The Effect of Work First Job Placements on the Distribution of Earnings: An Instrumental Variable Quantile Regression Approach

David H. Autor, *Massachusetts Institute of Technology and National Bureau of Economic Research*

Susan N. Houseman, *W. E. Upjohn Institute*

Sari Pekkala Kerr, *Wellesley College*

Government employment programs for low-skilled workers typically emphasize rapid placement of participants into jobs, of which many are temporary-help jobs. Using data from Detroit's welfare-to-work program and the Chernozhukov-Hansen instrumental variables quantile regression method, we find that neither direct-hire nor temporary-help job placements significantly affect the lower tail of the earnings distribution. In the upper tail, direct-hire placements yield sizable earnings increases for over half of participants, while temporary-help placements yield significant earnings losses at higher quantiles. Our results cast doubt on the efficacy of employment programs' exclusive focus on rapid job placement and their widespread reliance on temporary-help placements.

I. Introduction

Compared to other advanced economies, the United States spends relatively little on active labor market programs. Instead, US programs targeting

We are grateful to Christian Hansen for many valuable comments on the paper and extensive guidance on the IVQR method. We thank Josh Angrist, Brigham

[*Journal of Labor Economics*, 2017, vol. 35, no. 1]
 0734-306X/2017/3501-0002$10.00
Submitted March 28, 2012; Accepted March 1, 2016; Electronically published October 28, 2016

disadvantaged workers focus primarily on providing job search and job placement services rather than skills development.[1] Although evaluation evidence suggests that programs emphasizing job placement are successful on average in raising earnings and employment of participants (Bloom et al. 2005; King and Mueser 2005; Dyke et al. 2006; Autor and Houseman 2010), the emphasis on job placement is controversial. Average earnings gains of program participants may mask considerable heterogeneity in program effects and high rates of failure, particularly among the most disadvantaged participants. Many argue that alternative strategies are needed, though cost-effective alternatives have been elusive (see, e.g., Fraker et al. 2004).

One particularly controversial aspect of government job placement programs such as the Workforce Investment Act (WIA) and welfare-to-work is that these programs place a large number of participants in employment with temporary-help agencies rather than directly with employers. In the Detroit welfare-to-work program that we study in this paper, 20% of the job placements obtained through the program were with temporary-help agencies versus 80% with direct-hire employers. Available evidence indicates that such high placement rates are the norm rather than the exception. For example, Heinrich, Mueser, and Troske (2009) find that participation in government employment programs in Missouri is associated with a 50%–100% increase in the incidence of temporary-help employment relative to employment in other industries.[2] Debate over the impact of temporary-help employment has spurred numerous studies of its effects on low-skilled workers' labor market advancement in the United States and Europe.[3]

Frandsen, Patrick Kline, and Blaise Melly for excellent suggestions, the participants of the NBER Labor Studies summer institute for helpful discussion, and Lillian Vesic-Petroivic for research assistance. Sari Pekkala Kerr gratefully acknowledges the Yrjö Jahnsson Foundation, and David Autor and Susan Houseman thank the Russell Sage Foundation for financial support of this research. Contact the corresponding author, David Autor, at dautor@mit.edu. Information concerning access to the data used in this article is available as supplementary material online.

[1] OECD publishes cross-country comparisons of expenditures on labor market programs: http://stats.oecd.org/index.aspx?r=488782.

[2] Administrative data from various states show that 15%–40% of recent welfare leavers who found employment worked in the temporary-help sector (Pawasarat 1997; Cancian et al. 1999; Autor and Houseman 2002; Heinrich, Mueser, and Troske 2005). Many of these individuals would have participated in welfare-to-work programs. Given that temporary-help employment represents about 2% of daily payroll employment in the United States, the incidence of temporary-help employment in this population is especially striking.

[3] US studies include Ferber and Waldfogel (1998), Lane et al. (2003), Corcoran and Chen (2004), Benner, Leete, and Pastor (2007), and Autor and Houseman (2010). Autor and Houseman (2010) contains citations to many recent European studies. Those critical of placing low-skilled workers with temporary-help agencies argue that these jobs tend to be unstable and low-paying and offer few chances for skills develop-

To our knowledge, all studies analyzing the causal effect of either temporary-help or direct-hire placements on outcomes among participants in government employment programs focus on mean effects, that is, average gains in earnings and employment. This exclusive focus on mean effects is a potentially important shortcoming, since it seems unlikely that most participants obtain the "average" benefit or even close to it. Given the range of skills deficits that such participants present, there is likely to be considerable heterogeneity in the causal effects of direct-hire and temporary-help employment on the distribution of their subsequent earnings outcomes.[4] Of particular interest is whether either temporary-help or direct-hire jobs improve outcomes for the least advantaged—those in the lower tail of the earnings distribution.

The current paper offers the first evidence of which we are aware on these distributional questions. Drawing on a unique data set of Detroit's welfare-to-work program used in Autor and Houseman (2010), we estimate the impact of welfare-to-work job placements on the distribution of participants' earnings over a 7-quarter period. Participants in Detroit's welfare-to-work program, known as "Work First," are assigned on a rotational basis to one of two or three contractors operating in their district of residence. Rotational assignment—which is functionally equivalent to random assignment—among contractors with systematically different job placement rates enables us to separately identify the causal effects of both temporary-help and direct-hire placements on the distribution of earnings outcomes.

Our earlier work using these data found large positive and significant mean effects of direct-hire job placements on subsequent earnings but negative, though largely insignificant, mean effects of temporary-help job placements on earnings outcomes. This paper explores the entire distribution of causal effects using the instrumental variables quantile regression (IVQR) method developed by Chernozhukov and Hansen (2004a, 2005, 2006). This tool has seen limited applications in empirical work to date, and we are not aware of any prior paper that applies this estimator to a setting with multiple endogenous variables and multiple instruments.

Applying the Chernozhukov-Hansen IVQR technique reveals that the effects of job placement differ substantially across percentiles of the conditional earnings distribution. We find that neither direct-hire nor temporary-

ment or advancement (Parker 1994; Pawassarat 1997; Jorgensen and Riemer 2000; Benner et al. 2007). Others point out that temporary-help jobs may serve as important ports of entry into employment for low-skilled workers. Temporary-help jobs may directly lead to employment with the client company or help workers build skills and experience, thereby facilitating transition to more stable direct-hire jobs (Abraham 1988; Katz and Krueger 1999; Autor 2001, 2003; Houseman 2001; Autor and Houseman 2002; Kalleberg, Reynolds, and Marsden 2003).

[4] Corcoran and Chen (2004) and Andersson, Holzer, and Lane (2009) conduct some subgroup analyses of temporary-help employment.

help job placements significantly affect the lower tail of the earnings distribution. Direct-hire placements, however, substantially raise the upper tail, yielding sizable earnings increases for more than 50% of participants over the medium term (1–2 years following placement). Conversely, temporary-help placements have zero or negative earnings impacts at all quantiles. At higher quantiles, these effects are economically large and are significantly different both from zero and from the estimated effects of direct-hire placements. Unusual among quantile instrumental variables analyses, our analysis statistically rejects the hypothesis that the heterogeneity we detect in treatment effects arises by chance; that is, treatment effect differentials between the top and bottom quartiles of the effects distribution are, in the case of direct-hire placements, both economically and statistically significant.[5]

Analyses of the dynamics of job placements provide insights into the mechanisms underlying the disparate effects that direct-hire and temporary-help placements have in the upper tail of the earnings distribution. Among higher potential earners, we find that direct-hire job placements foster further direct-hire earnings and longer job tenures. In contrast, while temporary-help placements may increase future earnings in the temporary-help sector, they simultaneously reduce direct-hire earnings and job tenures among those in the upper tail of the earnings distribution. Thus, it appears that temporary-help placements fail to improve subsequent earnings among these workers because the temporary assignments are short-lived and do not generally serve as stepping stones to more stable, direct-hire jobs. By implication, those with relatively high potential earnings among the disadvantaged Work First population may fare better finding jobs on their own than they would accepting placements with temporary-help agencies through the Work First program.

Substantively, our findings raise concerns about the extensive use of temporary-help agencies in government employment programs. In addition, our findings that neither direct-hire nor temporary-help job placements improve subsequent earnings among those in the lower tail of the earnings distribution reinforce skepticism that programs focused primarily on job placement can help the hardest to serve.

The remainder of the paper is organized as follows. Section II provides background on the Detroit Work First program, the data used in our analysis, and the characteristics of our participant sample. Sections III and IV present our econometric framework and tests of the validity of our research design. Section V presents our empirical findings, and Section VI concludes.

II. Description of the Program, Data, and Participant Characteristics

Welfare reform legislation passed in 1996 created financial incentives for states to set minimum mandatory work requirements as a condition for re-

[5] Chernozhukov and Hansen (2004a) also reject the null of constant treatment effects in their study of the effect of 401K eligibility on wealth accumulation.

ceipt of Temporary Assistance for Needy Families (TANF) benefits. In Michigan, applicants who do not meet mandatory work requirements specified in the state legislation must participate in the state's welfare-to-work program, Work First. Refusal to participate may result in a reduction of welfare checks and food stamps. As is apparent in the program's title, the primary goal of Work First is to place participants rapidly into jobs.

A. The Detroit Work First Program

In the Detroit Work First program that we study, participants are assigned to a contract service provider who operates in the geographic district in which they reside. Program operations are divided into 16 districts or neighborhoods, and in 14 of these districts, two or three Work First providers serve the district. Contracts with service providers are written each year, with the set of contractors servicing a district occasionally changing from one year to the next. Importantly, when at least two contractors operate in a district, Work First participants are assigned to a contractor on a rotating basis, meaning that the contractor to which a participant is assigned is determined solely by the participant's application date. This procedure is functionally equivalent to random assignment of participants to contractors, as we demonstrate formally below.

All contractors provide a standard 1-week training course aimed at improving job applications and other skills of the participants. Under the program, each participant develops a résumé and is guided through the proper techniques for completing job applications and handling interviews. In addition, all participants are eligible for support services, such as childcare and transportation, that are provided outside of the Work First program. The Work First program, however, emphasizes intensive full-time job search and placement of participants into jobs. During a Work First spell, program participants may be placed with a temporary-help agency or directly with an employer (a direct-hire job). Alternatively, a participant may leave the program without a job placement. By the second quarter following entry, nearly all participants either are placed in a job or exit the program without having obtained a job.

By design, contractors have little scope for affecting participant outcomes other than through job placements. The training and support services provided by Work First contractors are minimal and do not differ measurably among contractors. Despite this, contractors display systematic differences in their propensities to place participants into direct-hire jobs, temporary-help jobs, or no jobs at all. These systematic differences in placement rates across contractors with statistically identical populations, stemming from differences in contractor practices, enable us to estimate the effects of job placement type on the distribution of subsequent employment outcomes.[6]

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

Evidence presented below indicates that, in our sample, the effect of contractor assignment on the probability that a participant is placed into a direct-hire, temporary, or no job does not systematically vary according to participant characteristics. This allows us to interpret the heterogenous effects of job placements on earnings as reflecting heterogeneity in treatment effects rather than heterogeneity in the subpopulation "treated" across contractors.

B. The Data and the Sample

Our data on participants in the Detroit Work First study come from two sources. The first is administrative data from the Detroit Work First program. The administrative data cover all Work First spells that commence between the fourth quarter of 1999 and the first quarter of 2003 and include the name of the employer for all participants placed into jobs during their Work First spells. Using detailed lists of temporary-help firms operating in the Detroit metropolitan area, we code whether the Work First placement was a temporary-help or a direct-hire job. The Detroit Work First administrative data also contain information on the occupation (26 categories), hourly wages, and weekly hours of jobs that participants obtain through the program. These Work First administrative data are linked to Unemployment Insurance (UI) earnings records from the state of Michigan. From the state data we have information on UI earnings and industry of employment for each job held during the 8 quarters before and the 8 quarters following a participant's entry into the Work First program.[7] Therefore, while the state UI data provide total quarterly earnings on each job held, the hourly wage, hours worked, and duration of the job within the quarter is unknown. We are generally unable to determine whether the employer of the Work First job is the same as an employer during the postplacement follow-up. In addition, it is important to note that when a firm hires a worker through a temporary-help agency, the temporary agency is the employer of record and in neither the Work First administrative data nor the state UI data is the identity of the client firm recorded.

most effective and hence pursue different policies. Contractors do not have access to UI wage records data (used in this study to assess participants' labor market outcomes), and they collect follow-up data only for a short time period and only for individuals placed in jobs. Therefore, they cannot rigorously assess whether job placements improve participant outcomes or whether specific job placement types matter. During in-person and phone interviews conducted by the authors, contractors expressed considerable uncertainty, and differing opinions, about the long-term consequences of temporary job placements (Autor and Houseman 2006).

[7] Earnings of federal and state workers and the self-employed are excluded from these data.

The data set used in our analysis covers 30,522 Work First spells. Some participants have more than one Work First spell.[8] Our data include only participants who initiated their Work First spell in a district that had at least two contractors, who were age 16–65 at the beginning of the spell, and who earned less than $15,000 per calendar quarter during the 7-quarter follow-up period. In addition, we drop two districts where the participant assignment was not rotated among contractors but rather was based on language needs. We exclude any Work First spells in districts where at least one contractor was not assigned any program participants during the calendar quarter in which the participant entered. Finally, as discussed further below, we exclude instances in which the effect of contractor assignment on job placement type varied systematically according to participant characteristics.

C. Participant Characteristics

Table 1 summarizes key demographic, work history, and employment and earnings outcomes for our Work First sample, both for the full set of Work First spells and separately for spells ending in each of three Work First placement outcomes: direct-hire placement, temporary-help placement, or no job placement. Of the 30,522 Work First spells, 38% lead to direct-hire job placement, another 9% lead to a temporary-help placement, and 53% of spells end without any job placement. Nearly all Work First participants in our sample are black women. The jobs that participants obtain during their Work First spells are, as expected, correlated with their demographic characteristics, prior labor market history, and labor market outcomes. Those who are not placed into any job during their Work First spell are less educated and have lower earnings prior to entering the program relative to those who were placed in a job during the program. Although the administrative records provide data on education in only 81% of the Work First spells (with the remainder missing education data), these figures indicate that a small fraction of the population has some postsecondary education and a large fraction dropped out of high school. No comprehensive national data on the demographic characteristics of participants in welfare-to-work programs exist, but our Detroit study population appears typical of these populations in large urban areas, according to data compiled in a study of welfare-to-work participants enrolled in programs in 18 cities from 1999 to 2002, a time period that coincides with that covered by our Detroit data. Welfare-to-work participants are predominantly female and low-educated. While the racial and ethnic composition of participants varies according to locale, participants are disproportionately minorities. During the same time period as our study, the African American share of welfare-to-work

[8] Autor and Houseman (2010) show that results based on a sample limited to participants' first spell are closely comparable to those based on the full sample of Work First spells.

Table 1
Summary Statistics for Primary Sample of Work First Participants, 1999–2003: Overall and by Job Placement Outcome

	Job Placement Outcome during Work First Spell							
	All		No Employment		Direct Hire		Temporary Help	
	Mean	SE	Mean	SE	Mean	SE	Mean	SE
Percentage of sample	100.0		53.0		37.9		9.0	
	A. Demographics							
Age	29.6	.05	29.3	.06	29.7	.07	30.4	.15
Female (%)	94.1	.13	94.4	.18	93.9	.22	93.3	.48
Black (%)	97.2	.09	97.1	.13	97.0	.16	98.3	.25
White (%)	2.1	.08	2.2	.11	2.3	.14	1.2	.21
Other (%)	.7	.05	.7	.01	.7	.08	.5	.14
< High school (%)	36.9	.28	39.9	.38	33.3	.43	34.4	.90
High school (%)	36.1	.27	34.0	.37	38.2	.45	39.6	.93
> High school (%)	7.8	.15	7.2	.20	8.7	.26	8.0	.52
Unknown (%)	19.1	.22	18.8	.31	19.8	.37	17.9	.73
	B. Job Placement Outcomes during Work First Assignment for Employed Participants							
Hourly wage ($)	7.53	.02	NA		7.45	.02	7.89	.04
Hours per week	34.1	.06	NA		33.5	.07	36.6	.12
Total earnings ($)	260	.80	NA		253	.90	289	1.64

	C. Work History in 8 Quarters Prior to Contractor Assignment: Quarterly Means							
Total wage earnings ($)	1,171	9	1,039	11	1,309	14	1,366	29
Direct-hire earnings ($)	1,032	8	915	11	1,172	14	1,129	28
Temporary-help earnings ($)	139	2	124	3	136	4	237	10
	D. Labor Market Outcomes in 7 Quarters (2–8) Following Contractor Assignment: Quarterly Means							
Employed Q2–Q4 post–Work First (%)	67.5	.3	58.4	.4	77.6	.4	78.2	.8
Employed Q5–Q8 post–Work First (%)	67.5	.3	61.3	.4	74.5	.4	74.6	.8
Total wage earnings ($)	1,229	9	935	12	1,575	16	1,499	32
Direct-hire earnings ($)	1,078	9	817	11	1,429	16	1,138	30
Temporary-help earnings ($)	136	3	108	3	128	5	338	15
Longest spell earnings ($)	955	8	731	10	1,229	14	1,118	28
N	30,522		16,177		11,583		2,762	

NOTE.—The sample is all Work First spells initiated from the fourth quarter of 1999 through the first quarter of 2003 in 12 Work First randomization districts in Detroit, Michigan. Participants may have multiple spells in the data. The data source is administrative records data from Work First programs linked to quarterly earnings from Michigan Unemployment Insurance wage records; statistics reported in panels A and B come from the Detroit Work First program administrative data, while the statistics reported in panels C and D are derived from the state Unemployment Insurance (UI) wage records data. Job placement outcomes are coded using Detroit administrative records. Temporary-help versus direct-hire employers during the pre- and post-program periods are identified using industry codes in the state UI wage records data. All earnings are inflated to 2003 dollars using the Consumer Price Index (CPI-U).

participants was 92% in Chicago, 89% in Philadelphia, and 87% in Nashville (Fraker et al. 2004, Exhibit II B).

As previously noted, the Work First administrative data provide information on the occupation, hourly wage, and weekly hours in jobs obtained through the program. Notably, panel B of table 1 shows that, as compared to Work First direct-hire jobs, temporary-help jobs pay a somewhat higher average hourly wage ($7.89 vs. $7.45), have longer weekly work hours (36.6 vs. 33.5 hours per week), and so have higher implied weekly earnings ($289 vs. $253). Consistent with national data, temporary-help placements in our Detroit Work First data are heavily concentrated in industrial, general labor, and clerical occupations, which together account for 63% of all temporary-help placements compared to 22% of direct-hire placements (appendix table A1). We examine the extent to which these large differences in occupational distribution account for the higher wages and weekly earnings in the temporary-help jobs obtained through Work First. A simple decomposition shows that $0.17 of the $0.44 hourly wage differential (38%) is accounted for by the fact that temporary-help jobs are concentrated in occupations with higher average hourly wages, while the remainder reflects higher hourly wages of temporary-help workers within occupations. Similarly, differences in the occupational distribution of temporary-help and direct-hire jobs explain 53% of the weekly earnings differential, while 47% is explained by higher weekly earnings of temporary-help workers within occupations.[9]

Using Michigan UI earnings records data matched with Work First administrative data, we display the earnings of Work First participants in the 8 quarters prior to program entry in panel C of table 1. Although the earnings differences between those receiving some type of job placement and those with no Work First job placement are particularly stark, notable differences among those placed into temporary-help and direct-hire jobs are also evident. Those placed with temporary-help agencies have slightly higher total earnings and earnings from temporary-help agencies but somewhat lower earnings from direct-hire employers in the 8 quarters prior to entering the program than those placed directly with employers.

We also track labor market outcomes of Work First participants in quarters 2–8 following Work First entry (panel D, table 1).[10] Participants are

[9] The differential accounted for by differences in the occupational distribution of temporary help and direct-hire placements is $\Sigma_i[s_{i,\,\mathrm{temp}}w_i - s_{i,\,dh}w_i]$, where s_i is the proportion of temporary-help or direct-hire placements in occupation i and w_i is the average wage or weekly earnings in occupation i.

[10] By the second quarter following Work First entry, virtually all participants have been either been placed into a job or terminated from the program: among those placed into a job, 99.6% have been placed by the second quarter following entry; among those terminated without a placement, 97.6% have been officially terminated by the second quarter, according to Work First administrative records. Thus, we treat employment and earnings in these 7 quarters as post-program outcomes, and we do not include the first post-entry quarter in our outcome data.

coded as employed in a quarter if they have any UI earnings during that quarter. Average employment is defined as the fraction of quarters with nonzero UI earnings over the follow-up period. This measure of nonemployment is admittedly crude, but, as noted, state UI earnings data do not provide information on the duration of jobs. Therefore, spells of nonemployment that last less than 3 month will be missed, and, depending on the date they commence, nonemployment spells lasting between 3 and 6 months may be missed in the data.[11] With this caveat, those not placed into a job during the Work First spell are less likely to be employed than those placed into a direct-hire or temporary-help job in quarters 2–8 following program entry. They also experience lower earnings in the 7-quarter follow-up period.

During quarters 2–8 following Work First assignment, the incidence of employment is slightly higher but not significantly different for those receiving temporary-help placements compared with those placed directly with employers (panel D), though again these statistics must be interpreted with caution because the state UI data only capture long spells of nonemployment. Despite the higher weekly earnings evidenced in their Work First jobs, those placed into temporary-help jobs have modestly lower average quarterly earnings (−$76) compared with those placed into direct-hire jobs in post-assignment quarters 2–8.

Panel D of table 1 also reports earnings from direct-hire and temporary-help jobs and from the longest continuously held job during post-assignment quarters 2–8 based on employer information contained in the UI data. In identifying the longest-held job, we selected the job with the highest earnings in cases of ties (i.e., a participant holding more than one job lasting the same number of quarters). Notably, the overwhelming majority of earnings in quarters 2–8 derive from direct-hire jobs; even for those receiving a temporary-help placement, 76% of post-assignment earnings, on average, come from direct-hire jobs. This figure is 91% for those with a direct-hire placement and 87% for those with no job placement.[12] In addition, over the 7-quarter follow-up period, more than three-fourths of earnings derive from a single employment spell, on average, with little variation according to Work First job placement type. These descriptive statistics suggest a strong link between durable employment spells and overall earnings.

The empirical focus in this paper concerns the causal effects of temporary-help and direct-hire job placements on the distribution of subsequent earn-

[11] If, for example, a participant worked on January 1 and on June 30 but was unemployed during all intervening days, the participant would appear as employed for the first 2 quarters of the year. Any longer spell of continuous nonemployment would necessarily generate at least 1 quarter with zero earnings.

[12] Because participants' industry of employment—used to code whether the employer is a temporary-help firm or a direct-hire employer—is missing in a small fraction of cases, direct-hire and temporary-help earnings do not sum precisely to total earnings.

ings. Table 2 provides summary statistics of mean quarterly earnings in post-assignment quarters 2–8 for all Work First spells and by placement type at selected percentiles of the earnings distribution. Not surprisingly, the entire distribution of earnings outcomes is lower for those who did not receive a Work First job placement compared with those who did. A sizable share—21% of all participant outcomes and 27% of those whose Work First spell ended without any placement—had no UI earnings in the 7-quarter follow-up period.

Panel B of table 2 shows the share of earnings over the follow-up period coming from direct-hire jobs at various points in the earnings distribution. Notably, the direct-hire share is the lowest (and the temporary share the highest) in the lowest earnings quantiles.[13] At the 25th percentile of total earnings, only 68% of earnings come from direct-hire employment, while at the 75th percentile, 85% of earnings come from direct-hire jobs. Also notable is that, though lower than for the other groups, for those with temporary-help placements, 64%–75% of earnings in the follow-up period come from direct-hire jobs. This fact implies that transitions from temporary-help to direct-hire jobs are common in this low-skill group.

III. The IVQR Method and Estimation

To analyze the effects of Work First job placements on the distribution of earnings requires a methodology that allows for causal inference in a quantile regression framework. We utilize the instrumental variable quantile regression method (IVQR) proposed by Chernozhukov and Hansen (2004a, 2005, 2006), which proves well suited to our quasi-experimental setting, albeit at the expense of imposing somewhat restrictive assumptions on the quantile process.[14] The basic assumptions and structure of the model are discussed in detail by Chernozhukov and Hansen and summarized here.

The econometric model is estimated on a data set with n observations, a continuous outcome variable Y, a treatment indicator D, an instrument Z (binary or otherwise), and a vector of covariates X. In the Work First case, Y is post-placement earnings, D is a vector of dummies indicating placement

[13] The direct-hire share is computed as the average share for persons within a 1 centile range. For example, the share of direct-hire earnings at the 50th centile is the average share of direct-hire earnings among individuals whose earnings lie between the 50th and 51st centiles of the distribution. A majority of individuals derive their earnings during the 7-quarter follow-up period entirely from direct-hire jobs or entirely from temporary help jobs.

[14] An alternative quantile treatment effects estimator is provided by Abadie, Angrist, and Imbens (2002). This method is, however, only applicable for the case of a single binary treatment and binary instrument for a "just identified" model. Chernozhukov and Hansen (2004a) show that, despite different assumptions and estimation methods, the results obtained by these two techniques are closely comparable in the applications that they consider.

Table 2
Summary Statistics for Primary Sample of Work First Participants: Post-Placement Earnings Centiles during Quarters 2–8 by Earnings Centile and Decomposed by Type

	Job Placement Outcome During Work First Spell			
Earnings Interval	All	No Employment	Direct-Hire	Temporary-Help
	A. Total Wage Earnings, Average ($)			
Centile 15	0	0	12	22
Centile 25	34	0	178	176
Centile 50	548	292	953	874
Centile 75	1,792	1,230	2,420	2,232
Centile 85	2,778	2,095	3,362	3,267
	B. Proportion from Direct-Hire Earnings, Average (%)			
Centile 15	NA	NA	71	64
Centile 25	68	NA	85	66
Centile 50	82	76	88	75
Centile 75	85	86	94	72
Centile 85	85	86	92	65

NOTE.—The sample is all Work First spells initiated from the fourth quarter of 1999 through the first quarter of 2004 in 12 Work First randomization districts in Detroit, Michigan. Participants may have multiple spells in the data. The data source is administrative records data from Work First programs linked to quarterly earnings from Michigan Unemployment Insurance (UI) wage records. Job placement outcomes are coded using Detroit administrative records. Temporary-help versus direct-hire employers are identified using UI records industry codes. All earnings are inflated to 2003 dollars using the Consumer Price Index (CPI-U). The proportion of earnings from direct-hire earnings is calculated by dividing the sample into 100 centiles by total wage earnings and then calculating the direct-hire share for all persons within the centile.

into a temporary-help or direct-hire job, and Z is an indicator of the rotational Work First contractor assignment.

The causal effects of interest are defined using potential outcomes Y_d that are indexed against the treatment d. For each individual, only one component of the vector of potential latent outcomes $\{Y_d\}$ is observed. In particular, we are interested in the *conditional quantiles* of the potential outcomes, $\{QY_d(\tau|x), \tau\epsilon(0, 1)\}$, where τ indicates the quantile index. The quantile treatment effects reveal the causal effect of D on Y, holding unobserved heterogeneity (U_D) constant at $U_D = \tau$. Here U_D is the so-called rank variable, which characterizes heterogeneity among observationally similar individuals (i.e., in terms of their covariates and treatment status). The quantile treatment effect can then be written simply as $(\partial/\partial d)QY_d(\tau|x)$ or $QY_d(\tau|x) - QY_{d'}(\tau|x)$. If the treatment effect is nonconstant (heterogeneous), these effects will vary across quantiles τ. In most cases, there are plausible reasons to believe that the mean effect will not capture the treatment effect for all parts of the outcome distribution.

If the treatment is not selected in relation to $\{Y_d\}$, conventional quantile regression (QR) will estimate the conditional quantile treatment effects (Koenker and Bassett 1978). If, however, treatment status is determined endogenously, the estimates will be biased, and it is necessary to use a quantile model with instrumental variables. Assuming we have an instrument Z that is uncorrelated with the potential outcome other than through the treatment, we can recover the causal effect of D on Y over the whole distribution of Y.

The main assumptions of the model as given by Chernozhukov and Hansen (2004a, 737–38) are: (A1) The *potential outcomes* can be expressed $Y = q(d, x, U_d)$, where $U_d \sim U(0, 1)$, and $q(d, x, U_d)$ is strictly increasing and left-continuous in U_d. (A2) Given $X = x$, $\{U_d\}$ is *independent* of Z. (A3) Given $X = x$ and $Z = z$, $D = \delta(z, x, V)$ for any unknown function δ and random vector V. This is the selection equation. (A4) For each d and d', given (V, X, Z), U_d is equal in distribution to $U_{d'}$. In other words, the method requires *rank similarity*.[15] (A5) The researcher observes $Y = q(D, X, U_D)$, $D = \delta(Z, X, Y)$, X, and Z.

To estimate the model in a finite sample framework, consider the usual quantile regression (QR) objective function, which can be written as

$$q_n(\tau, \alpha, \beta, \gamma) = \sum \rho_\tau (Y_i - D_i'\alpha - X_i'\beta - Z_i'\gamma) V_i. \tag{1}$$

Here D is, again, the vector of endogenous variables, X is the vector of exogenous covariates, $Z_i = f(X_i, Z_i)$ is the vector of instrumental variables, and $V_i = V(X_i, Z_i) > 0$ is a scalar weight. Estimating the Chernozhukov and Hansen (2004a, 2004b) IVQR model involves several steps. First, define $\| x \| a = \sqrt{x'Ax}$, where $A(\tau)$ is a uniformly positive definite matrix. Second, for a given value of the structural parameter (α), run the usual quantile regression to obtain

$$(\widehat{\beta}(\alpha, \tau), \widehat{\gamma}(\alpha, \tau)) = \arg\min q_n(\tau, \alpha, \beta, \gamma). \tag{2}$$

Then, to find an estimate for $\alpha(\tau)$, seek the value of α that makes the coefficient on the instrumental variable, $\widehat{\gamma}(\alpha, \tau)$, as close to 0 as possible, since the instrument should only affect the outcome through its effect on treatment status.

In our Work First context, Y will be a measure of earnings following contractor assignment, D will indicate placement into employment through the Work First program, and Z will be an indicator of the contractor assignment. As we are interested in the effects of different types of employment, we categorize job placements as temporary-help (T) jobs or direct-hire jobs (D).

Specifically, our empirical conditional quantile models are of the form

$$\begin{aligned} q_\tau(Y_i | T_i, D_i, X_i, t, q) = {} & \alpha_\tau + \beta_{1\tau} T_i + \beta_{2\tau} D_i \\ & + X_i' \lambda_\tau(U) + \theta_{\tau, r(c(i))} + \delta_{\tau, t, q} + \gamma_{\tau, r(c(i)), t}, \end{aligned} \tag{3}$$

[15] Rank similarity requires that each individual's rank in the conditional outcome distribution is invariant in expectation, regardless of the treatment state. Controlling for covariates may be important for achieving rank similarity.

where the subscripts refer to participant Work First spell i in contractor c in randomization district r in assignment year t and quarter q. We denote contractors as $c(i)$ and randomization districts as $r(c(i))$ to indicate that each contractor is associated with one randomization district and each participant Work First spell with one contractor. The binary variables T and D indicate whether the participant obtained a temporary-help job or a direct-hire job, respectively. The vector of covariates (X) includes gender, white and Hispanic race, age and its square, and total UI earnings and quarters of employment in the 8 quarters preceding Work First assignment. Finally, the vector θ contains randomization district dummies, the vector δ contains year-by-quarter of assignment dummies, and the vector γ contains all two-way interactions between district and year.

To estimate the IVQR, valid instrumental variables are required. In our setting, exogenous variation in job placements is generated by the rotational placement of Work First participants with contractors. The randomization of participants to contractors occurs within districts during the specific program year. Importantly for the current purpose, there are significant, persistent differences across contractors in their placement rates into temporary-help and direct-hire jobs.[16] This makes it possible to use contractor assignments as instruments for the two types of job placements.

In principle, we could use contractor-by-year assignment dummies directly as instrumental variables in the IVQR model. In practice, the computational burden imposed by using dozens of instruments makes this approach infeasible. In place of these dummies, we generate two continuous instrumental variables that capture each contractor's average excess probability of placement into temporary-help and direct-hire employment.[17] Thus, to instrument for T_i and D_i in (3), we use the excess probabilities of placement into temporary-help and direct-hire employment by contractor, $\hat{P}^T_{ct}$ and $\hat{P}^D_{ct}$, estimated from linear probability models.

For contractor assignments to serve as a valid instrumental variable for participant job placement types, the estimated placement rates $\hat{P}^T_{ct}$ and $\hat{P}^D_{ct}$ must be independent of potential outcomes. In practice, independence is almost guaranteed by random assignment. In addition, contractors' placement rates of participants into temporary-help or direct-hire employment must be independent of other contractor characteristics that might influence participant outcomes. This assumption allows for the possibility that contractors

[16] Autor and Houseman (2010) provide a detailed discussion of the sources of these contractor differences and their validity as instrumental variables for job placements.

[17] Specifically, we estimate a linear probability model for job placement type (temporary-help and direct-hire), where the right-hand-side variables consist of the X's used in the quantile regression, while contractor-by-year-dummies are absorbed. Residuals from this regression, calculated by contractor-year, form the excess employment probabilities that we use as instruments in the IVQR estimation.

influence participants' post-program outcomes through mechanisms other than job placements so long as these contractor effects are not systematically related to placement rates. Autor and Houseman (2010) provide a detailed discussion of this important identifying assumption as well as several falsification tests. Most relevantly, they demonstrate that there is no statistically significant heterogeneity in contractor effects on participant earnings or employment that is not explained by contractor placement rates into temporary-help and direct-hire jobs.[18]

IV. Verifying the Research Design

Prior to implementing the analysis, we perform two checks on the validity of the research design. Since the objective of the IVQR analysis is to study the heterogeneous treatment effects of job placements on Work First participants, it is important to check, first, that the participants assigned to different treatments are ex ante comparable and, second, that the treatments that these participants receive do not differ systematically with participants' characteristics. If either condition is violated, we may confound heterogeneity in the treated populations or heterogeneity in the treatments administered with heterogeneity in the effects of treatment, which is the empirical object of interest.

Both of these potential threats to validity correspond to violations of assumption A2 (Independence). In particular, A2 requires that, conditional on the control variables, a participant's rank in the latent outcome distribution U_d is independent of the instruments. Because we do not observe latent ranks, this independence assumption is formally untestable. However, we can use as a rough proxy for participants' earnings ranks their observed earnings in the 8 quarters prior to contractor assignment. Not surprisingly, past earnings are highly predictive of future earnings: in an OLS regression of earnings in quarters 2–8 following contractor assignment on 8-quarter prior earnings, year-by-quarter dummies, and contractor by year-of-assignment dummies, the coefficient on prior earnings is 0.51 (SE = .006).

To use prior earnings to assess the plausibility of the independence assumption, we divide participants into three terciles based on prior earnings and then test whether contractor effects on placement rates differ systematically among participants drawn from different prior earnings terciles assigned to the same contractor. Under the assumption that prior earnings terciles are an informative proxy for latent earnings ranks, the independence assumption implies that if, for instance, a contractor increases the average probability of placing participants into temporary-help jobs by 2 percentage points relative to other contractors operating in the district, that contractor should likewise increase the probability by 2 percentage points for

[18] Formally, this is is shown using an overidentification test.

all of its participants irrespective of their characteristics (in particular, earnings tercile).

We implement this test using the following model:

$$D_{i,\, k(i)} = \alpha_{k(i)} + X_i' \lambda_{k(i)} + \pi_{k(i),\, c(i),\, t} + \theta_{k(i),\, r(c(i))} + \delta_{k(i),\, t,\, q} + \gamma_{k(i),\, r(c(i)),\, t} + \varepsilon_{k(i),\, c(i),\, t}, \tag{4}$$

where D_i is a dummy variable equal to one if during the participant's Work First spell i, the participant in prior earnings tercile k assigned to contractor c serving assignment district r in year t and quarter q received a direct-hire or temporary-help placement during the participant's assignment spell (with separate dichotomous variables for each outcome).[19] The vector θ contains dummies indicating randomization districts, the vector δ contains a complete set of year-by-quarter of assignment dummies, the vector γ contains all two-way interactions between district and year, and the vector X contains participant characteristics.

Of interest in this equation is π, a vector of contractor-by-year assignment dummies for each prior earnings tercile k. Within each contractor-year cell, we test the equivalence of the coefficient estimates on π across terciles. A low p-value for this test corresponds to a rejection of the null hypothesis that, in a particular year, a contractor's effect on the probability that its participants were placed into temporary-help or direct-hire jobs did not systematically differ according to participants' prior earnings tercile. A joint test of the equivalence of these coefficient estimates for all contractor-year cells provides an omnibus test of the null.

Table 3 displays the results of this exercise. In most cases, we accept the hypothesis that a contractor's effects on direct-hire and temporary-help placement probabilities do not differ systematically across the terciles of prior earnings. However, there are a total of 13 of 100 contractor-year cells for which we reject the equality of placement effects across earnings terciles. Most of these cases correspond to contractors serving a smaller number of participants, which may lead to the estimated heterogeneity in their placement effects. We eliminate these cells from the analysis, which reduces the sample size by 6,639 observations, or roughly 17%.[20] The final analytic sample consists of 30,522 observations. With these problematic cells removed, these tests readily accept the null of equality with p-values exceeding 0.75.

[19] In reality, the SUR model involves a matrix of dependent variables and error terms. Expositionally, it is sufficient to consider the single equation case.

[20] Their elimination also required us to drop 7 additional contractor-year cells for which only one contractor remained in a district-year. The median number of participants served by the 13 cells dropped due to rejection of the homogeneity null is 235, as compared to 330 participants for those cells retained. The median number of participants in the 7 additional cells that were dropped due to lack of a comparison contractor in the district-year was 339.

Table 3
Do Contractor Placement Rates Vary Systematically by Pre-program Characteristics? Testing for the Equality of Contractor Dummies by Tercile of Prior Earnings

	Probability of Direct-Hire Placement		Probability of Temporary-Help Placement	
	F-Value	Prob > *F*	*F*-Value	Prob > *F*
Test for equality of contractor dummies across prior earnings terciles	1.51	.00	1.21	.06
Full sample *N*	37,161		37,161	
Test for equality of contractor dummies across prior earnings terciles	.86	.83	.89	.77
Limited sample *N*	30,522		30,522	

NOTE.—The sample is all Work First spells initiated from the fourth quarter of 1999 through the first quarter of 2004 in 12 Work First randomization districts in Detroit, Michigan. Participants may have multiple spells in the data. The data source is administrative records data from Work First programs linked to quarterly earnings from Michigan Unemployment Insurance wage records. Job placement outcomes are coded using Detroit administrative records. Temporary-help versus direct-hire employers are identified using UI records industry codes.

We restrict our subsequent analysis to this sample, though we note that our findings are essentially unaffected if we instead use the full sample.[21]

The second validity test we perform is a check on covariate balance among participants assigned to contractors within each district and year. We apply a SUR (seemingly unrelated regressions) model to test for balance of the following covariates: sex, white race, other (nonwhite) race, age and its square, average employment probability in the 8 quarters before program entry, average employment probability with a temporary agency in these prior 8 quarters, average quarterly earnings in these prior 8 quarters, and average quarterly earnings from temporary agencies in the prior 8 quarters. Following our approach above, we performed this test for the full sample and separately by earnings tercile. If the assignment of participants to contractors is balanced within district-years as expected, these covariates should not systematically differ across contractors within district-year cells, either overall or by prior earnings tercile (our summary measure of potential earnings). In all cases, the data accept the null by a comfortable margin, with p-values in excess of .50.[22]

It deserves emphasis that neither acceptance of the null for equality of placement rates within contractor-year by prior-earnings tercile nor balance

[21] Similarly, we have tested whether, among individuals assigned to a particular contractor in a specific program year, job placement probabilities vary systematically according to an individual's quartile of prior earnings. Using this more stringent test leads to a slightly smaller sample (29,851), but again we find that our results are little affected by the sample used.

[22] Estimates are omitted for brevity.

of covariates by earnings tercile across contractor-years confirms that the latent rank assumptions of the Chernozhukov-Hansen model are satisfied or that the rotational assignment of participants effectively balances unobservable participant characteristics among contractors within a district-year cell. The fact that we are unable to reject these null hypotheses, however, supports the plausibility of the assumptions.

V. Main Results: The Effect of Work First Placements on the Earnings Distribution

This section presents estimates of the causal effect of Work First placements on the distribution of participants' quarterly earnings during quarters 2–8 following Work First contractor assignment, and it contrasts estimates obtained from ordinary least squares (OLS), two-stage least squares (2SLS), ordinary Quantile Regression (QR), and IVQR models. We begin in table 4 by estimating the relationship between any job placement (temporary-help or direct-hire) during the Work First spell and earnings. In table 5, we consider the separate causal effects of temporary-help and direct-hire placements. All models use the full sample of 30,522 spells and include the full set of covariates noted in equation (3). To facilitate interpretation of the ordinary least squares (OLS) models, we recenter all control variables by subtracting the mean for participants who did not obtain a job during their Work First spell. Thus, by construction, the intercept in the OLS estimates equals the mean of the outcome variable for Work First participants who were not placed into jobs.

A. Earnings Effects of Any Job Placements

Panel A of table 4 presents descriptive OLS estimates of equation (3). Participants who obtain a job placement during their Work First spell earn on average $498 more per quarter over the 7 subsequent quarters than participants who obtain no placement. This point estimate corresponds to an earnings gain of more than 50% relative to nonplaced participants, whose quarterly earnings average $935. The OLS model is likely to provide an upward-biased estimate of the causal effect of job placements, however, since less than half of all participants obtain employment during their Work First spell, and those who do obtain employment have higher average prior earnings and labor force attachment than those who do not. Using contractor assignments as instruments for job placements, the two-stage least squares model in the panel B of the table confirms this expectation. We estimate that job placement raises subsequent quarterly earnings by $299, which is 40% smaller than the OLS estimate, though still highly significant.[23]

[23] We tested the instruments based on contractor-year of assignment for validity and strength using the first-stage *F*-statistic, the Angrist-Pischke first-stage chi-squared test of underidentification, and the Angrist-Pischke *F*-statistics test

Table 4
The Effect of Work-First Job Placements on Subsequent Earnings Quarters 2–8 Following Work First Assignment: Single Endogeneous Variable

	Mean Effect	Conditional Quantile Treatment Effects .15	.25	.50	.75	.85
	A. OLS	C. Quantile Regression				
Any job placement	498***	20***	72***	336***	748***	953***
	(20)	[6]	[7]	[14]	[28]	[36]
Constant	935***	39***	178***	599***	1,321***	1,929***
	(10)	[4]	[6]	[9]	[15]	[22]
	B. 2SLS	D. IVQR				
Any job placement	299**	13	44	209***	352**	260
	(113)	[41]	[46]	[73]	[170]	[239]
Constant	1,026***	40**	187***	637***	1,478***	2,256***
	(50)	[15]	[19]	[28]	[74]	[127]
Wald test for constant treatment effects:						
Any job placement		Wald statistic (p-value)			10.03 {.007}	

NOTE.—N = 30,522. Robust standard errors, clustered on contractor, are in parentheses for the ordinary least squares (OLS) and two-stage least squares (2SLS) models. Conventional standard errors are in brackets for the conventional quantile regression (QR) and instrumental variable quantile regression method (IVQR) models. Each column corresponds to a separate regression. All models include dummy variables for year by quarter of assignment and assignment-district by year of assignment, and controls for age and its square, gender, white and Hispanic race, and total UI earnings and total quarters of employment in 8 quarters prior to Work First assignment. Earnings values are inflated to 2003 dollars using the Consumer Price Index (CPI-U). The Wald test for constant treatment effects compares the 15th and 75th quantiles; Wald statistic p-values are in curly braces.
** Significant at the .05 level.
*** Significant at the .01 level.

The OLS and 2SLS models estimate the conditional mean effect of Work First placements on participant outcomes, but they are not informative about the distributional impacts of these placements. Panel C presents descriptive (QR) estimates analogous to the OLS estimates in panel A. The association between job placement during the Work First spell and post-assignment earnings is significantly positive at all quantiles, ranging from $20 per quarter at the 15th percentile to $953 per quarter at the 85th percentile. Notably, the point estimate and the intercept at the 50th percentile are considerably smaller than the OLS analogs, indicating that the distribution of quarterly earnings outcome is right skewed.

of weak identification for models with a single endogenous variable, reported in table 4, and for models with two endogenous variables, reported in table 5 (see Angrist and Pischke [2009, 218], as well as the formula correction posted on the mostlyharmlesseconometrics.com website on October 30, 2009. The tests are available as part of Stata's IVREG2 package). In all cases, our instruments pass these tests with very high levels of significance.

Like the OLS estimates above, these conventional QR models are unlikely to be informative about causal effects of job placements. Panel D reports causal effects estimates using the IVQR model, in which we instrument for participants' job placements using the average excess job placement probabilities of Work First contractors in the year in which the participant entered the Work First program. The computation of the IVQR is conducted over a parameter space centered on the 2SLS estimate.[24]

Consistent with the above contrast between OLS and 2SLS estimates, the IVQR estimates are uniformly smaller than the conventional quantile estimates and are insignificant in some cases. The IVQR estimate for the effect of job placement at the 50th conditional quantile is $209, as compared to $336 for the corresponding QR estimate. Figure 1 provides additional detail on these results by plotting the estimated QR and IVQR relationships between job placements and quarterly earnings at percentiles 10–90 (accompanied by 95% confidence intervals). The causal effects of job placements on subsequent earnings are quite heterogeneous. Below the 35th percentile, the estimated treatment effect is close to zero,with a relatively narrow confidence band. From the 35th to 60th percentile, this effect rises nearly monotonically from approximately $100 to $250 per quarter. The estimated treatment effect is fairly uniform above this level, though precision is greatly reduced at higher quantiles. To formally test for the heterogeneity of treatment effects, we estimate a Wald test for the null hypothesis of constant quantile treatment effects. The test compares the IVQR estimates for quantiles 15 and 75 and finds that the constant treatment effects hypothesis can be rejected at the 1% level (panel D of table 4).

B. Distinguishing between Direct-Hire and Temporary-Help Placements

Table 5 enriches the previous models to separately identify the earnings impacts of temporary-help and direct-hire placements. The benchmark OLS estimates in panel A indicate that direct-hire jobs are associated with an increase in participants' subsequent quarterly earnings of $519 during

[24] Estimation is performed in Matlab using software developed by Chernozhukov and Hansen and available for download at http://faculty.chicagobooth.edu/christian.hansen/research/. As noted above, we use a scalar instrumental variable in the IVQR model (and two scalars in the models that distinguish temporary-help from direct-hire placements) because estimating the IVQR models with 80 contractor-year dummy variables proved computationally infeasible. Our two-step procedure for constructing the instruments using excess placement residuals in the second stage produces numerically identical estimates to conventional 2SLS models. For the IVQR models, we are able to make the direct comparison for a subsample of three large districts. In this comparison, our two-step IVQR procedure produces point estimates that are identical to the single step IVQR procedure and standard errors that are slightly more conservative (i.e., larger). The results are reported in appendix table A2.

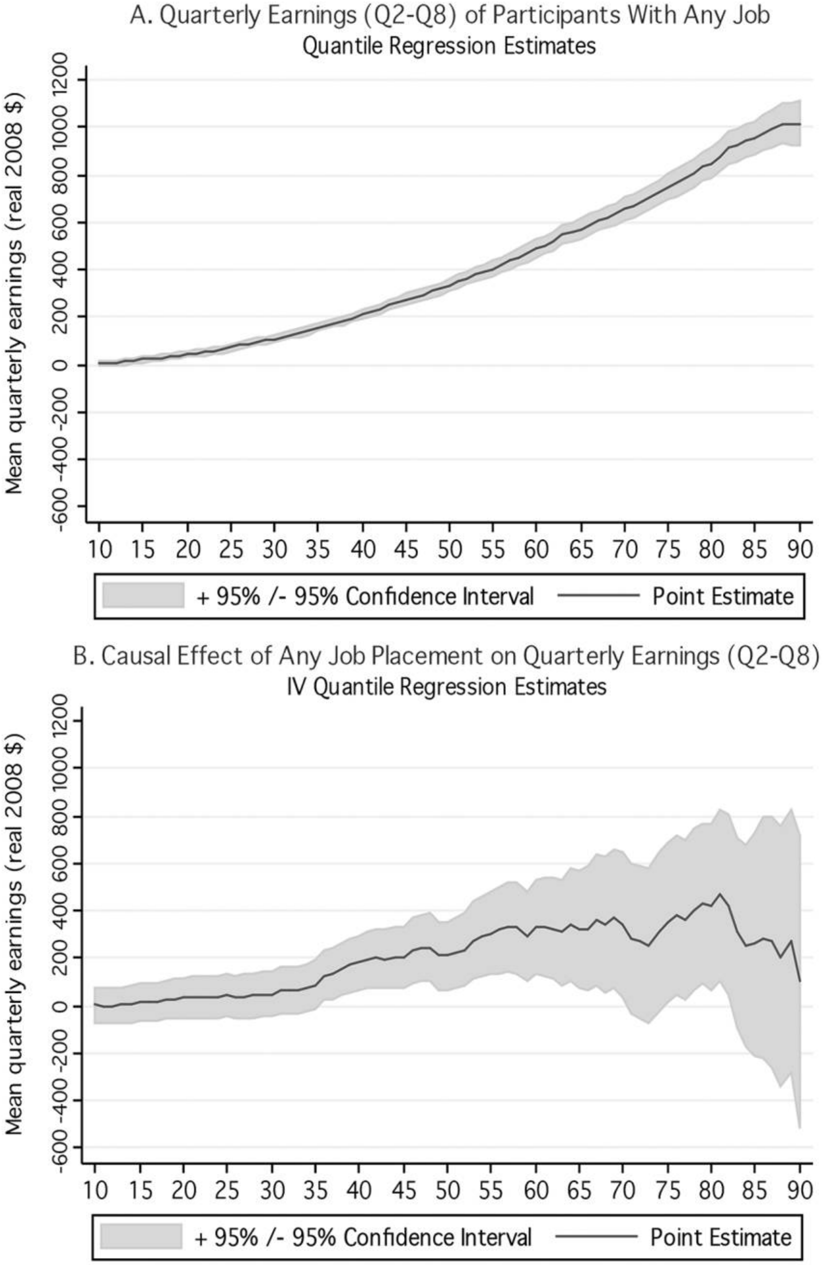

FIG. 1.—QR and IVQR estimates for earnings quarters 2–8 following assignment: single endogenous variable. Coefficient estimates are on the vertical axis and the quantile index is on the horizontal axis. The shaded region is the 95% confidence interval. A color version of this figure is available online.

Table 5
The Effect of Work-First Job Placements on Subsequent Earnings Quarters 2–8 Following Work First Assignment: Two Endogenous Variables

	Mean Effect	Quantile Treatment Effects at Quantile				
		.15	.25	.50	.75	.85
	A. OLS	C. Quantile Regression				
Direct-hire placement	410***	19***	77***	350***	783***	995***
	(31)	[7]	[8]	[15]	[30]	[39]
Temporary-help placement	519***	23*	59***	269***	551***	784***
	(23)	[12]	[14]	[26]	[48]	[72]
Constant	935***	39***	178***	599***	1,275***	1,931***
	(10)	[4]	[6]	[9]	[14]	[22]
	B. 2SLS	D. IVQR				
Direct-hire placement	503***	0	53	236*	661**	1,046**
	(159)	[0]	[75]	[138]	[270]	[478]
Temporary-help placement	−57	0	7	106	−254	−977***
	(201)	[1]	[106]	[192]	[277]	[209]
Constant	982***	0	181***	628***	1,452***	2,060***
	[56]	[0]	[21]	[34]	[70]	[135]
Wald test for constant treatment effects:						
Direct-hire placement		Wald statistic {*p*-value}			12.18	
					{.002}	
Temporary-help placement		Wald statistic {*p*-value}			1.00	
					{.608}	
Joint test for the two treatments		Wald statistic {*p*-value}			14.33	
					{.006}	
Wald test for equality of direct-hire and temporary-help placement effects:						
Wald statistic	3.73	.39	2.88	10.81	13.98	21.72
p-value	{.063}	{.824}	{.237}	{.005}	{.000}	{.000}

NOTE.—$N = 30{,}522$. Robust standard errors, clustered on contractor, are in parentheses for ordinary least squares (OLS) and two-stage least squares (2SLS) models. Conventional standard errors are in square brackets for the conventional quantile regression (QR) and instrumental variable quantile regression method (IVQR) models. Each column corresponds to a separate regression. All models include dummy variables for year by quarter of assignment and assignment-district by year of assignment, and controls for age and its square, gender, white and Hispanic race, and total UI earnings and total quarters of employment in 8 quarters prior to Work First assignment. Earnings values are inflated to 2003 dollars using the Consumer Price Index (CPI-U). The Wald test for constant treatment effects compares the 15th and 75th quantiles. Wald test *p*-values are in curly braces.
* Significant at the .10 level.
** Significant at the .05 level.
*** Significant at the .01 level.

quarters 2–8, while temporary-help placements are associated with a \$410 quarterly earnings gain. These OLS results are comparable to those reported in earlier literature on temporary help employment among low-wage workers in the United States, suggesting that there is nothing unusual about our

Detroit welfare sample.[25] The 2SLS estimates confirm, as above, that the OLS estimates are upward biased. Notably, the bias is far greater for temporary-help placements. After accounting for endogeneity, the effect of direct-hire placements on quarterly earnings remains significantly positive at $503, while the effect of temporary-help placements is weakly negative (−$57) and insignificant. The 2SLS results underscore the importance of accounting for selection bias, and they challenge the conventional wisdom that all job placements positively affect welfare recipients' labor market outcomes.

We explore the relationship between temporary-help and direct-hire placements and the conditional earnings distribution in panels C and D. Conventional QR estimates (panel C) find that both direct-hire and temporary-help placements are associated with higher subsequent earnings. At the conditional median, a direct-hire placement is associated with $350 higher quarterly earnings and a temporary-help placement with $269 higher quarterly earnings. Figure 2, which plots the entire quantile process for the QR model, indicates that direct-hire placements are associated with higher earnings than are temporary-help placements at essentially every quantile, with the greatest differences at higher quantiles.

Instrumental variables quantile estimates present a strikingly different picture of the causal effect of job placements on quarterly earnings. The IVQR estimates reveal that the mean effects estimates from the 2SLS models mask considerable heterogeneity. The effects of direct-hire placements are never negative, but they range from zero at the lowest quantiles, to $236 at the median, to $1,046 at the 85th percentile. These quantile treatment effects are generally significant at percentiles 50–85. By contrast, the estimates for temporary-help jobs start at zero and become negative at higher quantiles. This indicates that conditional on pre-program earnings and other observables, participants who rank higher in the earnings distribution benefit more from direct-hire placements and are more adversely affected by temporary-help placements than are those who rank lower in the conditional earnings distribution (in both cases, relative to those not placed in positions). For temporary-help placements, we cannot distinguish the IVQR estimate from zero for the lower quantiles, but we do see a significant negative effect towards the top of the conditional earnings distribution.

Figure 3, which displays the entire quantile process for the IVQR estimates, indicates that temporary-help placements do not appear to have positive impacts at any point in the quantile index, while the causal effects estimates above the 80th percentile are significantly negative and large. A

[25] See, e.g., Ferber and Waldfogel (1998), Lane et al. (2003), Corcoran and Chen (2004), Andersson, Holzer, and Lane (2005, 2009), and Heinrich et al. (2005, 2009). Autor and Houseman (2010) provide a detailed discussion of the close comparability of the OLS results from our Detroit sample with those in Heinrich et al. (2005) based on a sample of low-wage workers in North Carolina and Missouri.

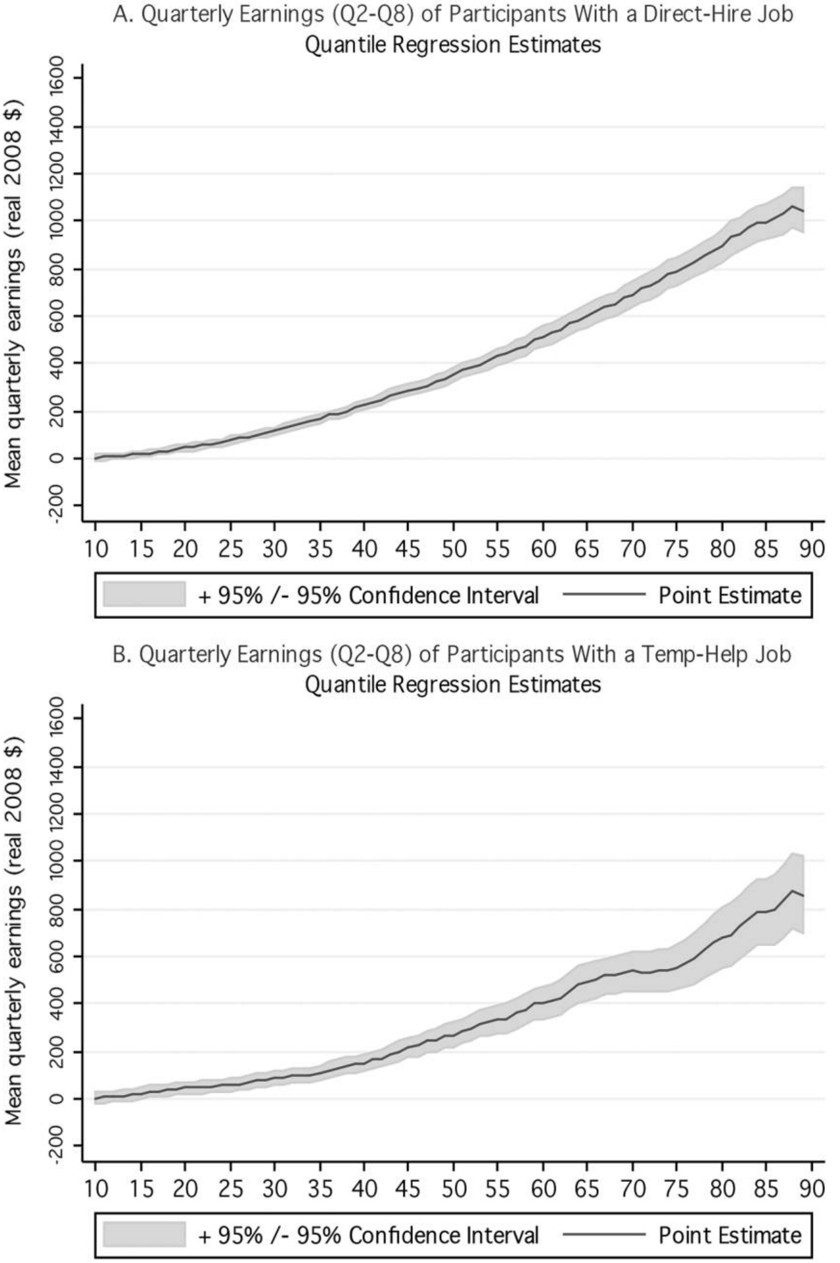

FIG. 2.—QR estimates for earnings quarters 2–8 following assignment: two endogenous variables. Coefficient estimates are on the vertical axis and the quantile index is on the horizontal axis. The shaded region is the 95% confidence interval. A color version of this figure is available online.

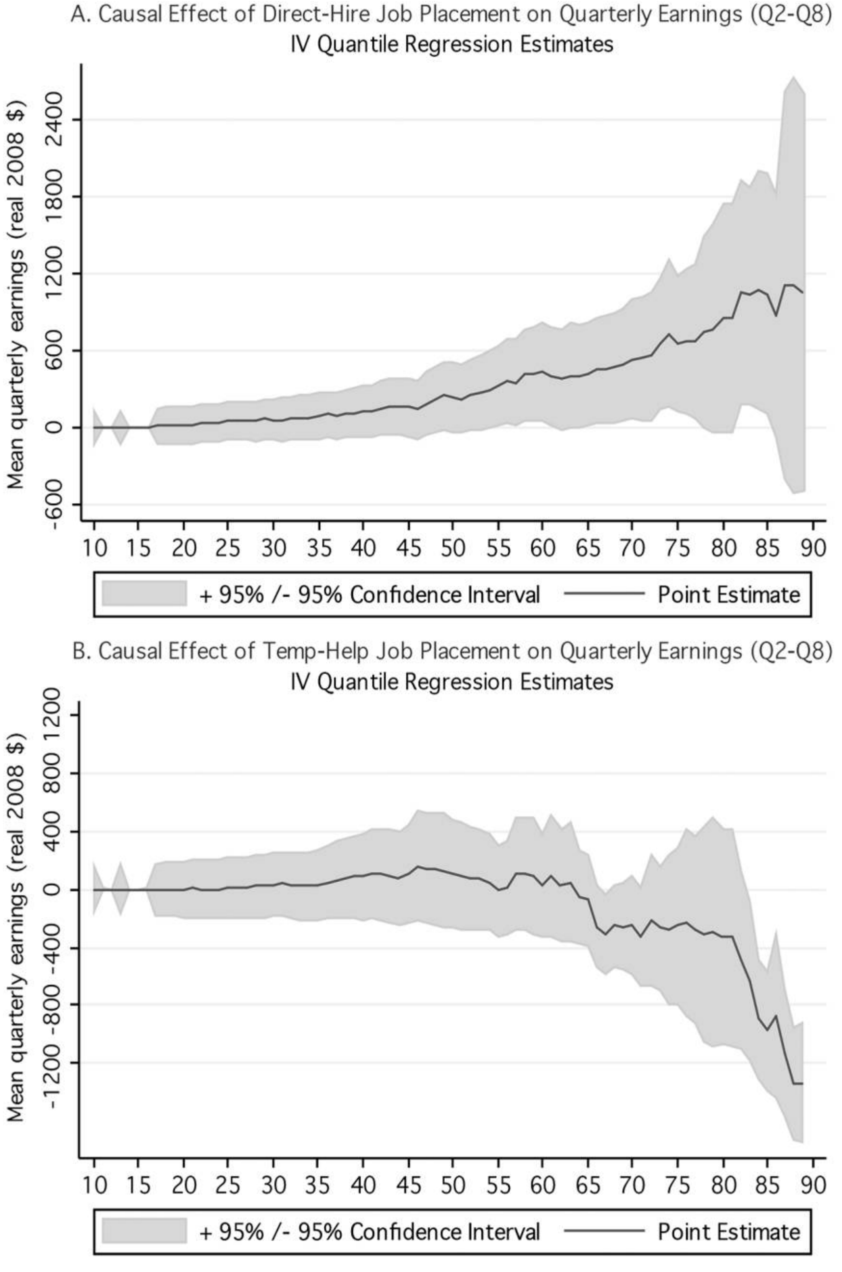

FIG. 3.—IVQR estimates for earnings quarters 2–8 following assignment: two endogenous variables. Coefficient estimates are on the vertical axis and the quantile index is on the horizontal axis. The shaded region is the 95% confidence interval. A color version of this figure is available online.

Wald test comparing estimates at the 15th and 75th quantiles rejects the null of constant quantile treatment effects at the 1% level for direct-hire placements and jointly for direct-hire and temporary help placements. Although constant quantile treatment effects cannot be rejected for temporary-help placements, owing to imprecision in these coefficient estimates, the effects of direct-hire and temporary-help placements on participant earnings are significantly different from one another at the 50th and higher quantiles (table 5, panel D).[26]

On net, these estimates reveal that the modest overall causal effects of job placements on participant earnings in the upper half of the conditional earnings distribution (table 4) mask two countervailing effects: relatively large direct-hire placement effects—ranging from about $250 to $1,000 per quarter over the 50th through 85th percentiles of the conditional earnings distribution—and imprecisely estimated but nevertheless large and negative effects of temporary-help placements on the conditional earnings distribution in higher quantiles. Under the maintained assumption of rank invariance, these estimates imply that participants with the highest potential earnings in direct-hire employment are those who suffer the greatest earnings losses from temporary-help placements.

One subtlety in interpreting these results lies in the relationship between conditional and unconditional quantiles. Because our main estimates condition on a rich set of covariates, it is not immediately apparent how the estimated causal effects of temporary-help and direct-hire placements on the conditional distribution of earnings correspond to their effects on the overall (unconditional) distribution of earnings.[27] To illuminate these relationships, we reestimate the IVQR without any person-level covariates. While these covariates serve a useful purpose in the main models—improving the precision of the estimates and increasing the plausibility of the rank invariance assumption—they complicate interpretation.[28] Alternative estimates that exclude person level covariates are reported in appendix table A3, with a detailed depiction of the quantile process shown in figure 4. While the exclusion of covariates modestly affects the shape of the treatment effect distribution and the magnitude of standard errors, the overall pattern of the

[26] We pool earnings over the 7 quarters of our follow-up period to improve precision of our IVQR estimates. Autor and Houseman (2010) estimate 2SLS models earnings for each of the 7 follow-up quarters and show that the mean effects of direct-hire and temporary-help placements on earnings dissipate over time.

[27] Firpo, Fortin, and Lemieux (2009) propose a useful technique for estimating the effect of covariates on unconditional outcome quantiles. We are not aware of an instrumental variables analog of this technique.

[28] Chernozhukov and Hansen (2006, 496) emphasize this point, stating that "the rank variable U . . . is made invariant to d, which ascribes an important role to conditioning on covariates X. Having a rich set of covariates makes rank invariance a more plausible approximation."

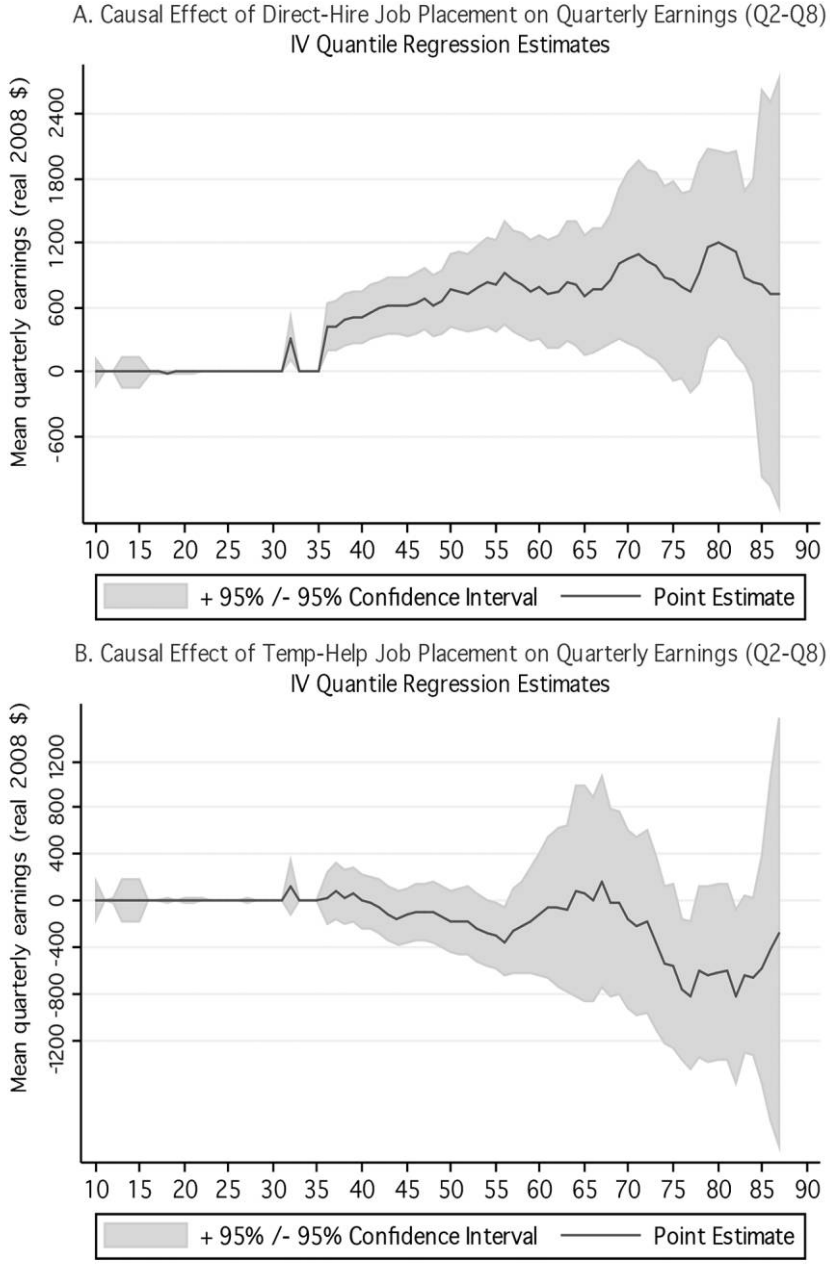

FIG. 4.—IVQR estimates for earnings quarters 2–8 following assignment: two endogenous variables and no individual-level covariates. Coefficient estimates are on the vertical axis and the quantile index is on the horizontal axis. The shaded region is the 95% confidence interval. A color version of this figure is available online.

quantile treatment effects is quite similar to the earlier models containing rich covariates: direct-hire placements have no effect at lower quantiles and large and often significant positive effects at higher conditional earnings quantiles; temporary-help placements negatively affect quarterly earnings for those in higher quantiles, though these estimates excluding covariates are not statistically significant. In net, these estimates support the interpretation given to the earlier results.

The Chernozhukov-Hansen IVQR model relies on strong assumptions about the structural relationship between job placements and earnings. Most significantly, the IVQR model assumes rank invariance. In our application this means that a participant whose contractor assignment leads to a job placement and post-placement earnings at percentile p' of the conditional earnings distribution of placed workers would, counterfactually, have had earnings at percentile p' of the conditional distribution of nonplaced workers had the participant's contractor assignment instead induced that outcome. Although Chernozhukov and Hansen explain that this assumption can be weakened to rank similarity, meaning that the assignment mechanism does not lead to systematic changes in ranks across treatment outcomes, it still rules out the possibility of comparative advantage. If, for example, a different set of skills is rewarded in temporary-help and direct-hire jobs, rank similarity would be violated.

To shed light on whether our findings are sensitive to the rank similarity assumption in the Chernozhukov-Hansen model, we estimate complementary nonstructural models. Specifically, we estimate a set of 2SLS models in which the dependent variable indicates whether average quarterly earnings during quarters 2–8 following assignment exceed various thresholds: no earnings (which corresponds to the 21st percentile of the sample distribution), median earnings ($548), and earnings at the 75th percentile ($1,792).[29] The results from these distributional treatment effects models are shown in table 6. Although the outcome measure in these models is quite different from that in the IVQR model, the pattern of the coefficient estimates is similar. Neither direct-hire nor temporary-help placements has a significant impact on the share with no earnings, consistent with the findings reported in table 5 that neither type of job placement affects earnings in the lower tail of the earnings distribution. At greater earnings thresholds, direct-hire placements have large positive, statistically significant effects on the share with higher earnings: placement into a direct-hire job raises the probability of having earnings above the median by about 25 percentage points and having earnings above the 75th percentile by about 14 percentage points. In contrast, the estimated effects of a temporary-help placement on the probability of having earnings above these higher thresholds is negative. Although never statistically significant, the negative effect of a temporary help placement on the probability of having earnings exceed earnings at the

[29] We thank Blaise Melly for suggesting this test.

Table 6
Two-Stage Least Squares (2SLS) Linear Probability Models of the Effect of Work-First Job Placements on the Probability That Average Quarterly Earnings Quarters 2–8 Following Work First Assignment Exceed Various Thresholds

	> $0	> Median ($548)	> 75th Percentile ($1,792)
Direct-hire placement	.0850	.246***	.137**
	(.052)	(.056)	(.052)
Temporary-help placement	.0463	−.0490	−.0324
	(.051)	(.045)	(.053)
Constant	.744***	.394***	.187***
	(.018)	(.019)	(.018)
Wald test for equality of direct-hire and temporary-help placement effects:			
Wald statistic	.24	12.64	3.87
p-value	{.625}	{.000}	{.049}

NOTE.—N = 30,522. Robust standard errors, clustered on contractor, are in parentheses. Each column corresponds to a separate regression. All models include dummy variables for year by quarter of assignment and assignment-district by year of assignment, and controls for age and its square, gender, white and Hispanic race, and total UI earnings and total quarters of employment in 8 quarters prior to Work First assignment. Earnings values are inflated to 2003 dollars using the Consumer Price Index (CPI-U). Wald statistic p-values are in curly braces.
** Significant at the .05 level.
*** Significant at the .01 level.

50th and 75th percentiles is significantly lower than the effect of direct-hire placements at these earnings thresholds. The consistency of patterns of the coefficient estimates from the distributional treatment effects models with those from the IVQR models suggests that the latter are not unduly affected by the rank similarity assumption.

C. The Dynamics of Job Placements: Earnings by Sector and by Longest Job Spell

Why do direct-hire placements raise subsequent earnings while temporary-help placements fail to do so? The earnings that workers receive while employed in temporary-help and direct-hire jobs are unlikely to provide the answer. As shown in table 1, average hourly wages and weekly earnings are actually higher in temporary-help jobs than in direct-hire jobs obtained through the Work First program. Corroborating this evidence, Hamersma, Heinrich, and Mueser (2014) use administrative data from the state of Wisconsin to show that, although disadvantaged workers with temporary help jobs have lower quarterly earnings than their counterparts in direct-hire jobs, their hourly wages are significantly higher. Complementary evidence is provided by Houseman and Heinrich's (2015) analysis of employment records from a large nationally representative temporary help firm. At this firm, the median duration of a temporary agency assignment is 21 days and only 7% of workers are hired into a direct-hire position by the client firm. Thus, temporary-help placements offer slightly higher earnings than direct-hire placements but extremely short durations. Such placements could nevertheless provide workers with valuable experience and employer contacts that lead to more stable subsequent employment and higher earnings—something that the literature terms a stepping stone effect.[30] Do we see this effect in practice?

We explore evidence for a stepping stone effect in table 7, where we reestimate our 2SLS and IVQR models for the impact of placements on subsequent earnings, in this case distinguishing between subsequent earnings in direct-hire and temporary-help employment. This analysis answers the question of whether temporary-help placements ultimately raise direct-hire (as well as temporary-help) earnings, and conversely whether direct-hire placements also raise earnings in temporary help employment. We find that if placed in a direct-hire job during the Work First spell, the median participant (i.e., at the 50th quantile of the conditional earnings distribution) increases subsequent quarterly direct-hire earnings by $237 (panel A), with no effect on subsequent temporary-help earnings (panel B). Conversely, participants placed in temporary-help jobs see a small, insignificant $37 increase in direct-hire earnings at the median (panel A) and no increase in temporary-help earnings (panel B). At higher quantiles, we see larger positive effects of direct-hire and temporary-help placements on earnings in their respective job types. Simultaneously, crowd-out is large at higher quantiles: at high values of the quantile index, participants placed in direct-hire jobs have the largest earnings gains in direct-hire jobs and forgo the largest earnings in temporary-help jobs and vice versa. The net effects of direct-hire placements are generally positive, but those for temporary-help placements are generally negative. Wald tests fail to reject the null hypothesis of constant treatment effects for direct-hire and temporary-help earnings, but they do reject the equality of the direct-hire and temporary-help placement effects at the 50th and 75th quantiles. In summary, direct-hire and temporary-help job placements primarily affect future earnings in the sectors into which workers are placed, and they generally crowd out earnings in alternative sectors: direct-hire placements generate additional earnings in direct-hire but not in temporary-help employment, and similarly, temporary-help placements increase earnings in temporary-help employment but do not serve as a stepping stone into direct-hire jobs.

Since temporary-help and direct-hire placements primarily affect earnings in the sectors in which workers are placed, a potential explanation for

[30] For this reason, much of the research in Europe and the United States on temporary-help employment has focused on whether these jobs are stepping stones to direct-hire employment. Autor and Houseman (2010) provides an overview of this literature. Placements of the unemployed into temporary help jobs also could benefit workers if the alternative is unemployment. Our results demonstrate, however, that Work First participants appear to obtain at least equivalent employment outcomes without the direct assistance of government return-to-work programs.

Table 7
The Effect of Work-First Job Placements on Subsequent Direct-Hire and Temporary-Help Earnings Quarters 2–8 Following Work First Assignment

		IVQR				
	2SLS	.15	.25	.50	.75	.85
	A. Direct-Hire Earnings					
Direct-hire placement	518***	1	1	237*	594**	1,061**
	(142)	[63]	[79]	[119]	[252]	[441]
Temporary-help placement	−139	−1	−1	37	−424***	−309
	(163)	[68]	[71]	[137]	[152]	[440]
Constant	841***	1	74***	456***	1,288***	1,926***
	(45)	[16]	[19]	[19]	[76]	[123]
Wald test for constant treatment effects:						
Direct-hire placement			Wald statistic {p-value}		4.24	
					{.120}	
Temporary-help placement			Wald statistic {p-value}		.73	
					{.693}	
Joint test for the two treatments			Wald statistic {p-value}		5.64	
					{.228}	
Wald test for equality of direct-hire and temporary-help placement effects:						
Wald statistic	5.82	.00	.00	5.89	25.63	3.78
p-value	{.022}	{.998}	{.999}	{.053}	{.000}	{.151}

	B. Temporary-Help Earnings					
Direct-hire placement	−19	0	0	0	−296*	−2,664
	(63)	[5]	[11]	[0]	[163]	[8,505]
Temporary-help placement	97	0	0	0	1,344	2,120
	(70)	[0]	[19]	[3]	[5,141]	[26,570]
Constant	128***	0	0	0	296**	2,664
	(26)	[0]	[4]	[0]	[139]	[8,441]
Wald test for constant treatment effects:						
Direct-hire placement			Wald statistic {p-value}		2.58	
					{.275}	
Temporary-help placement			Wald statistic {p-value}		.72	
					{.698}	
Joint test for the two treatments			Wald statistic {p-value}		.72	
					{.949}	
Wald test for equality of direct-hire and temporary-help placement effects:						
Wald statistic	2.68	.00	.00	.00	2.71	1.01
p-value	{.112}	{1.000}	{1.000}	{1.000}	{.257}	{.604}

NOTE.—N = 30,522. Robust standard errors, clustered on contractor, are in parentheses for 2SLS models. Conventional standard errors are in squared brackets for the instrumental variable quantile regression method (IVQR) models. Each column corresponds to a separate regression. All models include dummy variables for year by quarter of assignment and assignment-district by year of assignment, and controls for age and its square, gender, white and Hispanic race, and total UI earnings and total quarters of employment in 8 quarters prior to Work First assignment. Earnings values are inflated to 2003 dollars using the Consumer Price Index (CPI-U). The Wald test for constant treatment effects compares the 15th and 75th quantiles. Wald statistic p-values are in curly braces.

* Significant at the .10 level.

** Significant at the .05 level.

*** Significant at the .01 level.

why direct-hire placements increase net earnings by more than temporary-help placements is that direct-hire placements are more durable.[31] Indeed, the summary statistics in table 1 underscore that the large majority of earnings for Work First participants in quarters 2–8 following placement derive from a single job. The final set of tables and figures (table 8 and fig. 5) explore the role played by durable jobs in the earnings of Work First participants by estimating models for the effect of placements on total wage earnings during the longest post-placement job spell.[32] Focusing on direct-hire placements, the panel B estimates show considerable treatment effect heterogeneity across the conditional earnings distribution. The estimated earnings increases resulting from direct-hire placements in the IVQR range from $4 to $929 at the 15th and 85th percentiles respectively, and vary between –$1 and –$609 for temporary-help placements over the same quantile range. During the longest post-placement job spell, direct-hire placements create significant positive earnings effects that increase with the conditional earnings quantile. As shown in panel A, temporary-help placements are not predictive of such positive effects and appear to significantly reduce longest-job earnings at the higher tail of the conditional earnings distribution. Wald tests confirm the heterogeneity of estimated treatment effects for temporary-help placements and jointly for temporary-help and direct-hire placements.

Finally, when comparing the IVQR results from quantile to quantile (fig. 5), it is clear that the patterns are not always monotonic, instead exhibiting some occasional peaks and troughs. We believe that these local dips are not necessarily indicative of actual drastic changes in the treatment effect but rather are a result from the lack of support for the instrument at these locations.[33] Chernozhukov, Fernandez-Val, and Galichon (2009) show that it is possible to reorder the quantiles (point estimates and standard errors) to satisfy the monotonicity requirements and thereby improve upon the orig-

[31] In theory, workers with relatively high hourly wages and weekly work hours but short job durations could attain relatively high earnings over the medium term by securing a series of short-term jobs. In practice, workers in temporary positions are likely to experience some spell of nonemployment between jobs, which on balance may result in lower medium-term earnings. Indeed, Hamersma et al. (2014) find that the short duration of temporary help jobs largely explains (proximately) why workers in these positions have lower quarterly earnings despite having higher hourly wages.

[32] The longest job spell does not necessarily correspond to the Work First placement job. As noted above, information on job placements comes from Work First administrative data while information on employment during the 7-quarter follow-up period comes from state UI wage records. In general, we cannot tell whether a job held in the follow-up period is the same as the job obtained through the Work First program.

[33] Plots of the concentrated objective function over the coefficients of the endogenous variables support this conclusion. There appears to be little density around certain locations, making the parameter identification weaker in those areas.

Table 8
The Effect of Work-First Job Placements on Subsequent Earnings in the Longest Job Spell during Quarters 2–8 Following Work First Assignment

		IVQR				
	2SLS	.15	.25	.50	.75	.85
	A. Single Endogenous Variables					
Any job placement	199**	10	23	140***	189	421*
	(87)	[32]	[36]	[50]	[138]	[224]
Constant	814***	28**	115***	411***	1,138***	1,663***
	(39)	[12]	[15]	[20]	[61]	[100]
Wald test for constant treatment effects:						
Any job placement		Wald statistic {p-value}			2.33 {.312}	
	B. Two Endogenous Variables					
Direct-hire placement	397***	4	28	153	532*	929**
	(130)	[57]	[63]	[95]	[292]	[425]
Temporary-help placement	−146	−1	16	79	−430***	−609***
	(155)	[72)	[84]	[136]	[118]	[198]
Constant	771***	28*	113***	400***	1,072***	1,662***
	[45]	[15]	[18]	[25]	[75]	[112]
Wald test for constant treatment effects:						
Direct-hire placement		Wald statistic {p-value}			1.09 {.579}	
Temporary-help placement		Wald statistic {p-value}			7.52 {.023}	
Joint test for the two treatments		Wald statistic {p-value}			32.38 {.000}	
Wald test for equality of direct-hire and temporary-help placement effects:						
Wald statistic	5.30	.08	3.73	3.80	30.46	4.17
p-value	{.028}	{.963}	{.155}	{.150}	{.000}	{.124}

NOTE.—N = 30,522. Robust standard errors, clustered on contractor, are in parentheses for the two-stage least squares (2SLS) models. Conventional standard errors are in squared brackets for the instrumental variable quantile regression method (IVQR) models. Each column corresponds to a separate regression. All models include dummy variables for year by quarter of assignment and assignment-district by year of assignment, and controls for age and its square, gender, white and Hispanic race, and total UI earnings and total quarters of employment in 8 quarters prior to Work First assignment. Earnings values are inflated to 2003 dollars using the Consumer Price Index (CPI-U). The Wald test for constant treatment effects compares the 15th and 75th quantiles. Wald statistic p-values are in curly braces.
* Significant at the .10 level.
** Significant at the .05 level.
*** Significant at the .01 level.

inal estimates. We do not apply this rearrangement procedure here because the departures from monotonicity are modest in our application and hence the rearrangement makes little substantive difference.[34]

[34] Plots using the rearrangement procedure are available from the authors.

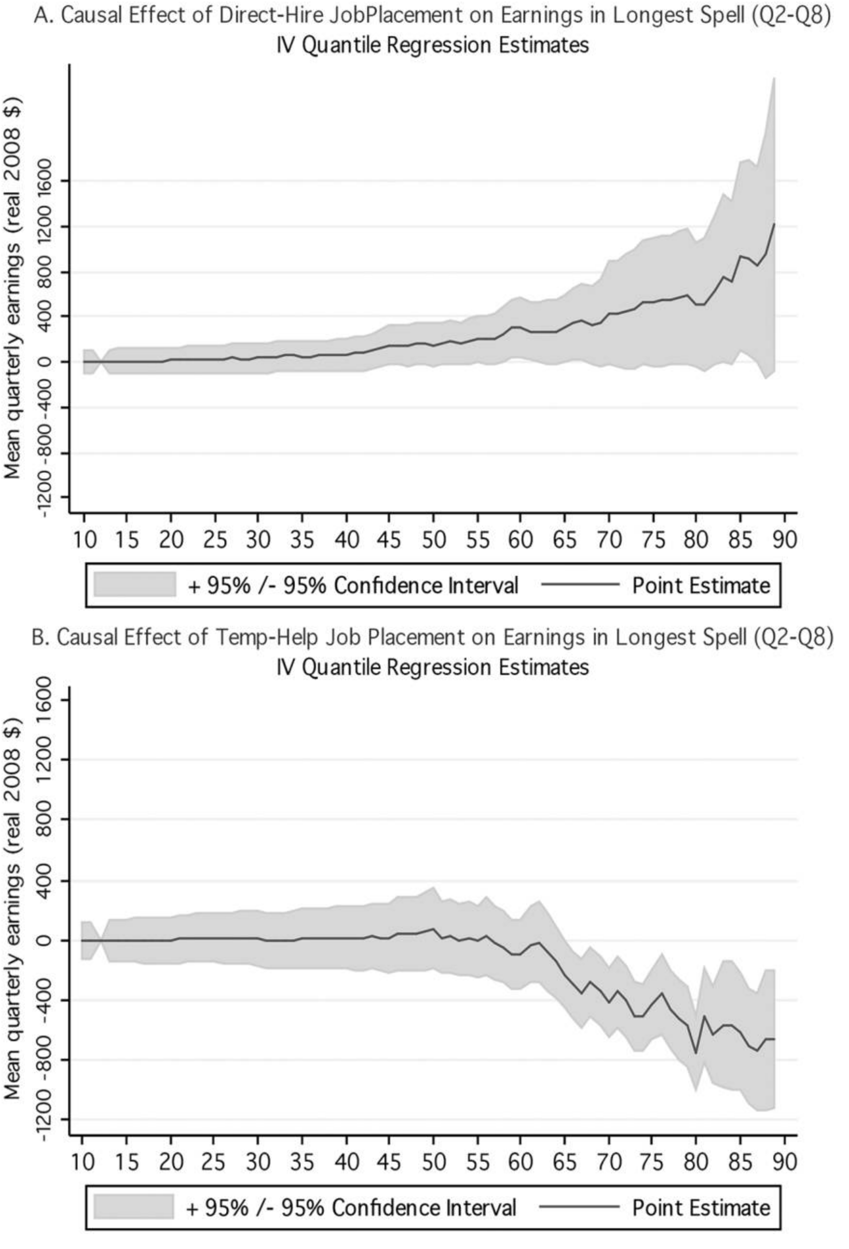

FIG. 5.—IVQR estimates for earnings in longest job spell in quarters 2–8 following assignment: two endogenous variables. Coefficient estimates are on the vertical axis and the quantile index is on the horizontal axis. The shaded region is the 95% confidence interval. A color version of this figure is available online.

VI. Conclusions

This paper applies the instrumental variable quantile regression estimator developed by Chernozhukov and Hansen (2004a, 2005, 2006) to study job placement and earnings data from Detroit's Work First program. Following Autor and Houseman (2010), we use the rotational assignment of participants to contractors as instrumental variables for direct-hire and temporary-help job placements, and this allows us to estimate the causal effects of placements on the distribution of participants' subsequent earnings. Distinct from Autor and Houseman (2010), we apply a quantile instrumental variables model to estimate the effects of direct-hire and temporary-help placements over the entire distribution of participants earnings. This approach provides a nuanced depiction of the causal effects of welfare-to-work job placements on participants' long-term labor market outcomes that is not captured by conventional OLS and IV methods.

We document that the effects of job placements on labor market outcomes vary substantially across percentiles of the conditional earnings distribution for both direct-hire and temporary-help placements and, further, that they differ qualitatively and quantitatively from one another. Direct-hire placements are estimated to significantly increase subsequent earnings over 1–2 years for half or more of all placed participants. By contrast, temporary-help placements have uniformly zero or negative effects on the earnings distribution, and these effects are large and significant at high quantiles. Even at the top of the earnings distribution, the positive effects generated by the Work First program are only manifested in direct-hire earnings and total wage earnings but not in temporary-help earnings.

The mechanisms underlying these findings appear to operate through the effect of job placement type on subsequent employment duration. Among those with higher potential earnings, placements into direct-hire jobs facilitate more direct-hire employment and longer job tenures, whereas the temporary help jobs into which participants are placed are short-lived and do not serve as stepping stones into more durable direct-hire jobs. Neither direct-hire nor temporary help-job placements improve subsequent earnings among those with lower potential earnings, however. Unusual among quantile instrumental variables analyses, our setting provides sufficient power to statistically reject the hypothesis that the heterogeneity in direct-hire treatment effects we detect arises by chance; the differential in treatment effects between the top and bottom quartiles of the effects distribution for this group are both economically and statistically significant.

Substantively, these results cast doubt on whether the widespread use of temporary-help agencies by government programs is a sound public investment. More fundamentally, they highlight the possibility that interventions focused solely on job placement do little to raise the earnings of those in the lower end of the conditional earnings distribution.

Appendix

Table A1
Occupational Distribution of Work First Job Placements in Direct-Hire and Temporary-Help Jobs

Occupation	Direct-Hire (%)	Temporary-Help (%)	Difference
Industrial	5.4	31.3	−25.8
	(.2)	(.9)	(.6)
General labor	6.2	20.0	−13.8
	(.2)	(.8)	(.6)
Clerical	8.1	12.4	−4.3
	(.3)	(.7)	(.6)
Other	80.2	36.3	43.9
	(.4)	(1.0)	(.9)

NOTE.—Figures in the first two columns show the occupational distribution in percent of direct-hire Work First job placements and of temporary-help Work First job placements, respectively; the third column shows the difference between the direct-hire and temporary-help distributions. Standard errors are in parentheses. All differences are significant at the .01 level. Occupational categories correspond to those in the Detroit Work First administrative data.

Table A2
Comparison of Estimated Effects from Models Using a Series of Binary Instrumental Variables versus the Residualized Continuous Instruments

	2SLS	IVQR Dummy Instruments	IVQR Residualized Instruments
Temporary-help placement	−165	−468	−468
	(447)	[462]	[470]
Direct-hire placement	810***	768**	768**
	(162)	[322]	[325]
Constant	870***	652***	652***
	(37)	[82]	[79]

NOTE.—N = 5,082. Robust standard errors, clustered on contractor, are in parentheses for two-stage least squares (2SLS) models. Conventional standard errors are in brackets for instrumental variable quantile regression method (IVQR) models. The sample includes districts 11, 12, and 122. Each column corresponds to a separate regression. All models include dummy variables for year by quarter of assignment and assignment-district by year of assignment, and controls for age and its square, gender, white and Hispanic race, and total UI earnings and total quarters of employment in 8 quarters prior to Work First assignment. Earnings values are inflated to 2003 dollars using the Consumer Price Index (CPI-U).
** Significant at the .05 level.
*** Significant at the .01 level.

Table A3
The Effect of Work-First Job Placements on Subsequent Earnings Quarters 2–8 following Work First Assignment: Two Endogenous Variables, No Person-Level Covariates

	Mean Effect	Quantile Treatment Effects at Quantile .15	.25	.50	.75	.85
	A. OLS	C. Quantile Regression				
Direct-hire placement	641***	12***	170***	664***	1,181***	1,307***
	(28)	[1]	[9]	[18]	[36]	[51]
Temporary-help placement	554***	22***	164***	570***	1,027***	1,168***
	(41)	[1]	[17]	[18]	[61]	[86]
Constant	935***	1	11**	304***	1,234***	2,085***
	(13)	[1]	[5]	[12]	[23]	[33]
	B. 2SLS	D. IVQR				
Direct-hire placement	604***	0	0	769***	852*	821
	(203)	[75]	[176]	[172]	[476]	[919]
Temporary-help placement	−149	0	0	−181	−566	−588
	(267)	[94]	[1,959]	[172]	[360]	[497]
Constant	1,011***	0	0	370***	1,464***	2,458***
	(79)	[19]	[31]	[34]	[151]	[330]
Wald test for constant treatment effects:						
Direct-hire placement		Wald statistic (p-value)			.70 {.706}	
Temporary-help placement		Wald statistic (p-value)			.30 {.862}	
Joint test for the two treatments		Wald statistic (p-value)			.94 {.919}	
Wald test for equality of direct-hire and temporary-help placement effects:						
Wald statistic	5.24	.00	.00	9.60	.89	.46
p-value	{.029}	{1.000}	{1.000}	{.008}	{.641}	{.793}

NOTE.—N = 30,522. Robust standard errors, clustered on contractor, are in parentheses for the ordinary least squares (OLS) and two-stage least squares (2SLS) models. Conventional standard errors are in brackets for the conventional quantile regression (QR) and instrumental variable quantile regression method (IVQR) models. Each column corresponds to a separate regression. All models include dummy variables for year by quarter of assignment and assignment-district by year of assignment. Earnings values are inflated to 2003 dollars using the Consumer Price Index (CPI-U). Wald test p-values are in curly braces.
* Significant at the .10 level.
** Significant at the .05 level.
*** Significant at the .01 level.

References

Abadie Alberto, Josh Angrist, and Guido Imbens. 2002. Instrumental variables estimates of the effect of subsidized training on the quantiles of trainee earnings. *Econometrica* 70, no. 1:91–117.

Abraham, Katharine G. 1988. Flexible staffing arrangements and employers' short-term adjustment strategies. In *Employment, unemployment, and labor utilization*, ed. Robert A. Hart, 288–311. Boston: Unwin Hyman.

Andersson, Fredrik, Harry J. Holzer, and Julia I. Lane. 2005. *Moving up or moving on: Who advances in the labor market?* New York: Russell Sage Foundation.

———. 2009. Temporary help agencies and the advancement prospects of low earners. In *Studies of labor market intermediation*, ed. David Autor. Chicago: University of Chicago Press.

Angrist, Joshua D., and Jörn-Steffen Pischke. 2009. *Mostly harmless econometrics: An empiricist's companion*. Princeton, NJ: Princeton University Press.

Autor, David H. 2001. Why do temporary help firms provide free general skills training? *Quarterly Journal of Economics* 116, no. 4:1409–48.

———. 2003. Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing. *Journal of Labor Economics* 21, no. 3:1– 42.

Autor, David H., and Susan N. Houseman. 2002. The role of temporary employment in welfare to work: Part of the problem or part of the solution? *Focus* 22:63–70.

———. 2006. Temporary agency employment: A way out of poverty? In *Working and poor: How economic and policy changes are affecting low-wage workers*, ed. Rebecca M. Blank, Sheldon H. Danziger, and Robert F. Schoeni, 312–37. New York: Russell Sage Foundation.

———. 2010. Do temporary help jobs improve labor market outcomes for low-skilled workers? Evidence from "Work First." *American Economic Journal: Applied Economics* 2, no. 3:96–128.

Benner, Chris, Laura Leete, and Manuel Pastor. 2007. *Staircases or treadmills? Labor market intermediaries and economic opportunity in a changing economy*. New York: Russell Sage Foundation.

Bloom, Dan, Richard Hendra, Karin Martinson, and Susan Scrivener. 2005. *The employment retention and advancement project: Early results from four sites*. New York: Manpower Research Demonstration Corporation.

Cancian, Maria, Robert Haveman, Thomas Kaplan, and Barbara Wolfe. 1999. Post-exit earnings and benefit receipt among those who left AFDC in Wisconsin. Special Report no. 75, Institute for Research on Poverty, University of Wisconsin–Madison.

Chernozhukov, Victor, and Christian Hansen. 2004a. The effects of 401(k) participation on the wealth distribution: An instrumental variables quantile regression analysis. *Review of Economics and Statistics* 86, no. 3:735–51.

———. 2004b. Instrumental variables quantile regression. Unpublished manuscript, Department of Economics, Massachusetts Institute of Technology.

———. 2005. An IV model of quantile treatment effects. *Econometrica* 73, no. 1:245–61.

———. 2006. Instrumental quantile regression inference for structural and treatment effect models. *Journal of Econometrics* 132, no. 2:491–525.

Chernozhukov, Victor, Ivan Fernandez-Val, and Alfred Galichon. 2009. Improving point and interval estimates of monotone functions by rearrangement. *Biometrica* 96, no. 3:559–75.

Corcoran, Mary, and Juan Chen. 2004. Temporary employment and welfare-to-work. Unpublished manuscript, University of Michigan, Ann Arbor.

Dyke, Andrew, Carolyn J. Heinrich, Peter R. Mueser, Kenneth R. Troske, and Kyung-Seong Jeon. 2006. The effects of welfare-to-work program activities on labor market outcomes. *Journal of Labor Economics* 24, no. 3: 567–607.

Ferber, Marianne A., and Jane Waldfogel. 1998. The long-term consequences of nontraditional employment. *Monthly Labor Review* 121, no. 5:3–12.

Firpo, Sergio, Nicole M. Fortin, and Thomas Lemieux. 2009. Unconditional quantile regressions. *Econometrica* 77, no. 3:953–73.

Fraker, Thomas M., Dan M. Levy, Irma Perez-Johnson, Alan M. Hershey, Demetra S. Nightingale, Robert B. Olsen, and Rita A. Stapulonis. 2004. The National Evaluation of the Welfare-to-Work Grants Program: Final report, Mathematica Policy Research, Inc., Washington, DC.

Hamersma, Sarah, Carolyn Heinrich, and Peter Mueser. 2014. Temporary help work: Earnings, wages, and multiple job holding *Industrial Relations* 53, no. 1:72–100.

Heinrich, Carolyn J., Peter R. Mueser, and Kenneth R. Troske. 2005. Welfare to temporary work: Implications for labor market outcomes. *Review of Economics and Statistics* 87, no. 1:154– 73.

———. 2009. The role of temporary-help employment in low-wage worker advancement. In *Studies of labor market intermediation*, ed. David Autor. Chicago: University of Chicago Press.

Houseman, Susan N. 2001. Why employers use flexible staffing arrangements: Evidence from an establishment survey. *Industrial and Labor Relations Review* 55, no. 1:149– 70.

Houseman, Susan, and Carolyn Heinrich. 2015. Temporary help employment in recession and recovery. Working Paper no. 15–227, Upjohn Institute for Employment Research, Kalamazoo, MI.

Jorgensen, Helene, and Hans Riemer. 2000. Permatemps: Young temp workers as permanent second class employees. *American Prospect* 11, no. 18:38–40.

Kalleberg, Arne L., Jeremy Reynolds, and Peter V. Marsden. 2003. Externalizing employment: Flexible staffing arrangements in U.S. organizations. *Social Science Research* 32:525–52.

Katz, Lawrence F., and Alan B. Krueger. 1999. The high-pressure U.S. labor market of the 1990s. *Brookings Papers on Economic Activity*, no. 1: 1–65.

King, Christopher T., and Peter R. Mueser. 2005. *Welfare and work: Experiences in six cities*. Kalamazoo, MI: Upjohn Institute for Employment Research.

Koenker, Roger, and Gilbert W. Bassett. 1978. Regression quantiles. *Econometrica* 46, no. 1:33–50.

Lane, Julia, Kelly S. Mikelson, Pat Sharkey, and Doug Wissoker. 2003. Pathways to work for low-income workers: The effect of work in the temporary help industry. *Journal of Policy Analysis and Management* 22, no. 4: 581–98.

Parker, Robert E. 1994. *Flesh peddlers and warm bodies: The temporary help industry and its workers*. New Brunswick, NJ: Rutgers University Press.

Pawasarat, John. 1997. The employer perspective: Jobs held by the Milwaukee County AFDC single parent population (January 1996–March 1997). Milwaukee: Employment and Training Institute, University of Wisconsin–Milwaukee.