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

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

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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 minority and 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 (DiNatale 2002). Recent analyses of state administrative welfare data reveal that 15 to 40 percent of former welfare recipients who obtained employment in the years following the 1996 U.S. welfare reform took jobs in the temporary help sector.[1] 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, Reynolds, and Marsden 2003).

[1] See Autor and Houseman (2002b) on Georgia and Washington state; Cancian et al. (1999) on Wisconsin; Heinrich, Mueser, and Troske (2005) on North Carolina and Missouri; and Pawasarat (1997) on Wisconsin.

In contrast to this view, numerous scholars and practitioners have argued that temporary help agencies provide little opportunity or incentive for workers to invest in human capital or develop productive job search networks and instead offer workers a series of unstable and primarily low-skilled jobs (Parker 1994; Pawasarat 1997; Jorgenson and Riemer 2000). In support of this hypothesis, Segal and Sullivan (1997) find that while mobility out of the temporary help sector is high, a disproportionate share of leavers enters unemployment or exits the labor force. 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 is that there are economically large, but typically unmeasured, differences in skills and motivation of workers taking temporary help and direct-hire jobs, as we show below. Cognizant of these sample-selection problems, several 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 parallel European literature evaluates whether temporary help employment, as well as fixed-term contracts, provide a “stepping stone” into stable employment. Notably, these recent U.S. and European studies, without exception, find that temporary help jobs provide a viable port of entry into the labor market and lead to longer-term labor market advancement.[2]

[2] Given the diversity of labor market institutions in European economies, there is no presumption that the cross-country findings should be comparable. This makes it all the more striking that the thirteen (by our count) studies in this literature have developed such consistent results.

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, selection-adjustment, and structural estimation techniques to account for the likely non-random selection of workers with different earnings capacities into different job types. The veracity of the findings therefore depends critically on the efficacy of these methods for drawing causal inferences from non-experimental data.

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 in which welfare recipients participating in a return-to-work program (Work First) were, in effect, randomly assigned across a large number of service providers (contractors). As we demonstrate below, Work First participants randomly assigned to different contractors had significantly different placement rates into direct-hire or temporary help jobs but otherwise received similar services. We analyze this randomization using an "intention to treat" framework whereby randomization alters the probabilities that 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 Unemployment Insurance (UI) wage records for the entire State of Michigan for approximately 39,000 Work First spells initiated from 1999 to 2003. The Work First data include demographic information on Work First participants and detailed information on jobs found during the program. The UI wage records enable us to track earnings of all participants over time, as well as provide labor market histories on participants before program entry. Among Work First participants who found employment, about 20 percent held temporary help jobs.

Our primary finding is that “marginal” direct-hire Work First placements—those induced by the random assignment of participants to Work First contractors—increase payroll earnings by several thousand dollars, increase time employed by one to two quarters, and lower the probability of recidivism into the Work First program by 20 percentage points over the subsequent two years. These relationships are significant, consistent across randomization districts, and economically large. By contrast, we find that temporary help placements improve employment and earnings outcomes only in the very short-term. Over time horizons of one to two years, temporary help placements do not improve—and quite possibly worsen—these labor market outcomes. Rather than promoting transitions to direct-hire jobs, temporary help placements primarily displace employment and earnings from other (direct-hire) jobs.

We also consider and present strong evidence against two potential threats to validity. One is that the adverse findings we document for temporary help job placements could be driven by a general association between “bad contractor” practices and use of temporary help placements. To address this concern, we first establish that the estimated negative consequences of temporary help placements are evident in almost all of the randomization districts in our sample, and hence that our findings are not driven by the poor practices of one or more aberrant contractors. Second, to explore the concern that there may be other important unmeasured contractor practices (e.g., additional supports and services) that explain the link between contractor random assignments and participants’ outcomes, we test and confirm that there is no remaining, significant variation in the effects of Work First contractors on participant outcomes that is not captured by contractor placement rates. Third, we find that direct-hire and temporary agency job placement rates are positively and significantly correlated across contractors, a fact that reduces the plausibility of a scenario in which "bad" contractors primarily place participants in temp agency jobs and "good" contractors primarily place participants in direct-hire jobs. These findings suggest that it is job placement rates themselves—not other confounding factors—that account for our main findings.

A second concern we tackle is the possibility of parameter instability. Because contractors have internal discretion about which clients to encourage toward which job types, our estimates might not necessarily identify a stable "intention to treat" relationship, as would occur if random assignments uniformly raised or lowered the probability that each participant obtained a given job placement (temporary help, direct-hire, non-employment). To address this issue, we exploit the panel structure of the data to analyze the labor market outcomes of participants who experience multiple Work First spells during the sample window and who are assigned to multiple contractors (because of the repeated randomization). Fixed-effects instrumental variables models estimated on this subsample affirm the main findings: using only within-person, over-time variation in outcomes for participants randomly assigned to contractors with differing placement practices, we estimate that direct-hire jobs induced by random assignments raise post-assignment earnings and employment, while temporary help placements retard them. Corroborating this evidence, we demonstrate that "marginal" workers placed in temporary help positions have comparable pre-placement earnings histories to marginal workers placed in direct-hire positions, again indicating that the contrast between the positive labor market outcomes of direct-hire placements and the generally negative outcomes of temporary help placements result from differences in the quality of jobs, not from differences in the quality of workers placed in these jobs.

In addition to presenting findings from models based on the quasi-experiment, we 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 opposite to our main, quasi-experimental estimates—OLS and fixed-effects estimates indicate that workers who take temporary help jobs fare almost as well as those taking direct-hire positions. The contrast with our core findings suggests either that non-experimental estimates are biased by the endemic self-selection of workers into job types according to unmeasured skills and motivation, or that there are substantial differences between the "marginal" treatment effects recovered by our quasi-experiment analysis and "average" treatment effects of temporary help placements observed in non-experimental data. 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 carefully considered in light of the evidence from random assignments.[3]

## 1. Prior evidence 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, 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 participants who take direct-hire jobs (see Table 1). Not surprisingly, the contrast with those who take no employment during their Work First spell is

[3] Our microeconomic evidence answers the question of whether temporary help jobs benefit the individuals who take them, but it does not inform the question of 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, 2005).

much more pronounced. These contrasts underscore the difficulty of disentangling the effects of job-taking on subsequent labor market outcomes from the causes that determine what jobs are taken initially.

Several recent studies attempt to overcome problems of sample selection. 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 they suggest the use of temporary help jobs by welfare agencies as a means to improve labor market outcomes. However, they acknowledge that in their Survey of Income and Program Participation data it was infeasible to construct comparison groups that were well-matched on earnings histories but differed on job types, which led to a potential bias in the estimates.

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 from those 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.[4] Like Lane et al. (2003), they find that the initial earnings of those taking temporary help jobs are lower than of those taking direct-hire jobs but that they are significantly better than

[4] 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.

of those who are not employed and tend to converge over two years toward the earnings of those initially taking direct-hire jobs.

An alternative approach, pursued by Ferber and Waldfogel (1998) and Corcoran and Chen (2004), is to estimate fixed-effects regressions to assess whether individuals who move into temporary help and other non-traditional jobs generally experience improvements in labor-market outcomes. A virtue of the fixed-effect model is that it will purge time-invariant unobserved heterogeneity in individual earnings levels that might otherwise be a source of bias. However, if there is heterogeneity in earnings trajectories (rather than in earnings levels) that is correlated with job-taking behavior, the fixed-effects model will not resolve this bias.[5] As is consistent with other work, the studies by Ferber and Waldfogel (1998) and Corcoran and Chen (2005) find that temporary help and other non-standard work arrangements are associated with improvements in individuals' earnings and employment.

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, relative to starting off unemployed, being in a temporary help job significantly increases the probability of finding permanent employment within 18 months. In a similar vein, Gerfin et al. (forthcoming) use matching techniques to estimate the effect of subsidized temporary help placements on the labor market prospects of unemployed workers in Switzerland and find significant benefits to these placements. Booth, Francesconi, and Frank (2002) and García-Pérez and Muñoz-Bullón (2002) study the effects on subsequent employment outcomes of temporary (agency and fixed-term) employment in Britain and temporary agency

[5] The fixed-effects estimator is ideally suited to a problem where successive outcome observations for each individual 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

employment in Spain, respectively. Their empirical strategies are similar to those used in Heinrich, Mueser, and Troske (2005), and they find generally positive effects of temporary employment, as well. Using matching and regression control techniques, studies by Andersson and Wadensjö (2004), Amuedo-Dorantes, Malo, and Muñoz-Bullón (2005), and Kvasnicka (2005) also find positive effects of temporary help employment on labor market advancement for workers in Sweden, Spain, and Germany, respectively. Zijl et al. (2004) apply a structural duration model to estimate the effect of temporary help job-taking on durations to direct-hire ("regular") work in the Netherlands and conclude that temporary help jobs substantially reduce unemployment durations and increase subsequent job stability.

While all of these non-experimental studies conclude that temporary help jobs improve subsequent labor market outcomes, we believe that the importance of the research question also warrants an experimental (or quasi-experimental) evaluation to explore the robustness of these conclusions. We pursue such an approach here.[6]

### b. Our Approach: The Michigan Work First quasi-experiment

Most recipients of TANF ('Temporary Assistance for Needy Families') benefits must fulfill mandatory minimum work requirements. In Michigan, those applying for TANF benefits who do not meet these work requirements must begin participating in a Work First program designed to help place them in employment. For administrative purposes, welfare and Work First services in the metropolitan area we study are divided into geographic districts, which we refer to as randomization districts. The Work First program is administered by a city agency, but the

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model. In Section 3, we assess whether fixed-effects models resolve the biases stemming from self-selection and conclude that they do not.

[6] The approach taken in this paper follows our earlier pilot study (Autor and Houseman 2002a), which 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 earlier study and the current work both find positive short-term effects of temporary help placements on earnings. By utilizing UI records to analyze long-term outcomes, the current study demonstrates that these short-term benefits wash out rapidly.

actual provision of services is contracted out to non-profit or public organizations. Within each geographic district, one to three Work First contractors provide services for TANF recipients residing in the district in each program year. When multiple contractors provide Work First services within a district, they alternate taking in new participants. Thus, the contractor to which a participant is assigned depends on the date that he or she applied for benefits. As we demonstrate formally below, this intake procedure is functionally equivalent to random assignment.[7]

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 childcare and transportation are provided by outside agencies and are available on an equal basis to participants at all contractors.

By the second week of the program, participants are expected to search intensively for employment and are formally <u>required</u> to take any job offered to them provided it pays the federal minimum wage and satisfies work hours requirements. Although Work First participants may find jobs on their own, job developers at each contractor play an integral role in the process.[8] This role includes encouraging and discouraging participants from applying for specific jobs and to specific employers, referring participants directly to job sites for specific openings, and arranging on-site visits by employers—including temporary help agencies—that screen and recruit participants at the Work First office. For example, Autor and Houseman (2005, Table 1) report that 24 percent of contractors surveyed in this metropolitan area refer participants to

[7] Participants reentering the system for additional Work First spells follow the same assignment procedure and thus may be reassigned to another contractor.

[8] In a survey of contractors operating in this city, half indicated they were directly involved in 75 percent or more of Work First participant job placements, and 85 percent of contractors took credit for more than 50 percent of the jobs obtained in their program (Autor and Houseman 2005).

temporary help jobs on a weekly basis, while 38 percent make such referrals only sporadically or never. Similarly, 14 percent of contractors directly invite temporary help agencies on-site weekly or monthly to recruit participants, while 29 percent of contractors never do so. The correlations between these frequencies and contractors' (self-reported) temporary agency placement rates are 0.29 for on-site visits and 0.53 for temporary agency referrals, the latter of which is highly significant. This indicates that the jobs that participants take depend in part on contractors' employer contacts and, more generally, on policies that foster or discourage temporary agency employment among participants.

It is logical to ask why contractors' placement practices significantly vary. The most plausible answer is that contractors are uncertain about which types of job placements are most effective and hence pursue different policies. Contractors do not have access to UI wage records data (used in this study to assess participants' labor market outcomes), and they collect follow-up data only for a short time period and only for individuals placed in jobs. Hence, they cannot rigorously assess whether job placements improve participant outcomes or whether specific job placement types matter. During in-person and phone interviews conducted for this study, contractors expressed considerable uncertainty, and differing opinions, about the long-term consequences of temporary job placements (Autor and Houseman 2005).

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, a direct-hire job, or no job during the program.

Our methodology does not assume that contractors have no effect on participant outcomes other than through their effects on job placements—only that any other practices affecting participant outcomes are uncorrelated with contractor placement rates. However, few resources are spent on anything but job development (Autor and Houseman 2005). 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. Survey evidence collected for the majority of contractors in our sample confirms that Work First services other than job placements are almost entirely standardized across contractors operating in this metropolitan area (Autor and Houseman 2005). In Section 4, 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 2003 in the metropolitan area. The administrative data contain detailed information on jobs obtained by participants while in the Work First program. 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.[9] 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

[9] Particularly helpful was a comprehensive list of temporary agencies operating in our metropolitan area as of 2000, developed by David Fasenfest and Heidi Gottfried.

an internet search or telephone contact. We also use the administrative data to calculate the implied weekly earnings for each Work First job by multiplying the hourly wage rate by weekly hours.

The 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.[10]

In 14 of the districts in the metropolitan area, two or more Work First contractors served the district over the time period studied. In two districts, however, one contractor in each district was designated to serve primarily ethnic populations, and participants were allowed to choose contractors based on language needs. We drop these two districts from our sample. We further limit the sample to spells initiated when participants were between the ages of 16 and 64 and drop spells where reported pre- or post-assignment quarterly UI earnings values exceed $15,000 in a single calendar quarter. These restrictions reduce the sample by less than 1 percent. Finally, we drop all spells initiated in a calendar quarter in any district where one or more participating contractors received no clients during the quarter, as occasionally occurred when contractors were terminated and replaced.[11]

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. The sample is predominantly female (94 percent) and black (97 percent). Slightly under half (47 percent) of Work First spells resulted in job placements. Among spells resulting in jobs, 20 percent have at least one job with

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

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.[12]

The average characteristics of participants vary considerably according to job outcome. Those who do not find jobs while in Work First are more likely to have dropped out of high school, to have worked fewer quarters before entering the program, and to 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 while 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 reveals one further noteworthy pattern: hourly wages, weekly hours, and weekly earnings are uniformly <u>higher</u> for participants in temporary help jobs than for those in direct-hire jobs. 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; DiNatale 2001). Although it is possible that this pattern is specific to the regional labor market we study, 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

[11] This further reduced the final sample by 3,091 spells, or 7.4 percent. We have estimated the main models including these observations with near-identical results.

but not hours of work. Because temporary help jobs are generally transitory, the absence of hours information in UI data may lead to the inference that temporary help jobs pay lower hourly wages when in fact they simply provide fewer total hours.

**b. Testing the efficacy of the random assignment**

If Work First assignments are functionally equivalent to random assignment, observed characteristics of clients assigned to contractors within a randomization district should be statistically indistinguishable. We test the random assignment across contractors within randomization district for each program year by comparing the following ten participant characteristics : gender, white race, other (non-white) race, age, elementary-school-only education, post-elementary high-school drop-out education, number of quarters worked in the eight quarters before 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 in the prior eight quarters from quarters where a temporary agency was the primary employer.

With ten 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 (e.g., less educated participants are more likely to be minorities). To resolve these confounding factors, we use a Seemingly Unrelated Regression (SUR) system to estimate the probability that the observed distribution of participant covariates across contractors within each randomization district and year is consistent with chance.[13] The SUR accounts for both the multiple comparisons (ten) simultaneously in each district and the correlations among demographic characteristics across participants at each contractor.

[12] Note that because participants who do not find jobs during their Work First assignments face possible sanctions, unsuccessful participants continue to face strong work incentives after leaving Work First.

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

Formally, let $X_{idt}^{k}$ be a $k \times 1$ vector of covariates containing individual characteristics for participant $i$ assigned to one 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) $$X_{dt} = (I_k \otimes (Z_{dt} \;\; 1))\theta + \psi \qquad X_{dt} = (X_{dt}^{1}{}', \ldots, X_{dt}^{k}{}')'.$$

Here, $X_{dt}$ is a stacked set of the participant covariates, the set of control variables include contractor assignment dummies and a constant, and $\psi$ 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 participants assigned to different contractors within a district and year; a high p-value corresponds to an acceptance of this null.

Table 2 provides the chi-square statistics and p-values for the significance of $Z$ in estimates of Equation (1) for each of the 41 district-by-year cells in our sample.[15] Consistent with the hypothesis that assignment of participants across contractors operating within each district is functionally equivalent to random assignment, we find that 46 of 48 comparisons accept the null hypothesis at the 10 percent level and 47 of 48 at the 5 percent level. We next perform grouped statistical tests to evaluate the validity of the randomization for the entire experiment. Since participant assignments are independent across districts and over time, the chi-square test

[14] Since the contractor assignment dummies in $Z$ are mutually exclusive, one is dropped.

[15] Seven of 48 district-by-year cells are dropped because there is only one (or in some cases no) participating contractor in the district for most or all of the year. In two district-by-year cells, one matching characteristics (race or education) was identical for all randomly-assigned participants; we therefore did not test for equality of this characteristic within the cell, and the degrees of freedom for the chi-square statistic are reduced accordingly.

statistics in each cell can be combined to form an overall chi-squared test statistic (DeGroot and Schervish 2002, Theorem 7.2.1). As is shown in the final row and column of Table 2, the overall p-value of the randomization across all 41 cells in our samples is 0.33, with 587 degrees of freedom. Moreover, the null of participant balance across contractors within districts is accepted at the 5 percent level or better in each of the 12 districts and in all four years of the sample. In sum, the data appear to affirm the efficacy of the random assignment.

**c. The effect of contractor assignments on job placements**

Our research design also requires that contractor random assignments significantly affect participant job placement outcomes. To test whether this occurs, we estimated a set of SUR models akin to equation (1) where the dependent variables are participant Work First job outcomes (direct-hire, temporary help, non-employment). These tests provide 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. The omnibus test for all 41 comparisons also rejects the null at well below the 1 percent level.[16]

Are the effects of randomization on participant job placement outcomes economically large in addition to being statistically significant? To answer this question, we calculate 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 0.019 for any employment, 0.013 for temporary help employment, and 0.011 for direct-hire employment. We benchmark these values against the partial R-squared values from a set of regressions of the three job placement outcomes on <u>all other</u> pre-determined covariates in our estimates including the ten demographic and earnings history variables discussed above 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 0.036 for any employment, 0.024 for temporary help employment, and 0.026 for direct-hire employment. A comparison of the two sets of partial R-squared values shows that the random assignments explain 40 to 55 percent as much of the variation in job placement outcomes among participants as do the combined effects of demographics, earnings history, and district and time effects. We conclude that the economic magnitude of the randomization on job-taking outcomes is substantial.

## 3. Main results: The effects of job placements on earnings and employment

We now use the linked quarterly earnings records from the state of Michigan's unemployment insurance system to assess how Work First job placements affect participants' earnings and employment over the subsequent eight calendar quarters following random assignment.[17] Our primary empirical model is:

$$(2) \qquad Y_{icdt} = \alpha + \beta_1 T_i + \beta_2 D_i + X_i{}' \beta_3 + \gamma_d + \theta_t + (\gamma_d \times \theta_t) + \varepsilon_{icdt} ,$$

where the dependent variable is real UI earnings or quarters of UI employment following the quarter of Work First assignment. Subscript $i$ refers 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.

[16] Tables displaying these results are available from the authors. At the district-by-year level, we reject the null hypothesis of no contractor effects on job placement outcomes in 36 of 41 district-year cells at the 1 percent level, and we reject at the 5 percent level in 39 of 41 cells.

The vectors $\gamma$ and $\theta$ contain dummies for randomization districts and year by quarter of random assignment.

The coefficients of interest in this model are $\beta_1$ and $\beta_2$, 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 38,689 participant spells initiated between 1999 and 2003 in the 12 randomization districts in our sample. To account for the grouping of participants within contractors, we use Huber-White robust standard errors clustered at the contractor × year of assignment level.[18]

In subsequent two-stage least squares models (2SLS), we instrument $T$ and $D$ with contractor-assignment-by-year dummy variables. For purposes of the 2SLS models, use of these contractor-by-year dummy variables is almost identical to using contractor-year placement job rates (by job type) as instrumental variables.[19] Accordingly, this model can be conveniently approximated as

(3) $$Y_{icdt} = \alpha + \pi_1 \hat{P}^T_{ct} + \pi_2 \hat{P}^D_{ct} + X_i\beta + \gamma_d + \theta_t + (\gamma_d \times \theta_t) + \nu_{ct} + \omega_{icdt},$$

where $\hat{P}^T_{ct}$ and $\hat{P}^D_{ct}$ are contractor × year temporary help and direct-hire placement rates, and where the error term is partitioned into two additive components, $e_{icdt} = \nu_{ct} + \omega_{icdt}$. The first is a contractor-by-year random effect, reflecting unobserved contractor heterogeneity. The second is a participant-spell specific iid random error component. Equation (3) underscores the two key

[17] It is not yet feasible 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.

[18] These standard errors do not, however, account for the fact that there are 25,802 unique individuals represented in our data and so some participants have repeat 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 (see also Appendix Table 1).

conditions that our identification strategy requires for valid inference. First, it must be the case that $\omega$ is uncorrelated with $\hat{P}_{ct}^{T}$ and $\hat{P}_{ct}^{D}$, a condition that is (almost) guaranteed to be satisfied by the randomization. The second condition is that contractor-by-year random effects are mean independent of contractor placement rates, i.e., $E(\nu_{ct} P_{ct}^{T}) = E(\nu_{ct} P_{ct}^{D})$. It is therefore not problematic for our estimation strategy if contractors have significant effects on participant outcomes through mechanisms other than job placements (e.g., other activities and supports) provided that these effects are not systematically related to contractor job placement rates. We proceed for now under the assumption that this condition is satisfied and examine corroborating evidence in Section 4.

**a. Ordinary least squares estimates**

To facilitate comparisons with earlier empirical work, we begin our analysis with ordinary least squares (OLS) estimates of Equation (2). The first two columns of Table 3 presents OLS estimates of Equation (2) for real earnings and quarters of employment for the first four calendar quarters following Work First assignment for all 38,689 spells in our data. As shown in column (1), participants who obtained any employment during their Work First spell earned \$789 more in the calendar quarter following UI placement than did clients who did not obtain employment. Interestingly, there is little difference between the post-assignment earnings of participants taking direct-hire and temporary help jobs. First quarter earnings are estimated at \$803 and \$731, respectively. These contrasts are significantly different from zero but not significantly different from one another at the 5 percent level ( $p = .09$ ).

Additional rows of Table 3 repeat the OLS estimates for total UI earnings in the four quarters following program entry. Participants who obtained any employment during their Work

[19] It is almost identical because means and dummy variables will differ slightly if there is any sample correlation

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. Panel B, which presents comparable OLS models for quarters of employment following Work First assignment, shows that participants who obtained direct-hire or temporary help jobs worked about 0.9 calendar 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.[20] Over this period, participants who obtained temporary help and direct-hire placements earned $3,385 and $4,212 more than those who did not find a job and worked an additional 1.2 and 1.3 quarters respectively (both significant at $p = 0.01$).

### b. Instrumental variables estimates

The preceding OLS estimates are consistent with existing research, most notably with Heinrich et al. (2005), who find that Missouri and North Carolina welfare recipients taking temporary help jobs earn almost as much over the subsequent two years as those obtaining direct-hire employment—and much more 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.[21] Instrumental variables estimates for the labor market consequences of Work First placements appear initially to be consistent with the OLS models. The 2SLS models in columns (3) and (4) of Table 3 confirm an economically large and statistically significant earnings gain accruing from Work First job

between contractor dummies and participant characteristics. However, we have already established that, because of the random assignment, this correlation is not significantly different from zero.

[20] To include UI outcomes for eight calendar quarters following assignment, we must drop all Work First spells initiated after 2002. This reduces the sample to 27,029 spells.

[21] All of our main models control for demographic and earnings history covariates as well as for time and district dummies and their interaction. OLS (but not IV) estimates of wage and employment effects of direct-hire and temporary help

placements during the first post-assignment quarter. The estimated gain to a Work First job placement, \$559 ($t = 5.8$), is about 25 percent less than the analogous OLS estimate.

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 \$460 and \$622 respectively. Both are statistically significant. While available precision does not allow us to reject the null hypothesis that these point estimates are drawn from the same distribution ($p = 0.49$), it is noteworthy that the IV estimate for the earnings gain from temporary help placements is approximately 25 percent 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. Placements in direct-hire jobs raise the probability of any employment in the first post-assignment quarter by 36 percentage points ($t = 6.1$). By contrast, placements in temporary help jobs raise the probability of first quarter employment by only 12 percentage points. This point estimate is not distinguishable from zero ($t = 1.7$), but it is significantly different from the point estimate for direct-hire placements.

When the wage and employment analysis is extended beyond the first post-assignment quarter, a far more substantial disparity is evident. In the first four calendar quarters following assignment, Work First clients placed in temporary help jobs earn \$2,470 less than those receiving a direct-hire placement and \$306 <u>less</u> than those receiving no placement at all (though this latter contrast is insignificant). Estimates for quarters of employment tell a comparable story. Direct-hire placements raise total quarters employed by 0.90 over the subsequent four calendar

placements are about 20 percent larger when these demographic and earnings history controls are excluded (estimates available from the authors).

quarters ($t = 6.5$), while temporary help placements have an economically small and statistically insignificant effect on total quarters worked in the first year.

Examining outcomes over a two-year period following Work First assignment (Table 4) adds to the strength of these conclusions. Estimated losses associated with temporary help job placements are sizable, $2,176 in earnings and 0.16 calendar quarters of employment, though not statistically significant. By contrast, direct-hire placements raise earnings by $6,407 and total quarters of employment by 1.56 over two years. For both estimates, we can easily reject the null hypothesis that the effects of direct-hire and temporary help job placements are equal. The clear picture that emerges from these 2SLS models is that temporary help placements do not improve—and potentially harm—labor market outcomes for the Work First population.[22]

### c. The dynamics of earnings, employment, and Work First recidivism

To better understand the disparate impacts of temporary help and direct-hire job placements, we explore the dynamics underlying these outcomes. We first 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.[23]

[22] 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. We evaluate the importance of serial correlation by estimating key models using only the first Work First spell per participant. These first-spell estimates, shown in Appendix Table 1, are closely comparable to our main models for earnings and employment in Table 3. Notably, given the one-third reduction in sample size, the slight reduction in the precision of the estimates indicates that the precision of our primary estimates is not substantially affected by serial correlation.

[23] For a small set of cases, the industry code is missing from the UI data (though we do measure total earnings and employment). These observations are included in the Table 5 analysis but the outcome measures are coded as zero for both direct-hire and temporary help earnings and employment. Consequently, the Table 5 point estimates do not sum precisely to the totals in Tables 3 and 4. In the Work First administrative case data used to code job types obtained during the Work First spell, job types (temporary help or direct-hire) are identified by employer names in all cases.

Table 5 shows that marginal temp workers earn an additional $999 and work an additional 0.48 quarters in temporary help jobs in the first calendar year following random assignment. (Both are significant.) However, these gains in temporary help earnings and employment appear to come at the expense of earnings and employment in direct-hire jobs. We estimate that temporary help placements displace $1,486 in direct-hire earnings and 0.48 quarters in direct-hire employment in the first year. On net, the first-quarter benefits to temporary help placements, clearly apparent in Table 4, wash out entirely over the first year. As shown in the bottom panel of Table 5, direct-hire placements continue to have large positive and significant impacts on direct-hire earnings and employment in the second post-assignment year, whereas temporary help placements have no statistically significant effect on employment and earnings in either direct-hire or temporary agency jobs over this horizon. Thus, the positive short-term benefits of temporary help placements displayed in Table 3 derive entirely from increased employment in the temporary help sector; we find no evidence that temporary agency placements help workers transition to direct-hire jobs.

To further explore the dynamics of job placement and job holding, we also examine how job placement type affects Work First program recidivism. Using Work First administrative data, we implement a variant of Equation (2) where the dependent variable is an indicator variable equal to one if a participant returns to Work First within 360 or 720 days of the commencement of the prior spell. 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. Table 6 shows that participants who obtain jobs during their Work First spells are substantially less likely to recidivate within a year or two. Those taking direct-hire jobs are 12 and 11 percentage points less likely to recidivate over one and two years, respectively (33 and 22 percent less than average). Those taking temporary help jobs are 7 and 5 percentage points (19 and 10 percent) less likely to recidivate over one and two years. These OLS models are unlikely to reveal causal relationships.

When we estimate the recidivism models using Work First random assignments as instruments for job attainment, we find that only direct-hire jobs reduce the probability of recidivism. Point estimates for temporary help jobs are positive, indicating a higher probability of recidivism, but neither is significant. However, we can readily reject the null hypothesis that the effects of direct-hire and temporary help job placements on two-year recidivism are equivalent. Thus, consistent with the findings pertaining to employment and earnings, only direct-hire placements appear to help participants reduce program recidivism, presumably because they most likely to lead to stable employment.[24]

## 4. Bad jobs or bad contractors?

A potential objection to the interpretation of our core results is that they may conflate the effect of contractor quality with the effect of job type. 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 the sake of 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

[24] A key question that our data do not yet allow us to answer is whether Work First job placements fostered by random assignments reduce state welfare payments. In future work, we will obtain linked welfare payment data from the state of Michigan to analyze the fiscal impacts of Work First job placements.

other quality differentials. The above scenario violates this assumption since it implies that $E(\nu_{ct}P_{ct}^{T})<0$ or $E(\nu_{ct}P_{ct}^{D})>0$ (or both).

We view the "bad contractor" scenario as improbable. Based on a survey of Work First contractors serving this metropolitan area (Autor and Houseman 2005), we document that program funding is tight and few resources are spent on anything but job development. A standardized program of general or life skills training is provided in the first week of the program at all contractors. After the first week, all contractors focus on job placement. Support services intended to aid job retention, such as childcare and transportation, are equally available to participants from all contractors and are provided outside the program. It also bears emphasis that direct-hire and temporary agency job placement rates are positively and significantly correlated across contractors, implying that contractors with high job placement rates tend to be strong on both placement margins; this fact reduces the plausibility of a scenario in which "bad" contractors primarily place participants in temp agency jobs and "good" contractors primarily place participants in direct-hire jobs.[25] Nevertheless, we believe the bad contractor concern deserves close scrutiny and so provide two formal checks on it below.

**a. Exploiting the 12 experiments to gauge the consistency of the estimates**

A first test is to reestimate our main models separately for each of the 12 randomization districts in our sample. If the aggregate results are driven by outlying contractors or aberrant randomization districts, these models will reveal this fact. Appendix Table 2a presents OLS and 2SLS models by district for the two-way contrast between employment and non-employment. As is consistent with the pooled, district estimates in Table 3, eight of 12 2SLS point estimates for the effect of job placements on earnings are positive and five are statistically significant. Of the three negative point estimates, none is statistically significant (though one is marginally so). Similarly, 11 of 12 2SLS estimates for the effects of job placement on quarters of employment are positive and eight are statistically significant.

In Appendix Table 2b, we provide estimates for the contrast between direct-hire employment, temporary help employment and non-employment. These estimates use the sub-sample of districts (7 of 12) where participants were randomly assigned among three contractors during at least some part of the three-year sample window. The results, summarized in Figure 1, provide consistent support for the main inferences. In five of seven randomization districts, the point estimate for the effect of temporary help placements on four-quarter earnings is substantially less positive (or more negative) than for direct-hire placements (by at least $2,000), and three of these five contrasts are significant.[26] Similarly, the estimated effect of direct-hire placements on four-quarter employment exceeds that of temporary help placements in six of seven districts, and three of these contrasts are statistically significant. These disaggregated estimates confirm that our core findings reflect a robust and pervasive feature of the data.

**b. A test of contractor heterogeneity**

As noted above, a survey of contractors failed to uncover systematic differences in contractor practices aside from differences in their job placement rates. Here, we provide a formal test of the existence of other differences in contractor practices that affect participant outcomes. Referring to equation (3), the reduced form version of our main estimating equation, the presence of sizable contractor heterogeneity in earnings or employment outcomes (large $\sigma_{\nu}^{2}$) indicates that contractors have substantial impacts on Work First participants that are

[25] The correlation between contractor-by-year temporary help and direct-hire placement rates is 0.241 ( $p = .02$ ). A regression of direct-hire placement rates on temporary help placement rates, year dummies, and a constant yields a coefficient on the temporary help placement rate variable of 0.389 ( $p = .01$ ).

[26] One countervailing contrast is also significant at $p = .05$.

independent of their placement practices. While not intrinsically a problem for our identification strategy, this finding would suggest that our statistical model, focused on job placements, provides a limited empirical characterization of how contractors affect participant outcomes. Moreover, if these other contractor effects were correlated with placement practices, this would cause us to (at least partly) misattribute the consequences of other contractor practices to job placement practices. By contrast, a small (or insignificant) value of $\sigma_{\nu}^{2}$ indicates that placement rates for a temporary help, direct-hire, or no job capture the entire effect that contractors have on participant outcomes.

We test the magnitude of $\sigma_{\nu}^{2}$ by first estimating equation (3) by OLS and retaining the residuals. We next re-estimate Equation (3), replacing $\hat{P}_{ct}^{T}$ and $\hat{P}_{ct}^{D}$ with a complete set of contractor-by-year dummies, also retaining the residuals. We then test for the significance of $\sigma_{\nu}^{2}$ by using a conventional F-test to evaluate whether the unrestricted model, containing the 59 contractor-by-year dummy variables, has significantly more explanatory power for participant outcomes than the restricted model in which these dummies are parameterized using only two measures, $\hat{P}_{ct}^{T}$ and $\hat{P}_{ct}^{D}$.[27]

These F-tests yield a surprisingly strong result. For both participant outcomes (four-quarter earnings and four-quarter employment), we accept at the 16 percent level or better the null hypothesis that the 59 contractor-by-year dummy variables have <u>no additional explanatory power</u> for participant outcomes beyond simple mean contractor-by-year job placement rates, $\hat{P}_{ct}^{T}$ and $\hat{P}_{ct}^{D}$. We therefore can reject the possibility that there is any significant, non-placement-

[27] There are 100 contractor-by-year cells and 40 district-by-year dummy variables plus an intercept. This leaves 59 contractor-by-year dummies as instruments. The F-test of these restrictions is distributed F(J-M, N-J), where N is the total count of observations, J is the number of parameters in the unrestricted model, and J-M is the number of parameters in the restricted model.

related effect of contractors on participant outcomes.[28] This finding demonstrates that we are not misattributing the effects of other contractor policies to contractor job placement rates unless these other contractor policies are virtually collinear with job placement rates – a possibility that we view as highly unlikely.

## 5. Interpreting the parameter estimates

### a. Instrumental variables fixed-effects estimates

One complication for the interpretation of our estimates is that contractors may exercise discretion about which of their randomly assigned participants to encourage toward temporary help and direct-hire jobs. For example, some contractors may encourage the most "work-ready" participants to obtain temporary help jobs and others the least work-ready. If contractors follow substantially different practices regarding the types of individuals they refer to temporary help and direct-hire jobs and if treatment effects are strongly heterogeneous within the Work First population (i.e., a particular job type has quite different effects on different individuals), our estimates, while still unbiased, will identify an unknown interaction of job placements and unobserved worker quality.[29]

To address this concern, we ideally would repeatedly randomize job placements for the same Work First participants to form within-person, cross job-type (temporary help, direct-hire, none) contrasts in employment outcomes. Fortuitously, an exercise akin to this is feasible in our sample. Using the panel structure of the research database, we can estimate fixed-effects versions of our main 2SLS models using the sub-sample of participants who experience multiple Work First spells during the sample window and who are assigned to multiple contractors (because of

[28] By contrast, when placement rates are parameterized using a single placement measure that does not distinguish between temporary help and direct-hire jobs ( $\overline{E}_{ct} = \overline{T}_{ct} + \overline{D}_{ct}$ ), the F-test rejects the null at the 7 percent level for both outcome measures.

the repeated randomization). Because of the inclusion of individual fixed effects, these models identify the coefficients of interest using only within-person, over-time variation in outcomes for participants randomly assigned to contractors with differing placement practices.[30]

Fixed effects estimates of the causal effects of job placements on employment and earnings, shown in Table 7, are remarkably similar to 2SLS estimates that do not include fixed effects. In fact, the most notable difference is that inclusion of fixed effects enhances the precision of the estimates. Consistent with our main estimates in Tables 3 and 4, pooled and fixed-effects 2SLS models estimated on the multiple-spell sample indicate that direct-hire placements raise four-quarter earnings and employment substantially—by $2,301 and 0.75 quarters respectively in the fixed-effects 2SLS models—while temporary help placements have consistently negative (though typically insignificant) impacts on both outcomes.[31] The close comparability of the pooled and fixed-effects 2SLS estimates indicates that our main estimates accurately portray the person-level effects of job placements on post-assignment earnings and employment.

For comparison with prior fixed-effects estimates of the effect of temporary help jobs on the earnings of low-skilled workers (e.g., Segal and Sullivan 1997, 1998; Ferber and Waldfogel 1998; Corcoran and Chen 2004), we also present in Table 7 a set of OLS (i.e., non-instrumental variables) models estimated both including and excluding fixed effects. Inclusion of individual fixed-effects in the OLS models reduces the estimated earnings and employment consequences

[29] The estimates will identify a stable "intention to treat"relationship if treatment effects are homogenous or if random assignments uniformly raise or lower the probability that each participant obtains a given job placement (temporary help, direct-hire, non-employment).

[30] The primary sample has 38,689 spells experienced by 25,802 participants. The sample of participants with multiple spells includes 8,418 participants and 21,305 spells. In this multiple-spell sample, 6,053 participants (16,024 spells) are randomly assigned to two or more distinct contractors. An unattractive feature of the fixed-effects approach is that we are forced to limit the sample to participants who experience multiple Work First spells during the sample window, which is a form of selection on the outcome variable. For this reason, we do not use fixed-effects models for our primary estimates.

[31] In this case, the estimated four-quarter earnings loss of $1,265 for a temporary help placement is statistically significant.

of direct-hire and temporary help placements by about half—from approximately $2,000 to $1,000 for four-quarter earnings, and from approximately 0.90 to 0.45 quarters for four-quarter employment. Yet, even the OLS fixed-effects models show a significant, positive effect of temporary help jobs on post-assignment earnings and employment, indicating that fixed-effect models are inadequate for obtaining unbiased estimates of the consequences of job placements on labor market outcomes.

**b. Who is the marginal worker?**

The preceding analysis establishes that our estimates capture the effects of job placements on individual worker outcomes. In this final section, we explore the characteristics of the "marginal" temporary agency and direct-hire workers for whom these outcomes are estimated. While it is not possible to individually identify the participants whose employment outcomes are directly altered by the quasi-experiment—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.

Consider the following regression model:

$$1[D_i + T_i = 1] \bullet X_{icdt} = \alpha + \kappa_1 T_i + \kappa_2 D_i + \gamma_d + \theta_t + (\gamma_d \times \theta_t) + \varsigma_{icdt}. \quad (4)$$

Here, $X$ is a predetermined participant characteristic of interest (e.g., pre-assignment earnings or employment), $1[\bullet]$ is the indicator function, and $D$ and $T$ are dummy variables indicating whether participant $i$ obtained a direct-hire job or a 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 employment during the Work First spell and zero otherwise.

If Equation (4) is fit using OLS, the parameters $\hat{\kappa}_1$ and $\hat{\kappa}_2$ 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 Equation (4) in column (1) of Table 8 show that participants who found any employment during their Work First spell earned an average of $4,685 and worked 2.15 quarters in the four calendar quarters <u>prior to</u> 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 one 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 previous four quarters than those who took direct-hire placements (and lower direct-hire earnings and employment by approximately offsetting amounts).

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, $\hat{\kappa}_1$ and $\hat{\kappa}_2$ 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), and in particular that random assignment to treatment ( $Z = 1$ ) weakly increases the odds that each participant obtains employment during her Work First spell. In this case, a Wald estimate of Equation (4) yields the following quantity:

(5) $$\hat{\kappa}_{wald} = \frac{E[X \mid J = 1, Z = 1] \bullet E[J \mid Z = 1] - E[X \mid J = 1, Z = 0] \bullet E[J \mid Z = 0]}{E[J \mid Z = 1] - E[J \mid Z = 0]}.$$

The numerator of this expression is a scaled contrast between the average $X$ of employed participants in the treatment and control groups. The denominator rescales this contrast by the effect of the random assignment on employment odds. The ratio of these two expressions provides an estimate of the average $X$ of marginal workers—workers whose employment status was changed by the random assignment.[32]

Two-stage least squares estimates of Equation (4), found in columns (3) and (4) of Table 8, establish two key results. First, the earnings histories of "marginal workers" are significantly weaker than those of average workers. Specifically, prior-year earnings of marginal workers are about $500 below those of average workers while prior-year labor force participation is lower by between 0.05 and 0.15 quarters. Hausman tests for the equality of OLS and 2SLS coefficients (bottom row of each panel) confirm that these differences in earnings histories are statistically significant. It thus appears that random assignments alter employment outcomes among Work First participants by moving those with relatively weak earnings histories into or out of the labor force, which appears eminently sensible.

The second result established in Table 8 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 significantly from one another.[33] (Although, notably, prior earnings and employment in the temporary help sector are higher among marginal temporary help workers

[32] 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 Work First spells. Also assume that 70 percent of treated participants who find jobs are high school dropouts versus 50 percent of untreated participants. If one uses Equation (4), these numbers imply that 90 percent of marginally employed are high school dropouts. The intuition for this result is that the marginal 10 percent of employed participants must be 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.

[33] Estimates akin to those in Table 8 for the demographic characteristics of marginal versus average workers (available from the authors), indicate that "marginal temps" are slightly more likely than average temporary workers or marginal direct-

than among marginal direct-hire workers, and this contrast approaches statistical significance.) The near comparability of the marginal temporary help and direct-hire populations is important 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 similar populations. We therefore conclude that "marginal temporary help workers" in our sample would likely have fared significantly better had they instead been randomized into direct-hire jobs, and vice versa for "marginal direct hires."[34]

## 6. Conclusion

The primary finding of our analysis is that direct-hire placements induced by the random assignment of low-skilled workers to Work First contractors significantly increase payroll earnings and quarters of employment for marginal participants—by several thousand dollars over the subsequent two years. This effect of direct-hire placements on post-assignment earnings and employment is significant, consistent across randomization districts, and economically large. By contrast, we find that although temporary help placements increase participants' earnings over the near term, these placements do not raise—and may 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 many permutations of sampled districts and post-assignment time intervals in our data. They are corroborated by evidence from Work First administrative records that marginal temporary help placements, unlike marginal direct-hire placements, do not lower and possibly raise Work First recidivism.

hires to be female or non-white. These contrasts are statistically significant but economically small – no larger than 5 percentage points.

[34] If instead the two marginal populations were disjointed, 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 direct-hire jobs, and vice versa.

Why do 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. Because the short-term earnings gains from temporary help jobs are offset over time by forgone earnings in direct-hire employment, it appears that temporary help placements primarily serve to displace future direct-hire employment rather than to help workers transition to direct-hire jobs. Moreover, although never statistically significant, 2SLS coefficient estimates of the effects of temporary help placements on earnings over the subsequent one to two year period are always negative—and estimated effects on Work-First recidivism always positive—suggesting that in some cases participants may be better off passing up temporary help positions and continuing to search for better, possibly direct-hire, jobs.

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 temporary help placements generate significant benefits. 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 can improve participants' labor market outcomes by placing more clients in temporary agency positions. Our analysis suggests not. 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.

Finally, our research 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 results for low-skilled U.S. workers apply generally to these disparate labor markets and worker populations, it is notable that comparable non-experimental methodologies applied to the same empirical question in the United States and Europe produce comparable findings—namely, that temporary help jobs foster positive labor market outcomes. Notably, we also obtain results comparable to these prior studies when we apply conventional OLS and fixed-effects models to our data. Our quasi-experimental evidence suggests that these non-experimental methods may be inadequate to resolve the endemic self-selection of workers into job types according to unmeasured skills and motivation. We conclude 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.

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**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 | | 52.8 | | 37.6 | | 9.6 | |
| | | | | *A. Demographics* | | | | |
| Age | 29.6 | (0.41) | 29.3 | (0.06) | 29.8 | (0.06) | 30.3 | (0.13) |
| Female (%) | 94.3 | (0.12) | 94.6 | (0.16) | 93.9 | (0.20) | 94.0 | (0.39) |
| Black (%) | 97.3 | (0.08) | 97.2 | (0.12) | 97.1 | (0.14) | 98.2 | (0.22) |
| White/Other (%) | 2.7 | (0.08) | 2.8 | (0.12) | 2.9 | (0.14) | 1.8 | (0.22) |
| < High school (%) | 36.8 | (0.25) | 39.4 | (0.34) | 33.6 | (0.39) | 35.1 | (0.78) |
| High school (%) | 35.3 | (0.24) | 33.5 | (0.33) | 37.3 | (0.40) | 37.6 | (0.80) |
| > High school (%) | 7.6 | (0.13) | 7.0 | (0.18) | 8.5 | (0.23) | 7.8 | (0.44) |
| Unknown (%) | 20.2 | (0.20) | 20.1 | (0.28) | 20.6 | (0.34) | 19.5 | (0.65) |
| | *B. Work History in Four Quarters Prior to Contractor Assignment* | | | | | | | |
| Wage earnings | 4,210 | (31) | 3,781 | (41) | 4,682 | (52) | 4,723 | (100) |
| Qtrs employed | 1.98 | (0.01) | 1.84 | (0.01) | 2.14 | (0.01) | 2.18 | (0.02) |
| Direct hire earnings | 3,520 | (30) | 3,170 | (39) | 3,996 | (50) | 3,578 | (94) |
| Qtrs direct hire employment | 1.51 | (0.01) | 1.39 | (0.01) | 1.68 | (0.01) | 1.48 | (0.02) |
| Temp help earnings | 515 | (10) | 457 | (12) | 487 | (15) | 940 | (42) |
| Qtrs temp employment | 0.37 | (0.00) | 0.35 | (0.01) | 0.34 | (0.01) | 0.57 | (0.02) |
| | *C. Job Placement Outcomes during Work First Assignment (if employed)* | | | | | | | |
| Hourly wage | 7.53 | (0.01) | n/a | | 7.45 | (0.02) | 7.84 | (0.03) |
| Weekly hours | 34.2 | (0.05) | n/a | | 33.5 | (0.06) | 36.7 | (0.10) |
| Weekly earnings | 260 | (0.70) | n/a | | 253 | (0.80) | 287 | (1.38) |
| | *D. Labor Market Outcomes in Four Quarters Following Contractor Assignment* | | | | | | | |
| Wage earnings | 4,203 | (30) | 2,860 | (35) | 5,759 | (53) | 5,497 | (104) |
| Qtrs employed | 1.96 | (0.01) | 1.50 | (0.01) | 2.47 | (0.01) | 2.48 | (0.02) |
| Direct hire earnings | 3,425 | (28) | 2,313 | (33) | 5,019 | (51) | 3,303 | (89) |
| Qtrs direct hire employment | 1.48 | (0.01) | 1.09 | (0.01) | 2.05 | (0.01) | 1.34 | (0.02) |
| Temp help earnings | 550 | (11) | 371 | (11) | 450 | (16) | 1,935 | (63) |
| Qtrs temp employment | 0.34 | (0.00) | 0.28 | (0.00) | 0.26 | (0.01) | 0.99 | (0.02) |
| Work First reentry: | | | | | | | | |
| 360 days (%) | 36.1 | (0.24) | 41.5 | (0.34) | 29.2 | (0.38) | 33.9 | (0.78) |
| 720 days (%) | 51.4 | (0.25) | 56.0 | (0.35) | 45.3 | (0.41) | 50.2 | (0.82) |
| N | 38,689 | | 20,437 | | 14,544 | | 3,708 | |

Sample: All Work First spells initiated from the fourth quarter of 1999 through the first quarter of 2003 in 12 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 Chi-Square Tests of Random Assignment of Participant Demographic Characteristics across Work First Contractors with Randomization Districts, 1999–2000**

| | Randomization District | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | I | II | III | IV | V | VI | VII | VIII | IX | X | XI | XII | All |
| 1999–2000 | | | | | | | | | | | | | |
| $\chi^2$ | 5.0 | 35.1 | 9.2 | 13.2 | | 14.5 | 5.9 | | 6.6 | 7.6 | 9.2 | 9.0 | 115.2 |
| DF | 10 | 20 | 10 | 10 | | 10 | 10 | | 10 | 10 | 10 | 10 | 110 |
| P-value | 0.89 | 0.02 | 0.52 | 0.21 | | 0.15 | 0.82 | | 0.76 | 0.67 | 0.51 | 0.53 | 0.35 |
| N | 1,952 | 755 | 721 | 1,425 | n/a | 963 | 822 | n/a | 748 | 844 | 713 | 720 | 9,663 |
| 2000–2001 | | | | | | | | | | | | | |
| $\chi^2$ | 19.9 | 25.7 | 29.4 | 10.3 | | 27.0 | 22.4 | | 14.6 | 8.2 | 8.5 | 21.1 | 187.1 |
| DF | 20 | 20 | 20 | 10 | | 20 | 20 | | 9 | 10 | 10 | 20 | 159 |
| P-value | 0.47 | 0.17 | 0.08 | 0.41 | | 0.14 | 0.32 | | 0.10 | 0.61 | 0.58 | 0.39 | 0.06 |
| N | 1,554 | 1,474 | 505 | 1,405 | n/a | 974 | 692 | n/a | 160 | 900 | 558 | 1,590 | 9,812 |
| 2001–2002 | | | | | | | | | | | | | |
| $\chi^2$ | 19.7 | 22.9 | 23.6 | 10.0 | 20.5 | 6.6 | 9.2 | 11.4 | | 7.4 | 11.8 | 16.5 | 159.8 |
| DF | 20 | 20 | 20 | 10 | 20 | 10 | 10 | 10 | | 10 | 10 | 20 | 160 |
| P-value | 0.48 | 0.29 | 0.26 | 0.44 | 0.43 | 0.76 | 0.51 | 0.33 | | 0.68 | 0.30 | 0.68 | 0.49 |
| N | 2,093 | 1,651 | 1,051 | 1,436 | 994 | 970 | 939 | 1,166 | n/a | 822 | 453 | 1,693 | 13,268 |
| 2002–2003 | | | | | | | | | | | | | |
| $\chi^2$ | 22.0 | 11.3 | 8.8 | 8.7 | 9.7 | 21.6 | 14.6 | 10.5 | | 11.5 | | 20.7 | 139.6 |
| DF | 20 | 20 | 10 | 10 | 20 | 20 | 18 | 10 | | 10 | | 20 | 158 |
| P-value | 0.34 | 0.94 | 0.55 | 0.56 | 0.97 | 0.36 | 0.69 | 0.39 | | 0.32 | | 0.41 | 0.85 |
| N | 775 | 649 | 337 | 724 | 673 | 437 | 513 | 394 | n/a | 431 | n/a | 1,013 | 5,946 |
| All Years | | | | | | | | | | | | | |
| $\chi^2$ | 66.6 | 95.0 | 71.0 | 42.2 | 30.2 | 69.6 | 52.1 | 22.0 | 21.3 | 34.7 | 29.5 | 67.3 | 601.6 |
| DF | 70 | 80 | 60 | 40 | 40 | 60 | 58 | 20 | 19 | 40 | 30 | 70 | 587 |
| P-value | 0.59 | 0.12 | 0.16 | 0.37 | 0.87 | 0.19 | 0.69 | 0.34 | 0.32 | 0.71 | 0.49 | 0.57 | 0.33 |
| N | 6,374 | 4,529 | 2,614 | 4,990 | 1,667 | 3,344 | 2,966 | 1,560 | 908 | 2,997 | 1,724 | 5,016 | 38,689 |

Each cell provides the chi-squared value, degrees of freedom, p-value and number of observations for the null hypothesis that the 10 main sample covariates are balanced across clients assigned to Work First contractors within the relevant randomization district × year cell. Covariates tested are gender, white race, other race, age, primary school education, post-primary high school dropout education, total quarters employed and total employment earnings in eight quarters prior to Work First assignment, total quarters employed in temporary help agency work and total temporary help agency earnings in eight quarters prior to Work First assignment. Far column and bottom row provide analogous test statistics pooling across districts either within a year or across years within a district. Bottom right-hand cell provides the chi-square test for all districts and years simultaneously. Cells marked "n/a" indicate that there was only one contractor operating in the district during most or all of the indicated year.

**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) | (1) | (2) | (3) | (4) |
| | | | | *First Quarter* | | | | |
| Any job | 789 | | 569 | | 0.36 | | 0.28 | |
| | (16) | | (100) | | (0.01) | | (0.04) | |
| Temp agency job | | 731 | | 460 | | 0.38 | | 0.12 |
| | | (38) | | (206) | | (0.01) | | (0.07) |
| Direct-hire job | | 803 | | 622 | | 0.35 | | 0.36 |
| | | (18) | | (127) | | (0.01) | | (0.06) |
| $R^2$ | 0.19 | 0.19 | | | 0.20 | 0.20 | | |
| $H_0$: Temp = Direct | | 0.09 | | 0.53 | | 0.03 | | 0.02 |
| | | | | *Quarters 2–4* | | | | |
| Any job | 1,686 | | 783 | | 0.53 | | 0.35 | |
| | (53) | | (266) | | (0.02) | | (0.08) | |
| Temp agency job | | 1,493 | | −765 | | 0.49 | | −0.04 |
| | | (108) | | (647) | | (0.03) | | (0.15) |
| Direct-hire job | | 1,735 | | 1,542 | | 0.54 | | 0.54 |
| | | (56) | | (352) | | (0.02) | | (0.08) |
| $R^2$ | 0.20 | 0.20 | | | 0.16 | 0.16 | | |
| $H_0$: Temp = Direct | | 0.04 | | 0.01 | | 0.10 | | 0.00 |
| | | | | *Quarters 1–4* | | | | |
| Any job | 2,475 | | 1,352 | | 0.88 | | 0.63 | |
| | (64) | | (349) | | (0.02) | | (0.11) | |
| Temp agency job | | 2,224 | | −306 | | 0.87 | | 0.08 |
| | | (134) | | (816) | | (0.03) | | (0.20) |
| Direct-hire job | | 2,537 | | 2,164 | | 0.89 | | 0.90 |
| | | (68) | | (456) | | (0.02) | | (0.13) |
| $R^2$ | 0.22 | 0.22 | | | 0.21 | 0.21 | | |
| $H_0$: Temp = Direct | | 0.03 | | 0.02 | | 0.60 | | 0.00 |

N = 38,689. 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 and UI quarters worked 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) | (1) | (2) | (3) | (4) |
| | | | | | *Quarters 1–4* | | | |
| Any job | 2,360 | | 1,320 | | 0.84 | | 0.54 | |
| | (73) | | (532) | | (0.03) | | (0.13) | |
| Temp agency job | | 2,031 | | −1,059 | | 0.82 | | 0.01 |
| | | (145) | | (1,010) | | (0.03) | | (0.25) |
| Direct-hire job | | 2,447 | | 3,053 | | 0.84 | | 0.93 |
| | | (77) | | (669) | | (0.03) | | (0.17) |
| $R^2$ | 0.23 | 0.23 | | | 0.21 | 0.21 | | |
| $H_0$: Temp = Direct | | 0.01 | | 0.00 | | 0.50 | | 0.01 |
| | | | | | *Quarters 5–8* | | | |
| Any job | 1,686 | | 1,470 | | 0.44 | | 0.29 | |
| | (73) | | (511) | | (0.02) | | (0.13) | |
| Temp agency job | | 1,372 | | −1,117 | | 0.37 | | −0.17 |
| | | (140) | | (1,179) | | (0.03) | | (0.23) |
| Direct-hire job | | 1,765 | | 3,354 | | 0.45 | | 0.62 |
| | | (85) | | (835) | | (0.02) | | (0.19) |
| $R^2$ | 0.18 | 0.18 | | | 0.14 | 0.14 | | |
| $H_0$: Temp = Direct | | 0.19 | | 0.01 | | 0.02 | | 0.01 |
| | | | | | *Quarters 1–8* | | | |
| Any job | 4,046 | | 2,790 | | 1.28 | | 0.83 | |
| | (128) | | (986) | | (0.04) | | (0.23) | |
| Temp agency job | | 3,385 | | −2,176 | | 1.19 | | −0.16 |
| | | (263) | | (2,086) | | (0.06) | | (0.46) |
| Direct-hire job | | 4,212 | | 6,407 | | 1.30 | | 1.56 |
| | | (143) | | (1412) | | (0.04) | | (0.33) |
| $R^2$ | 0.24 | 0.24 | | | 0.21 | 0.21 | | |
| $H_0$: Temp = Direct | | 0.01 | | 0.00 | | 0.06 | | 0.01 |

N = 27,029. 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 and UI quarters worked 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 vs. 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 | 542 | | 841 | | 0.13 | | 0.51 | |
| | (218) | | (242) | | (0.07) | | (0.09) | |
| Temp agency job | | 999 | | −1,486 | | 0.48 | | −0.48 |
| | | (467) | | (526) | | (0.16) | | (0.13) |
| Direct-hire job | | 319 | | 1,981 | | −0.04 | | 0.99 |
| | | (223) | | (379) | | (0.08) | | (0.10) |
| $H_0$: Temp = Direct | | 0.19 | | 0.00 | | 0.01 | | 0.00 |
| N | 38,689 | | | | | | | |
| *Quarters 1–4: Participants assigned 1999–2002* | | | | | | | | |
| Any job | 549 | | 686 | | 0.16 | | 0.36 | |
| | (319) | | (353) | | (0.10) | | (0.12) | |
| Temp agency job | | 812 | | −2,158 | | 0.39 | | −0.54 |
| | | (607) | | (582) | | (0.21) | | (0.15) |
| Direct-hire job | | 357 | | 2,757 | | −0.01 | | 1.01 |
| | | (343) | | (488) | | (0.13) | | (0.13) |
| $H_0$: Temp = Direct | | 0.53 | | 0.00 | | 0.17 | | 0.00 |
| N | 27,029 | | | | | | | |
| *Quarters 5–8: Participants assigned 1999–2002* | | | | | | | | |
| Any job | −99 | | 1,680 | | −0.10 | | 0.42 | |
| | (162) | | (512) | | (0.06) | | (0.11) | |
| Temp agency job | | 79 | | −1,234 | | −0.02 | | −0.23 |
| | | (279) | | (1002) | | (0.12) | | (0.18) |
| Direct-hire job | | −228 | | 3,803 | | −0.16 | | 0.90 |
| | | (216) | | (835) | | (0.09) | | (0.18) |
| $H_0$: Temp = Direct | | 0.41 | | 0.00 | | 0.39 | | 0.00 |
| N | 27,029 | | | | | | | |

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 and UI quarters worked 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 | −0.11 | | −0.01 | |
| | (0.01) | | (0.04) | |
| Temp agency job | | −0.07 | | 0.04 |
| | | (0.01) | | (0.08) |
| Direct-hire job | | −0.12 | | −0.03 |
| | | (0.01) | | (0.05) |
| $R^2$ | 0.03 | 0.04 | | |
| $H_0$: Temp = Direct | | 0.00 | | 0.47 |
| Number of observations | 38,689 | | | |
| | *Return within 720 days of assignment* | | | |
| Any job | −0.10 | | −0.07 | |
| | (0.01) | | (0.06) | |
| Temp agency job | | −0.05 | | 0.10 |
| | | (0.01) | | (0.08) |
| Direct-hire job | | −0.11 | | −0.20 |
| | | (0.01) | | (0.08) |
| $R^2$ | 0.05 | 0.05 | | |
| $H_0$: Temp = Direct | | 0.00 | | 0.01 |
| N | 27,029 | | | |

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 and UI quarters worked 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 Placement Models on Earnings and Employment in First Year Following Work First Assignment**

| | OLS | | | | 2SLS | | | |
|---|---|---|---|---|---|---|---|---|
| | Pooled | | Fixed-Effects | | Pooled | | Fixed-Effects | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | *A. Earnings: Quarters 1–4* | | | | | | | |
| Any job | 2,249 | | 1,239 | | 922 | | 848 | |
| | (88) | | (81) | | (421) | | (398) | |
| Temp agency job | | 2,048 | | 968 | | −367 | | −1,265 |
| | | (160) | | (126) | | (567) | | (515) |
| Direct-hire job | | 2,305 | | 1,319 | | 1,794 | | 2,301 |
| | | (93) | | (84) | | (552) | | (483) |
| $R^2$ | 0.07 | 0.07 | 0.73 | 0.73 | | | | |
| $H_0$: Temp = Direct | | 0.12 | | 0.01 | | 0.01 | | 0.00 |
| | *B. Quarters Employed: Quarters 1–4* | | | | | | | |
| Any job | 0.95 | | 0.48 | | 0.38 | | 0.41 | |
| | (0.03) | | (0.02) | | (0.13) | | (0.12) | |
| Temp agency job | | 0.92 | | 0.43 | | 0.02 | | −0.10 |
| | | (0.04) | | (0.04) | | (0.18) | | (0.15) |
| Direct-hire job | | 0.96 | | 0.50 | | 0.62 | | 0.75 |
| | | (0.03) | | (0.03) | | (0.16) | | (0.15) |
| $R^2$ | 0.12 | 0.12 | 0.71 | 0.71 | | | | |
| $H_0$: Temp = Direct | | 0.33 | | 0.10 | | 0.02 | | 0.00 |
| N | 16,024 spells (6,053 participants) | | | | | | | |

Robust standard errors in parentheses are clustered on Work First contractor assignment × year. All models include individual participant fixed effects, 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. Models for the Average and Marginal Characteristics of Participants Obtaining Temporary Help and Direct-Hire Jobs during their Work First Spells**

| | A. Earnings History | | | | B. Employment History | | | |
|---|---|---|---|---|---|---|---|---|
| | OLS | | 2SLS | | OLS | | 2SLS | |
| | (1) | (2) | (3) | (4) | (1) | (2) | (3) | (4) |
| | *Earnings in Prior Year* | | | | *Quarters Worked in Prior Year* | | | |
| Any job | 4,685 | | 4,078 | | 2.15 | | 1.99 | |
| | (85) | | (416) | | (0.02) | | (0.07) | |
| Temp agency job | | 4,712 | | 4,284 | | 2.18 | | 2.14 |
| | | (134) | | (614) | | (0.03) | | (0.12) |
| Direct-hire job | | 4,678 | | 3,978 | | 2.14 | | 1.91 |
| | | (88) | | (548) | | (0.02) | | (0.10) |
| $R^2$ | 0.24 | 0.24 | | | 0.52 | 0.52 | | |
| $H_0$: Temp = Direct | | 0.79 | | 0.71 | | 0.27 | | 0.17 |
| $H_0$: $B_{OLS} = B_{2SLS}$ | | | 0.03 | 0.10 | | | 0.02 | 0.03 |
| | *Temporary Help Earnings in Prior Year* | | | | *Temporary Help Quarters in Prior Year* | | | |
| Any job | 583 | | 396 | | 0.39 | | 0.30 | |
| | (17) | | (86) | | (0.01) | | (0.03) | |
| Temp agency job | | 943 | | 622 | | 0.57 | | 0.38 |
| | | (44) | | (178) | | (0.02) | | (0.08) |
| Direct-hire job | | 492 | | 286 | | 0.34 | | 0.26 |
| | | (17) | | (116) | | (0.01) | | (0.05) |
| $R^2$ | 0.05 | 0.05 | | | 0.11 | 0.12 | | |
| $H_0$: Temp = Direct | | 0.00 | | 0.16 | | 0.00 | | 0.31 |
| $H_0$: $B_{OLS} = B_{2SLS}$ | | | 0.04 | 0.02 | | | 0.01 | 0.00 |

N=38,689. 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).

**Appendix Table 1. 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 | 2,580 | | 1,327 | | 0.86 | | 0.62 | |
| | (80) | | (370) | | (0.03) | | (0.11) | |
| Temp agency job | | 2,327 | | −906 | | 0.84 | | −0.17 |
| | | (165) | | (1,022) | | (0.37) | | (0.24) |
| Direct-hire job | | 2,642 | | 2,417 | | 0.86 | | 1.00 |
| | | (86) | | (592) | | (0.03) | | (0.17) |
| $R^2$ | 0.22 | 0.22 | | | 0.21 | 0.21 | | |
| $H_0$: Temp = Direct | | 0.08 | | 0.02 | | 0.56 | | 0.00 |
| N | 25,802 | | | | | | | |

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 and UI quarters worked 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 2a. 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 | | | B. Quarters Worked | |
|---|---|---|---|---|---|
| | OLS | 2SLS | | OLS | 2SLS |
| I | 2,523 | 1,067 | | 0.79 | 0.28 |
| | (181) | (658) | | (0.07) | (0.13) |
| | | | 6,374 | | |
| II | 2,587 | 1,024 | | 0.84 | 0.40 |
| | (172) | (437) | | (0.05) | (0.15) |
| | | | 4,529 | | |
| III | 2,836 | -209 | | 1.08 | 0.81 |
| | (302) | (959) | | (0.12) | (0.32) |
| | | | 2,614 | | |
| IV | 2,429 | 1,430 | | 0.83 | 0.17 |
| | (214) | (708) | | (0.05) | (0.23) |
| | | | 4,990 | | |
| V | 2,371 | -65 | | 0.93 | 1.16 |
| | (305) | (2,762) | | (0.09) | (1.08) |
| | | | 1,667 | | |
| VI | 2,408 | 957 | | 0.92 | 0.40 |
| | (235) | (219) | | (0.09) | (0.23) |
| | | | 3,344 | | |
| VII | 2,611 | 1,885 | | 1.01 | 1.20 |
| | (172) | (684) | | (0.06) | (0.19) |
| | | | 2,966 | | |
| VIII | 2,574 | 105 | | 1.00 | 0.19 |
| | (199) | (269) | | (0.11) | (0.04) |
| | | | 1,560 | | |
| IX | 2,071 | -3,440 | | 0.75 | -0.69 |
| | (200) | (1,849) | | (0.08) | (0.71) |
| | | | 908 | | |
| X | 2,170 | -876 | | 0.81 | 0.56 |
| | (170) | (195) | | (0.05) | (0.06) |
| | | | 2,997 | | |
| XI | 2,746 | 344 | | 1.08 | 0.99 |
| | (216) | (568) | | (0.08) | (0.19) |
| | | | 1,724 | | |
| XII | 2,290 | 3,673 | | 0.84 | 1.29 |
| | (150) | (649) | | (0.04) | (0.12) |
| | | | 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 and UI quarters worked 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 2b. 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 | | | | |
| Temp agency job | 2,375 | -3,062 | 0.83 | -0.14 |
| | (224) | (3,433) | (0.06) | (0.60) |
| Direct-hire job | 2,561 | 5,622 | 0.77 | 0.74 |
| | (193) | (3,768) | (0.07) | (0.60) |
| $H_0$: Temp = Direct | 0.39 | 0.25 | 0.06 | 0.47 |
| | | N=6,374 | | |
| II | | | | |
| Temp agency job | 1,791 | -4,323 | 0.64 | -0.50 |
| | (365) | (1,805) | (0.09) | (0.39) |
| Direct-hire job | 2,832 | 5,732 | 0.90 | 1.20 |
| | (155) | (1,633) | (0.05) | (0.33) |
| $H_0$: Temp = Direct | 0.01 | 0.01 | 0.02 | 0.02 |
| | | N=4,529 | | |
| III | | | | |
| Temp agency job | 2,560 | -2,485 | 1.11 | -0.18 |
| | (474) | (1,604) | (0.15) | (0.71) |
| Direct-hire job | 2,903 | 31 | 1.07 | 0.92 |
| | (281) | (1,096) | (0.11) | (0.30) |
| $H_0$: Temp = Direct | 0.34 | 0.13 | 0.61 | 0.11 |
| | | N=2,614 | | |
| V | | | | |
| Temp agency job | 2,423 | -21,575 | 1.11 | -5.73 |
| | (552) | (5,297) | (0.10) | (1.21) |
| Direct-hire job | 2,360 | -5,923 | 0.89 | -0.72 |
| | (267) | (1,746) | (0.09) | (0.65) |
| $H_0$: Temp = Direct | 0.87 | 0.01 | 0.09 | 0.00 |
| | | N=1,667 | | |
| VI | | | | |
| Temp agency job | 2,506 | 2,731 | 0.96 | 1.09 |
| | (405) | (0,710) | (0.09) | (0.74) |
| Direct-hire job | 2,378 | -72 | 0.91 | 0.00 |
| | (269) | (0,631) | (0.10) | (0.43) |
| $H_0$: Temp = Direct | 0.79 | 0.05 | 0.56 | 0.34 |
| | | N=3,344 | | |
| VII | | | | |
| Temp agency job | 1,883 | 4,056 | 0.97 | 0.75 |
| | (282) | (2,408) | (0.11) | (0.73) |
| Direct-hire job | 2,749 | 2,012 | 1.02 | 1.17 |
| | (229) | (604) | (0.07) | (0.23) |
| $H_0$: Temp = Direct | 0.08 | 0.34 | 0.66 | 0.45 |
| | | N=2,966 | | |
| XII | | | | |
| Temp agency job | 2,664 | 1,016 | 0.85 | 0.34 |
| | (476) | (1,850) | (0.09) | (0.38) |
| Direct-hire job | 2,202 | 4,953 | 0.84 | 1.75 |
| | (172) | (1483) | (0.04) | (0.35) |
| $H_0$: Temp = Direct | 0.41 | 0.16 | 0.83 | 0.04 |
| | | N=5,016 | | |

Notes. See Appendix Table 2a.

**Figure 1. Two-Stage Least Squares Estimates of the Differential Impact of Direct-Hire vs. Temporary Help Job Placements on UI Earnings and Quarters of Employment for Work First Clients over Four Subsequent Quarters**