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Do Temporary-Help Jobs Improve Labor Market Outcomes for Low-Skilled Workers? Evidence from "Work First"[†]

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Temporary-help jobs offer rapid entry into paid employment, but they are typically brief and it is unknown whether they foster longer term employment. We utilize the unique structure of Detroit's welfare-to-work program to identify the effect of temporary-help jobs on labor market advancement. Exploiting the rotational assignment of welfare clients to numerous nonprofit contractors with differing job placement rates, we find that temporary-help job placements do not improve and may diminish subsequent earnings and employment outcomes among participants. In contrast, job placements with direct-hire employers substantially raise earnings and employment over a seven quarter follow-up period. (*JEL* J22, J23, J24, J31, J68)

Temporary-help firms employ a disproportionate share of low-skilled and minority US workers (US Department of Labor, Bureau of Labor Statistics 2005). Within the low-wage population, employment in temporary help is especially prevalent among participants in public employment and training programs. Although the temporary-help industry accounts for less than 3 percent of average daily employment in the United States, state administrative data show that 15 to 40 percent of former welfare recipients who obtained employment in the years following the 1996 US welfare reform took jobs in the temporary-help sector.[1] Comparing the industry distribution of employment of participants in welfare, job training, and labor exchange programs in Missouri before and immediately following program participation, Heinrich, Mueser, and Troske (2007) find that participation in government programs is associated with a 50 to 100 percent increase

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[†] To comment on this article in the online discussion forum, or to view additional materials, visit the articles page at http://www.aeaweb.org/articles.php?doi=10.1257/app.2.3.96.

[1] See Autor and Houseman (2002) on Georgia and Washington state; Maria Cancian et al. (1999) on Wisconsin; Carolyn J. Heinrich, Peter R. Mueser, and Kenneth R. Troske (2005) on North Carolina and Missouri; and John Pawasarat (1997) on Wisconsin.

in employment in temporary-help firms and that no other industry displays such a spike in employment.

The concentration of low-skilled workers in the temporary-help sector and the high incidence of temporary-help employment among participants in government employment programs have catalyzed a debate as to whether temporary-help jobs facilitate or hinder labor market advancement. Lack of employment stability is the principal obstacle to economic self-sufficiency among the low-skilled population, and thus a main goal of welfare-to-work and other employment programs targeting low-skilled workers is to help participants find stable employment (Dan Bloom et al. 2005). Temporary-help jobs are typically less stable than regular ("direct-hire") jobs (Christopher T. King and Peter R. Mueser 2005). Nevertheless, by providing an opportunity to develop contacts with potential employers and acquire other types of human capital, temporary-help jobs may allow workers to transition to more stable employment than they otherwise would have attained. Moreover, because temporary-help firms face relatively low screening and termination costs, numerous researchers have posited that these firms may hire individuals who otherwise would have difficulty finding any employment, and that this may lead directly or indirectly to employment in direct-hire positions (Katharine G. Abraham 1988; Lawrence F. Katz and Alan B. Krueger 1999; Autor 2001 and 2003; Houseman 2001; Autor and Houseman 2002; Houseman, Arne J. Kalleberg, and George A. Erickcek 2003; Kalleberg, Jeremy Reynolds, and Peter V. Marsden 2003).

Some scholars and practitioners have countered that temporary-help firms primarily offer unstable and low-skilled jobs, which provide little opportunity for workers to invest in human capital or engage in productive job search (Robert E. Parker 1994; Pawasarat 1997; Helene Jorgensen and Hans Riemer 2000; Chris Benner, Laura Leete, and Manuel Pastor 2007). This argument, however, only implies that temporary-help jobs inhibit labor market advancement if these jobs displace more productive employment activities. Temporary-help jobs may nevertheless increase employment and earnings if they substitute for spells of unemployment. Thus, a central question for evaluation is whether temporary-help positions on average augment or displace other job search and human capital acquisition activities.

Because it is inherently difficult to differentiate the effects of holding given job types from the skills and motivations that cause workers to hold these jobs initially, distinguishing among these competing hypotheses is an empirical challenge. This study exploits a unique aspect of the city of Detroit's welfare-to-work program (Work First) to identify the causal effects of temporary-help and direct-hire jobs on the subsequent labor market advancement of low-skilled workers. Welfare participants in Detroit are assigned, on a rotating basis, to one of two or three not-for-profit program providers—termed contractors—operating in the district where they reside. Contractors operating in a given district have substantially different placement rates into temporary-help and direct-hire jobs, but offer otherwise standardized services. Contractor assignments, which are functionally equivalent to random assignments, are uncorrelated with participant characteristics but, due to differences in contractor placement practices, are correlated with the probability that participants are placed into a direct-hire job, a temporary-help job, or no job during their Work First spells. These program features enable us to use contractor assignments as instrumental variables for job-taking.

Our analysis draws on administrative records from the Detroit Work First program linked with unemployment insurance (UI) wage records for the State of Michigan for over 37,000 Work First spells commencing between 1999 and 2003. The administrative data provide person-level demographic information on Work First participants and the jobs they obtain during their Work First spells. The UI wage records track participants' quarterly earnings in each job held for two years before and after entering the program. Consistent with welfare populations studied in other states, the incidence of temporary-help employment in Detroit is high: one in five jobs obtained during Work First is with a temporary-help firm. This provides ample variation to simultaneously analyze the causal effects of direct-hire and of temporary-help jobs on subsequent labor market outcomes.

The analysis yields two main insights. Placements into *direct-hire jobs* significantly improve subsequent earnings and employment outcomes. Over a seven-quarter follow-up period, direct-hire placements induced by contractor assignments raise participants' payroll earnings by $493 per quarter (approximately a 50 percent increase over baseline for this low-skill population) and increase the probability of employment per quarter by 15 percentage points (about a 33 percent increase over baseline). These effects are highly statistically significant and are economically large. *Temporary-help placements*, by contrast, do not improve, and may even harm, subsequent employment and earnings outcomes. The precision of our estimates rules out any moderately positive effects of temporary-help placements. Thus, although we find that job placements, overall, significantly improve affected workers' long-term employment and earnings outcomes, consistent with results of large-scale random assignment studies (see Howard S. Bloom et al. 1997, and Bloom and Charles Michalopoulos 2001 for summaries), the benefits of job placement services derive entirely from placements into direct-hire jobs. This finding places an important qualification on the conventional wisdom that placement into any job is better than no job.

We provide a variety of tests of the plausibility and robustness of these results. The use of contractor assignments as instrumental variables for job placement types requires that either contractors only affect participant outcomes through their influence on the types of jobs that they take or, alternatively, that any other effects that contractors may have on participant outcomes is orthogonal to the effect operating through job placement. We argue that, by design, contractors have little scope for affecting participant outcomes other than through job placements and, for the limited set of other services provided, there is little variation among contractors. Consistent with this view, we demonstrate that the effect of contractor assignments on participant outcomes is fully captured by contractors' placement rates into temporary-help and direct-hire jobs. We also demonstrate that our findings are robust to alternative specifications of the instrumental variables, that our results do not suffer from weak instruments biases, and that our findings cannot be ascribed to differences in the occupational distribution of temporary-help and direct-hire jobs.

Complementary analyses provide insights into why direct-hire placements are found to improve long-term labor market outcomes while temporary-help placements are not. Exploiting employer-level data in the UI wage records, we find that the key observable difference between these job placements is their effect on job stability. Over the seven-quarter follow-up period, the bulk of the earnings gain enjoyed by participants placed into direct-hire jobs derives from a single, continuous job spell. Direct-hire placements generate durable earnings effects in part because the placement jobs themselves last and in part because the placement jobs serve as stepping stones into stable jobs. In contrast, placement jobs in the temporary-help sector reduce job stability by all measures we are able to examine. Temporary-help placements increase multiple job holding and reduce tenure in the longest-held job, both indicators of job churn. Rather than helping participants transition to direct-hire jobs, temporary-help placements initially lead to more employment in the temporary-help sector, which serves to crowd out direct-hire employment.

We emphasize that our findings pertain to the marginal temporary-help job placements induced by the randomization of Work First clients across contractors, and therefore do not preclude the possibility that infra-marginal temporary-help placements generate significant benefits. However, our findings address the most pertinent policy issue: whether increased (or decreased) use of temporary-help firms in job placement of low-skilled workers will improve participant outcomes.

Our study is the first to exploit a plausibly exogenous source of variation in temporary-help job taking to examine the effects of temporary-help employment on long-term labor market outcomes among low-wage workers. Notably, our conclusions are at odds with those of several recent US and European studies that find that temporary-help employment provides a stepping stone into stable employment.[2] We point out that our OLS estimates are closely comparable to those in the literature, implying any unique feature of our Detroit sample cannot explain our discrepant findings. Substantial differences between the marginal treatment effects of temporary-help placements recovered by our instrumental variables estimates and the average treatment effects recovered by estimators in other studies could account for these disparate findings. Alternatively, the statistical techniques used in previous studies may be unable to fully differentiate the causal effects of holding given job types from the unmeasured skills and motivations that cause self-selection into these jobs.

I. Context: Work First Contractor Assignments in Detroit

Our study exploits the unique structure of Detroit's welfare-to-work program to identify the long-term consequences of temporary-help and direct-hire employment on labor market outcomes of low-skilled workers. Most recipients of Temporary Assistance for Needy Families (TANF) benefits must fulfill mandatory minimum

[2] US studies include Marianne A. Ferber and Jane Waldfogel (1998); Julia Lane et al. (2003); Mary Corcoran and Juan Chen (2004); Fredrik Andersson, Harry J. Holzer, and Lane (2005, 2009); Heinrich, Mueser, and Troske (2005, 2007); and Benner, Leete and Pastor (2007). Studies on temporary help employment in Europe include Alison L. Booth, Marco Francesconi, and Jeff Frank (2002); J. Ignacio García-Pérez and Fernando Muñoz-Bullón (2003); Pernilla Andersson and Eskil Wadensjö (2004); Marloes Graaf-Zijl, Gerard J. van den Berg, and Arjan Hemya (2009); Andrea Ichino, Fabrizia Mealli, and Tommaso Nannicini (2005, 2008); Catalina Amuedo-Dorantes, Miguel A. Malo, and Muñoz-Bullón (2008); René Böheim and Ana Rute Cardoso (2009); Michael Kvasnicka (2009). With the exception of Benner, Leete, and Pastor (2007), these US and European studies uniformly conclude that temporary-help jobs benefit workers, either by facilitating longer term labor market attachment or, at a minimum, by substituting for spells of unemployment.

work requirements. TANF applicants in Michigan who do not already meet these work requirements are assigned to Work First programs, which serve to place them in employment. For administrative purposes, Detroit's welfare and Work First programs are divided into 19 geographic districts. TANF participants are assigned to districts according to zip code of residence. The city of Detroit administers the Work First program, but the provision of services is contracted out to nonprofit or public organizations. One to three Work First contractors service each district, and when multiple contractors provide Work First services within a district, the city's Work First office rotates the assignment of participants to contractors. The contractor to which a participant is assigned thus depends on the date that he or she applies for TANF.

The Work First program is designed to provide short-term, intensive job placement services. All contractors operating in Detroit offer a fairly standardized one-week orientation, which includes life-skills training. Following orientation, few resources are spent on anything other than job development, and, as the program name implies, the emphasis is on rapid placement into jobs. Participants are expected to search for work on a full-time basis. Besides monitoring participants' job search efforts, contractors play a direct role in job placement by referring participants to employers or by hosting events at which employers recruit participants at the Work First program site. Although participants may find jobs on their own, most contractors in our study reported that they are directly involved in half or more of their job placements. Among those who are successfully placed into a job, three-fourths are placed within six weeks of program entry. Virtually all participants are placed into a job or are terminated from the program without a placement within six months of entry.[3] 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 (Autor and Houseman 2006). Participants who do not find jobs during their Work First assignments face possible sanctions. Consequently, unsuccessful participants continue to have strong incentives to work after leaving Work First.

Figure 1 provides a schematic diagram of Detroit's Work First program and the rotational assignment of participants to contractors. Upon entry, participants, who vary in terms of their personal characteristics and work histories, are assigned to a contractor operating in their district.[4] Contractors play an integral role in helping to place participants into jobs, but systematically vary in their propensities to place participants into direct-hire, temporary-help, or, indeed, any job at all.

It is logical to ask why contractors' placement practices vary. The most plausible answer is that contractors are uncertain about which type of job placement is 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

[3] Individuals may be terminated from Work First if they fail to find a job or if they fail to meet job search requirements.

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

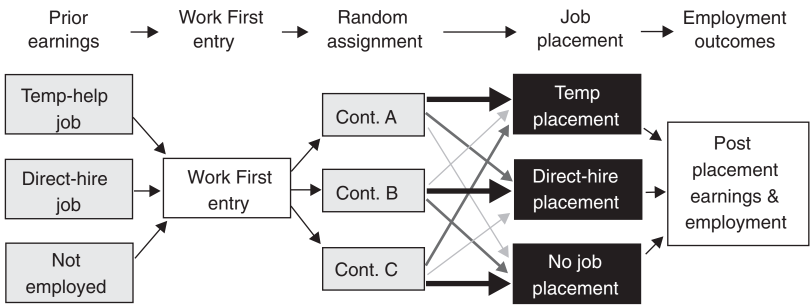

FIGURE 1. RESEARCH DESIGN

for individuals placed in jobs. Therefore, 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 2006).

II. The Research Design

Central to our research design are two features of the Detroit Work First environment: contractors operating in a given district have substantially different placement rates into temporary-help and direct-hire jobs, but offer otherwise standardized services; and the rotational assignment of participants to contractors is functionally equivalent to random assignments (as we show immediately below) so that contractor assignments are uncorrelated with participant characteristics. Under the plausible assumption (explored in detail below) that contractors only systematically affect participant outcomes in the post-program period through their effect on job placements, we can use contractor assignments as instrumental variables to study the causal effects of temporary-help and direct-hire placements on the employment and earnings of welfare recipients.

Our analysis draws on a unique database containing administrative records on the jobs obtained by participants while in the Work First program linked to their quarterly earnings from the State of Michigan's unemployment insurance wage records database. These administrative data document all jobs obtained by participants while in the program for all Work First spells initiated from the fourth quarter of 1999 through the first quarter of 2003 in Detroit. Work First job placements are classified as either direct-hire or temporary-help using a carefully compiled list of all temporary-help agencies in the metropolitan area.[5] The Work First data are matched to statewide Unemployment Insurance data that record total earnings and industry of

[5] Particularly helpful was a comprehensive list of temporary agencies operating in our metropolitan area as of 2000, developed by David Fasenfest and Heidi Gottfried, associate professors at Wayne State University. In a small number of cases where the appropriate coding of an employer was unclear, we collected additional information on the nature of the business through an internet search or telephone contact.

employment by participant for each employer for each calendar quarter. The UI data allow us to construct pre- and post-Work First UI earnings for each participant for the eight quarters before and after the quarter of program entry.[6] By the second quarter following Work First entry, virtually all participants have been either placed into a job or terminated from the program. Thus, we treat employment and earnings in these seven quarters as post-program outcomes, and we do not include the first post-entry quarter in our outcome data. Including this quarter has little substantive effect on our results, however, as shown in an earlier working paper version of this study.[7]

In the time period studied, 14 districts in Detroit were served by two or more Work First contractors, thus making these districts potentially usable for our analysis. In two districts with large ethnic populations, the assignment of participants to contractors was not done on a rotating basis, but rather was 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 65 and drop spells where reported pre- or post-assignment quarterly UI earnings 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.[8]

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 placement outcome during the Work First spell: direct-hire placement, temporary-help placement, or no job placement. The sample is predominantly female (94 percent) and black (97 percent). Slightly under half (48 percent) of Work First spells resulted in job placements. Among spells resulting in jobs, 20 percent have at least one job with a temporary agency. Interestingly, average weekly earnings are somewhat higher in temporary help jobs than in direct-hire jobs obtained in Work First.

The bottom panel of Table 1 reports average quarterly earnings and employment probability in quarters two through eight following the quarter of Work First entry. Participants are coded as employed in a particular quarter if they have any UI earnings during that quarter. Average employment probability is defined as the average of those employment dummy variables over the follow-up period. The average quarterly earnings and employment probabilities over quarters 2–8 following program entry are comparable for those obtaining temporary agency and direct-hire placement jobs, while earnings and the probability of employment for those who do not obtain employment during the Work First spell are 40 to 50 percent lower.

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

[7] This paper is available http://web.mit.edu/dautor/www/ah-detroit-january-2008.pdf. Among those placed into a job, 99.6 percent have been placed by the second quarter following entry, and among those terminated without a placement, 97.6 percent have been officially terminated by the second quarter, according to Work First administrative records. Because a high fraction of participants who unsuccessfully exit the program in quarter two or subsequently actually have UI earnings in the first quarter, it is likely that de facto time to exit among participants not placed into jobs is actually shorter than indicated in the administrative data. Participants who are placed into jobs officially remain in the program for up to three months, and their employers are periodically surveyed to check on their employment status.

[8] 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.

TABLE 1—SUMMARY STATISTICS FOR WORK FIRST PARTICIPANTS RANDOMLY ASSIGNED TO CONTRACTORS 1999–2000: OVERALL AND BY JOB PLACEMENT OUTCOME DURING WORK FIRST SPELL

	Job placement outcome during Work First spell							
	All		No employment		Direct hire		Temporary help	
	Mean	SE	Mean	SE	Mean	SE	Mean	SE
Percent of sample	100.0		51.9		38.4		9.8	
Panel A. Demographics								
Age	29.6	(0.04)	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)	93.9	(0.40)
Black (%)	97.3	(0.08)	97.3	(0.12)	97.1	(0.14)	98.2	(0.22)
White/other (%)	2.7	(0.08)	2.7	(0.12)	2.9	(0.14)	1.8	(0.22)
< High school (%)	37.2	(0.25)	40.1	(0.35)	33.8	(0.40)	35.3	(0.79)
High school (%)	35.5	(0.25)	33.6	(0.34)	37.6	(0.41)	37.8	(0.80)
> High school (%)	7.6	(0.14)	6.9	(0.18)	8.5	(0.23)	7.8	(0.44)
Unknown (%)	19.7	(0.21)	19.4	(0.28)	20.1	(0.34)	19.1	(0.65)
Panel B. Work history in eight quarters prior to contractor assignment: quarterly means								
All earnings/qtr	1,149	(8)	1,014	(10)	1,289	(13)	1,312	(25)
Direct-hire earnings/qtr	995	(7)	877	(10)	1,137	(12)	1,060	(24)
Temp-help earnings/qtr	136	(2)	121	(3)	133	(3)	229	(9)
Any employment in qtr	0.52	(0.00)	0.48	(0.00)	0.56	(0.00)	0.56	(0.01)
Any direct-hire employment in qtr	0.42	(0.00)	0.38	(0.00)	0.46	(0.00)	0.42	(0.01)
Any temp-help employment in qtr	0.09	(0.00)	0.09	(0.00)	0.09	(0.00)	0.14	(0.00)
Panel C. Job placement outcomes during Work First assignment (if employed)								
Hourly wage	7.51	(0.01)	n/a		7.43	(0.02)	7.83	(0.03)
Weekly hours	34.2	(0.05)	n/a		33.5	(0.06)	36.7	(0.10)
Weekly earnings	259	(0.71)	n/a		252	(0.80)	287	(1.40)
Panel D. Labor market outcomes in seven quarters (2–8) following contractor assignment: quarterly means								
All earnings/qtr	1,221	(8)	922	(11)	1,561	(15)	1,472	(28)
Direct-hire earnings/qtr	1,072	(8)	807	(10)	1,419	(14)	1,121	(26)
Temp-help earnings/qtr	134	(3)	105	(3)	123	(4)	330	(13)
Any employment in qtr	0.49	(0.00)	0.41	(0.00)	0.57	(0.00)	0.56	(0.01)
Any direct-hire employment in qtr	0.41	(0.00)	0.33	(0.00)	0.50	(0.00)	0.40	(0.01)
Any temp-help employment in qtr	0.07	(0.00)	0.07	(0.00)	0.06	(0.00)	0.15	(0.00)
Observations	37,161		19,277		14,255		3,629	

Notes: Sample is comprised of all Work First spells initiated from the fourth quarter of 1999 through the first quarter of 2003 in 12 Work First assignment districts in Detroit, Michigan. Data source is Detroit administrative records data from Work First programs linked to quarterly earnings from Michigan unemployment insurance wage records. Job placement outcomes and hourly earnings during Work First spell are coded using Detroit administrative records. Quarterly temporary-help and direct-hire earnings in eight quarters pre and post contractor assignment are coded using state of Michigan unemployment insurance records, where employer type is determined by industry codes. Work First participants may have multiple spells. All earnings are inflated to 2003 dollars using the Consumer Price Index (CPI-U).

The average characteristics of participants vary considerably according to job placement outcome. Compared to those who found jobs while in Work First, those who do not find jobs are more likely to have dropped out of high school and to have worked fewer quarters and had lower earnings before entering the program. Among those placed in jobs, those taking temporary-help jobs actually have slightly higher average prior earnings and employment than those taking direct-hire jobs. Not surprisingly, those who take temporary-help 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.[9]

Before turning to detailed tests of the research design, we depict the main results of analysis in a set of scatter plots comparing average UI employment and earnings outcomes for Work First participants by contractor by year of assignment against contractor-year placement rates into temporary-help and direct-hire jobs. As noted above, randomization of Work First participants to contractors occurs within districts within a specific program year. To purge district-year effects from these plots, we first estimate person-level OLS regressions of job placement type obtained during Work First (direct-hire, temporary-help, or no job) and post-program quarterly UI employment and earnings on a complete set of district by year of assignment dummy variables. We calculate the contractor-year specific component of each variable (temporary-help placement, direct-hire placement, UI earnings, UI employment) as the mean residual for each regression by contractor and year of assignment. By purging year and district effects, this procedure isolates the variation on which our research design relies: variation among contractors operating in the same district at the same time.

Figure 2A plots participants' post-program quarterly employment probabilities—defined as the fraction of quarters two through eight following contractor assignment in which they have positive earnings—against their contractors' direct-hire placement and temporary-help placement rates.[10] This figure reveals that participants assigned to contractors with high direct-hire placement rates have substantially higher average employment rates in the post-program period. There is no similar relationship, however, between contractors' temporary-help placement rates and post-program employment probabilities of the participants assigned to them. An analogous scatter plot for post-program earnings over post-assignment quarters two through eight (Figure 2B) tells a similar story: participants assigned to contractors with high direct-hire placement rates have substantially higher average quarterly earnings in quarters two through eight following program assignment, while the locus relating to temporary-help placement rates and post-program earnings is essentially flat.

Our subsequent analysis tests the validity of this research design and applies it—with many refinements—to produce estimates of the causal effects of job placements on earnings and employment and to explore the channels though which these

[9] In a small percentage of cases, employers' industry codes are missing in the UI wage records. For this reason, earnings and employment in temporary-help and direct-hire employment do not sum to corresponding total earnings.

[10] In essence, Figure 2 is the reduced form of our IV models.

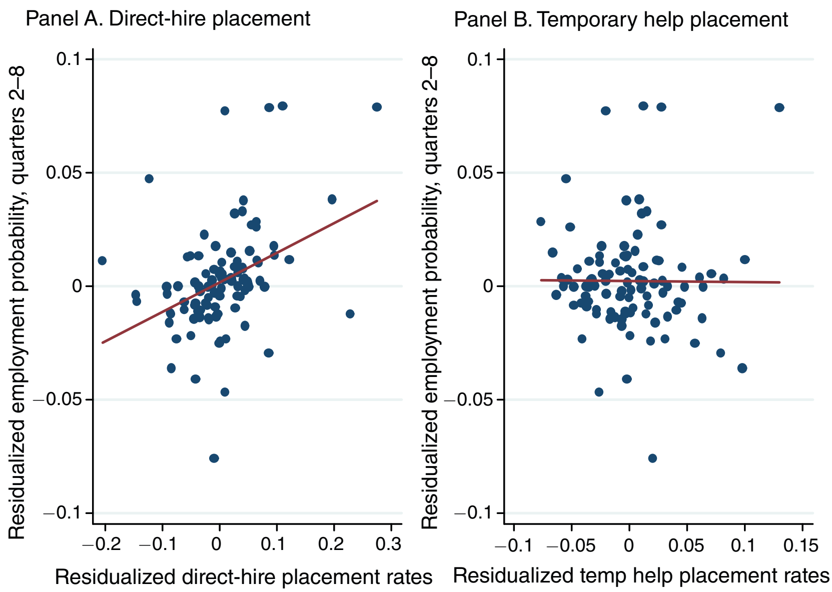

Figure 2A. Participant Employment Rates in Quarters 2 through 8 Following Contractor Assignment and Contractor-Year Placement Rates into Direct-Hire and Temporary-Help Jobs

Notes: Placement rates are the means of contractor-by-year residuals from OLS regressions of indicator variables for participant placements into direct-hire and temporary-help jobs on district-year dummy variables. Employment rate variables are contractor-year mean residuals from an analogous OLS regression of average participant employment rates in post-assignment quarters 2–8 on district-year dummy variables corresponding to the district and year in which participants were assigned to Work First contractors.

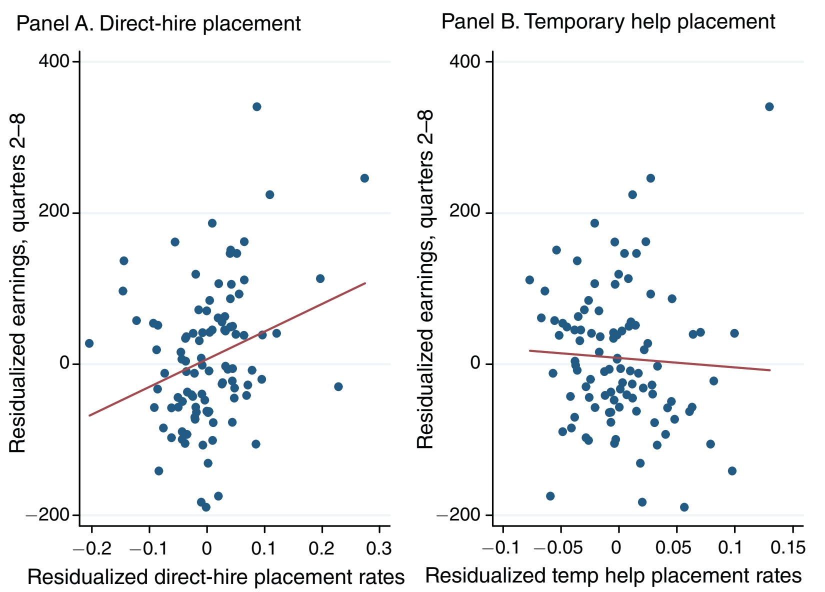

Figure 2B. Participant Earnings in Quarters 2 through 8 Following Contractor Assignment and Contractor-Year Placement Rates into Direct-Hire and Temporary-Help Jobs

Notes: Placement rates are the means of contractor-by-year residuals from OLS regressions of indicator variables for participant placements into direct-hire and temporary-help jobs on district-year dummy variables. Earnings variables are contractor-year mean residuals from an analogous OLS regression of average participant quarterly earnings in post-assignment quarters 2–8 on district-year dummy variables corresponding to the district and year in which participants were assigned to Work First contractors.

causal effects arise. The bottom line of our analysis, however, is already visible in Figure 2.

A. *Testing the Research Design*

Our research design requires that the rotational assignment of participants to contractors effectively randomizes participants to contractors operating within each district in a given program year. We test whether the data are consistent with random assignment by statistically comparing the following eight characteristics of participants assigned to contractors within each district and year: sex, white race, other (nonwhite) race, age and its square, average employment probability in the eight quarters before program entry, average employment probability with a temporary agency in these prior eight quarters, average quarterly earnings in these prior eight quarters, and average quarterly earnings from temporary agencies in the prior eight quarters.[11]

In testing the comparability of participants across these eight characteristics, we are likely to obtain many false rejections of the null, and this is exacerbated by the fact that participant characteristics are not fully independent (e.g., participants with high prior employment rates are also likely to have high prior earnings). To account for these confounding factors, we estimate a seemingly unrelated regression (SUR), which addresses both the multiple comparisons problem and the correlations among demographic characteristics across participants at each contractor.[12] This procedure can be readily described with a single equation regression model:

$$X^k_{icdt} = \alpha + \gamma_d + \varphi_t + \theta_{dt} + \lambda_{ct} + \omega_{icdt}, \tag{1}$$

where X^k_{icdt} is one of the eight measures used for the comparison (e.g., prior employment, gender, etc., all indexed by k) for participant i assigned to contractor c serving assignment district d in year t. The vectors γ and φ contain a complete set of dummies indicating randomization districts and year-by-quarter of contractor assignment, respectively, while the vector θ contains all two-way interactions between district and year.[13]

Of central interest in this equation is λ, a vector of contractor-by-year of assignment dummies, with one contractor-by-year dummy dropped for each district-year pair. The p-value for the hypothesis that the elements of λ are jointly equal to zero provides an omnibus test for the null hypothesis that participant covariates do not differ significantly among participants assigned to different contractors within a district-year pair. A high p-value corresponds to an acceptance of this null. We use SUR to estimate this model simultaneously for all eight covariates in X to account

[11] Because of the large number of missing values for the education measures, and because some contractors were apparently more diligent than others about recording participant education, we exclude education variables from both the randomization test and subsequent statistical analysis. Regression results that include these variables (including an "education missing" variable) are nearly identical to our main results.

[12] This method for testing randomization across multiple outcomes is proposed by Jeffrey R. Kling et al. (2004) and Kling, Jeffrey B. Liebman, and Katz (2007).

[13] To conserve degrees of freedom, we do not include district by year by calendar quarter interactions. Models that include these additional dummy variables produce near-identical results and are available from the authors.

for the correlations among these variables. If we estimated this system equation-by-equation using OLS, we would obtain identical point estimates, but the standard errors would be incorrect for the hypotheses of interest.

The results of the tests of randomization are highly consistent with a chance distribution of covariates. The top panel of Appendix Table 1 provides p-values for estimates of equation (1) applied to the full sample and fit separately to each of the 41 district-by-year cells. The overall p-value for the full sample pooled across districts and years is 0.44. Among 41 separate district-by-year comparisons, 38 accept the null hypothesis at the 10 percent level or higher, and only one comparison rejects the null at conventional levels of significance. The final row and column of the table provide p-values for the comparison test for each year, pooling across districts, and for each district, pooling across years. All but one of these 16 tests readily accepts the null at conventional levels of significance. These results strongly support the hypothesis that the rotational assignment of participants across contractors generates variation that can be treated as random.

The research design also requires that random assignment to contractors significantly affects participant job placements. To confirm this, 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, nonemployment). Here, our expectation is that job placement outcomes should differ significantly across contractors within a district and year. Tests of this hypothesis in panel B of Appendix Table 1 provide strong support for the efficacy of the research design. The omnibus test for cross-contractor, within district-year differences in job placement outcomes rejects the null at below the 1 percent level for the full sample, as do 16 of 17 tests for significant differences in placement rates across all districts within a year or within a district across all years.[14]

III. Main Results: The Effects of Job Placements on Earnings and Employment

We 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 quarters two through eight following the calendar quarter of random assignment to contractor. Our primary empirical model is

$$Y_{icdt} = \alpha + \beta_1 D_i + \beta_2 T_i + X_i'\lambda + \gamma_d + \varphi_t + \theta_{dt} + e_{icdt}, \tag{2}$$

where the dependent variable is average real quarterly earnings or quarterly employment (from UI records) defined over the follow-up period. Notation for district,

[14] We also calculate partial R-squared values from a set of regressions of job placement type (any job placement, direct-hire job placement, temporary-help job placement) on dummy variables indicating contractor-by-year of assignment after first orthogonalizing these job placement types with respect to demographic, earnings history, and time variables. Conversely, we compute partial R-squared values from regressions of job placement types on demographic, earnings history, and time variables after first orthogonalizing the dependent variable with respect to contractor assignment. We find that contractor assignment explains 85 to 130 percent as much variation in job placement type as do demographic, earnings history, and time variables combined.

time, and district-by-year vectors is the same as in equation (1), with subscripts i, c, d, and t referring, respectively, to participants, contractors, placement districts, and year-by-quarter of participant assignment. The binary variables D_i and T_i indicate whether participant i obtained either a direct-hire or temporary-help job placement during her Work First spell (with both equal to zero if no placement was obtained). To account for the grouping of participants within contractors, we use Huber-White robust standard errors clustered by contractor (33 clusters).[15]

It bears emphasis that there is not a mechanical linkage between job placements occurring during the Work First spell and earnings and employment outcomes observed in the UI data in the follow-up period. The job placement variables on the right-hand side of equation (2), D and T, refer to jobs obtained *during* the Work First spell and are coded using welfare case records from the city of Detroit. The dependent variable, by contrast, is obtained from state of Michigan unemployment insurance records and measures labor market outcomes in the specified quarters *following* Work First assignment. We examine outcomes beginning in the second quarter following Work First assignment because, as noted above, virtually all participants have either been placed into a job or exited the program by that time. It is therefore possible—in fact, commonplace—for a participant who obtains a job placement during Work First to have no earnings in the second and subsequent quarters following program entry and, conversely, for a participant who receives no placement to have positive earnings in the second and subsequent post-assignment quarters.

In general, we would not expect equation (2) to recover unbiased estimates of the effects of job placements on participant outcomes when estimated using Ordinary Least Squares. Only about half of Work First participants in our sample obtain employment during their Work First spell (Table 1), and this set of participants is likely to be more skilled and motivated to work than average participants. Unless these attributes are fully captured by the covariates in X, estimates of β_1 and β_2 are likely to be biased.

We address this bias by instrumenting D and T in equation (2) with contractor-by-year-of-assignment dummy variables as outlined in Section II. Our use of contractor-by-year dummy variables as instruments is equivalent to using contractor-by-year placement rates as instruments. To facilitate exposition, we can therefore rewrite equation (2) as

$$(3) \qquad Y_{icdt} = \alpha + \pi_1 \overline{D}_{ct} + \pi_2 \overline{T}_{ct} + \gamma_d + \varphi_t + \theta_{dt} + \nu_{ct} + \iota_{icdt},$$

where $\overline{D}_{ct}$ is the observed direct-hire placement rate of contractor c in year t, $\overline{T}_{ct}$ is the corresponding placement rate in temporary-help employment, and we omit the

[15] All models also include the vector of eight pre-determined covariates used in the randomization test: sex, race (white, black, or other), age and age-squared, and measures of quarters of UI employment and real UI earnings in direct-hire and in temporary-help employment in the eight quarters prior to contractor assignment. We suppress these terms here to simplify the exposition of the 2SLS models.

X vector for simplicity.[16] This equation underscores that our instruments enable the identification of the causal effects of placements into direct-hire and temporary-help jobs through variation in job placement rates among contractors that have statistically identical populations. In general, these models will yield estimates of the causal effects of temporary and direct-hire job placements for the "marginal" placements—i.e., those whose job placement type was altered by contractor assignment.[17]

The error term in equation (3) is partitioned into two additive components, ν_{ct} and ι_{icdt}, to underscore the two key conditions that our identification strategy requires for valid inference. The first is that unobserved participant-specific attributes that affect earnings (ι_{idct}) must be uncorrelated with $\overline{D}_{ct}$ and $\overline{T}_{ct}$. The evidence above suggests that this condition is met by the rotational assignment design. The second condition is that if there is any unobserved contractor-by-year heterogeneity that affects participant outcomes, but does not operate through job placement rates (ν_{ct}), it must be mean independent of contractor placement rates, i.e., $E(\nu_{ct}\overline{D}_{ct}) = E(\nu_{ct}\overline{T}_{ct}) = 0$. This latter condition highlights that the research design does *not* require that contractors only affect participant outcomes through job placements. However, it does require that any nonplacement effects are uncorrelated with contractor job placement rates, since this correlation would cause 2SLS estimates to misattribute the effects of unobserved contractor practices to job placement rates. As outlined at the beginning of this paper, almost all Work First resources are devoted to job placement, and few other support services are provided to participants beyond the set of standardized services offered by the city of Detroit to all participants. Exploiting the fact that we have more instruments than endogenous right-hand-side variables, we report below the results of overidentification tests, which provide strong statistical evidence of the validity of this assumption.

One other element of this specification deserves note. Our use of contractor-by-year of assignment dummy variables as instruments for temporary-help and direct-hire job placements, rather than simply contractor of assignment dummies, allows for an interaction between contractor placement and time period. This is useful because even if contractors operating in a district have stable (but different) placement policies, the differences in temporary-help and direct-hire placements among contractors may vary over time in response to changes in the local economy

[16] If the vector of participant characteristics were also included, equation (3) would differ slightly from 2SLS to the degree that there is sample correlation between contractor dummies and participant characteristics (though in practice, this correlation is insignificant, as shown in Appendix Table 1). Kling (2006) implements an instrumental variables strategy analogous to equation (3), in which means of the assignment variable are used as instruments rather than fixed effects.

[17] In an extended working paper version of this paper (see link in footnote 7), we provide a formal analysis of the conditions under which the coefficients from our IV models yield causal effects estimates for individuals whose job placement was affected by contractor assignment. If the effect of temporary and direct-hire placements is constant among these marginal placements—what we term locally constant treatment effects—our IV models yield causal effects estimates for these individuals. We note that an assumption of locally constant treatment effects is less restrictive than the common assumption of constant treatment effects for the entire sample population. Alternatively, if assignment to a given contractor affects the probability that workers take temporary-help or direct-hire jobs (but not both), our IV estimates may be interpreted within the Local Average Treatment Effects framework of Guido W. Imbens and Joshua D. Angrist (1994). In the extended working paper, we provide empirical evidence that our IV estimates may be interpretable under the LATE framework.

or changes in the average characteristics of participants entering the program.[18] In Section IV, we show that estimates of the effects of placement type on earnings and employment outcomes using contractor of assignment as instruments are similar to those obtained using contractor-by-year of assignment as instruments.

A. *Ordinary Least Squares Estimates*

To facilitate comparison with prior studies of the impact of temporary-help and direct-hire job taking on labor market advancement of welfare participants and other low-earnings workers (e.g., Andersson, Holzer, and Lane 2005, 2007; Heinrich, Mueser, and Troske 2005, 2007), we begin our analysis with ordinary least squares (OLS) estimates of equation (2). Table 2 presents OLS estimates for average real quarterly earnings and quarterly employment for Work First participants in quarters two through eight following their assignment to Work First contractors using all 37,161 spells in our data. For ease of interpretation, we re-center all control variables by subtracting the mean for participants who did not obtain a job during their Work First spell. Thus, by construction, the intercept in equation (2) equals the mean of the outcome variable for Work First participants not placed into jobs.

The first column of Table 2 shows that, conditional on detailed controls for race, age, and prior employment and earnings, earnings in post-assignment quarters two through four among participants who obtained any employment during their Work First spell were on average \$573 more per quarter than earnings for clients who did not obtain employment during their Work First spell. Over that same horizon, the probability of employment was 17 percentage points higher per quarter among those placed into a job during their Work First spell compared to those who were not. As indicated by the intercepts of these equations, average quarterly earnings were \$817, and the average probability of employment was only 40 percent among participants who did *not* obtain employment during their Work First spell.

Column (2) distinguishes outcomes for those taking temporary-help from those taking direct-hire jobs during their Work First spells. In quarters two through four following Work First assignment, participants who obtained a temporary-help position during their Work First spell were slightly less likely to be employed and averaged \$101 less per quarter than participants who obtained a direct-hire placement, though neither difference is statistically significant. Subsequent columns of Table 2 summarize outcomes over longer time horizons following Work First assignment. Participants who obtained a job placement during Work First earned an average of 53 percent more per quarter (\$493) and on average were 34 percent more likely to be employed (14 percentage points) over the entire seven-quarter follow-up period compared to participants who did not obtain a job while in Work First. The earnings gap between those obtaining temporary-help and direct-hire jobs during Work First cumulates slightly over this longer time frame, but is small relative to the substantial

[18] For example, when temporary-help positions are scarce, observed percentage point differences among contractors in temporary-help placement rates are likely to contract. Survey results in Autor and Houseman (2006) also indicate that some contractors have amended their placement polices in recent years, with a significant fraction reporting having reduced their use of temporary-help placements.

TABLE 2—OLS ESTIMATES OF THE RELATIONSHIP BETWEEN WORK FIRST JOB PLACEMENTS AND EARNINGS AND EMPLOYMENT QUARTERS 2–8 FOLLOWING WORK FIRST ASSIGNMENT

	Quarters 2–4		Quarters 5–8		Quarters 2–8	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Quarterly earnings						
Any job placement	573**		433**		493**	
	(19)		(21)		(19)	
Direct-hire job placement		593**		455**		514**
		(22)		(23)		(21)
Temp-help job placement		492**		343**		407**
		(33)		(31)		(29)
Constant	817	817	1001	1001	922	922
	(11)	(11)	(11)	(11)	(10)	(10)
R^2	0.23	0.23	0.21	0.21	0.25	0.25
H_0:Temp=Direct		0.01		0.00		0.00
Panel B. Quarterly employment						
Any job placement	0.17**		0.11**		0.14**	
	(0.01)		(0.00)		(0.01)	
Direct-hire job placement		0.18**		0.11**		0.14**
		(0.01)		(0.00)		(0.01)
Temp-help job placement		0.16**		0.10**		0.13**
		(0.01)		(0.01)		(0.01)
Constant	0.40	0.40	0.41	0.41	0.41	0.41
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.17	0.17	0.15	0.15	0.20	0.20
H_0:Temp=Direct		0.20		0.02		0.05

Notes: $N = 37{,}161$. Robust standard errors in parentheses are clustered on Work First contractor (33 clusters). Each column corresponds to a separate OLS regression. The dependent variable in the top panel is mean quarterly earnings over the indicated period. Employment is measured as a dummy variable equal to one if the participant has any UI earnings in a particular quarter; the dependent variable in the lower panel is the average of these employment dummy variables over the indicated period. All models include dummy variables for year by quarter of assignment and assignment-district by year of assignment, and controls for sex, white or Hispanic race, other race, age and age-squared, total quarters employed and total earnings in eight quarters prior to Work First assignment, total quarters employed in temporary-help work and total temporary-help earnings in the eight quarters prior to Work First assignment. Earnings values are inflated to 2003 dollars using the Consumer Price Index (CPI-U).

~ Significant at the 0.10 level.
* Significant at the 0.05 level.
** Significant at the 0.01 level.

gap in employment and earnings between those who took jobs during Work First and those who did not. Over the seven quarter period, earnings of those placed into temporary-help jobs were 93 percent of earnings of those placed into direct-hire jobs, and the earnings difference between those with a temporary-help versus a direct-hire placement was just 26 percent of the earnings gap between those with a temporary-help versus no job placement.

These OLS estimates are consistent with other published findings, most notably with Heinrich, Mueser, and Troske (2005 and 2007). They find that Missouri and North Carolina welfare recipients who obtained temporary-help jobs in 1993 and 1997 earned almost as much over the subsequent two years as those who obtained direct-hire employment—and earned much more than did non job-takers. Like Heinrich, Mueser, and Troske (2005), our primary empirical models for earnings and employment are estimated for a relatively homogeneous and geographically concentrated population and include detailed controls for observable participant demographic characteristics and prior earnings. Similar to our estimates, Heinrich, Mueser, and Troske (2005) report that welfare participants taking temporary-help jobs earned at least 85 percent of that of workers taking nontemporary-help jobs over the subsequent two years and that the dollar decrement over this period to having started in a temporary help versus a direct-hire job was less than one-third the positive effect of a temporary job relative to no job.[19] Though less directly comparable, our findings also echo those of Andersson, Holzer, and Lane (2005, 2009) who report that low-skilled and low-earnings workers who obtain temporary-help jobs typically fare relatively well in the labor market over the subsequent three years, despite starting with lower earnings.

These observations provide assurance that our sample from the city of Detroit is comparable to those used in other studies of job-taking among welfare recipients and other low-skilled workers. Moreover, the similarity between our OLS estimates and those of Heinrich, Mueser, and Troske for the relationship between temporary-help job-taking and subsequent earnings suggests that the differences in causal estimates that we report below from instrumental variable models are due to substantive differences in research design rather than to differences in sample frame.[20]

B. *Instrumental Variables Estimates*

Table 3 reports instrumental variables estimates of equation (2) for the impact of Work First job placements on subsequent employment and earnings, where employment placements during the Work First spell are instrumented by contractor-by-year assignments. The estimate in column (1) confirms an economically large and statistically significant effect of Work First job placements on earnings and employment in quarters two to four following Work First assignment. Obtaining any job placement is estimated to raise the average employment probability in post-assignment quarters 2–4 by 13 percentage points and increase average quarterly earnings by \$301. These effects are highly significant and are more than half the corresponding OLS estimates (Table 2).[21]

Column 2 distinguishes between the causal effects of temporary-help placements and the effects of direct-hire job placements. These estimates reveal that the entirety

[19] Henrich, Mueser, and Troske (2005, 165–166).

[20] To control for possible selection bias in the decision to take a temporary agency job, Heinrich, Mueser, and Troske 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. Their empirical strategy thus 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. This correction has little effect on their regression estimates, suggesting either that the selection problem is unimportant or that their instruments do not effectively control for selection on unobservable variables.

[21] The standard errors in Table 3 do not account for potential serial correlation in outcomes among participants with multiple spells. The 37,161 Work First spells in our data correspond to 24,903 unique participants, 67 percent of whom have one spell, 22 percent of whom have 2 spells, and 11 percent of whom have 3 or more spells. To assess the importance of this issue, we reestimated models for total earnings and quarters worked over eight quarters using only the first Work First spell per participant observed in our data. These first-spell estimates, available from the authors, are closely comparable to our main estimates for earnings and employment in Table 3.

Table 3—Instrumental Variables Estimates of the Effect of Work First Job Placements on Earnings and Employment Quarters 2–8 Following Work First Assignment

	Quarters 2–4		Quarters 5–8		Quarters 2–8	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Quarterly earnings						
Any job placement	301**		209*		248**	
	(106)		(98)		(96)	
Direct-hire job placement		577**		430**		493**
		(149)		(153)		(147)
Temp-help job placement		−246~		−228		−235~
		(127)		(143)		(123)
Constant	943	891	1105	1064	1036	990
	(47)	(54)	(45)	(56)	(43)	(53)
R^2	0.22	0.21	0.20	0.20	0.24	0.24
H_0:Temp=Direct		0.00		0.00		0.00
Over-ID test	0.20	0.42	0.29	0.38	0.20	0.36
Panel B. Quarterly employment						
Any job placement	0.13**		0.06*		0.09**	
	(0.03)		(0.03)		(0.03)	
Direct-hire job placement		0.20**		0.11**		0.15**
		(0.04)		(0.04)		(0.04)
Temp-help job placement		−0.01		−0.05		−0.03
		(0.03)		(0.04)		(0.03)
Constant	0.42	0.41	0.44	0.42	0.43	0.42
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
R^2	0.17	0.16	0.15	0.14	0.19	0.18
H_0:Temp=Direct		0.00		0.02		0.00
Over-ID test, P-value	0.06	0.21	0.06	0.14	0.02	0.09

Notes: $N = 37{,}161$. Robust standard errors in parentheses are clustered on Work First contractor (33 clusters). Each column corresponds to a separate 2SLS regression. Instrumental variables for jobs obtained (any, direct-hire and temporary-help) are contractor by year of assignment dummies. Sample and specification are identical to Table 2.

~Significant at the 0.10 level.
*Significant at the 0.05 level.
**Significant at the 0.01 level.

of the positive effect of Work First job placements derives from placements into direct-hire jobs. Direct-hire placements induced by contractor assignment raise average quarterly earnings by $577 and increase the average quarterly employment probability by 20 percentage points in post-assignment quarters 2–4. In marked contrast, the point estimate of the effect of temporary-help placements on employment probability is close to 0 and insignificant, while the estimated effect on earnings is negative (−$246 per quarter) and weakly significant.

Subsequent columns of Table 3 show that the employment and earnings effects of direct-hire placements persist into the second year following Work First assignment. In quarters 5–8, direct-hire placements induced by contractor assignments raise average quarterly earnings by $430 and the employment probability by 11 percentage points. Over the seven follow-up quarters, direct-hire placements induced by contractor assignment raise cumulative earnings by an estimated $3,451, an effect that is highly significant and economically large.

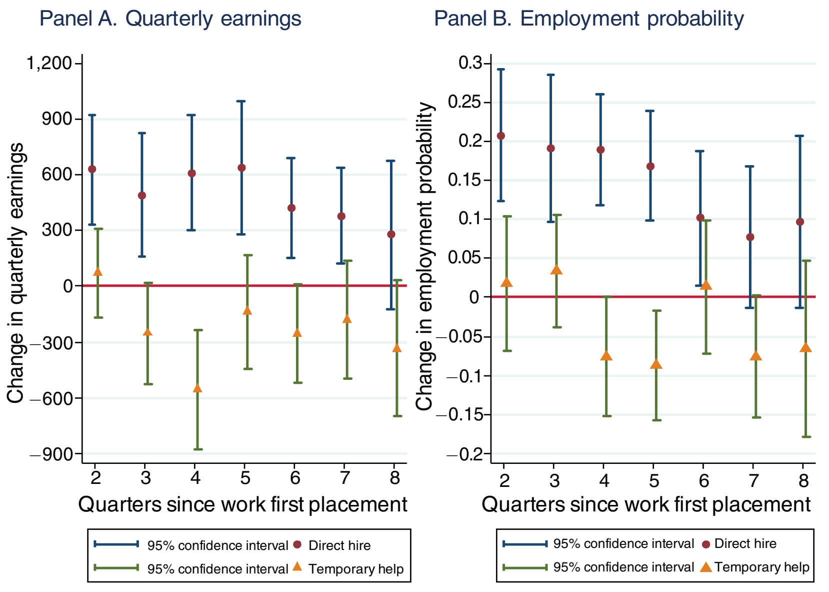

Figure 3. Two-Stage Least Squares Estimates of the Effect of Direct-Hire and Temporary-Help Job Placements on Quarterly Earnings and Probability of Employment in Quarters 2–8 Following Work First Contractor Assignment

Notes: Each pair of plotted points is from a separate 2SLS regression of the indicated outcome variable for the relevant quarter on the direct-hire and temporary-help job placements instrumented by contractor-by-year of assignment. Confidence intervals are estimated with robust standard errors that are clustered on contractor assignment.

Conversely, the estimated impact of a temporary-help placement on employment in quarters two through eight is insignificantly different from zero, and the effect on quarterly earnings is weakly negative. The 95 percent confidence interval of the estimates excludes earnings gains larger than $5 per quarter and increases in the probability of employment of greater than 2 percentage points. In all cases, we reject the hypothesis that the impacts of temporary-help and direct-hire placements either on earnings or on employment are comparable.

Figure 3 provides further detail on these results by plotting point estimates and 95 confidence intervals for analogous 2SLS estimates of the effect of direct-hire and temporary-help job placements on earnings and employment probability in each of the 7 quarters in our follow-up period. The figure shows that direct-hire placements significantly raise both earnings and the probability of employment in the first six quarters and five quarters of the follow-up period, respectively. These impacts begin to diminish after the fifth quarter, consistent with some fade-out of benefits.[22] By contrast, estimated impacts of temporary-help placements on employment and earnings generally are not significantly different from zero. While this

[22] The evidence suggesting that the benefits of job placements fade with time echoes that in David Card and Dean R. Hyslop (2005) who find, in the context of a Canadian welfare program, that initial job accessions induced by a time-limited earnings subsidy tend to peak after approximately 15 months and, in the limit, do not produce permanent earnings gains. Nevertheless, from a policy perspective, a job placement that raises earnings and employment for two full years may still be viewed as successful.

figure makes clear that direct-hire job placements induced by Work First contractor assignments substantially increase earnings and employment of Work First clients over the subsequent two years, we find no evidence, in contrast to prior research, that comparable benefits accrue from temporary-help placements.

One noteworthy pattern in these results is that the difference between the estimated effects of direct-hire and temporary-help placements on employment and earnings are larger in IV than in OLS models (compare Tables 2 and 3). Under the assumption that the effects of job placement type are homogenous across participants, this pattern would suggest that those taking temporary-help positions are more positively selected than those taking direct-hire jobs, which is counterintuitive and appears inconsistent with the patterns found in our OLS models reported in Table 2.[23]

Tempering this interpretation is the fact that the IV models identify the effect of job placements on marginal workers, i.e., those whose job placements are causally affected by contractor assignments. If the effects of job placement type are heterogeneous between marginal and inframarginal workers, IV models are not necessarily informative about the direction of bias in OLS estimates.[24] It is plausible that these marginal workers, on average, differ from those whose placement job is unaffected by contractor assignment and experience different treatment effects.

Why might marginal direct-hire placements have such great benefit, while temporary-help placements have so little? Work First participants differ in the degree to which they rely on employer contacts provided by the contractor to find jobs. Many participants find jobs on their own, drawing upon employer contacts from prior work experience or from family and friends. Those who are most reliant on contractor input, however, are likely to have relatively few personal contacts and less wherewithal to find good jobs on their own. The job placements obtained by these workers are most likely to be causally affected by their contractor assignments. Arguably, these are also the workers who stand to benefit most from obtaining placement into a stable job. Thus, the marginal benefit of a direct-hire placement may be relatively high and the marginal benefit of a temporary-help placement may be relatively low for these participants. Indeed, the results in Table 3 imply that direct-hire jobs are scarce. Marginal participants placed in direct-hire jobs, on average, would not have obtained jobs that were equally durable or remunerative had they not received these placements. Conversely, the IV results suggest that marginal workers placed in temporary-help positions would, on average, have fared equally well, or somewhat better, without such placements. Consistent with this

[23] The OLS estimates in Table 2 compare mean earnings and employment of participants who found direct-hire or temporary-help jobs during the Work First spell relative to participants who found *no* employment during the spell. Workers self-selecting into direct-hire jobs obtain significantly higher post-program earnings and employment than workers self-selecting into temporary-help jobs, and both obtain higher earnings and employment than those with no Work First job. This pattern accords with the standard intuition that workers found in temporary-help jobs are less positively selected than workers found in direct-hire jobs.

[24] To see this, consider a case where self-selection into both temporary-help and direct-hire employment is functionally equivalent to random assignment, so OLS estimates recover the mean causal effects for both types of job placements for those who gain employment. This mean effect may include a mixture of positive, negative, and zero effects, though the weighted average of these effects is assumed to be positive. Even in this scenario, the causal effect of temporary-help employment for the *marginal* temporary-help job taker may be negative. This would be the case if the compliers to the assignment mechanism primarily included the subset of workers for whom temporary-help assignments crowd out superior employment outcomes.

interpretation, we show in Section V that marginal temporary-help placements raise earnings in temporary help positions but crowd out earnings in direct-hire positions at a greater than one-to-one rate, thus lowering earnings in net.

IV. Testing the Identification Framework

This section explores two central aspects of the identification framework. We first consider the validity of using contractor random assignments as instrumental variables for Work First participants' placement into temporary-help and direct-hire jobs. Next, we test the robustness of the instrumental variables results to plausible alternative specifications of the instruments.[25]

A. *Validity of the Instruments*

Our identification strategy rests on the assumption that contractor assignments are valid instrumental variables for Work First participants' employment in temporary-help and direct-hire jobs. Validity requires two conditions: contractors causally affect the probability that Work First participants obtain direct-hire and temporary-help jobs, a condition directly verified in Section IIA; and contractors only systematically affect Work First participants' employment outcomes in quarters two through eight following Work First assignment through placements into direct-hire and temporary-help jobs during the Work First spell. If this latter (exclusion) condition were violated—that is, contractors affected participant outcomes through channels other than temporary-help or direct-hire job placements, and these other contractor impacts were correlated with job placement rates—our instrumental variables would be correlated with the error terms of our 2SLS models, and the estimates would be biased.

While the restriction that contractors only systematically affect participant outcomes through job placements is fundamentally untestable, we can directly evaluate the importance of heterogeneity in contractor effects on participant outcomes by taking advantage of the fact that we have 59 instruments (contractor-by-year dummy variables) and only two endogenous variables (temporary-help and direct-hire placements).[26] This permits us to use an overidentification test to assess whether a saturated model using 59 contractor-by-year dummies as distinct instrumental variables for participant outcomes is statistically equivalent to a far more restrictive parameterization in which contractor effects on participant outcomes operate exclusively through direct-hire and temporary-help placements.

[25] In Web Appendix Table A, we also examine whether the large differences in the consequences of temporary-help and direct-hire job placements are attributable to differences in the types of positions obtained in temporary-help versus direct-hire employment rather than to differences in the employment arrangements per se. Production jobs are heavily overrepresented in temporary-help placements. Using our IV framework to estimate four endogenous variables (temporary-help placements in production and nonproduction jobs and direct-hire placements in production and nonproduction jobs), we show that our results are not attributable to occupational differences in temporary-help and direct-hire jobs. Direct-hire placements into both production and nonproduction jobs significantly improve subsequent employment and earnings, while temporary-help placements into both production and nonproduction jobs do not improve and, in the case of production jobs, may harm subsequent earnings and employment outcomes.

[26] 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 overidentification test reveals a compelling result. We detect no statistically significant effect of contractor assignment on participant outcomes that is *not* captured by temporary-help and direct-hire placements. In other words, the data accept the null hypothesis that the 59 contractor-by-year dummy variables have no significant explanatory power for participant outcomes beyond their effects on temporary-help and direct-hire job placements. This result holds for the full seven quarter outcome period and for both sub-periods, as is visible in the bottom row of each panel of Table 3. For earnings outcomes, the p-values of the overidentification tests range from 0.36 to 0.42. For quarterly employment outcomes, the p-values range from 0.09 to 0.21 (see the bottom row of each panel of Table 3).

It is widely recognized that overidentification tests may have low power against the null (see, Angrist and Jörn-Steffen Pischke 2004, section 4.2.2). That is not the case here, however. If we instead compare the unrestricted model 2SLS model (i.e., with 59 dummies) against a parameterization that collapses temporary-help and direct-hire placements into a single employment category (thus reducing 59 parameters to 1 rather than 2), the overidentification test rejects the null at the 2 percent level for 7 quarter employment and accepts it at the 20 percent level for 7 quarter earnings (down from 36 percent). Thus, a parameterization that distinguishes between the causal effects of temporary-help and direct-hire placements is both necessary and sufficient to statistically capture the full effect of contractor assignments on participant outcomes.

These results demonstrate that any set of contractor practices that systematically affects participant outcomes, but does not operate through job placements, would have to be collinear with—and, hence, statistically indistinguishable from—contractor job placements. We view this possibility as unlikely. Based on a detailed survey of the Work First contractors in the Detroit area analyzed by this study (Autor and Houseman 2006), we document that program funding is tight and few resources are spent on anything other than job placement. A standardized program of general or life skills training is provided in the first week of the program by 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. Consequently, there is little scope for contractors to substantially affect participant outcomes other than through job placements, and what other services do exist are fairly uniform across contractors; thus their provision should be uncorrelated with contractor job placements.

Adding to this body of evidence, we find in the Detroit data that direct-hire and temporary-help job placement rates are positively and significantly correlated across contractors, implying that contractors with direct-hire placement rates tend to have high temporary-help placement rates. This fact reduces the plausibility of a scenario in which another set of contractor practices, collinear with job placements, accounts for our IV estimates showing divergent effects of direct-hire and temporary-help job placement.[27]

[27] In the working paper version of this article, found at http://web.mit.edu/dautor/www/ah-detroit-january-2008.pdf we show in Appendix Table 3 that if separate 2SLS models for the causal effects of temporary-help and

B. *Robustness and Power of the Instruments*

The 2SLS models in Table 3 use contractor-by-year of assignment dummy variables as instruments for temporary-help and direct-hire job placements. These instruments allow for differences in placement rates among contractors operating in a particular district to change over time due to, for example, changes in the local economy, changes in average participant characteristics, or changes in placement policies by individual contractors. Although allowing for some interaction between contractor dummy variables and time is efficient, as a robustness check on our parameterization of the instrumental variables, we report, in Table 4, estimates for the main empirical model in which we use contractor assignments as instruments rather than contractor-by-year assignments. We also repeat the baseline models in column 1 for reference. Point estimates from these contractor-only 2SLS models found in column 3 prove quite comparable to the baseline estimates. Although standard errors are slightly larger, as expected, these models clearly affirm the prior conclusions: direct-hire placements significantly raise employment and earnings; direct-hire effects are significantly larger than the corresponding effects for temporary-help placements; the effects of temporary-help placements on employment and earnings are always negative and in some cases significant. In net, the results are quite robust to discarding the year-to-year variation in contractor placement rates.

A further concern with use of contractor assignments as instruments is that they may suffer from the weak instruments problem highlighted by John Bound, David A. Jaeger, and Regina M. Baker (1995). According to conventional rule of thumb tests (see, James H. Stock, Jonathan H. Wright, and MotohiroYogo 2002), weak instruments should not be an issue in our application. The chi-square statistics for our instrumental variables are 895, 634, and 548 for overall employment, temporary-help employment, and direct-hire employment, respectively. As a further check, we report in even-numbered columns of Table 4 models for the main outcomes that use a limited information maximum likelihood (LIML) estimator in place of 2SLS. Unlike 2SLS, LIML is approximately unbiased in the case of weak instruments (Angrist, Imbens, and Krueger 1999; Angrist and Krueger 2001). For either set of dummy instrumental variables—contractor-by-year dummies or contractor dummies—LIML point estimates are closely comparable to their 2SLS counterparts, while standard errors are about 50 percent larger. Thus, weak instruments do not appear to be a concern.

V. Interpreting the Findings: Job Transitons and Employment Stability

Why do temporary-help and direct-hire placements yield such divergent impacts on subsequent earnings and employment? In this section, we analyze job transitions

direct-hire job placements are estimated using the full set of contractor-by-year instruments in each model, the resulting point estimates continue to indicate that direct-hire placements have large positive impacts on earnings and employment (comparable to the main models in Table 3), while temporary-help placements have small and insignificant effects on these margins. Given the significant positive correlation between temporary-help and direct-hire placement rates, these results suggest that "bad contractors" cannot be responsible for the lack of beneficial impacts of temporary-help job placements on participant outcomes. If bad contractors were responsible, these adverse effects should load onto both direct-hire and temporary-help point estimates in these by-placement-type models given the positive correlation between direct-hire and temporary-help placement rates.

Table 4—Comparison of Alternative Instrumental Variables and Estimators for the Effect of Job Placements on Employment and Earnings Quarters 2–8 Following Work First Assignment

	IVs: Contractor by year dummies		IVs: Contractor dummies	
	2SLS (1)	LIML (2)	2SLS (3)	LIML (4)
Panel A. Quarterly earnings				
Direct-hire job placement	493**	518**	547*	569**
	(147)	(141)	(243)	(182)
Temp-help job placement	−235~	−314	−345*	−392
	(123)	(212)	(164)	(245)
Constant	990**	988**	980**	976**
	(53)	(51)	(83)	(65)
R^2	0.24	0.23	0.23	0.23
H_0:Temp=Direct	0.00	0.00	0.01	0.01
Panel B. Quarterly employment				
Direct-hire job placement	0.15**	0.16**	0.15*	0.16**
	(0.04)	(0.03)	(0.07)	(0.04)
Temp-help job placement	−0.03	−0.06	−0.08*	−0.10~
	(0.03)	(0.05)	(0.04)	(0.06)
Constant	0.42**	0.42**	0.42**	0.42**
	(0.01)	(0.01)	(0.02)	(0.02)
R^2	0.18	0.17	0.17	0.16
H_0:Temp=Direct	0.00	0.00	0.02	0.00

Notes: $N = 37{,}161$. Robust standard errors in parentheses are clustered on Work First contractor (33 clusters). Odd-numbered columns contain two-stage least squares estimates. Even-numbered columns contain limited information maximum liklihood (LIML) estimates. The instrument in columns 1 and 2 is the assigned contractor by year and in columns 3 and 4 the assigned contractor. Sample and specification are otherwise identical to prior tables.

~Significant at the 0.10 level.
*Significant at the 0.05 level.
**Significant at the 0.01 level.

following Work First program entry to explore the central link between job placements and subsequent employment and job stability.

The objective of Work First job placements is to foster sustained employment. Ideally, participants placed into jobs during the program would remain in those jobs indefinitely or would change employers with little or no interruption to employment. As a descriptive matter, the bulk of earnings among our sample of Work First participants during the period following contractor assignment derives from continuous employment with a single employer. Among Work First participants with any earnings in the second through eighth post-assignment quarters, the average ratio of earnings from the longest-held job to total earnings was 77 percent. Given this fact, we conjecture that a central reason why direct-hire job placements increase participants' subsequent employment and earnings is that they foster stable employment, either because these placements are often durable or because they frequently serve as stepping stones into other employment that proves stable. Because temporary-help jobs are intrinsically short-lived, temporary-help job placements clearly will not offer durable employment. Nevertheless, they may foster stable employment if

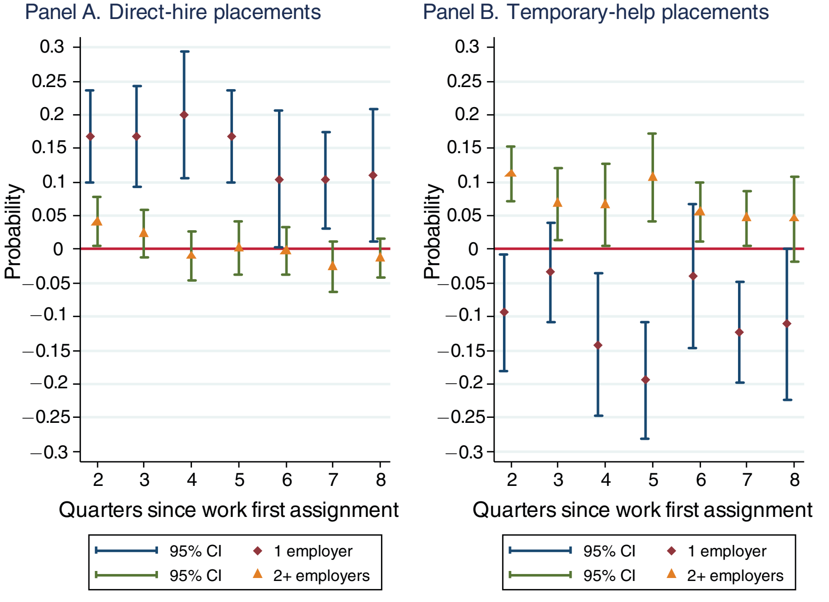

Figure 4. Two-Stage Least Squares Estimates of the Effect of Direct-Hire and Temporary-Help Job Placements on Probability of Holding One or Two-Plus Jobs in Quarters 2–8 Following Work First Contractor Assignment

Notes: Estimates for each outcome (one employer, two-plus employers) are from separate 2SLS regressions of the indicated outcome variable for the relevant quarter on the direct-hire and temporary-help job placements instrumented by contractor-by-year of assignment. Confidence intervals are estimated with robust standard errors that are clustered on contractor assignment.

they serve as stepping stones into other durable jobs. We next explore the degree to which direct-hire and temporary-help job placements in Work First lead to stable employment.

We begin with an examination of the effect of job placement on the number of employers in a quarter.[28] To assess the effects of job placement on multiple job holding, we estimate a set of 2SLS linear models for the probability that participants hold employment with either a single employer or multiple employers (with no employer as the residual category) separately for each quarter in our follow-up period. These estimates, summarized in Figure 4, reveal that direct-hire placements significantly raise the probability that participants work for a single employer (though not necessarily the same employer) in each of the seven quarters. This effect is substantial, ranging from 11 to 20 percentage points. Direct-hire placements also raise the probability that participants work for multiple employers in the second post-assignment quarter, suggesting an initial increase in job shopping or churn. But this effect becomes insignificant by the third quarter, and the point estimate is

[28] The UI data do not show when within a quarter a job is held, so when the UI data record multiple employers for an individual during a quarter, it is impossible to tell whether that individual is working multiple jobs at the same time or whether the jobs are held sequentially. To the extent that it reflects the latter, having multiple employers in a given quarter is an indicator of job churn.

essentially zero thereafter. Thus, direct-hire placements lead to a near-term increase in multiple job-holding, and a near and longer-term increase in single job-holding.

By contrast, Figure 4 reveals that temporary-help placements significantly raise the probability that participants work for multiple employers in six of the seven post-assignment quarters. Simultaneously, they significantly reduce the probability that participants work for a single employer in four of seven quarters and, in net, have no significant effect on the probability that participants have any employment in a quarter (see also Figure 3). To the extent that multiple job holding reflects job changes (and not second jobs), these results offer an explanation for the surprising finding that temporary-help placements may reduce earnings on balance (Table 3): temporary help placements appear to decrease job stability in net, and so participants' earnings may suffer because of gaps in employment between job spells.

The sharp differences in the effects of direct-hire and temporary-help placements on patterns of single and multiple job holding broadly suggest that stable jobs play an important role in improving employment and earnings outcomes.[29] We drill down on job stability further by using the unique employer identifiers in the UI data to study the effect of placements on ongoing employment and earnings in particular jobs—or more precisely, in ongoing job spells with particular employers. For each participant, we code earnings and employment in the longest held job observed in the seven quarter follow-up period. We use the IV model to estimate how job placements affect the duration and earnings in this spell.[30]

Table 5 illustrates the centrality of long job spells to the earnings and employment effects of direct-hire placements. Of the \$493 total effect of a direct-hire placement on quarterly earnings over quarters 2–8, \$398 derives from increased earnings in the longest spell. Similarly, of the 15 percentage point gain in average quarterly employment probability, 11 percentage points accrue in the longest spell. Consistent with earlier results, temporary-help placements are not found to foster long job spells. Instead, these placements are found to reduce tenure and earnings in the longest job spell. (The adverse earnings effect of \$297 per quarter is statistically significant.) Notably, the \$310 estimated reduction in earnings in the longest held job is *larger* than the estimated net earnings loss from a temporary-help placement of \$235 per quarter, indicating that participants placed in temporary-help jobs partly compensate for increased instability through greater employment and earnings in other jobs.

How much of the total earnings impact of a job placement derives from earnings in the specific job in which the participant is placed and how much from other job spells that are fostered by the placement? To make this assessment, we define the "exit job" for those who receive a placement during Work First as the job in which the participant is placed. For participants who leave the program without a placement, we define the exit job as the longest-held job obtained in the quarter of exit. By construction, earnings in the exit job in quarters two onward will be zero if the exit job spell has ended prior to the second quarter. We use the IV model to estimate

[29] The estimates in Figure 4 are consistent with direct-hire/temporary-help placements increasing/reducing on-going employment with a single employer (i.e., job stability). However, the positive effects of temporary-help placements on multiple job holding could reflect an increase in second jobs, not job switching.

[30] Where participants have multiple jobs of the same length (in quarters), we break ties by using the highest earnings spell.

Table 5—Instrumental Variables Estimates of the Effect of Work First Job Placements on Earnings and Employment over Quarters 2–8 Following Work First Assignment: Overall, in Longest Job Spell, and in Exit Job

	Panel A. Earnings			*Panel B. Employment*		
	All (1)	Longest job spell (2)	Exit job spell (3)	All (1)	Longest job spell (2)	Exit job spell (3)
Direct-hire placement	493**	398**	213**	0.15**	0.11**	0.08**
	(147)	(133)	(59)	(0.04)	(0.04)	(0.01)
Temp-help placement	−235~	−310**	−53	−0.03	−0.04	0.00
	(123)	(110)	(138)	(0.03)	(0.02)	(0.03)
Constant	990**	777**	209**	0.42**	0.30**	0.07**
	(53)	(46)	(24)	(0.01)	(0.01)	(0.01)
R^2	0.24	0.19	0.11	0.18	0.14	0.09
H_0: Temp = Direct	0.00	0.00	0.09	0.00	0.00	0.06

Notes: $N = 37{,}161$. Robust standard errors in parentheses are clustered on Work First contractor (33 clusters). Each column corresponds to a separate 2SLS regression. A job spell is a set of contiguous quarters with earnings from the indicated employer. The exit job refers to the placement job for those placed into employment while in Work First and to the longest job obtained in the quarter of exit for those not placed while in Work First. Sample and specification are identical to prior tables.

~Significant at the 0.10 level.
*Significant at the 0.05 level.
**Significant at the 0.01 level.

the impact of placements on earnings and employment in the exit job. Implicitly, these models estimate the difference in earnings and employment stemming from the Work First placement job relative to the job that the participant would (in expectation) have found on her own.

One complication for this approach is that if participants hold multiple jobs in the quarter of job placement or program exit (that is, there are multiple candidate ‘exit jobs’), we cannot precisely identify which job is the placement job or the first job taken.[31] In these circumstances, we define the exit job as the longest job commencing in the quarter of job placement for those placed into jobs during Work First, and, analogously, as the longest job observed in the quarter of exit for those not placed.[32]

Instrumental variables estimates for exit job earnings and employment in Table 5 show that direct-hire placements raise earnings in the exit job by $213 per quarter and increase the probability of ongoing employment in that job by 8 percentage points. These effects represent about half (43 percent and 53 percent, respectively) of the overall earnings and employment gain from a direct-hire placement. What accounts for the other half of the gain? Since the complement of employment and earnings in the “exit job” is employment and earnings in all other jobs, we conclude that this “other half” is attributable to the stepping-stone effect of direct-hire placements on further employment.

[31] The textual employer names in the Work First data cannot be matched to the numeric employer ID’s in the UI data.

[32] Our definition of the longest spell does not count interrupted spells. Thus, for instance, if we observe earnings from the exit employer in quarter 3, but not in quarter 2, exit job earnings and employment are still set equal to zero.

Table 6—OLS and Instrumental Variables Estimates of the Effect of Work First Job Placements on Earnings by Sector over Quarters 2–8 Following Work First Assignment: Direct-Hire and Temporary Help Jobs

	All earnings		Direct-hire earnings		Temp-help earnings	
	Qtrs 2–4 (1)	Qtrs 5–8 (2)	Qtrs 2–4 (3)	Qtrs 5–8 (4)	Qtrs 2–4 (5)	Qtrs 5–8 (6)
Panel A. OLS						
Direct-hire job placement	593**	455**	582**	438**	5	10~
	(22)	(23)	(21)	(22)	(5)	(5)
Temp-help job placement	492**	343**	187**	215**	293**	122**
	(33)	(31)	(18)	(25)	(25)	(14)
Constant	817	1001	702	885	106	104
	(11)	(11)	(10)	(10)	(4)	(4)
R^2	0.23	0.21	0.20	0.18	0.07	0.04
H_0: Temp=Direct	0.01	0.00	0.00	0.00	0.00	0.00
Panel B. 2SLS						
Direct-hire job placement	577**	430**	496**	427**	89	−7
	(149)	(153)	(126)	(154)	(65)	(36)
Temp-help job placement	−246~	−228	−427**	−156	157*	−56
	(127)	(143)	(113)	(160)	(70)	(45)
Constant	891	1064	791	924	87	127
	(54)	(56)	(43)	(55)	(28)	(15)
R^2	0.21	0.20	0.19	0.18	0.05	0.03
H_0: Temp=Direct	0.00	0.00	0.00	0.02	0.23	0.31

Notes: $N = 37{,}161$. Robust standard errors in parentheses are clustered on Work First contractor (33 clusters). Each column corresponds to a separate OLS or 2SLS regression. Instrumental variables for jobs obtained (any, direct-hire and temporary-help) are contractor by year of assignment dummies. Sample and specification are identical to prior tables.

~ Significant at the 0.10 level.
* Significant at the 0.05 level.
** Significant at the 0.01 level.

Turning to analogous estimates for temporary-help placements, in Table 5, we find that these placements neither improve nor diminish outcomes in the exit job. This finding is consistent with the hypothesis that temporary-help jobs are not scarce for the marginal Work First participant. However, the lack of long-term employment and earnings benefits from temporary-help job placements (Table 3) also implies that these jobs do not foster transitions into other employment—that is, they do not serve as stepping-stones.

Table 6 confirms this hypothesis. In this table, we estimate the impact of temporary-help and direct-hire placements separately on earnings from temporary help employment and direct-hire employment in post-assignment quarters two through eight.[33] The top panel of the table reports OLS models. These descriptive estimates show, as expected, that participants who take a temporary-help job during Work First have higher average quarterly earnings and employment rates in both temporary-help *and* direct-hire employment in the follow-up period. We read this as confirmation of selection bias: participants placed into temporary-help jobs have

[33] Analogous estimates for employment, reported in Web Appendix Table B, are qualitatively similar to the results for earnings in Table 6.

comparatively strong employment prospects relative to participants who are not placed into a job during Work First. Because participants who take temporary-help jobs during Work First typically have substantial earnings in direct-hire jobs thereafter, one naïve reading of these results is that temporary-help jobs lead to direct-hire employment.

The IV estimates in the bottom panel of Table 6 do not support this interpretation. Temporary-help placements induced by contractor assignment significantly raise subsequent earnings in temporary help jobs, echoing the finding that temporary-help placements increase multiple job holding within quarters (Figure 4).[34] At the same time, temporary help jobs significantly crowd out earnings in direct-hire employment. In post-assignment quarters 2–4, temporary-help placements increase quarterly earnings in temporary-help jobs by $157, but reduce quarterly earnings in direct-hire jobs by $427. In follow-up quarters five through eight, temporary-help placements significantly affect earnings in neither temporary-help nor direct-hire jobs.

Finally, consistent with earlier findings, IV estimates show that direct-hire placements significantly increase earnings in direct-hire jobs throughout the seven quarter follow-up period, though the effect is not as large in the second year as the first. Direct-hire job placements have no effect on subsequent earnings in the temporary-help sector, however. By implication, direct-hire placements increase subsequent employment outside the "exit" job by raising employment in other direct-hire jobs.

VI. Conclusion

Our analysis yields two primary findings. Direct-hire placements induced by the rotational assignment of Work First participants to contractors significantly increase subsequent payroll earnings and employment. The increase in earnings, which amounts to almost $3,500 over a seven quarter follow-up period, is economically large, representing a 50 percent earnings gain. In contrast, temporary-help placements fail to improve employment outcomes, and, on net, may even moderately lower earnings over the follow-up period. Thus, despite much descriptive evidence to the contrary, our analysis indicates that temporary-help placements have no net beneficial effect for the earnings, employment, and labor market advancement of low-skilled workers.

The link between job placements and job stability is central to understanding the disparate impacts direct-hire and temporary-help placements have on subsequent employment outcomes. Direct-hire placements generate durable earnings and employment effects by fostering stable employment; on average, the placement jobs themselves are relatively durable and further serve as a stepping stone into stable jobs. By contrast, temporary-help placements, on average, reduce subsequent job stability by fostering greater job churn and, at least initially, raising employment in the temporary-help sector at the expense of opportunities in direct-hire employment.

[34] Because temporary-help jobs are intrinsically short-term, an increase in temporary-help employment should be accompanied by an increase in job turnover.

We find no evidence that temporary-help placements provide a port of entry into stable employment.

These findings are pertinent to the economics literature on active labor market programs designed to improve employment and earnings among low-skilled workers. Large-scale random assignment experiments conducted with welfare-to-work and adult disadvantaged populations in the 1990s generally found that, compared to more costly intervention strategies, job placement services were as effective or more effective at improving subsequent labor market outcomes. On-going random assignment experiments at 15 sites in 8 states are currently assessing the efficacy of various strategies that are intended to address persistent problems of job instability and lack of advancement in the welfare population (Bloom et al. 2005). Studies in this vein typically assess the net effect of various program features—in addition to job search assistance—on Work First participant outcomes.[35] Our study is the only analysis of which we are aware that directly assesses causal effects of job placement, per se, on the recipients who receive them. This distinction proves important here. Although, consistent with the experimental literature, we find that job placements significantly improve long-term employment and earnings outcomes on average, the analysis also reveals that the benefits of job placement services derive entirely from placements into direct-hire jobs.

We emphasize that our results pertain to the marginal temporary-help job placements induced by the randomization of Work First participants across contractors. They therefore do not preclude the possibility that infra-marginal temporary-help placements generate significant benefits. Our findings are nevertheless 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-help positions. Our analysis suggests not. While participants placed in direct-hire jobs benefit substantially, workers induced to take temporary-help jobs by contractor assignments are no better off than they would have been without any job placement. Putting greater emphasis on placing participants in direct-hire jobs appears to be a more promising approach for increasing earnings and employment stability in this population.

[35] Bloom et al. (1997) summarizes the results from 16 random assignment studies of the efficacy of services provided to participants in JTPA Title II-A programs, which serviced disadvantaged adults. Table 4 of that study compares the estimated effects of programs that rely on classroom training compared to programs that provide job placement and on-the-job training services. Bloom and Michalopoulos (2001) summarize the results from a series of studies of welfare initiatives, all of which used random assignment research designs. These studies included analysis of the impact on annual earnings of programs emphasizing job search first and programs emphasizing education first.

Appendix

Table 1—*P*-Values of Tests of Random Assignment of Participant Demographic Characteristics and of Equality of Job Placement Probabilities across Work First Contractors within Randomization Districts, 1999–2003

District	1999–2000	2000–2001	2001–2002	2002–2003	All years
Panel A. Test of covariate balance: P-value (N)					
I	0.52 (1,863)	0.35 (1,462)	0.13 (2,006)	0.38 (717)	0.21 (6,048)
II	0.10 (720)	0.14 (1,380)	0.10 (1,589)	0.95 (634)	0.13 (4,323)
III	0.65 (708)	0.01 (498)	0.07 (1,042)	0.34 (332)	0.02 (2,580)
IV	0.23 (1,412)	0.31 (1,384)	0.33 (1,423)	0.95 (715)	0.48 (4,934)
V	n/a	n/a	0.34 (923)	0.81 (642)	0.64 (1,565)
VI	0.12 (954)	0.55 (954)	0.44 (957)	0.58 (436)	0.41 (3,301)
VII	0.80 (807)	0.98 (682)	0.73 (932)	0.65 (476)	0.98 (2,897)
VIII	n/a	n/a	0.35 (1,102)	0.18 (382)	0.21 (1,484)
IX	0.79 (697)	0.66 (145)	n/a	n/a	0.84 (842)
X	0.89 (794)	0.85 (849)	0.99 (784)	0.76 (419)	1.00 (2,846)
XI	0.86 (690)	0.92 (527)	0.63 (372)	n/a	0.96 (1,589)
XII	0.66 (676)	0.25 (1,484)	0.49 (1,614)	0.08 (978)	0.18 (4,752)
All	0.63 (9,321)	0.34 (9,365)	0.18 (12,744)	0.76 (5,731)	0.44 (37,161)
Panel B. Test of equality of job placement probabilities by contract-year within districts: P-value (N)					
I	0.00 (1,863)	0.00 (1,462)	0.00 (2,006)	0.00 (717)	0.00 (6,048)
II	0.22 (720)	0.00 (1,381)	0.00 (1,589)	0.00 (634)	0.00 (4,323)
III	0.12 (708)	0.01 (498)	0.00 (1,042)	0.02 (332)	0.00 (2,580)
IV	0.00 (1,412)	0.00 (1,384)	0.00 (1,423)	0.15 (715)	0.00 (4,934)
V	n/a	n/a	0.39 (923)	0.26 (642)	0.31 (1,565)
VI	0.00 (954)	0.00 (954)	0.01 (957)	0.00 (436)	0.00 (3,301)
VII	0.06 (807)	0.93 (682)	0.00 (932)	0.00 (476)	0.00 (2,897)
VIII	n/a	n/a	0.00 (1,102)	0.00 (382)	0.00 (1,484)
IX	0.00 (697)	0.69 (145)	n/a	n/a	0.00 (842)
X	0.02 (794)	0.25 (849)	0.00 (784)	0.00 (419)	0.00 (2,846)
XI	0.24 (690)	0.05 (527)	0.19 (372)	n/a	0.06 (1,589)
XII	0.00 (676)	0.00 (1,484)	0.00 (1,614)	0.00 (978)	0.00 (4,752)
All	0.00 (9,321)	0.00 (9,365)	0.00 (12,744)	0.00 (5,731)	0.00 (37,161)

Notes: Panel A: Each cell provides the *p*-value from a Seemingly Unrelated Regression for the null hypothesis that the 10 main sample covariates are balanced across participants assigned to Work First contractors within the relevant assignment district and year cell. Covariates tested are sex, white or Hispanic race, other race, age and age-squared, total quarters employed and total earnings in eight quarters prior to Work First assignment, total quarters employed in temporary help work and total temporary help earnings in eight quarters prior to Work First assignment. Right-hand 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 test statistic 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. Panel B: Each cell gives the *p*-value for the null hypothesis that placement rates are comparable across contractors within a district-year cell. 2-way tests compare overall job placement probabilities (any placement versus no placement). 3-way tests compare placement probabilities into direct-hire, temporary-help and non-employment simultaneously. Sample is identical to panel A.

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