12-2002

Do Temporary Help Jobs Improve Labor Market Outcomes? A Pilot Analysis with Welfare Clients

David H. Autor
Massachusetts Institute of Technology

Susan N. Houseman
W.E. Upjohn Institute for Employment Research, houseman@upjohn.org

Authors
David H. Autor, *Massachusetts Institute of Technology*
Susan N. Houseman, *W.E. Upjohn Institute for Employment Research*

Upjohn Author(s) ORCID Identifier
https://orcid.org/0000-0003-2657-8479

Do Temporary Help Jobs Improve Labor Market Outcomes?
A Pilot Analysis with Welfare Clients

David H. Autor
MIT and NBER

Susan N. Houseman
W.E. Upjohn Institute for Employment Research[1]

December 2002

Abstract

We draw upon administrative data from an unusual policy experiment in the state of Michigan to study the effects of temporary agency employment among welfare-to-work clients on job retention, program recidivism, and earnings. To identify these effects, we exploit the fact that welfare-to-work clients in one Michigan county were randomly assigned to service providers who had substantially different placement rates in temporary agencies but otherwise similar policies. Our findings indicate that moving welfare clients who otherwise would have been unemployed into temporary agency jobs provides some benefits to these workers, primarily by increasing their short-term earnings. Temporary agency jobs also may slightly reduce program recidivism in the first year. However, these jobs do not appear to help these clients attain steady employment–defined as 90 days of continuous employment–nor do they reduce program recidivism over the longer term.

[1] This research was supported by the Russell Sage Foundation and the Rockefeller Foundation. We thank seminar participants at the MIT Sloan School, the Upjohn Institute, and the University of Michigan for valuable suggestions. We are indebted to Lillian Vesic-Petrovic for superb research assistance and to Anne Schwartz for assistance with the data.

Introduction

The temporary help industry became a leading port of labor market entry for welfare recipients during the 1990s. Recent analyses of state administrative welfare data reveal that 15 to 40 percent of former welfare recipients who obtained employment in the years following welfare reform (PRWORA, 1996), took jobs in the temporary help sector.[1] These numbers are especially striking in light of the fact that the temporary help industry accounts for less than 3 percent of average U.S. daily employment.

The substantial overrepresentation of welfare clients in temporary help positions likely reflects a confluence of three forces. First, the temporary help industry underwent rapid demand growth throughout the 1990s–accounting for 10 percent of net U.S. job creation during the decade–and consequently would have accounted for a much larger fraction of employment among new hires than among the all workers. Second, the industry's growth was concentrated in low-paying service and production positions requiring relatively little education and training–jobs for which many welfare recipients qualify. Third, welfare reform's stated policy goal of moving welfare claimants rapidly into the workforce made temporary help agencies, which specialize in quickly matching workers with employers, a natural starting point for their job search.

Although it is easy to understand why temporary help agencies played a central–if unanticipated–role in the welfare-to-work process, far less obvious is whether welfare recipients' integration into the labor market was abetted or hindered by their temporary help employment positions. That is the question we address in this study. Drawing on unique data from a welfare-to-work program–Work First–in one Michigan county, we provide quasi-experimental evidence on how temporary help jobs affect the short-term earnings and longer-term independence of welfare clients who otherwise would have spent additional time searching for direct-hire positions.[2] Work First clients in this county were randomly assigned to Work First service providers. We study Work First spells at two service providers that had

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

different amounts of contact with temporary agencies and thus had systematically different rates of placement in temporary agency jobs. Importantly, the placement rates in direct-hire jobs did not differ across the two providers, nor did the job search and other services offered by these providers. These features of our data allow us to identify the marginal effects on client outcomes of obtaining a temporary agency job while in the Work First program relative to not obtaining employment in the program.

Our analysis indicates that temporary help jobs appear to benefit welfare clients who otherwise would have been non-employed, primarily by increasing their short-term earnings. Notably, the wages of marginal temporary workers are significantly below–by about 15 percent–those of infra-marginal temporary workers, suggesting that workers encouraged from non-employment into temporary employment by their program assignment had lower human capital than average temporary workers. The evidence in favor of an enduring impact on welfare clients' labor market attachment is less supportive, however. On the margin, temporary agency jobs appear neither to help Work First clients retain steady employment–defined as 90 days of continuous employment–nor to reduce program recidivism over the longer term. A caveat is that we are unable to track earnings progression—the outcome of greatest interest—in our data, and thus our findings on the longer-term consequences of temporary employment should be interpreted as tentative.

The paper proceeds as follows. Section 1 discusses contrasting hypotheses on the effects of temporary employment on subsequent employment outcomes, reviews methodological approaches that have been used to distinguish between these hypotheses, and outlines the approach we employ. Section 2 provides background information on Michigan's Work First program and discusses our administrative data. Section 3 details our econometric methodology, including the assumptions needed to identify the effects of temporary employment. Section 4 provides estimates of the effects of temporary employment on short-term earnings, and section 5 presents results for job retention and program recidivism. Section 6 summarizes the findings and discusses future work.

[2] We study the consequence of temporary help employment for individual workers but do not address the general equilibrium question of how the growth of the temporary help sector affects the overall distribution of employment opportunities and

1. Hypotheses and Empirical Approaches

a. Hypotheses

The consequences of temporary agency employment for the industry's workers have been a subject of controversy for at least a decade. Two plausible, but opposing, hypotheses of how temporary agency jobs may impact the career trajectories of welfare recipients and other low-skilled workers have been articulated.

One hypothesis is that because temporary help agencies typically face lower screening, and termination costs than other employers (Autor, 2003), they may hire individuals who otherwise would have been unemployed (Houseman, Kalleberg, and Erickcek, forthcoming; Autor and Houseman, 2002). Accordingly, spells in temporary help may reduce the time workers spend in unproductive, potentially discouraging job search, and facilitate rapid entry into employment. Temporary assignments may further permit workers to develop human capital and labor market contacts that lead, directly or indirectly, to longer-term jobs. Two pieces of evidence support the plausibility of this hypothesis. One is that the vast majority of temporary help workers leave the sector within one year, indicating high mobility (Segal and Sullivan, 1997). Second, a large and growing number of employers use temporary help assignments as a means to screen workers for direct-hire jobs (Abraham, 1988; Autor, 2001; Houseman 2001, Kalleberg et al. 2000), a practice that appears particularly prevalent in the manufacturing sector (Ballantine and Ferguson, 1999). Consistent with these observations, we find in our administrative data from the state of Michigan (described in detail below) that over 50 percent of temporary help positions taken by welfare recipients are in relatively high paying production positions–in contrast to fewer than 5 percent of direct-hire positions (see Figure 1). It thus appears conceivable that temporary agency employment may provide an 'on-ramp' into the labor market that improves short-term–and potentially long-term–work outcomes for individuals with poor labor market prospects.

---

outcomes, including for those not employed in the sector.

A contrasting but coherent alternative hypothesis is that the unstable and primarily low-skilled placements offered by temporary help agencies provide little opportunity or incentive for workers to invest in human capital or develop productive job search networks (Jorgenson and Riemer, 2000). In support of this view, several researchers document that workers in temporary jobs receive on average lower pay and fewer benefits than would be expected in direct-hire jobs (DiNatalie, 2000; General Accounting Office, 2000; Segal and Sullivan, 1998). And while mobility out of the temporary help sector is high, a disproportionate share of leavers enters unemployment or exits the labor force (Segal and Sullivan, 1997). These facts would not be of great concern if temporary help jobs exclusively substituted for spells of unemployment. But to the degree that spells in temporary help crowd out productive direct-hire job search, they may inhibit longer-term labor advancement. Under this 'cul de sac' scenario, the short term gains accruing from nearer-term employment in temporary help jobs may be offset by employment instability and poor earnings growth.

b. Previous empirical approaches

Distinguishing among these competing hypotheses is an empirical challenge. The characteristics of workers in temporary help, direct-hire and non-employment statuses differ greatly; in the cross section, temporary workers have less education, weaker labor force attachment and lower earnings than direct-hire workers (Autor and Houseman, 2002). Although some characteristics, such as education and labor force history, can be directly controlled in a regression, others like motivation and job-preparedness will impact earnings but are unobservable in conventional data sources. Consequently, analyses of the effects of temporary help employment on labor market progression that do not take adequate account of these confounding factors are likely to be unconvincing and potentially biased.

Several methodologies have been used to address these selection issues. An obvious but unsatisfying approach is to treat job status as exogenous–equivalent to assuming that there are no unobservable earnings-related differences among temporary help, direct-hire, and non-employed groups. A second approach, used by Segal and Sullivan (1997) and Waldfogel and Ferber (1998), is to estimate regression models with individual fixed effects (or first differences) that sweep out time-invariant individual level characteristics that affect the *level* of earnings. However, because workers reentering the labor market after spells of unemployment, training, or welfare receipt, are likely to be on a steep earnings *trajectory* (Card and Sullivan, 1998), this approach is likely to be highly imperfect. The recent studies by Booth, Francesconi and Frank (2002) and Heinrich, Mueser and Troske (2002) implement two additional methods. Both studies exploit detailed earnings history and demographic data to potentially control for individual determinants of earnings that might otherwise bias regression estimates. In addition, each implements a set of parametric selection-correction or instrumental variables models to adjust for non-random selection of workers into temporary help versus direct-hire positions. What all of these studies have in common is that they draw exclusively on observational data to ascertain causal relationships. Consequently, their findings will depend critically on the methods used to correct the non-experimental data for self-selection into temporary help, direct hire, and non-employment.

c. Our quasi-experimental approach

To potentially overcome these confounds, we exploit a unique, multi-year policy experiment in one Michigan county. During the years of 1997 to 2000, Work First clients in this county were randomly assigned to several not-for-profit service contractors. We analyze data for two of these contractors. As documented below, these Work First contractors provided substantially similar client support services–yet generated quite different rates of temporary help employment among their randomly assigned clients. At 'Program 1,' case workers encouraged clients to seek employment with temporary help agencies and frequently invited agencies on-site to screen clients for jobs. By contrast, case workers at 'Program 2' generally viewed temporary agency positions as counterproductive and provided clients with far fewer direct contacts with temporary agencies.[3]

These policies led to substantial differences in the rate of temporary help job-taking among clients of these two programs. In the four years of data we analyze–corresponding to almost 3,000 client spells–14.6 percent of all spells at Program 1 (25.1 percent of those that resulted in employment) yielded temporary

[3] This information was obtained from on-site interviews with the staff of Program 2 and from discussions with the program officers who oversaw the contract with Program 1 (which is now defunct).

help positions. At Program 2, these numbers were 8.9 percent of all spells and 17.6 percent of those generating employment. These differences appear quite persistent. In all four years of data, temporary employment was 50 to 100 percent higher at Program 1 then Program 2–in all cases a significant difference. Importantly, the placement rates in direct-hire jobs did not differ across the two providers, either overall or in any given year.

These features of the Michigan policy experiment allow us to identify the consequences of temporary help employment for client outcomes. Specifically, we use a client's exogenous Work First program assignment as an instrumental variable that affects her propensity to obtain a temporary help position but is not otherwise correlated with her potential earnings and recidivism outcomes. In addition to the random assignment provided by the Michigan program, our analysis relies on two additional behavioral assumptions discussed below. First, we assume–and provide evidence to document–that the minimal services provided by Programs 1 and 2 did not affect client outcomes *except* though their impact on job taking propensities (e.g., temporary help, direct hire, or non-employment). Second, we assume that program assignment only affected the odds of temporary help employment for clients who otherwise would not have found employment during their Work First spell; by implication, workers who take direct-hire jobs are unaffected by program assignment. While this latter assumption is a strong one, we provide several pieces of supporting evidence, most notably that: 1) direct-hire placement rates never differed between Programs 1 and 2; and 2) conditional on direct-hire employment, wage and employment outcomes in the two programs were near-identical. Under these assumptions, our econometric framework shows that random assignment of clients to programs identifies the consequences of temporary employment for welfare clients who otherwise would not have obtained employment during their Work First spell. We refer to this group as 'marginal' temporary workers.

Applying this framework to the data, we study how temporary agency jobs affect three sets of employment outcomes for marginal temporary workers: short term earnings obtained during the Work First spell; 90-day job retention; and program recidivism up to two years following the start of a Work First spell.

2. The Michigan Work First Program

a. Context

The Michigan Family Independence Agency (FIA) determines the eligibility of Temporary Assistance for Need Family (TANF) applicants for cash payments and administers these payments. FIA must assign eligible applicants to the state's Work First program within 10 days of their application. The stated objective of the Work First program is to "move people off welfare rolls and onto payrolls." Thus, the program emphasizes job search skills in order to place individuals in jobs as quickly as possible. At Work First, welfare clients attend basic job search workshops that include training in how to write a resume, how to fill out a job application, and how to interview for a job. After clients receive these core job search services, they are required to search intensively–20 to 40 hours per week–for a job.[4] In-depth assessment and more extensive skill training typically are only available to those who hold a job already or who have found it extremely difficult to find and retain employment.

Clients are expected to secure employment within four weeks of entering the Work First program. Individuals failing to comply with the job search and finding requirements are terminated from the Work First program and risk reduction or termination of benefits from FIA. Individuals who remain in qualified employment for 90 days are (successfully) terminated from the Work First program. Two measures of success are used to evaluate Michigan Work First programs: the fraction of clients finding employment and the fraction of those finding employment that remain employed for 90 days. Individuals may change jobs within the 90-day period, but the gap between jobs must be 7 days or fewer.

Our analysis concentrates on the Work First program in one Michigan county from 1997 to 2000. During this time, program operations were subcontracted to three non-profit service provider organizations.[5] One provider serviced all two-parent cases. Critically for our purposes, single-parent cases, which comprise the majority of the case population, were randomly assigned in predetermined

[4] During the time period covered by our data, single-parent clients with children under six years of age were required to engage in 20 hours of work or job search activities per week; single parents with children over six were required to work or search for employment for 30 hours per week. In 2002, work requirements were increased from 20 to 40 hours per week for all clients, including single parents.

proportions among the three service providers.[6] We provide evidence on the efficacy of this randomization below.

b. Data and randomization

Our administrative data encompass all Work First case spells in the county from the period from January 1997 through August 2002. We exclude two-parent families from our sample because these were exclusively assigned to one Work First provider. We further limit the sample to women, who make up the overwhelming majority (95 percent) of the single-parent cases. This leaves a sample of 2,778 spells for 2,003 individuals.[7] For each spell, we observe basic demographic information on the client (age, race, and completed schooling), as well as the beginning and ending date of the spell and the reason for its termination (e.g., 90-day job retention, sanction for non-compliance, etc.).

We also exploit detailed information on clients' employment during each Work First spell, including the beginning and ending date of each job obtained, the starting hourly wage and weekly hours, the employer's name, and client's occupation. Clients are required to report any employment to Work First staff, who then contact the employer to confirm all recorded information. The collection of accurate data on clients' employment and earnings is given high priority because this information is used to determine clients' welfare eligibility and the amount of any welfare payment. We coded jobs as temporary or direct-hire jobs based on the employer name.[8] By longitudinally linking welfare records, we are able to identify repeat spells in this county among Work First clients during the period of study, permitting us to study program recidivism for a minimum of two years following the client's previous program assignment.[9]

[5] The Michigan Work First program is administered locally by 26 Workforce Development Boards.

[6] For a two-year period, a pilot study was conducted in which half of the single-parent Work First clients were assigned by client characteristic to specific providers, while the other half continued to be randomly assigned. In our study, we included only clients who had been randomly assigned. Assignment to either the targeted or random assignment group during this period was itself random, and so the population of clients included in our analysis is fully representative of the client population in all years.

[7] The distribution of spells among clients is: 1,508 clients with 1 spell; 370 clients with 2 spells; 109 clients with 3 spells; 31 clients with 4 spells; 11 clients with 5 spells; and 4 clients with 6 spells. The econometric analysis in this preliminary paper does not adjust standard errors for the repeat occurrence of individuals in the sample. This will be corrected in the next version.

[8] In almost all cases, the employer was readily identifiable as a temporary agency or direct-hire employer. In a few cases, we telephoned the employer or contacted a Work First service provider for clarification on the nature of an employer's business.

[9] To allow the two year follow up window, we limit our sample to Work First spells originating on or before August 15, 2000. Spells in the subsequent two years are coded as recidivism but do not generate a new spell record in our analysis database.

The two programs on which we focus ('Programs 1 and 2') that have been found to provide similar (minimal) job search training and other support services to Work First clients. In particular, a detailed study of services given by these programs found that both programs engaged clients on average in fewer than 12 hours of assessment and employability training (11.2 and 7.3 hours for Programs 1 and 2 respectively) and that each provided 20 or more hours of service to slightly more than one-fourth of all clients (Eberts, 2002).[10] Following orientation, the primary service performed by each program was to monitor clients to ensure that they engaged in at least 20 hours of job search weekly, often using the telephone facilities provided at the program offices.

We test the efficacy of program randomization in Table 1. Panel A compares key demographic characteristics of 2,778 client spells assigned to Programs 1 and 2 over the period from January 1997 to August 2000. Examining race, age, and education characteristics from the administrative data, we find an extremely close demographic match across programs. To formally test for non-random assignment, Panel B present OLS models where client demographic variables were regressed on dummy variables for year-by-quarter of program assignment and a dummy variable indicating assignment to Program 1. If a particular demographic group was differentially assigned to either program, the Program 1 dummy variable would estimate this conditional mean difference. These models find only minute differences in demographic variables across programs, and these differences are insignificant.[11]

c. Program effects on the prevalence of temporary help employment

Welfare programs have historically eschewed temporary help positions because these jobs were typically deemed to offer unstable employment. More recently, due to philosophical shifts, the pressure for rapid placement of clients in jobs generated by welfare reform, and shifts in the industry toward more "temp-to-hire" placement services, many Work First programs have modified these policies. Many programs now encourage clients to seek temporary agency positions and some invite temporary agencies

[10] We exclude a third service provider that gave its clients much more intensive screening (Eberts, 2002) and displayed significantly higher rates of successful termination and significantly lower rates of recidivism within each employment outcome than the other programs.

to program offices to conduct interviews (Autor and Houseman, 2002). As discussed above, the two Work First programs in our experimental analysis differed substantially in the degree to which they encouraged clients to seek contacts with temporary employment agencies.[12]

Table 2 compares the frequency with which clients in Programs 1 and 2 attained three employment statuses–temporary help employment, direct-hire employment, and non-employment–during their Work First spells.[13] As is visible in the first row, the rate at which clients in Program 1 obtained temporary help employment is far higher than at Program 2: 14.6 percent versus 8.9 percent (t = 4.4) of all clients took temporary help jobs, and 25.1 versus 17.7 percent of clients obtaining employment took temporary help jobs. Notably, there is no significant difference in the rate of direct-hire employment among the two client populations (t = 1.0). Subsequent rows of the table compare employment status by program for each of the four years of our data. In all years, temporary help employment is 50 to 100 percent higher at Program 1 than 2, and in all cases, this difference is significant. By contrast, direct-hire employment never differs significantly between programs, nor is there a consistent sign to the year-to-year contrast between direct-hire frequencies at the two programs.

Table 3 presents a regression test of the impact of program assignment on temporary help and direct-hire job outcomes of Work First clients. We regress a dummy variable indicating temporary help (panel A) or direct-hire (panel B) employment on demographic controls (high school dropout, non-white, high school dropout times non-white, age, and age-squared), year-by-quarter of assignment dummies and a dummy indicating Program 1 assignment. Consistent with the unadjusted means in Table 2, the regression models indicate that clients randomly assigned to Program 1 were on average 6.5 percentage points more likely to obtain temporary help jobs during their Work First spell than Program 2 clients (t = 4.6). The

---

[11] We also estimated models comparing race by education categories across programs. These also yielded no significant differences.

[12] Besides providing more on-site contacts with temporary agencies, Program 1 also gave incentive pay to staff tied to the number of successful job placements. These incentives likely motivated staff to foster contacts with temporary agencies and to encourage clients to seek these positions.

[13] Employment outcomes refer to any employment held during the Work First spell, regardless of duration. If a client obtained both temporary and direct-hire employment during a spell, we code their employment as temporary employment. Only 4 percent of spells fell into this category.

close comparability of the regression estimate in Table 3 with the comparison of means in Table 2 further testifies to the efficacy of the random assignment: because covariates are well balanced between the two treatment groups, they have little impact on the point estimates. The corresponding regression model for direct-hire employment in panel B finds a minute 0.004 percentage point cross-program difference in the probability that a Work First spell yields a direct-hire job (t = 0.02).[14] On average, program assignment appears to only affect client's temporary agency employment propensity.

To test whether program assignment had differential effects on employment propensities by demographic group, a second regression specification in each panel interacts the Program 1 assignment variable with indicators for non-white race and less than high school education. These interaction terms provide no strong evidence of differential impacts by race or education.

Given the efficacy of randomization and the large and persistent cross-program differences in temporary help employment frequencies in all four years of our data, it appears clear that the higher level of temporary help employment of Program 1 versus Program 2 during 1997 to 2000 was a causal result of program assignment. We next discuss how this random assignment structure can be used to test the causal impact of temporary help employment on wage and employment outcomes of clients whose temporary help status was altered by program assignment.

3. Using Randomization to Study the Impact of Temporary Help Employment on Client Outcomes

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

[14] Logit models yield comparable marginal probability estimates.

Formally, define each Work First client's potential outcome set as the triple $Y_i \equiv \{Y_i^u, Y_i^t, Y_i^d\}$, where the superscripts t, d, u correspond to clients' potential outcomes after obtaining temporary help employment, direct-hire employment, or no employment during the Work First treatment period. There are two parameters of interest for our analysis, $\delta^{tu} = E[Y^t - Y^u]$ and $\delta^{td} = E[Y^t - Y^d]$. The first parameter is the causal effect of temporary help relative to unemployment, and the latter is the causal effect of temporary relative to direct-hire employment.

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

$$\begin{aligned} &E[Y \mid J = t] - E[Y \mid J = u] = E[Y^t \mid J = t] - E[Y^u \mid J = u] \\ &\quad = E[Y^t - Y^u \mid J = t] + \{E[Y^u \mid J = t] - E[Y^u \mid J = u]\}, \end{aligned} \tag{1}$$

In this equation, the first term in the bottom row is the causal effect of temporary employment relative to non-employment among clients who attained temporary help employment (the parameter of interest). The second term is the average difference in potential outcomes *conditional on non-employment* for the client group that obtained temporary help employment relative to the client group that did not. We would generally presume that this latter term is positive; clients who obtained temporary employment while in the Work First program would have fared on average better than those who remained unemployed regardless. Consequently, estimates of equation (1) will be upward biased.

To purge this bias requires a source of variation that influences clients' employment status but is independent of potential outcomes. The candidate instrument Z that we use is clients' assignment to Work First programs (i.e., Program 1 or 2). In our study, Z is coded to take on two values, 1 and 2, corresponding to the client's program assignment. Define $J_z \in \{u, t, d\}$ to be the employment status the client would attain during the program conditional on her program assignment. Note that J_z is assumed to exist for every person in the sample, although of course each person is only assigned to one program. For each sample member, we observe the triple (Z, J, Y) where Z is program assignment, $J = J_Z = (2-Z)\cdot J_1 + (Z-1)\cdot J_2$ is employment status attained during the program, and $Y = Y_J$ is the outcome variable.

To use program assignment as an instrumental variable for studying the causal impact of temporary employment on client outcomes, we make two key identifying assumptions. First, given random assignment of clients across programs, we assume plausibly that the random variables J_1, J_2, Y_1, Y_2 are jointly independent of Z. This assumption is supported by the evidence above that the demographics of clients randomly assigned to the two programs are statistically indistinguishable. Second, and equally critically, we assume that program assignment only affects client outcomes through its effect on employment status (temporary, direct-hire, or no employment) attained during the program. This amounts to assuming that the minimal client counseling and support services provided by the programs have negligible effects on client outcomes beyond their impacts on job placement. In Sections 4 and 5, we verify several implications of these assumptions.

Under these assumptions, the difference in the mean outcomes in Programs 1 and 2 is caused by the program level difference in the distribution of employment outcomes (direct-hire, temporary, and no employment). Formally, this difference may be decomposed as follows:

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

As this equation indicates, there are six possible changes in employment status that could be induced by a change in program assignment. A client could be induced to switch from: unemployment to temporary employment and visa versa; from direct-hire employment to unemployment and visa versa; and from direct-hire employment to temporary help employment and visa versa. Each term in equation (2) represents the average effect on the outcome variable for clients induced to switch from one particular status to another weighted by the share of clients induced to make this switch. Note that clients whose employment status is unaffected by program assignment do not contribute anything to the contrast of average outcomes by treatment status.

We would like to be able to isolate the marginal effect of moving from unemployment to temporary employment, δ^{tu} , which corresponds to the first two terms in equation (2). In order to do so, we must place additional structure on the problem. Either of two sets of assumptions is sufficient to identify the parameter of interest using our experimental setup. The first is a constant treatment effect assumption:

(3) $$Y_i^t - Y_i^u = \delta^{tu},\ Y_i^d - Y_i^u = \delta^{du} \Rightarrow Y_i^d - Y_i^t = \delta^{du} - \delta^{tu}.$$

Here, the difference in potential outcomes among the three employment states is assumed identical for each client. This reduces the number of terms in (2) from six to three and, in combination with the evidence above that Program 1 clients have identical rates of direct-hire employment to Program 2 clients, implies that the contrast in (2) stems solely from clients whose employment status was switched from unemployment to temporary employment by the program assignment. Equation (2) then reduces to

(4) $$\begin{aligned} &E[Y \mid Z=1] - E[Y \mid Z=2] \\ &= E[Y_1 - Y_2 \mid J_1 = t, J_2 = u]\cdot \Pr[J_1 = t, J_2 = u] + E[Y_1 - Y_0 \mid J_1 = u, J_2 = t]\cdot \Pr[J_1 = u, J_2 = t], \\ &= \delta^{tu}\cdot\{\Pr[J_1 = t, J_2 = u] - \Pr[J_1 = u, J_2 = t]\} \end{aligned}$$

which can be readily estimated using our quasi-experimental setup.

An alternative assumption allows us to interpret (2) within the Local Average Treatment Effect framework of Imbens and Angrist (1994). Specifically, we may assume that program assignment only changes the employment status of clients who would attain temporary employment if assigned to Program 1 but would attain non-employment if assigned to Program 2. Formally, this is:

(5) $$J_1 \neq J_2 \Rightarrow J_1 = t, J_2 = u.$$

This assumption embeds two behavioral postulates. First, no client is induced by program assignment to switch from direct-hire to temporary employment, direct-hire to unemployment, or the reverse.[15] Second, there is no client who would be unemployed if assigned to Program 1 but would obtain temporary help employment if assigned to Program 2. In the LATE framework, this 'monotonicity' condition rules out the possibility that individuals' employment status is perversely affected by the treatment. Under these behavioral assumptions, equation (2) becomes

$$(6) \qquad E[Y \mid Z=1]-E[Y \mid Z=2] \;=E[Y_1-Y_2 \mid J_1=t, J_2=u]\cdot \Pr[J_1=t, J_2=u],$$

which is the average causal effect on Y of a switch in status from non-employment to temporary help employment for the group of clients whose behavior is changed by program assignment (scaled by the share of such switchers in the client population).

With either set of assumptions, we can estimate the causal effect of temporary employment relative to unemployment using the Wald estimator:

$$(7) \qquad \hat{\delta}=\frac{E[Y \mid Z=1]-E[Y \mid Z=2]}{\Pr[J=t \mid Z=1]-\Pr[J=u \mid Z=2]},$$

(or, controlling for covariates in a two-stage least squares framework). The interpretation of $\hat{\delta}$ differs slightly, however, under the two cases. Under the constant treatment effect assumption, $\hat{\delta}$ is by definition an estimate of δ^{tu} for the entire population of Work First clients. Under the behavioral postulates in the LATE framework, $\hat{\delta}$ is an estimate of δ^{tu} only for the group of clients whose behavior is changed by program assignment.

If neither the constant treatment effects nor the monotonicty assumptions hold (but the assumption that program assignment affects outcomes only through its effect on employment status continues to hold), we may still ascribe a causal interpretation to differences in outcomes across programs. These differences would represent the marginal effect on outcomes of a treatment that induces higher rates of

[15] In other words, every individual who attained direct-hire employment in Program 1 would have done so in Program 2, and vice versa; all clients with no employment in Program 1 (which has the smaller share with no employment) would have also had no employment were they in Program 2; and all clients with temporary employment in Program 2 (which has the smaller share in temporary employment) would have had temporary employment were they in Program 1.

temporary employment. However, the mixture of employment status switches as shown in (2) would be unknown, and hence estimates would not have a direct interpretation as the causal effect of temporary help employment on a specific population.

4. Estimates of Temporary Help Employment on Earnings

Our causal analysis focuses on three outcome measures for Work First clients: in-program earnings, job retention, and program recidivism. In this section, we analyze earnings outcomes and focus on job retention and recidivism measures in Section 5. To evaluate the impact of temporary help employment on earnings, we would ideally exploit panel data on earnings for all individuals in the sample over a consistent time frame. Unfortunately, our data contain detailed earnings and hours data for Work First clients *only* during the time period when clients were enrolled in the program. Assuming that clients who do not find jobs in the Work First program have zero earnings outside the program, we may use these data to estimate the short-term earnings gains from temporary jobs. To the extent that non-employed program leavers have positive earnings outside the program, we will overestimate the gains to temporary employment. As discussed below, this source of bias would be unlikely to overturn our main findings.

a. Comparison of hours and earnings at temporary help and direct-hire positions

Table 4 compares the hours and earnings (in real 2001 dollars) of Work First clients who obtained temporary help and direct-hire jobs during their Work First spells. Clients who did not find employment in Work First have by definition zero hours and earnings. Hours and earnings data refer to those in the first direct-hire or temporary agency job a client held, for those holding multiple jobs in a Work First spell.[16] What is most striking in the table is that hourly wages, weekly hours, and weekly earnings are uniformly *higher* for Work First clients in temporary help jobs than in direct-hire jobs. This pattern holds true for clients in both Programs 1 and 2, and is apparent throughout the distribution of hours and earnings–at the mean, median, and 20th and 80th percentiles. Although this pattern stands in contrast to the conventional finding of lower wages in temporary help positions (Segal and Sullivan, 1998), it follows immediately in our data from the substantial differences in the occupational distribution of temporary help and direct-hire jobs held by Work First clients.

As depicted in Table 5 and Figure 1, production and health care (primarily nursing aids) positions account for over 70 percent of temporary agency placements. These occupations are among the highest paying occupations held by Work First clients, as is shown in the right-hand panel of Table 5. By contrast, direct-hire employment is dispersed across a variety of service occupations including cashier, health care, janitor, sales worker, and childcare occupations. Fewer than 1 in 20 direct-hire workers hold production positions as compared to 1 in 2 temporary help workers.

To assess whether temporary/non-temporary earnings differentials are primarily accounted for by differences in occupational composition, we regress client earnings measures on year-by-quarter of assignment dummies, standard demographic controls, and dummy variables for each of the occupation categories (the production occupation dummy is excluded). Whereas the raw earnings difference between temporary and direct-hire workers is approximately 60 cents per hour and 65 dollars per week, this difference falls to 19 cents per hour and 14.6 dollars per week conditional on these controls. This suggests that most–but not all–of the earnings contrast is explained by occupational composition. Moreover, the minimal overlap between the occupations filled by temporary help and direct-hire workers underscores that these employment venues provide distinct work opportunities that may ultimately affect client's longer term employment outcomes.

b. The causal effect of temporary employment on earnings

The comparisons of earnings in temporary help versus direct-hire employment thus far do not answer the causal question of how temporary help jobs remunerate those who would otherwise have been non-employed. To answer this causal question, we exploit the random assignment component of the Michigan Work First program to estimate the following regression model:

(8) $$Y_{it} = \alpha + \gamma_1 \cdot T_i + \gamma_2 \cdot D_i + X_i'\beta + \phi_t + \varepsilon_{it},$$

[16] In 4 cases, individuals holding a temporary agency job in a Work First spell held a direct-hire job as their first job. These individuals were classified as temporary and data for their second, i.e. temporary agency, job were used in all analyses involving

where Y is the outcome measure (earnings or hours) for client i during a Work First spell commencing in calendar year-by-quarter t, X is a vector of demographic controls, ϕ_t is a vector of year-by-quarter dummies, and T and D are indicator variables equal to one if the outcome Y corresponds to a temporary help or direct-hire job, respectively. As discussed above, OLS estimates of γ_1 (and γ_2) are likely to be biased. We can potentially obtain causal estimates of γ_1 by using client program assignment as an instrumental variable for temporary help employment status. Under the assumptions outlined in Section 3, two stage least squares (2SLS) estimates of equation (8) will identify hours and earnings in temporary employment of welfare clients who, if assigned to Program 2, would have remained non-employed during their Work First spell.[17] We stress that direct-hire employment remains endogenous in these models. Consequently, our estimates will *only* measure the causal effect of temporary help employment on outcomes for workers who would otherwise have remained non-employed.

OLS estimates of equation (8) are given in Table 7. These estimates confirm the bivariate comparisons visible in Table 4: workers in temporary help positions work more hours per week (6.6 hours), earn higher hourly wages (0.62 dollars) and receive greater weekly earnings (62.5 dollars) than do Work First clients in direct-hire jobs. In all cases, these differences are highly significant.

Panel B presents 2SLS estimates of these earnings model Notably, the estimated hours and earnings differences between temporary help and direct-hire employment are uniformly lower than in the corresponding OLS estimates. Specifically, the 2SLS models reduce the estimated hourly earnings advantage of temporary help workers by 25 percent and reduce the weekly hours and weekly earnings advantage by 40 percent. This pattern of results suggests that marginal temporary workers–that is, welfare clients induced by program assignment to take temporary help jobs–had on average weaker skills and were less productive than infra-marginal temporary workers.

---

hours and earnings.

[17] We also estimated but do not present non-linear 2SLS versions of these models. Non-linear 2SLS results are quite similar to conventional 2SLS estimates but, following Angrist (2001), we believe the linear models have a more natural interpretation in the causal effects analysis.

To see why this result arises, it is instructive to compare the hours and earnings of temporary workers across Programs 1 and 2 (Table 4). On all earnings and hours measures, and at almost all points in the distribution, Program 1 temporary help workers fare significantly worse than Program 2 temporary help workers. This pattern of lower earnings for Program 1 temporary workers is also confirmed in Appendix Table 1 (panel A), where we regress earnings and hours of temporary workers on demographic controls, year-by-quarter dummies, and a Program 1 dummy.

Our econometric framework implies that the cross-program earnings differences in temporary help employment are due to the lower human capital of marginal temporary workers. An alternative interpretation, however, is that the jobs (or job search services) available to Program 1 clients are simply of lower quality than those at Program 2 , and this induces lower earnings for Program 1 clients conditional on employment. On its face, this hypothesis seems improbable given that the frequency of job placements is *higher* at Program 1 than Program 2 in every year of our data. However, because this alternative interpretation would undermine the validity of our analysis, we provide two additional tests of differences in job quality by program.

First, we calculate the occupational distribution of employment by program for temporary help and direct-hire jobs. If jobs taken by Program 1 clients are typically of lower quality, this would likely be reflected in a larger share of Program 1 clients entering low wage occupations. As is apparent from Figure 2 and confirmed by chi-square tests, however, the occupational distributions of temporary help and direct-hire jobs are not significantly different across programs–though of course these distributions differ significantly from one another.

As a further test, we note that if Program 1 typically provides entrée to lower quality jobs than does Program 2, this disparity would likely affect the employment client outcomes in *both* direct-hire and temporary help positions. Hence, a second falsification test is to compare hours and earnings across programs in direct-hire jobs. This test is particularly relevant because our econometric framework assumes that program assignment has *no* impact on employment outcomes of direct-hire workers. Consequently, significant program-level differences in earnings or hours of direct-hire workers would cast doubt both on of our interpretation of the Table 7 findings and more generally on the underpinnings of our analysis. Yet, as is visible in Table 3, hours and earnings of direct-hire workers are near-identical across the two programs in each case. Appendix Table 1 (panel B) confirms this impression. In a regression of direct-hire outcomes on demographic controls, year by quarter of assignment dummies, and a Program 1 dummy, the hourly wages, weekly earnings and weekly earnings of direct-hire workers differ only minutely between Programs 1 and 2.

We conclude that the most plausible interpretation of the 2SLS earnings estimates is that clients encouraged by program assignment to enter temporary help employment had lower human capital than average temporary help workers and hence lower earnings. Because, as noted, we not observe client earnings outside of the Work First spell, it is almost certain that the 2SLS estimates somewhat overstate the temporary help earnings gains for non-employed; at least some (probably a small minority) of these clients would have had labor earnings outside of Work First. Note however that the 2SLS estimates indicate that the earnings of marginal temporary workers equaled or slightly exceeded those of direct-hire workers. Therefore, provided that the fraction of clients obtaining jobs outside of Work First is small, it is quite unlikely that client earnings outside the Work First program would entirely or even significantly offset the relatively high earnings of Program 1 clients induced to take temporary help jobs.[18] Whether these temporary help positions also facilitated clients' short-term job retention and longer term welfare independence is the question we address next.

5. Estimates of the Effect of Temporary Employment on Job Retention and Welfare Recidivism

Using longitudinally linked the Work First data, we are able to track clients' job retention and welfare recidivism for a minimum of two years following the commencement of each spell. To measure short-term job holding, we analyze the 90-day retention measure used by the state of Michigan as the primary criterion for evaluating Work First service providers. A Work First client is successfully terminated from

[18] As discussed in the conclusion, we hope to acquire more comprehensive earnings data for these Work First clients that will allow us to more accurately measure the effects of temporary employment on earnings.

the program is she holds a qualified job or jobs for 90 consecutive days. Job changes that result in an employment gap of less than one week are also counted as part of the same 90-day spell. As an evaluation tool, this 90-day retention measure has several shortcomings. First, because it is a short-term outcome, it may poorly predict whether clients remain independent from the welfare system. Second, it only tracks employment outcomes of clients while they are undergoing a Work First spell; if clients quit Work First and obtain steady employment outside of the program, this is not reflected in the retention measure. Finally, the time interval over which a client may obtain 90 days of continuous employment is open-ended, and so potentially subject to the discretion of program case workers.

To supplement this short-term outcome measure, we constructed a set of program recidivism variables that code whether clients reentered the Work First program within a closed interval after the commencement of a spell: 180, 270, 360, or 720 days following the orientation. For the small fraction of spells that had not yet terminated the program at the close of these time intervals, we coded these cases as "reentries" (i.e., failures) *unless* the individual was employed on that date (e.g., 180 days, 270 days, etc. following the orientation date).[19] Because Work First clients are frequently terminated and reenter within a short time horizon, we also construct a longer-term recidivism outcome that measures whether individuals re-entered the Work First program at any time between one and two years following their initial orientation date. This measure discards short-term recidivism that arguably may be considered part of the initial Work First spell. Hence it may capture more enduring outcomes.[20]

It bears note that because our data only capture Work First clients served in the one Michigan county, we do not observe if a client enters the Work First program elsewhere in the state or out of state. Because clients are randomly assigned to the two programs, there is no *a priori* reason for mobility rates to differ across programs. Mobility would pose a problem for our analysis, however, if it is correlated with employment outcomes that are affected by program assignment.

[19] 91.2 percent of spells ended within 180 days, 97.0 within 270 days, 98.6 within 360 days and 99.8 within 720 days.

[20] In a typical scenario, a service provider would terminate a client for failing to attend mandatory Work First meetings; the notice of termination would be communicated to the individual's FIA case worker, who would cut benefits unless the individual

a. Summary of outcome measures

Table 8 summarizes our job retention and recidivism measures for the 2,778 spells in our data. Only 21 percent of spells result in clients holding employment for 90 days. Program reentry is quite high. Thirty percent of clients reenter within 180 days, and 62 percent within 720 days. Almost half of Work First spells result in reentry one to two years later. The next three rows tabulate outcomes by whether the spell yielded a temporary job, a direct-hire job (and no temporary job), or no employment. The 90-day retention rate is considerably lower for those holding temporary help jobs than for those holding direct-hire jobs. By definition, the 90-day retention rate is zero for those who do not find jobs.

Although the reentry rate over short time horizons is lower for those who found temporary jobs than for those with no employment, the differences are not significant, and the recidivism rates in the two groups converge over time. Notably, the rate at which clients reenter the program one to two years following initial entry (that is, the long-term measure) does not differ substantially by employment status during the Work First spell. However, those with direct-hire jobs have significantly lower recidivism rates by all other metrics.

Figure 3 depicts the fraction of clients receiving services over time following entry into Work First, plotted separately by program for each employment outcome: temporary employment, direct-hire employment, and no employment. The fraction of recipients drops initially as clients exit the program and begins to rise after approximately 6 months as the number reentering the program exceeds the number exiting.[21] For the first six months following program entry, the fraction receiving program services is quite similar for those finding temporary and direct-hire jobs but much lower than for those not finding employment. By one year following program entry, the fraction reentering or still in the program is comparable for temporary workers and non-employed (approximately 53 percent), whereas the recidivism

---

reenrolled in the Work First program. If a client has not left the program within 361 days after first entry, we also count this as recidivism in our long-term recidivism measure.

[21] The slower exit from Work First by those who find employment is not, in itself, a negative outcome, but rather reflects the program's follow-up system. Those who find and remain in employment are tracked for at least 90 days before being terminated from Work First. Reflecting their shorter average duration in Work First, clients who fail to find a job typically reenter the program sooner.

rate for those finding direct-hire jobs is substantially lower (41 percent). This convergence in outcomes between temporary workers and non-employed–and its contrast with recidivism for direct-hires–remains relatively stable from 12 months forward.

The bottom two rows of Table 8 and the corresponding Figure 4 document overall recidivism by program following initial entry. Most notable from this comparison is that, despite the higher rate of temporary help employment and lower rate of non-employment in Program 1 relative to Program 2, job retention rates and program recidivism are extremely similar in the two programs. This simple comparison suggests that the additional temporary employment induced by Program 1 had at best a limited effect on these outcome measures. One caveat, however, is that the overall difference in temporary help employment and non-employment in Programs 1 and 2 is small (about 6 percentage points) relative to their commonality of employment outcomes (about 94 percentage points). Consequently, any effect of moving from non-employment into temporary help employment on individual outcomes will have a relatively small effect on mean program outcomes unless the effect size is large.[22]

b. Causal Estimates of Temporary Employment Impacts on Job Retention and Program Recidivism

Following the previous analysis, we utilize a two-stage least squares model to estimate the marginal effects of temporary employment on 90-day job retention and recidivism among Work First clients. Specifically, we use random client assignment to Program 1 versus 2 as an instrument for temporary employment *relative* to non-employment, and control for demographic characteristics–age, race, and education–and quarter-year of program entry in the regression equations. Before discussing these 2SLS estimates, we present in Table 9 OLS models of these outcomes with the endogenous variables, temporary help and direct-hire employment, included as regressors.

[22] For instance, if the marginal effect of placing non-employed into temporary jobs were to reduce the probability of program recidivism by 20 percentage points, and if 6 percent of the non-employed switched, the fraction returning to the program would decline by only 0.012. Given the relatively small size of our sample, it will be difficult to detect small and moderate marginal effects.

In the first model (column 1) predicting 90-day job retention in the Work First program, the point estimate for temporary help employment is 0.25 and highly significant. That is, workers obtaining temporary help employment have a 25 percent higher probability of successfully exiting the program. This relationship is close to mechanical, however. Those clients who did not find employment are necessarily coded as unsuccessful, and so temporary or direct-hire employment is necessary, though not sufficient, for successful exit.

Columns 2 – 7 present OLS estimates of the relationship between employment status and subsequent recidivism. These estimates reflect what is visible from Figure 3. In the first 180 to 270 days following entry, temporary help workers are 3 to 5 percentage points less likely than the non-employed to reenter the Work First program. From 360 to 720 days after entry, there are no significant differences in recidivism. Workers finding direct-hire positions have substantially lower recidivism in the first 720 days: they are 4 to 13 percentage points less likely to reenter the program, a difference that is always significant. Interestingly, there are no significant differences among temporary help, direct-hire, and non-employed workers in program reentry during days 361 – 720. This finding suggests that all differences in recidivism across employment outcomes are due to differences in recidivism in the first year.

Table 10 presents 2SLS estimates of these equations. Correcting for the endogeneity of temporary help employment, we find no significant impact of temporary help employment relative to non-employment on 90-day retention. The results for recidivism are only slightly different. Over the first year following program entry, the two-stage least square estimates suggest that temporary employment had a modest negative effect on recidivism. But this coefficient is imprecisely measured and never significant. After a year, temporary employment is estimated to actually increase recidivism–reflecting the fact that recidivism rates over longer time horizons are actually higher in Program 1 than in Program 2 (Table 8). These point estimates are also never statistically significant.

In interpreting these estimates, we stress that direct-hire employment remains endogenous. As above, our IV estimates only measure the causal effect of temporary help employment on outcomes for workers who would otherwise have remain non-employed (i.e., if assigned to Program 2 instead of Program 1).

The pattern of null results has two interpretations within our econometric framework. Under the assumption that the marginal effects of temporary employment are constant across individuals, our results imply (by definition) that moving from unemployment into a temporary agency job does not improve any individual's chances of remaining employed for 90 days or remaining out of Work First over the long term. Under the alternative monotonicity assumption, our estimates imply that there is no effect of temporary employment relative to non-employment on job retention and recidivism for the group of welfare clients induced by program assignment to take temporary employment. It is therefore possible under this assumption that temporary employment could improve program success and reduce recidivism for inframarginal temporary workers.

It is easy to see from Figure 3 (or Table 8) why temporary help employment is not found to impact Work First client recidivism. The key observation from this figure is that one year after program entry, program recidivism is essentially the same for temporary workers and non-employed. Hence, if the effect of Program 1 assignment was to induce workers who would have been non-employed in Program 2 to become temporary workers, and if these workers fared *as well as* infra-marginal temporary workers (measured by recidivism), we would still not predict any effect on recidivism. This observation demonstrates why it is quite unlikely that additional temporary employment induced by Program 1 assignment would tend to reduce measured recidivism in the Work First data.[23]

Before drawing conclusions from this exercise, we consider one possible explanation for why, in spite of the different rates of temporary employment across programs, outcome measures may not differ. We have assumed that service providers only affect client outcomes through their placement rates in temporary and direct-hire jobs. If this assumption were incorrect, it might bias our estimates. For example, if Program 2 provided better support services to unemployed clients than Program 1, this would explain why its average outcomes were similar to Program 1, despite lower rates of job placement.

[23] If clients were negatively selected into temporary employment relative to non-employment, it would, of course, be possible for the IV estimates to yield positive impacts of temporary employment on recidivism. It seems quite unlikely, however, that the employed are negatively selected relative to the non-employed.

The data in Figure 3 shed some light on the plausibility of this assumption. The three panels of this figure show that the rates of 90-day retention and recidivism *within* each employment outcome are always closely comparable across service providers, suggesting that employment status is the main determinant of program recidivism–not the direct program effect.[24] In the case of temporary employment and non-employment, this comparison potentially could be misleading since program assignment directly affects the composition of these two groups (i.e., Program 1 places more clients in temporary employment and fewer in non-employment). However, under our identifying assumption that there is no program impact on the composition of the direct-hire group, the cross-program comparison for direct-hire workers does not suffer from composition bias. It is thus important to observe that there are no significant program level differences in success or recidivism for the direct-hire groups.

6. Conclusion

Drawing on unique data in one Michigan county, our pilot analysis provides, we believe, the first quasi-experimental evidence on the causal impact of temporary help employment on earnings, job retention, and program recidivism among welfare clients. The random assignment of clients across Work First providers over a four year period provides closely matched samples of Work First clients for experimental analysis. The differential policies toward temporary help employment of the two Work First providers studied yields sizable, statistically significant, and persistent cross-program differences in the incidence of temporary help employment. We exploit these cross-program differences to study the causal impact of temporary help employment on outcomes.

In addition to the random assignment, two identifying assumptions are required to make a causal interpretation appropriate. First, we assume that program assignment only affects client outcomes through its effect on employment status—temporary, direct-hire, or no employment—attained during the program. Second, we adopt one of two statistical assumptions to permit interpretation of the parameter estimates: either a) a constant treatment effect assumption; or b) a monotonicity assumption where Program 1

[24] Detailed tabulations available from the authors demonstrate that conditional on employment status, 90-day retention and client recidivism never differ significantly across the two programs.

assignment *only* raises the odds of temporary employment for those who would otherwise have been non-employed (in Program 2). Under these assumptions, our findings may be interpreted as the causal impact of temporary employment on the various outcome measures for Work First clients who otherwise would have not obtained employment in that program.

We offer three pieces of evidence favoring the plausibility of these identifying assumptions:

1. As documented by Eberts (2002), the two programs we study offered only minimal services to Work First clients aside from job placement, and those services that they offered were closely comparable in content and client contact hours. These facts provide support for our first assumption, that program assignment affects outcomes only through its effect on employment status–temporary job, direct-hire job, or non-employment.

2. Consistent with the fact that Program 1 encourages marginal non-employed into temporary jobs, the earnings and hours of temporary help workers are significantly lower in Program 1 than in Program 2. By contrast, the earnings and hours of direct-hire workers are virtually identical across the programs. These patterns provide further support for our assumption that program assignment affects outcomes only through its effect on employment status and support the monotonicity assumption, i.e. that program assignment does not affect the composition of the direct-hire group.

3. Conditional on client employment status–temporary job, direct-hire job, or non-employment, we find no significant differences in client 90-day retention rates or recidivism between programs, supporting our first assumption that programs affect client outcomes through placement rates rather than other services. The close comparability of outcomes between programs is particularly noteworthy for clients with direct-hire jobs because this pattern again provides support for the monotonicity assumption.

Interpreting the findings in this framework, spells of temporary employment have sizable positive impacts on earnings and hours over the short term. In particular, marginal temporary workers on average earn $216 per week. This dollar figure is below the weekly earnings of inframarginal temporary workers, but is slightly above that of direct-hire workers in the Work First sample. This estimate could be biased upward, as noted, because we do not observe earnings and hours for non-employed clients who exit the program and find employment outside of Work First. However, given very high and rapid rates of Work First reentry for these non-employed clients, it is unlikely that this source of bias would entirely negate the positive impacts we find.

Our experimental evidence yields no significant impacts of temporary help employment on 90-day job retention or program recidivism for Work First clients. Although this finding is at odds with the the 'on ramp' hypothesis that temporary help employment facilitates labor market integration for Work First clients, it is not surprising in the context of our study. As our data indicate, recidivism is at the baseline indistinguishable between temporary workers and non-employed. Consequently, even if otherwise non-employed clients encouraged by program assignment to obtain temporary jobs fared as well as inframarginal temporary help workers, we would still predict no improvement in recidivism.

We hope to augment the study with richer outcome measures in the future. The primary outcome measures on which we currently focus–short-term earnings and program recidivism–are important metrics for state welfare agencies and are measured with great precision. They are not ideal gauges of clients' labor market progression, however, because they do not track earnings growth and labor force attachment over time. We therefore hope to supplement these measures with Unemployment Insurance data measuring employment and earnings of Work First clients over two or more years following initial Work First program assignment. This analysis might reveal, for example, that the comparable rates of recidivism among non-employed and temporary employed mask greater earnings and employment progression among temporary workers, as is suggested in the analysis by the Heinrich et al.(2002).

We believe our pilot analysis confirms that the client randomization fortuitously employed by the Michigan Work First program generates plausibly exogenous differences in employment statues among Work First clients. Our evidence suggests further that these differences can be used to identify the causal effects of temporary employment on client outcomes such as short-term earnings, 90-day job retention, and welfare recidivism. Enriching our set of labor market outcomes will substantially augment the policy-relevant information provided by this experiment.

References

Angrist, Joshua D. 2001. "Estimation of Limited Dependent Variable Models with Dummy Endogenous Regressors: Simple Strategies for Empirical Practice." *Journal of Business and Economic Statistics*, 19(1), 2–16.

Angrist, Joshua D. and Guido W. Imbens. 1995. "Two-Stage Least Squares Estimation of Average Causal Effects in models with Variable Treatment Intensity." *Journal of the American Statistical Association*, 90(43), 431–442.

Autor, David H. 2001. "Why Do Temporary Help Firms Provide Free General Skills Training?" *Quarterly Journal of Economics*, 116(4), November, 1409–1448.

Autor, David H. 2003. "Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing." *Journal of Labor Economics*, 21(3), January.

Autor, David H. and Susan N. Houseman. 2002. "The Role of Temporary Employment Agencies in Welfare to Work: Part of the Problem or Part of the Solution?" *Focus*, 22(1), 63–70.

Ballantine, John and Ronald F. Ferguson (1999) "Labor Demand for Non-College Educated Young Adults" mimeograph, Harvard University.

Booth, Alison L., Marco Francesconi and Jeff Frank (2002) "Temporary Jobs: Stepping Stones or Dead Ends?" *The Economic Journal*, 112 (June), F189–F213.

Cancian, Maria, Robert Haveman, Thomas Kaplan, and Barbara Wolfe. 1999. *Post-Exit Earnings and Benefit Receipt among Those Who Left AFDC in Wisconsin.* Institute for Research on Poverty, University of Wisconsin-Madison, Special Report no. 75.

Card, David and Daniel G. Sullivan. 1998. "Measuring the Effect of Subsidized Training Programs on Movements in and out of Employment." *Econometrica*, 56(3), 497–530.

DiNatale, Marisa. 2000. "Characteristics and preference for alternative work arrangements, 1999" *Monthly Labor Review*, 124(3), March, 28–49.

Eberts, Randall W. 2002. "Design, Implementation, and Evaluation of the Work First Profiling Pilot Project." Mimeo, W.E. Upjohn Institute for Employment Research, March.

Ferber, Marianne A. and Jane Waldfogel. 1998. "The Long-Term Consequences of Nontraditional Employment." *Monthly Labor Review*, 121(5), 3–12.

General Accounting Office. 2000. "Contingent workers: Incomes and benefits lag behind the rest of the workforce" GAO/HEHS-00-76, June, available at http://www.gao.gov/.

Heinrich, Carolyn J., Peter R. Mueser, and Kenneth R. Troske. 2002. "Welfare to Temporary Work: Implications for Labor Market Outcomes," Mimeo, University of Missouri-Columbia, August.

Houseman, Susan N., 2001. "Why Employers Use Flexible Staffing Arrangements: Evidence from an Establishment Survey." *Industrial and Labor Relations Review*, 55(1), October, 149–170.

Houseman, Susan N., Arne J. Kalleberg, and George A. Erickcek, forthcoming. "The Role of Temporary Help Employment in Tight Labor Markets." *Industrial and Labor Relations Review.*

Imbens, Guido W. and Joshua D. Angrist. 1994. "Identification and Estimation of Local Average Treatment Effects." *Econometrica*, 62(2), 467–475.

Jorgenson, Helene and Hans Riemer. 2000. "Permatemps: Young Temp Workers as Permanent Second Class Employees." *American Prospect*, 11(18), pp. 38-40.

Kalleberg, Arne L., Jeremy Reynolds, and Peter V. Marsden. 1999. "Externalizing Employment: Flexible Staffing Arrangements in U.S. Organizations." Unpublished paper, University of North Carolina at Chapel Hill.

Lerman, Robert I. and Caroline Ratcliffe. 2001. "Are Single Mothers Finding Jobs with Displacing other Workers?" *Monthly Labor Review*, July, 3-12.

Pawasarat, John. 1997. The Employer Perspective: Jobs Held by the Milwaukee County AFDC Single Parent Population (January 1996-March 1997). Employment and Training Institute, University of Wisconsin-Milwaukee.

Segal, Lewis M., and Daniel G. Sullivan. 1997. "The Growth of Temporary Services Work," *Journal of Economic Perspectives*, 11, 117–136.

Segal, Lewis M., and Daniel G. Sullivan. 1998. "Wage Differentials for Temporary Services Work: Evidence from Administrative Data." Federal Reserve Bank of Chicago Working paper, No. 98–23.

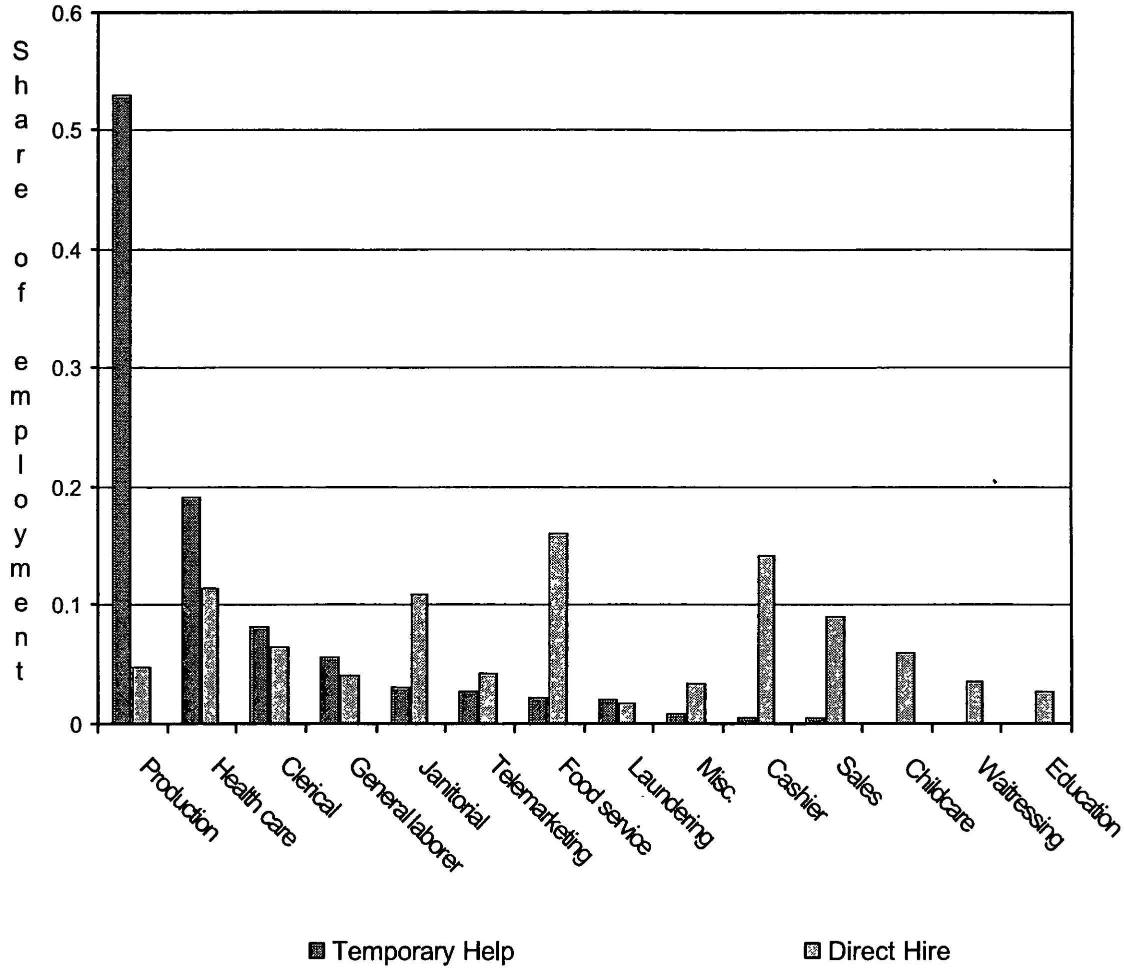

Figure 1. Occupational Distribution of Work First Clients in Temporary Help and Direct Hire Positions

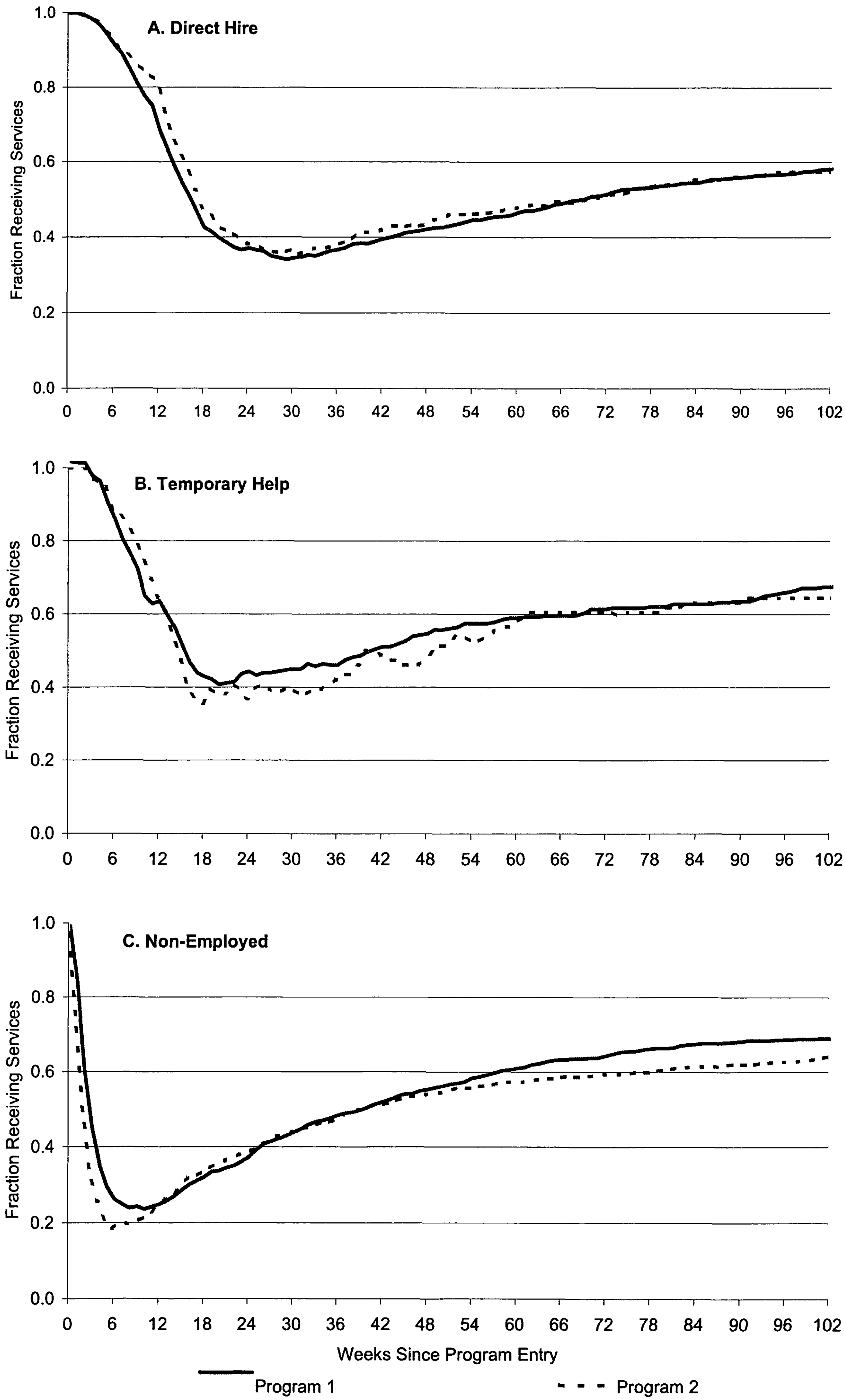

Figure 3. Comparison of Recidivism by Initial Employment Status at Programs 1 and 2

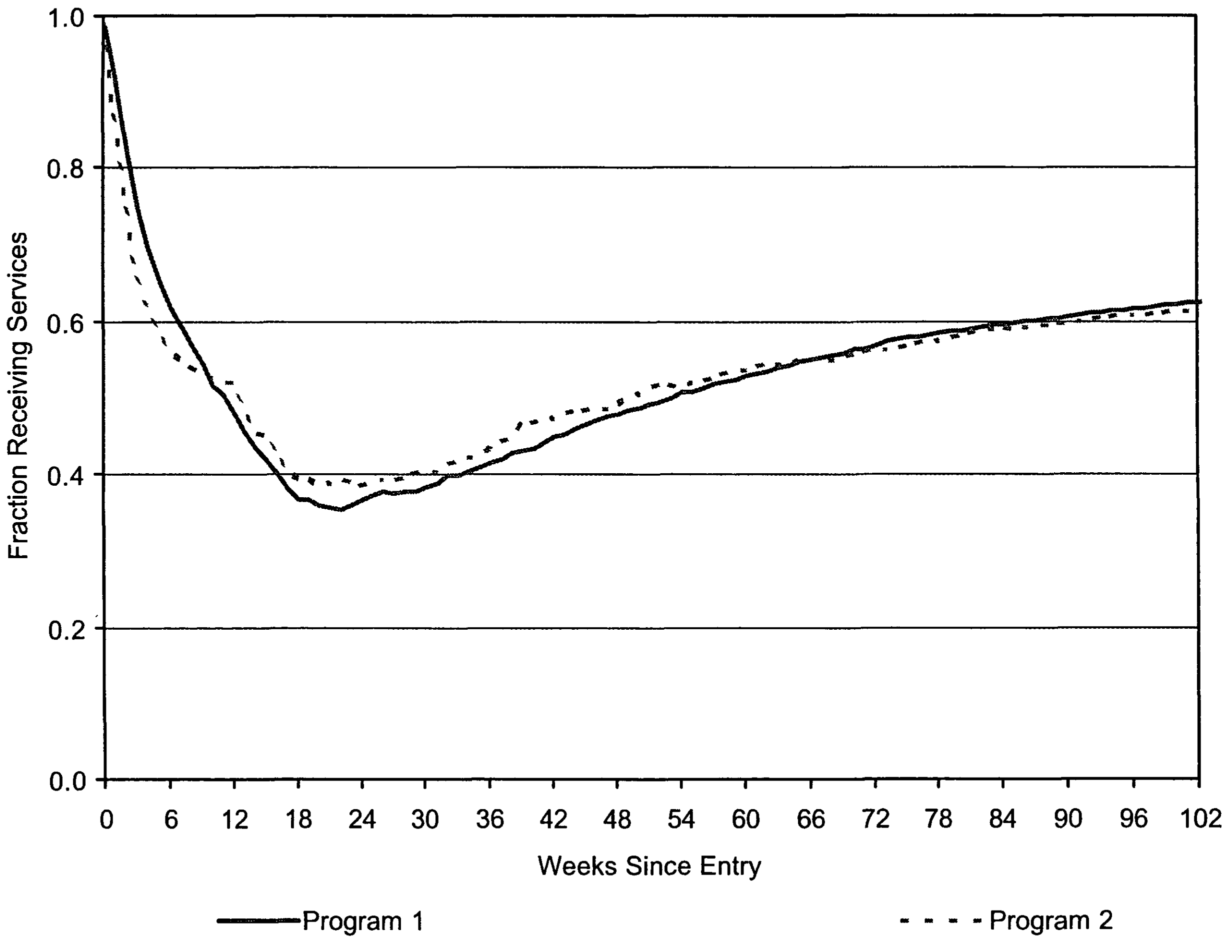

Figure 4. Cumulative Recidivism of Work First Clients Receiving Work First Services Following Date of Entry by Program

Table 1:
Means of Demographic Variables for Clients Randomly Assigned to Work First Providers and Test of Randomization.

A. Comparison of Means

	Black	HS Dropout	Black x HS Drop	Client Age
Overall	0.589	0.385	0.231	27.53
(n=2,778)	(0.009)	(0.009)	(0.008)	(0.14)
Program 1	0.585	0.386	0.227	27.66
(n=1,921)	(0.011)	(0.011)	(0.010)	(0.17)
Program 2	0.600	0.382	0.239	27.22
(n=857)	(0.017)	(0.017)	(0.015)	(0.24)
Program 1 - 2	-0.015	0.005	-0.012	0.44
Difference	(0.020)	(0.020)	(0.017)	(0.29)

B. Regression Test of Randomization

	Black	HS Dropout	Black x HS Drop	Client Age
	(1)	(2)	(3)	(4)
Program 1	-0.007	0.013	-0.003	0.22
	(0.021)	(0.020)	(0.018)	(0.30)
Constant	0.659	0.375	0.249	26.06
	(0.043)	(0.042)	(0.036)	(0.62)
R^2	0.011	0.007	0.010	0.010
n	2,778			

Standard errors in parentheses. Sample is Work First clients randomly assigned to programs during 1st quarter 1997 through 3rd quarter 2000. Each column of Panel B is from a separate OLS regression of the indicated demographic characteristic on tabulated variables plus dummies for year-by-calendar quarter of program assignment. Omitted category is program 2 assigment.

Table 2
Comparison of Employment Statuses Attained by Work First Clients by Program and Year

	Program 1				Program 2				Program 1 - 2 Difference		
	Temp Help	Direct Hire	Non-Employed	n	Temp Help	Direct Hire	Non-Employed	n	Temp Help	Direct Hire	Non-Employed
All Years	0.146	0.436	0.419	1,921	0.089	0.415	0.497	857	0.057	0.020	-0.078
	(0.008)	(0.011)	(0.011)		(0.010)	(0.017)	(0.017)		(0.013)	(0.020)	(0.020)
1997	0.125	0.530	0.345	530	0.062	0.517	0.421	178	0.063	0.013	-0.076
	(0.014)	(0.022)	(0.021)		(0.018)	(0.038)	(0.037)		(0.023)	(0.043)	(0.042)
1998	0.140	0.405	0.455	605	0.066	0.387	0.547	243	0.075	0.018	-0.093
	(0.014)	(0.020)	(0.020)		(0.016)	(0.031)	(0.499)		(0.021)	(0.037)	(0.499)
1999	0.147	0.433	0.419	434	0.083	0.441	0.473	145	0.065	-0.008	-0.053
	(0.017)	(0.024)	(0.024)		(0.023)	(0.041)	(0.041)		(0.029)	(0.048)	(0.048)
2000	0.185	0.349	0.466	352	0.124	0.364	0.512	291	0.061	-0.015	-0.046
	(0.021)	(0.025)	(0.027)		(0.019)	(0.028)	(0.029)		(0.028)	(0.038)	(0.040)

Standard errors in parentheses. Means for Work First clients randomly assigned to programs during 1st quarter 1997 through 3rd quarter 2000. Job status refers to employment obtained while client was in program. If more than one type of employment obtained, temporary help position is given precedence.

Table 3:
First Stage Models Linear Probability Models: Impact of Program Assignment On Probability of Obtaining Temporary Help or Direct Hire Job.

	A. Obtained Temp Help Job		B. Obtained Direct Hire Job	
	(1)	(2)	(1)	(2)
Program 1 Assignment	0.065	0.087	-0.004	-0.033
	(0.014)	(0.024)	(0.020)	(0.035)
Black x Program 1		-0.024		-0.003
		(0.028)		(0.041)
Less than HS x Program 1		-0.022		0.079
		(0.028)		(0.041)
Black	0.033	0.050	-0.054	-0.052
	(0.016)	(0.025)	(0.024)	(0.037)
Less than HS	0.009	0.024	-0.096	-0.152
	(0.020)	(0.028)	(0.030)	(0.041)
Black x Less HS	-0.001	-0.001	0.010	0.011
	(0.026)	(0.026)	(0.038)	(0.038)
Age	0.020	0.020	0.022	0.022
	(0.006)	(0.006)	(0.009)	(0.009)
$Age^2 \times 10^{-2}$	-0.028	-0.029	-0.038	-0.037
	(0.010)	(0.010)	(0.015)	(0.015)
Constant	-0.181	-0.203	0.146	0.174
	(0.097)	(0.099)	(0.142)	(0.144)
R^2	0.029	0.030	0.058	0.059
F-Stat for IVs	21.53	0.70	0.04	1.86
P value for IVs	0.000	0.495	0.839	0.156

n=2,778. Standard errors in parentheses. Sample is Work First clients randomly assigned to programs during 1st quarter 1997 through 3rd quarter 2000. Each columns is from a separate OLS regression. All models contain dummy variables for year-by-calendar quarter of program assignment. Omitted category is program 2 assigment. Job status refers to employment obtained while client was in work first program. If more than one type of employment obtained, temporary help position is given precedence.

Table 4:
Earnings and Hours of Clients Who Obtained Employment while in Work First Program by Job Type: Temporary Help or Direct Hire

	Program 1			Program 2			Program 1 - 2	
	Temp Help	Direct Hire	Diff	Temp Help	Direct Hire	Diff	Temp Help	Direct Hire
				A. Hourly Wages				
Mean	7.28	6.72	0.56	7.73	6.75	0.98	-0.45	-0.03
	(0.07)	(0.05)	(0.09)	(0.16)	(0.07)	(0.17)	(0.17)	(0.09)
20 pctile	6.52	5.71	0.81	6.66	5.68	0.98	-0.14	0.03
50 pctile	7.17	6.38	0.79	7.44	6.38	1.06	-0.27	0.00
80 pctile	7.91	7.20	0.71	8.70	7.61	1.09	-0.79	-0.41
				B. Weekly Earnings				
Mean	243.9	185.1	58.8	275.4	186.7	88.7	-31.5	-1.6
	(4.9)	(2.9)	(5.7)	(9.3)	(4.3)	(10.3)	(10.5)	(5.1)
20 pctile	149.3	119.6	29.7	204.7	121.3	83.4	-55.43	-1.76
50 pctile	267.3	163.1	104.2	287.8	164.1	123.8	-20.56	-1.02
80 pctile	304.4	245.8	58.5	327.5	246.7	80.8	-23.17	-0.90
				C. Weekly Hours				
Mean	33.5	27.0	6.5	35.6	27.2	8.3	-2.05	-0.18
	(0.6)	(0.3)	(0.7)	(0.9)	(0.5)	(1.0)	(1.10)	(0.54)
20 pctile	20.0	20.0	0.0	27.5	20.0	7.5	-7.50	0.00
50 pctile	40.0	25.0	15.0	40.0	25.0	15.0	0.00	0.00
80 pctile	40.0	37.0	3.0	40.0	36.0	4.0	0.00	1.00
n	280	837		75	356			

Standard errors in parenthese. Earnings and hours given for clients who obtained and completed 90 days of employment while in Work First programs during 1997 - 2000 period. If more than one type of employment obtained, temporary help position is given precedence. Earnings inflated to real 2001 dollars using the Consumer Price Index for urban consumers (series CUUR0000SA0).

Table 5:
Occupational Distributions and Occupational Earnings of Temporary Help and Direct Hire Workers

	A. Occupation Shares (%)			B. Weekly Earnings		
	Temp Help	Direct Hire	Diff	Temp Help	Direct Hire	Diff
Production	53.0	4.8	47.8	266.8	272.8	-6.0
	(2.7)	(0.6)	(2.7)	(4.8)	(13.5)	(14.3)
Health Services	19.2	11.4	8.0	199.8	236.0	-36.3
	(2.1)	(0.9)	(2.3)	(11.6)	(7.7)	(13.9)
Clerical	8.2	6.5	2.0	272.4	191.0	81.4
	(1.5)	(0.7)	(1.6)	(17.8)	(8.8)	(19.9)
General Labor	5.6	4.0	1.6	248.2	229.1	19.1
	(1.2)	(0.6)	(1.3)	(18.0)	(15.3)	(23.6)
Janitorial	3.1	10.9	-8.1	191.7	173.6	18.1
	(0.9)	(0.9)	(1.3)	(26.6)	(5.7)	(27.2)
Tele-Marketing	2.8	4.4	-1.5	291.3	194.4	96.9
	(0.9)	(0.6)	(1.1)	(19.3)	(8.8)	(21.2)
Food Services	2.3	15.9	-13.7	235.0	160.5	74.5
	(0.8)	(1.1)	(1.3)	(23.2)	(4.0)	(23.6)
Laundering	2.0	1.8	0.2	222.6	173.3	49.4
	(0.7)	(0.4)	(0.8)	(21.2)	(13.4)	(25.0)
Misc.	0.8	3.4	-2.6	267.3	211.0	56.3
	(0.5)	(0.5)	(0.7)	(74.1)	(16.3)	(75.9)
Cashier	0.6	14.2	-13.6	238.0	151.2	86.8
	(0.4)	(1.0)	(1.1)	(91.0)	(3.5)	(91.0)
Sales	0.6	9.1	-8.5	347.8	181.1	166.7
	(0.4)	(0.8)	(0.9)	.	(8.1)	.
Child care	0.0	6.0	-5.7	.	164.4	.
	(0.0)	(0.7)	(0.7)		(7.6)	
Waiter/ Waitress	0.0	3.6	-3.6	.	143.1	.
	(0.0)	(0.5)	(0.5)		(5.1)	
Education	0.0	2.8	-2.8	.	168.8	.
	(0.0)	(0.5)	(0.5)		(19.6)	
Unknown	2.0	1.4	0.5	273.3	239.0	34.3
	(0.7)	(0.3)	(0.8)	(39.4)	(38.6)	(55.2)
n	355	1,193		355	1,193	

Standard errors in parenthese. Occupational titles and weekly earnings given for clients who obtained and completed 90 days of employment while in Work First programs during 1997 - 2000 period. If more than one type of employment obtained, temporary help position is given precedence. Earnings in Panel B inflated to real 2001 dollars using the Consumer Price Index for urban consumers (series CUUR0000SA0).

Table 6:
OLS Estimates of Earnings and Hours Differentials in Temporary Help verus Direct Hire Jobs: Controlling for Occupation

	Hourly Wage	Weekly Earnings	Weekly Hours
Temporary Help Position	0.19	14.6	1.62
	(0.10)	(6.0)	(0.64)
Health Care	0.73	-37.0	-7.84
	(0.13)	(7.7)	(0.84)
Clerical	-0.13	-46.8	-5.81
	(0.16)	(9.5)	(1.03)
General Labor	-0.22	-25.7	-2.77
	(0.18)	(11.0)	(1.18)
Janitorial	-0.56	-79.5	-8.69
	(0.15)	(9.1)	(0.99)
Telemarketing	-0.23	-43.9	-5.00
	(0.19)	(11.5)	(1.24)
Food Services	-0.68	-87.7	-9.41
	(0.14)	(8.5)	(0.92)
Laundering	-0.90	-65.3	-5.13
	(0.26)	(15.7)	(1.69)
Cashier	-0.80	-100.7	-10.88
	(0.15)	(8.9)	(0.96)
Sales	-0.42	-68.3	-8.04
	(0.17)	(9.9)	(1.07)
Child care	-0.89	-90.0	-8.73
	(0.19)	(11.3)	(1.22)
Waiter/Waitress	-1.23	-110.4	-10.34
	(0.23)	(13.7)	(1.47)
Education	0.14	-86.6	-12.93
	(0.25)	(15.0)	(1.62)
R^2	0.218	0.246	0.225

n=1,548. Standard errors in parentheses. Omitted occupational category is Production positions. Miscellaneous and Unknown occupational category dummies are included in regression models but are not tabulated. In addition to occupation dummies, all models include age and its square, and dummies for non-white, less than high school education, and non-white x less than high school, and a constant. Occupational titles and weekly earnings given for clients who obtained and completed 90 days of employment while in Work First programs during 1997 - 2000 period. If more than one type of employment obtained, temporary help position is given precedence. Earnings inflated to real 2001 dollars using the Consumer Price Index for urban consumers (series CUUR0000SA0).

Table 7:
Regression Estimates of Work First Client Earnings and Hours: OLS Models and IV Models using Program Assignment as Instrument for Temporary Help Employment

	A. OLS Models			B. Program Assignment as IV for Temporary Help		
	Hourly Wage	Weekly Earnings	Weekly Hours	Hourly Wage	Weekly Earning	Weekly Hours
	(1)	(3)	(2)	(1)	(3)	(2)
Temporary Help Position	7.35	248.8	33.79	7.17	216.1	30.31
	(0.06)	(3.7)	(0.40)	(0.69)	(41.1)	(4.39)
Direct Hire Position	6.73	186.3	27.22	6.69	178.6	26.40
	(0.04)	(2.6)	(0.27)	(0.17)	(9.9)	(1.06)
Black	0.01	2.3	0.35	0.02	3.0	0.43
	(0.05)	(3.0)	(0.32)	(0.05)	(3.2)	(0.34)
Less than HS	-0.26	-3.5	0.64	-0.26	-3.9	0.60
	(0.06)	(3.8)	(0.40)	(0.06)	(3.9)	(0.41)
Black x Less HS	0.06	-3.8	-0.79	0.06	-3.8	-0.79
	(0.08)	(4.9)	(0.52)	(0.08)	(4.9)	(0.53)
Age	0.02	2.1	0.26	0.02	2.9	0.35
	(0.02)	(1.2)	(0.12)	(0.03)	(1.5)	(0.16)
$Age^2 \times 10^{-2}$	-0.02	-2.8	-0.39	-0.03	-4.0	-0.52
	(0.03)	(1.9)	(0.20)	(0.04)	(2.4)	(0.26)
Constant	-0.02	-24.5	-4.23	-0.04	-28.4	-4.65
	(0.30)	(17.9)	(1.92)	(0.32)	(18.8)	(2.01)
R^2	0.917	0.738	0.831	.	.	.

n=2,778. Standard errors in parentheses. Sample is Work First clients randomly assigned to programs during 1st quarter 1997 through 3rd quarter 2000. Job status, hours and earnings refer to period while client was in work first program. If more than one type of employment obtained, temporary help position is given precedence. Omitted employment category is non-employment. All models contain controls for age and its square, and dummy variables for year-by-calendar quarter of program assignment. Instrument for temporary help employment in Panel B is program assignment. Earnings inflated to real 2001 dollars using the Consumer Price Index for urban consumers (series CUUR0000SA0)

Table 8:
Means of Job Retention and Program Recidivism Outcomes:
Overall, by Work First Program and by Employment Status.

	90-day retention	Recidivism 180 day	270 day	360 day	720 day	1-2 Yrs
All (n=2,778)	0.21 (0.01)	0.30 (0.01)	0.40 (0.01)	0.48 (0.01)	0.62 (0.01)	0.48 (0.01)
Direct Hire (n=1,193)	0.42 (0.01)	0.22 (0.01)	0.33 (0.01)	0.41 (0.01)	0.58 (0.01)	0.47 (0.01)
Temp Help (n=355)	0.26 (0.02)	0.32 (0.02)	0.43 (0.03)	0.53 (0.03)	0.65 (0.03)	0.52 (0.03)
Non-Employed (n=1,230)	.	0.37 (0.01)	0.47 (0.01)	0.54 (0.01)	0.65 (0.01)	0.48 (0.01)
Program 1 (n=1.921)	0.21 (0.01)	0.30 (0.01)	0.40 (0.01)	0.48 (0.01)	0.62 (0.01)	0.49 (0.01)
Program 2 (n=857)	0.21 (0.01)	0.31 (0.02)	0.41 (0.02)	0.49 (0.02)	0.61 (0.02)	0.47 (0.02)

Standard errors in parentheses. Means for Work First clients who were randomly assigned to programs during 1st quarter 1997 through 3rd quarter 2000. Job status refers to employment obtained while client was in work first program. If more than one type of employment obtained, temporary help position is given precedence. 90-day retention indicates whether client held a job for 90 consecutive days during Work First spell. Recidivism measures whether client commenced a new work first spell (or initial spell was ongoing) by specified number of days following initial assignment.

Table 9:
OLS Models of Work First Client 90-Day Job Retention and Program Recidivism.

	Outcome Measures					
	(1)	(2)	(3)	(4)	(5)	(6)
	90-day	Recidivism				
	retention	180 day	270 day	360 day	720 day	1-2 Yrs
Temp Job	0.249	-0.052	-0.035	-0.005	0.015	0.050
	(0.021)	(0.027)	(0.029)	(0.029)	(0.028)	(0.029)
Direct Hire Job	0.381	-0.133	-0.118	-0.098	-0.038	0.019
	(0.015)	(0.019)	(0.020)	(0.020)	(0.019)	(0.020)
Black	-0.033	0.115	0.146	0.196	0.215	0.211
	(0.017)	(0.022)	(0.023)	(0.024)	(0.023)	(0.024)
Less than HS	-0.076	0.033	0.038	0.070	0.090	0.094
	(0.022)	(0.027)	(0.029)	(0.029)	(0.029)	(0.030)
Black x	0.032	0.074	0.085	0.054	0.041	0.016
Less HS	(0.028)	(0.035)	(0.038)	(0.038)	(0.037)	(0.038)
Age	0.006	0.001	-0.001	-0.009	-0.015	-0.008
	(0.007)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
$Age^2 \times 10^{-2}$	-0.006	-0.005	-0.002	0.008	0.015	0.001
	(0.011)	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)
Constant	-0.145	0.232	0.393	0.560	0.743	0.531
	(0.102)	(0.130)	(0.139)	(0.140)	(0.136)	(0.141)
R^2	0.276	0.072	0.080	0.102	0.095	0.084

n=2,778. Standard errors in parentheses. Sample is Work First clients randomly assigned to programs during 1st quarter 1997 through 3rd quarter 2000. Job status refers to employment obtained while client was in work first program. If more than one type of employment obtained, temporary help position is given precedence. Omitted category is no employment during program period. 90-day retention indicates that client retained employment for 90 days in a job obtained during program spell. Recidivism indicates that client either reentered the program during specified period or original spell was still underway and client was not currently employed. All regression models also control for age and its square, and dummy variables for year-by-calendar quarter of program assignment

Table 10:
Instrumental Variables Models of Work First Client 90-Day Job Retention and Program Recidivism Using Program Assignment as Instrumental Variable for Temporary Help Employment

	Outcome Measures					
	(1)	(2)	(3)	(4)	(5)	(6)
	90-day	Recidivism				
	retention	180 day	270 day	360 day	720 day	1-2 Yrs
Temp Job	-0.300	-0.132	-0.022	-0.053	0.304	0.498
	(0.258)	(0.294)	(0.313)	(0.315)	(0.312)	(0.331)
Direct Hire Job	0.253	-0.152	-0.115	-0.109	0.030	0.124
	(0.062)	(0.071)	(0.076)	(0.076)	(0.075)	(0.080)
Black	-0.021	0.117	0.146	0.197	0.209	0.202
	(0.020)	(0.023)	(0.024)	(0.024)	(0.024)	(0.026)
Less than HS	-0.082	0.032	0.038	0.070	0.094	0.099
	(0.024)	(0.028)	(0.029)	(0.030)	(0.029)	(0.031)
Black x	0.032	0.074	0.085	0.054	0.041	0.016
Less HS	(0.031)	(0.035)	(0.038)	(0.038)	(0.038)	(0.040)
Age	0.019	0.003	-0.001	-0.008	-0.022	-0.019
	(0.010)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)
$Age^2 \times 10^{-2}$	-0.026	-0.008	-0.002	0.006	0.025	0.017
	(0.015)	(0.017)	(0.018)	(0.018)	(0.018)	(0.019)
Constant	-0.210	0.223	0.394	0.551	0.777	0.584
	(0.118)	(0.135)	(0.144)	(0.145)	(0.143)	(0.152)

n=2,778. Standard errors in parentheses. Sample is Work First clients randomly assigned to programs during 1st quarter 1997 through 3rd quarter 2000. Job status refers to employment obtained while client was in work first program. If more than one type of employment obtained, temporary help position is given precedence. Omitted category is no employment during program period. 90-day retention indicates that client retained employment for 90 days in a job obtained during program spell. Recidivism indicates that client either reentered the program during specified period or original spell was still underway and client was not currently employed. All regression models also control for age and its square, and dummy variables for year-by-calendar quarter of program assignment. Instrumental variable for temporary help employment status is program assignment.

Appendix Table 1:
Impact of Program Assignment on Hours and Earnings of Clients who Obtained Employment While in Work First Programs.

	A. Temporary Help			B. Direct Hire Positions		
	Hourly Wage	Weekly Earning	Weekly Hours	Hourly Wage	Weekly Earning	Weekly Hours
	(1)	(3)	(2)	(1)	(3)	(2)
Program 1	-0.35	-19.65	-0.93	0.07	0.52	-0.26
	(0.17)	(11.03)	(1.35)	(0.09)	(5.34)	(0.55)
Black	-0.30	-2.10	0.83	0.12	5.37	0.52
	(0.18)	(11.43)	(1.40)	(0.10)	(5.84)	(0.60)
Less than HS	-0.61	5.05	3.23	-0.47	-9.22	0.77
	(0.23)	(15.05)	(1.84)	(0.13)	(7.65)	(0.79)
Black x	0.38	-32.56	-5.62	0.02	-1.89	-0.55
Less HS	(0.29)	(18.44)	(2.25)	(0.18)	(10.16)	(1.05)
Age	0.07	0.88	0.07	0.06	6.85	0.82
	(0.08)	(5.43)	(0.66)	(0.04)	(2.41)	(0.25)
$Age^2 \times 10^{-2}$	-0.09	-2.75	-0.37	-0.06	-9.40	-1.19
	(0.14)	(8.85)	(1.08)	(0.07)	(3.90)	(0.40)
Constant	7.11	303.28	38.22	6.24	76.59	12.78
	(1.30)	(83.73)	(10.22)	(0.66)	(37.65)	(3.89)
R^2	0.134	0.161	0.102	0.061	0.053	0.044
n		354			1192	

Standard errors in parentheses. Standard errors in parentheses. Sample is Work First clients randomly assigned to programs during 1st quarter 1997 through 3rd quarter 2000 who retained employment for 90 days during the program. Job status, hours and earnings refer to period while client was in work first program. If more than one type of employment obtained, temporary help position is given precedence. All models contain controls for age and its square, and dummy variables for year-by-calendar quarter of program assignment. Earnings inflated to real 2001 dollars using the Consumer Price Index for urban consumers (series CUUR0000SA0).