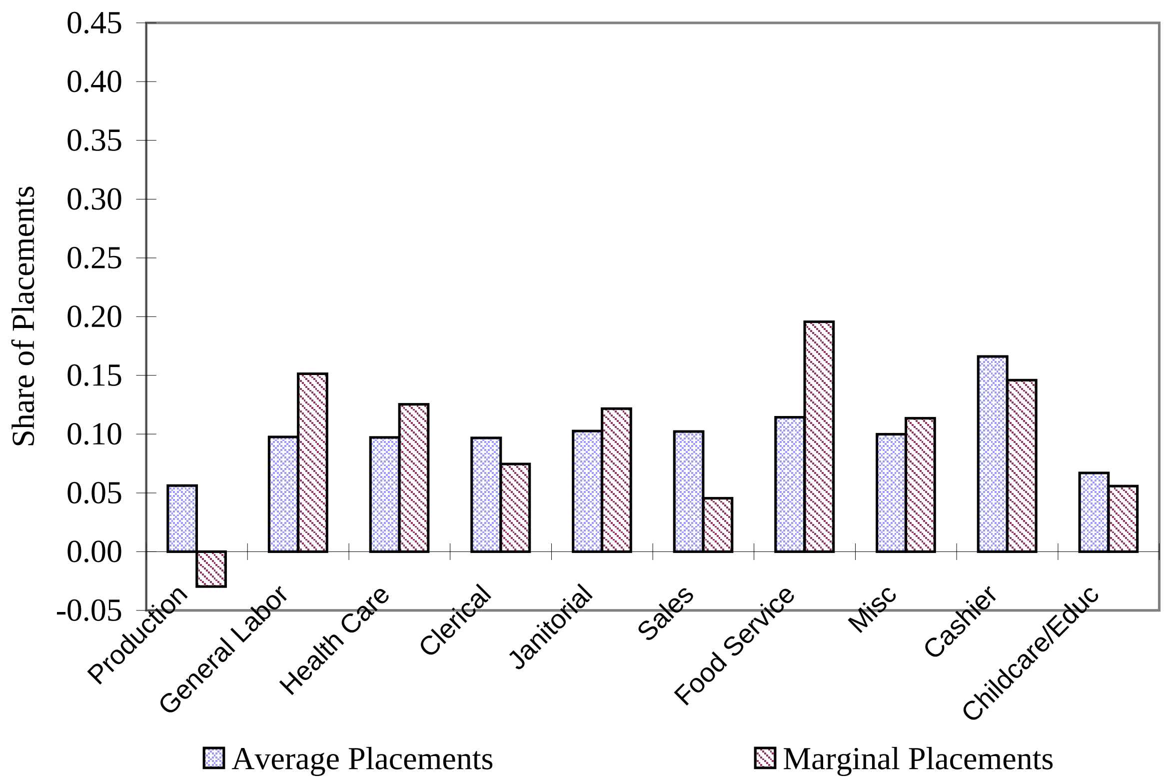


Figure 1. Occupational Distribution of Average and 'Marginal' Temporary Help and Direct-Hire Work First Job Placements

Table 1. Summary Statistics for Primary Sample of Work First Participants Randomly Assigned to Contractors 1999 - 2000: Overall and by Job Placement Outcome

	Job Placement Outcome During Work First Spell							
	All		No Employment		Direct Hire		Temporary Help	
	Mean	Std. error	Mean	Std. error	Mean	Std. error	Mean	Std. error
Percent of sample	100.0		52.9		37.4		9.7	
	A. Demographics							
Age	29.6	(0.04)	29.3	(0.06)	29.8	(0.07)	30.4	(0.13)
Female (%)	94.2	(0.12)	94.6	(0.16)	93.8	(0.21)	93.6	(0.41)
Black (%)	97.0	(0.09)	96.9	(0.12)	96.9	(0.15)	98.1	(0.23)
White/Other (%)	3.0	(0.09)	3.1	(0.12)	3.1	(0.15)	1.9	(0.23)
< High school (%)	35.9	(0.25)	38.6	(0.35)	32.7	(0.40)	33.3	(0.80)
High school (%)	35.3	(0.25)	33.5	(0.34)	37.0	(0.42)	38.3	(0.82)
> High school (%)	7.8	(0.14)	7.1	(0.19)	8.7	(0.24)	8.3	(0.47)
Unknown (%)	21.0	(0.21)	20.9	(0.29)	21.6	(0.35)	20.1	(0.68)
	B. Work History in Four Quarters Prior to Contractor Assignment							
Wage earnings	4,288	(32)	3,854	(43)	4,746	(54)	4,888	(105)
Qtrs employed	2.00	(0.01)	1.86	(0.01)	2.14	(0.01)	2.21	(0.03)
Direct hire earnings	3,589	(31)	3,237	(41)	4,060	(53)	3,689	(99)
Qtrs direct hire employment	1.52	(0.01)	1.40	(0.01)	1.69	(0.01)	1.49	(0.02)
Temp help earnings	520	(10)	462	(13)	484	(15)	975	(43)
Qtrs temp employment	0.37	(0.00)	0.35	(0.01)	0.34	(0.01)	0.59	(0.02)
	C. Job Placement Outcomes during Work First Assignment (if Employed)							
Hourly wage	7.32	(0.01)	n/a		7.24	(0.02)	7.65	(0.03)
Weekly hours	34.2	(0.06)	n/a		33.6	(0.06)	36.7	(0.11)
Weekly earnings	253	(0.71)	n/a		246	(0.81)	281	(1.41)
	D. Labor Market Outcomes in Four Quarters Following Contractor Assignment							
Wage earnings	4,277	(31)	2,941	(37)	5,811	(56)	5,658	(110)
Qtrs employed	1.97	(0.01)	1.52	(0.01)	2.47	(0.01)	2.49	(0.02)
Direct hire earnings	3,491	(30)	2,385	(35)	5,071	(54)	3,439	(95)
Qtrs direct hire employment	1.49	(0.01)	1.11	(0.01)	2.05	(0.01)	1.37	(0.02)
Temp help earnings	558	(11)	379	(11)	451	(17)	1,949	(65)
Qtrs temp employment	0.34	(0.00)	0.28	(0.01)	0.26	(0.01)	0.97	(0.02)
Work first reentry (%)	36.1	(0.25)	41.3	(0.36)	29.5	(0.39)	33.3	(0.80)
N	36,105		19,110		13,498		3,497	

Sampe: All Work First spells initiated from the fourth quarter of 1999 through the first quarter of 2004 in nine Work First randomization districts in a metropolitan area in Michigan. Individuals may have multiple spells in our data. Data source is administrative records data from Work First programs linked to quarterly earnings from Michigan unemployment insurance wage records. Temporary help versus direct hire employers are identified using unemployment insurance records industry codes. Recidvism measure identifies individuals who reentered the Work First program anywhere in the state of Michigan. All earnings inflated to 2003 dollars using the Consumer Price Index (CPI-U).

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: Assignment Years 1999 - 2003.

	Randomization District									
Assignment Year	I	II	III	IV	V	VI	VII	VIII	IX	All
1999 - 2000										
Randomization	0.79	0.02	0.46	0.17	0.17	0.67		0.59	0.42	0.13
Any employment	0.00	0.00	0.03	0.44	0.00	0.01		0.55	0.70	0.00
Temp v. direct v. none	0.00	0.00	0.16	0.00	0.00	0.04		0.00	0.00	0.00
N	1,952	1,216	900	1,425	963	822	n/a	844	720	8,842
2000 - 2001										
Randomization	0.10	0.10	0.03	0.41	0.26	0.08		0.57	0.22	0.24
Any employment	0.01	0.00	0.00	0.08	0.00	0.81		0.23	0.00	0.00
Temp v. direct v. none	0.00	0.00	0.00	0.00	0.00	0.22		0.09	0.00	0.00
N	2,026	1,474	887	1,405	974	913	n/a	900	1,590	10,169
2001 - 2002										
Randomization	0.34	0.18	0.21	0.61	0.61	0.53	0.33	0.80	0.59	0.80
Any employment	0.00	0.00	0.36	0.00	0.18	0.00	0.00	0.00	0.00	0.00
Temp v. direct v. none	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00
N	2,093	1,651	1,051	1,436	970	939	1,166	822	1,693	11,821
2002 - 2003										
Randomization	0.46	0.96	0.37	0.73	0.54	0.63	0.35	0.63	0.28	0.96
Any employment	0.02	0.00	0.02	0.06	0.00	0.00	0.00	0.00	0.00	0.00
Temp v. direct v. none	0.00	0.00	0.02	0.16	0.00	0.00	0.00	0.00	0.00	0.00
N	775	649	337	724	437	513	394	431	1,013	5,273
All Years										
Randomization	0.39	0.07	0.12	0.67	0.61	0.31	0.35	0.80	0.59	0.56
Any employment	0.00	0.00	0.02	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Temp v. direct v. none	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00
N	6,846	4,990	3,175	4,990	3,344	3,187	1,560	2,997	5,016	36,105

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 employent 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 randomization district. 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 randomization district.

Table 3. The Effect of Work-First Job Placements on Subsequent Earnings and Quarters of Employment One to Four Quarters Following Work First Assignment: Participants Assigned 1999 - 2003

	A. Earnings				B. Quarters Employed			
	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				First Quarter				
Any job	781		634		0.36		0.27	
	(18)		(108)		(0.01)		(0.04)	
Temp agency job		723		494		0.38		0.12
		(42)		(221)		(0.01)		(0.07)
Direct-hire job		795		705		0.35		0.35
		(19)		(133)		(0.01)		(0.06)
R^2	0.19	0.19			0.18	0.18		
H_0: Temp = Direct		0.11		0.44		0.04		0.04
				Quarters 2 - 4				
Any job	1,666		707		0.55		0.34	
	(54)		(291)		(0.02)		(0.09)	
Temp agency job		1,517		-626		0.52		-0.02
		(109)		(692)		(0.03)		(0.16)
Direct-hire job		1,704		1,380		0.55		0.52
		(59)		(376)		(0.02)		0.10
R^2	0.20	0.20			0.14	0.14		
H_0: Temp = Direct		0.12		0.02		0.27		0.01
				Quarters 1 - 4				
Any job	2,447		1,341		0.91		0.61	
	(67)		(378)		(0.02)		(0.12)	
Temp agency job		2,240		-132		0.90		0.10
		(136)		(868)		(0.04)		(0.21)
Direct-hire job		2,500		2,084		0.91		0.87
		(71)		(490)		(0.02)		(0.15)
R^2	0.22	0.22	0.21	0.21	0.18	0.18	0.17	0.16
H_0: Temp = Direct		0.08		0.05		0.91		0.01

N = 36,105. Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables, and controls for age and its square, gender, race, sum of UI earnings in four quarters prior to Work First assignment, and four education dummies (elementary education, less than high school, greater than high school and education unknown). Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Table 4. The Effect of Work-First Job Placements on Subsequent Earnings and Quarters of Employment One to Four Quarters Following Work First Assignment: Participants Assigned 1999 - 2002

	A. Earnings				B. Quarters Employed			
	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Quarters 1 - 4				
Any job	2,330		1,348		0.85		0.48	
	(77)		(596)		(0.03)		(0.15)	
Temp agency job		2,063		-941		0.85		-0.01
		(146)		(1081)		(0.04)		(0.27)
Direct-hire job		2,399		3,091		0.86		0.85
		(81)		(755)		(0.03)		(0.18)
R^2	0.23	0.23	0.22	0.20	0.17	0.17	0.16	0.15
H_0: Temp = Direct		0.03		0.00		0.96		0.01
				Quarters 5 - 8				
Any job	1,640		1,300		0.46		0.29	
	(71)		(584)		(0.02)		(0.13)	
Temp agency job		1,453		-1,363		0.41		-0.20
		(145)		(1,262)		(0.04)		(0.24)
Direct-hire job		1,688		3,328		0.47		0.67
		(86)		(899)		(0.02)		(0.18)
R^2	0.18	0.18			0.12	0.12		
H_0: Temp = Direct		0.19		0.01		0.14		0.01
				Quarters 1 - 8				
Any job	3,970		2,648		1.31		0.77	
	(130)		(1,110)		(0.04)		(0.26)	
Temp agency job		3,516		-2,304		1.27		-0.22
		(265)		(2,219)		(0.06)		(0.49)
Direct-hire job		4,086		6,420		1.32		1.51
		(148)		(1,555)		(0.04)		(0.32)
R^2	0.24	0.24			0.18	0.18		
H_0: Temp = Direct		0.06		0.00		0.36		0.01

N = 25,118. Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables, and controls for age and its square, gender, race, sum of UI earnings in four quarters prior to Work First assignment, and four education dummies (elementary education, less than high school, greater than high school and education unknown). Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Table 5. Two Stage Least Squares Estimates of the Effect of Work-First Job Placements on Earnings and Employment Distinguishing by Earnings Source: Temporary Help versus Direct-Hire Employer

	A. Earnings				B. Quarters Employed			
	Temporary Help		Direct Hire		Temporary Help		Direct Hire	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Quarters 1 - 4: Participants assigned 1999 - 2003							
Any job	580		789		0.14		0.48	
	(235)		(252)		(0.08)		(0.10)	
Temp agency job		968		-1,223		0.46		-0.39
		(493)		(570)		(0.17)		(0.14)
Direct-hire job		384		1,804		-0.02		0.91
		(235)		(385)		(0.09)		(0.10)
H_0: Temp = Direct		0.28		0.00		0.02		0.00
N				36,105				
	Quarters 1 - 4: Participants assigned 1999 - 2002							
Any job	591		667		0.16		0.30	
	(357)		(378)		(0.11)		(0.12)	
Temp agency job		681		-1,865		0.31		-0.44
		(633)		(657)		(0.22)		(0.16)
Direct-hire job		522		2,595		0.05		0.86
		(373)		(533)		(0.13)		(0.14)
H_0: Temp = Direct		0.82		0.00		0.34		0.00
N				25,118				
	Quarters 5 - 8: Participants assigned 1999 - 2002							
Any job	-47		1,474		-0.08		0.41	
	(176)		(579)		(0.06)		(0.11)	
Temp agency job		8		-1,277		-0.06		-0.15
		(270)		(1,109)		(0.12)		(0.18)
Direct-hire job		-89		3,569		-0.10		0.84
		(218)		(906)		(0.09)		(0.18)
H_0: Temp = Direct		0.77		0.00		0.81		0.00
N				25,118				

Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables, and controls for age and its square, gender, race, sum of UI earnings in four quarters prior to Work First assignment, and four education dummies (elementary education, less than high school, greater than high school and education unknown). Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Table 6. The Effect of Work-First Job Placements on Work First Program Recidivism

	OLS		2SLS	
	(1)	(2)	(3)	(4)
	Return within 360 days of Asssignment			
Any job	-0.10		0.05	
	(0.01)		(0.03)	
Temp agency job		-0.07		0.09
		(0.01)		(0.08)
Direct-hire job		-0.11		0.03
		(0.01)		(0.05)
R^2	0.03	0.03		
H_0: Temp = Direct		0.00		0.55
Number of observations		36,105		
	Return within 720 days of Assignment			
Any job	-0.09		-0.02	
	(0.01)		(0.06)	
Temp agency job		-0.05		0.13
		(0.01)		(0.09)
Direct-hire job		-0.10		-0.14
		(0.01)		(0.08)
R^2	0.05	0.05		
H_0: Temp = Direct		0.00		0.04
N		25,118		

Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables, and controls for age and its square, gender, race, sum of UI earnings in four quarters prior to Work First assignment and four education dummies (elementary education, less than high school, greater than high school and education unknown).

Table 7. Comparison of OLS, Fixed-Effects and Instrumental Variables estimates of the Effect of Work-First Job Placements Models on Earnings and Employment in First Year Following Work First Assignment

	OLS				2SLS			
	Pooled		Fixed-Effects		Pooled		Fixed-Effects	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	A. Earnings: Quarters 1 - 4							
Any job	2,295		1,227		1,069		635	
	(79)		(77)		(440)		(378)	
Temp agency job		2,105		1,026		48		-857
		(143)		(118)		(819)		(603)
Direct-hire job		2,346		1,286		1,694		1,719
		(87)		(83)		(645)		(502)
R^2	0.07	0.07	0.75	0.75				
H_0: Temp = Direct		0.13		0.04		0.17		0.00
	B. Quarters Employed: Quarters 1 - 4							
Any job	0.93		0.46		0.57		0.37	
	(0.03)		(0.03)		(0.12)		(0.09)	
Temp agency job		0.92		0.41		0.40		-0.02
		(0.04)		(0.04)		(0.19)		(0.13)
Direct-hire job		0.94		0.47		0.67		0.65
		(0.03)		(0.03)		(0.15)		(0.11)
R^2	0.11	0.11	0.72					
H_0: Temp = Direct		0.68		0.09		0.27		0.00
N	20,267							

Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables, and controls for age and its square. Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Table 8. The Relationship Between Post-Program Client Earnings and Job Placement Rates of their Assigned Work First Contractors
Dependent Variable: Participant Earnings in Four Quarters Following Program Entry

	All Participants		Participants Not Placed in Temporary Help Jobs		Participants Placed in Temporary Help Jobs	
	(1)	(2)	(3)	(4)	(5)	(6)
% Placed	12.92		11.93		-13.73	
	(4.22)		(3.93)		(18.53)	
% Placed in Temp Help		0.43		6.81		-139.66
		(8.82)		(9.44)		(21.69)
% Placed in Direct Hire		18.98		14.34		54.47
		(5.44)		(4.55)		(20.80)
R^2	0.041	0.041	0.041	0.041	0.045	0.060
H_0: Temp = Direct		0.101		0.497		0.000
N	36,105		32,608		3,497	

Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables, and controls for age and its square, gender, race, sum of UI earnings in four quarters prior to Work First assignment, and four education dummies (elementary education, less than high school, greater than high school and education unknown). Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Table 9. Models for the Average and Marginal Characteristics of Participants Obtaining Temporary Help and Direct-Hire Jobs during their Work First Spells

	A. Employment and Earnings History				B. Demographic Characteristics			
	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Earnings in Prior Year				*Female*			
Any job	4,772		4,243		0.937		0.955	
	(91)		(421)		(0.002)		(0.012)	
Temp agency job		4,879		4,322		0.936		0.997
		(141)		(637)		(0.004)		(0.020)
Direct-hire job		4,744		4,203		0.938		0.934
		(95)		(567)		(0.002)		(0.017)
R^2	0.238	0.238			0.888	0.888		
H_0: Temp = Direct		0.32		0.89		0.625		0.036
H_0: $B_{OLS} = B_{2SLS}$			0.081	0.196			0.130	0.038
	Quarters Worked in Prior Year				*Non-White*			
Any job	2.16		1.99		0.978		0.977	
	(0.02)		(0.08)		(0.002)		(0.007)	
Temp agency job		2.21		2.08		0.986		0.957
		(0.04)		(0.13)		(0.002)		(0.015)
Direct-hire job		2.15		1.94		0.976		0.987
		(0.02)		(0.11)		(0.003)		(0.008)
R^2	0.522	0.522			0.959	0.959		
H_0: Temp = Direct		0.069		0.431		0.001		0.081
H_0: $B_{OLS} = B_{2SLS}$			0.020	0.046			0.885	0.097
	Temporary Help Earnings in Prior Year				*Less than High School Education*			
Any job	589		386		0.331		0.335	
	(18)		(94)		(0.009)		(0.031)	
Temp agency job		980		558		0.338		0.308
		(48)		(187)		(0.012)		(0.040)
Direct-hire job		490		299		0.330		0.348
		(18)		(127)		(0.009)		(0.040)
R^2	0.046	0.055			0.213	0.213		
H_0: Temp = Direct		0.000		0.302		0.459		0.472
H_0: $B_{OLS} = B_{2SLS}$			0.033	0.013			0.893	0.739
	Temporary Help Quarters in Prior Year				*Years of Age*			
Any job	0.391		0.287		29.9		30.0	
	(0.011)		(0.034)		(0.1)		(0.3)	
Temp agency job		0.591		0.328		30.4		29.3
		(0.025)		(0.084)		(0.1)		(0.7)
Direct-hire job		0.341		0.266		29.8		30.3
		(0.011)		(0.057)		(0.1)		(0.4)
R^2	0.113	0.126			0.888	0.889		
H_0: Temp = Direct		0.000		0.620		0.000		0.250
H_0: $B_{OLS} = B_{2SLS}$			0.008	0.000			0.847	0.262

N=36,105. Robust standard errors (in parentheses) are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables. Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Table 10. The Effect of Work-First Job Placements on In-Program Earnings

	OLS		2SLS	
	(1)	(2)	(3)	(4)
		Hourly Wages		
Any job	7.27		7.12	
	(0.03)		(0.10)	
Temp agency job		7.64		7.03
		(0.07)		(0.24)
Direct-hire job		7.18		7.17
		(0.03)		(0.15)
R^2	0.89	0.89		
H_0: Temp = Direct		0.00		0.68
H_0: $B_{OLS} = B_{2SLS}$			0.104	0.005
		Weekly Hours		
Any job	34.06		35.82	
	(0.23)		(1.02)	
Temp agency Job		36.83		36.76
		(0.22)		(1.83)
Direct-hire job		33.42		35.41
		(0.25)		(1.28)
R^2	0.93	0.93		
H_0: Temp = Direct		0.00		0.55
H_0: $B_{OLS} = B_{2SLS}$			0.000	0.000
		Weekly Earnings		
Any job	250		256	
	(2)		(7)	
Temp agency Job		281		258
		(3)		(14)
Direct-hire job		243		255
		(2)		(10)
R^2	0.81	0.81		
H_0: Temp = Direct		0.00		0.87
H_0: $B_{OLS} = B_{2SLS}$			0.221	0.016

N=36,105. Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables, and controls for age and its square, gender, race, sum of UI earnings in four quarters prior to Work First assignment, and four education dummies (elementary education, less than high school, greater than high school and education unknown). Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Appendix Table 1a. Comparing of OLS and 2SLS Models for UI Wage Earnings in Quarters Following Work First Assignment

	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Earnings in Quarters 1 through 4							
Any job	2,851	2,447	1,281	1,341				
	(69)	(67)	(499)	(378)				
Temp agency job					2,678	2,240	-157	-132
					(154)	(136)	(981)	(868)
Direct hire job					2,896	2,500	2,004	2,084
					(74)	(71)	(611)	(490)
Age		161.7		192.3		162.6		192.4
		(23.8)		(25.4)		(23.8)		(26.0)
Age^2		-2.5		-3.0		-2.5		-2.9
		(0.4)		(0.4)		(0.4)		(0.4)
Female		-166.4		-200.4		-167.0		-196.3
		(141.7)		(142.3)		(142.0)		(143.8)
Race white		-221.6		-241.9		-232.1		-326.6
		(182.8)		(184.3)		(181.4)		(181.7)
Race other		145.9		105.5		139.4		60.8
		(441.0)		(448.1)		(440.0)		(442.0)
Elementary school only		-980.6		-1,036.3		-977.2		-992.3
		(109.4)		(113.1)		(109.7)		(116.1)
High school dropout		-739.0		-794.8		-738.7		-777.0
		(85.8)		(89.5)		(85.8)		(88.7)
More than high school		516.7		523.3		514.1		499.3
		(130.0)		(127.5)		(129.7)		(131.8)
Pre-Work-First earnings x 10^{-3}		364.7		369.8		364.7		368.8
		(9.6)		(9.7)		(9.6)		(9.6)
R^2	0.067	0.222			0.068	0.222		
H_0: Temp = Direct					0.197	0.077	0.076	0.045
N				36,105				

Robust standard errors (in parentheses) are clustered on Work First contractor assignment ´ year. All models include year ´ quarter of assignment and randomization-district ´ year of assignment dummy variables. Models in even numbered columns additionally contain a dummy for education unknown. Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Appendix Table 1b. Comparing of OLS and 2SLS Models for UI Wage Earnings in Quarters Following Work First Assignment

	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Quarters Worked in Quarters 1 through 4							
Any job	0.97	0.91	0.60	0.61				
	(0.02)	(0.02)	(0.13)	(0.12)				
Temp agency job					0.96	0.90	0.11	0.10
					(0.04)	(0.04)	(0.20)	(0.21)
Direct hire job					0.97	0.91	0.85	0.87
					(0.03)	(0.02)	(0.17)	(0.15)
Age		0.01		0.02		0.01		0.02
		(0.01)		(0.01)		(0.01)		(0.01)
Age^2		0.00		0.00		0.00		0.00
		(0.00)		(0.00)		(0.00)		(0.00)
Female		0.17		0.16		0.17		0.16
		(0.03)		(0.03)		(0.03)		(0.03)
Race white		-0.16		-0.17		-0.16		-0.20
		(0.04)		(0.04)		(0.04)		(0.04)
Race other		-0.16		-0.17		-0.16		-0.19
		(0.09)		(0.09)		(0.08)		(0.09)
Elementary school only		-0.19		-0.20		-0.19		-0.19
		(0.03)		(0.04)		(0.03)		(0.04)
High school dropout		-0.14		-0.15		-0.14		-0.15
		(0.03)		(0.03)		(0.03)		(0.03)
More than high school		-0.02		-0.01		-0.02		-0.02
		(0.03)		(0.03)		(0.03)		(0.03)
Pre-Work-First earnings x 10^{-3}		0.06		0.07		0.06		0.06
		(0.00)		(0.00)		(0.00)		(0.00)
R^2	0.111	0.180			0.111	0.180		
H_0: Temp = Direct					0.950	0.913	0.012	0.008
N				36,105				

Robust standard errors (in parentheses) are clustered on Work First contractor assignment ´ year. All models include year ´ quarter of assignment and randomization-district ´ year of assignment dummy variables. Models in even numbered columns additionally contain a dummy for education unknown. Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Appendix Table 2. The Effect of Work-First Job Placements on Wage and Salary Earnings during First Four Quarters Following Work First Assignment: Sample Limited to First Work-First Spell for Each Participant

	A. Earnings				B. Quarters Employed			
	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Any job	2,546		1,501		0.88		0.63	
	(86)		(377)		(0.03)		(0.13)	
Temp agency job		2,318		-817		0.87		-0.18
		(169)		(1,064)		(0.04)		(0.26)
Direct-hire job		2,603		2,594		0.88		1.02
		(93)		(618)		(0.03)		(0.18)
R^2	0.22	0.22			0.18	0.18		
H_0: Temp = Direct		0.12		0.02		0.73		0.00
N				23,746				

Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables, and controls for age and its square, race, sum of UI earnings in four quarters prior to Work First assignment, and four education dummies (elementary education, less than high school, greater than high school and education unknown). Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Appendix Table 3a. The Effect of Work-First Job Placements on Earnings and Employment during Four Quarters Following Random Assignment: Estimates by Randomization District

Randomization	A. Earnings		B. Quarters Worked	
District	OLS	2SLS	OLS	2SLS
I	2,451	896	0.80	0.19
	(155)	(724)	(0.06)	(0.15)
		n = 6,846		
II	2,617	1,106	0.85	0.32
	(140)	(430)	(0.04)	(0.11)
		n = 4,990		
III	2,612	-1,078	1.05	0.70
	(364)	(1,405)	(0.12)	(0.32)
		n = 3,175		
IV	2,400	1,475	0.86	0.16
	(205)	(683)	(0.05)	(0.24)
		n = 4,990		
V	2,358	937	0.97	0.43
	(236)	(218)	(0.08)	(0.18)
		n = 3,344		
VI	2,657	1,837	1.08	1.20
	(158)	(690)	(0.06)	(0.22)
		n = 3,187		
VII	2,605	80	1.03	0.17
	(212)	(223)	(0.12)	(0.02)
		1,560		
VIII	2,165	-847	0.85	0.64
	(159)	(195)	(0.06)	(0.03)
		n = 2,997		
IX	2,283	3,672	0.89	1.36
	(150)	(646)	(0.04)	(0.13)
		n = 5,016		

Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables, and controls for age and its square, gender, race, sum of UI earnings in four quarters prior to Work First assignment, and four education dummies (elementary education, less than high school, greater than high school and education unknown). Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Appendix Table 3b. The Effect of Work-First Job Placements on Earnings and Employment during Four Quarters Following Random Assignment: Estimates by Randomization District

Randomization District	A. Earnings OLS	A. Earnings 2SLS	B. Quarters Worked OLS	B. Quarters Worked 2SLS
I				
Temp-agency job	2,264	-2,809	0.86	0.07
	(199)	(3,088)	(0.05)	(0.48)
Direct-hire job	2,500	4,936	0.78	0.32
	(167)	(3,238)	(0.07)	(0.40)
H_0: Temp = Direct	0.24	0.24	0.02	0.77
		n = 6,846		
II				
Temp-agency job	1,807	-4,577	0.66	-0.54
	(330)	(1,718)	(0.09)	(0.36)
Direct-hire job	2,870	6,281	0.91	1.10
	(124)	(1,621)	(0.05)	(0.28)
H_0: Temp = Direct	0.01	0.01	0.03	0.02
		n = 4,990		
III				
Temp-agency job	2,208	-3,477	1.10	-0.22
	(571)	(2,768)	(0.16)	(0.71)
Direct-hire job	2,713	-1,219	1.04	0.65
	(328)	(1,584)	(0.11)	(0.35)
H_0: Temp = Direct	0.19	0.21	0.47	0.21
	0.23			
IX		n = 3,175		
Temp-agency job	2,656	1,011	0.91	0.38
	(475)	(1,843)	(0.10)	(0.39)
Direct-hire job	2,196	4,942	0.88	1.83
	(171)	(1,465)	(0.04)	(0.37)
H_0: Temp = Direct	0.41	0.16	0.78	0.04
		n = 5,016		

Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include year × quarter of assignment and randomization-district × year of assignment dummy variables, and controls for age and its square, gender, race, sum of UI earnings in four quarters prior to Work First assignment, and four education dummies (elementary education, less than high school, greater than high school and education unknown). Earnings values inflated to 2003 dollars using the Consumer Price Index (CPI-U).