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ABSTRACT

Federal and state employment programs for low-skilled workers typically emphasize rapid placement of participants into jobs and often place a large fraction of participants into temporary-help agency jobs. Using unique administrative data from Detroit's welfare-to-work program, we apply the Chernozhukov-Hansen instrumental variables quantile regression (IVQR) method to estimate the causal effects of welfare-to-work job placements on the distribution of participants' earnings. We find that neither direct-hire nor temporary-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 fifty percent of participants over the medium-term (one to two years following placement). Conversely, temporary-help placements have zero or negative earnings impacts at all quantiles, and these effects are economically large and significant at higher quantiles. In net, we find that the widespread practice of placing disadvantaged workers into temporary-help jobs is an ineffective tool for improving earnings and, moreover, that programs focused solely on job placement fail to improve earnings among those who are hardest to serve. Methodologically, one surprising result is that a reduced-form quantile IV approach, akin to two-step instrumental variables, produces near-identical point estimates to the structural IVQR approach, which is based on much stronger assumptions.

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I. Introduction

Compared to other advanced economies, the United States spends relatively little on active labor market programs, and U.S. programs targeting 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, for example, 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 with traditional direct-hire employers. Debate over the impact of temporary-help employment has spurred numerous studies in the United States and Europe of its effects on low-skilled workers' labor market advancement.[2] In the Detroit welfare-to-work program that we study in this paper, 20 percent of the job placements obtained through the program were with temporary-help agencies versus 80 percent 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 to 100 percent increase in the incidence of temporary-help employment relative to employment in other industries.[3]

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

2. U.S. 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 development or advancement (Parker 1994, Pawassarat 1997, Jorgensen and Riemer 2000, Benner, Leete, and Pastor 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 and 2003; Houseman 2001; Autor and Houseman 2002; Kalleberg, Reynolds, and Marsden 2003).

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

To our knowledge, all studies analyzing the causal effect of either temporary-help or direct-hire Work First job placements on participant outcomes 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 Work First 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 conditional 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 causal effects of welfare-to-work job placements on the distribution of participants' earnings over a seven-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 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 are, as anticipated, quite heterogeneous. We find that neither direct-hire nor temporary-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 fifty percent of participants over the medium-term (one to two 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 from both zero and from the estimated effects of direct-hire placements.

4. Corcoran and Chen (2004) and Andersson et al. (2009) conduct some sub-group analyses of temporary-help employment.

Unusual among quantile instrumental variables analyses, our analysis statistically rejects the hypothesis that the heterogeneity in treatment effects we detect for 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] Substantively, our findings raise concerns about the extensive use of temporary-help agencies in government employment programs. They also reinforce skepticism that programs focused on job placement can help the hardest to serve.

Alongside these substantive conclusions, our analysis provides one novel methodological finding. A potentially unattractive feature of the Chernozhukov-Hansen IVQR estimator is that it requires a strong, untestable assumption about the structural relationship between observed and counterfactual outcomes. Specifically, the IVQR model assumes *rank invariance*—meaning that an individual's rank in the conditional distribution of outcomes among those receiving the same treatment (e.g., direct-hire or temporary-help placement) is invariant to the treatment she receives. In our application, this assumption implies 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 *non-placed* workers had her contractor assignment instead induced that outcome. Though 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–for example, if a different set of skills is rewarded in temporary-help jobs than in direct-hire jobs.

As a reality check on the IVQR results, we complement these estimates with reduced form quantile regression (RFQR) models that use a simple two-stage procedure. In the first stage, we calculate average excess temporary-help and direct-hire placement rates by contractor-year. In stage two, we estimate quantile regressions for conditional earnings quantiles at the individual participant level using the excess contractor-year placement rate measure as our key explanatory variable (along with standard covariates). These RFQR models make no particular assumptions about the relationships among participant earnings ranks across treatment conditions. Rather, in the spirit of standard QR models, they simply estimate the effect of a treatment on the conditional quantiles of the outcome variable.

Somewhat unexpectedly, the coefficient estimates from the RFQR models are very closely comparable (and in many cases point identical) with the corresponding IVQR models. This close comparability provides both reassurance on the robustness of our results and caution

5. Chernozhukov and Hansen (2004a) reject the null of constant treatment effects for the effect of 401K eligibility on wealth accumulation.

in their interpretation. On the positive side, the estimates are clearly not sensitive to the estimation procedure (IVQR versus the simple reduced form model). On the negative side, the fact that RFQR yields near-identical results to the IVQR estimator raises some doubt as to whether the IVQR model is actually recovering the joint distribution of latent outcomes for participants at each location in the quantile index or whether it is merely estimating the causal effect of treatment on the conditional distribution of wages. Under either interpretation, our estimates clearly indicate that direct-hire placements have heterogeneous effects, with no significant impact at lower quantiles of the conditional earnings distribution and sizable and significant positive effects at higher quantiles. Conversely, while temporary-help placements also have heterogeneous effects on earnings, the impacts are negative at higher quantiles. Under the latter interpretation, however, we would not be justified in concluding that the participants who most benefit from direct-hire placements are those who are most harmed by temporary-help placements and vice versa.

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, Research Design, and Data

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

II.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 her application date. This procedure is functionally equivalent to random assignment of participants to contractors, as we demonstrate formally below.

All contractors provide a standard one-week training course aimed at improving job applications and other skills of the participants. Under the program, each participant develops a resumé 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 child care 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 significantly 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] 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.

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 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).

II.B. *The Data and 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. These Work First administrative data are linked to unemployment insurance wage 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 eight quarters before and eight quarter following a participant's entry into the Work First program. We generally are unable to determine whether the employer of the Work First job is the same as an employer during the post-placement follow-up.

The data set used in our analysis covers 30,522 Work First spells. Our data include only participants who initiated their Work First spell in a district that had at least two contractors, who were age 16 to 65 at the beginning of the spell, and who earned less than $15,000 per calendar quarter during the seven-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.

II.C. *Participant Characteristics*

Table 1 summarizes key demographic, work history, and employment and earnings outcomes for our Work First sample. The table displays these characteristics for the entire sample and separately by Work First placement outcome: direct-hire job, temporary-help job, or no job placement. Of the 30,522 Work First spells, 38 percent lead to direct-hire job placement, another 9 percent lead to a temporary-help placement, and 53 percent 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 percent of the Work First spells (with the remainder missing education data), these figures indicate that a small fraction of the population has any post-secondary education and a large fraction is high school drop-outs.

We track labor market outcomes of Work First participants in quarters 2 to 8 following Work First entry using matched Michigan unemployment insurance wage records data (panel D, Table 1).[7] Participants are 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 non-zero UI earnings over the follow-up period. Those not placed into a job during the Work First spell are less likely to be employed and have lower earnings in the seven-quarter follow-up period compared with those placed into a direct-hire or temporary-help job. 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 in the eight quarters prior to entering the program than those placed directly with employers. Interestingly, on average, temporary-help jobs obtained through the Work First program pay a somewhat higher hourly wage and have longer weekly work hours than Work First direct-hire jobs. During quarters 2 to 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. Despite this and the higher earnings evidenced in their Work First jobs, those placed into temporary-help jobs have somewhat lower average quarterly earnings (-$76) compared with those placed into direct-hire jobs in post-assignment quarters 2 to 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 to 8 based on employer information contained in the UI wage records 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 to 8 derive from direct-hire jobs. Even for those receiving a temporary-help placement, 76 percent of post-assignment earnings, on average, come from direct-hire

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

jobs. This figure is 91 percent for those with a direct-hire placement and 87 percent for those with no job placement.[8] In addition, over the seven-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 stable employment and higher earnings.

The empirical focus in this paper concerns the causal effects of temporary-help and direct-hire job placements on the distribution of subsequent earnings. Table 2 provides summary statistics of mean quarterly earnings in post-assignment quarters 2 to 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. Approximately 20 percent of all participants and 27 percent of those whose Work First spell ended without any placement had no UI earnings in the seven-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. At the 25th percentile of total earnings, only 68 percent of earnings come from direct-hire employment, while at the 75th percentile 85 percent 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 to 75 percent of earnings in the follow-up period come from direct-hire jobs. This fact indicates 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. In this paper, we rely on 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.[9] The basic assumptions and structure of the model are discussed in detail by Chernozhukov and Hansen and hence we present only a summary here.

8. 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. Consequently, direct-hire and temporary-help earnings do not sum to total earnings.

9. 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. 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.

The econometric model is estimated on a dataset 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 are the post-placement earnings, D is a vector of dummies indicating placement into a temporary-help or direct-hire job, and Z is an indicator of the rotational Work First contractor assignment. Here, capital letters denote random variables while lowercase letters denote the values these random variables take.

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 $U_D = \tau$. U_D is the so-called rank variable which characterizes heterogeneity among observationally similar individuals (that is, in terms of their covariates and treatment status). The quantile treatment effect can then be written simply as $\frac{\partial}{\partial d} QY_d\,(\tau \mid X)$ or $QY_{d(\tau \mid x)} - QY_{d'}\,(\tau \mid x)$. If the treatment effect is non-constant (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. Effectively, the estimator IVQR is a quantile analog of two stage least squares.

The main assumptions of the model as given by Chernozhukov and Hansen (2004a) are: $A1$. The *potential outcomes* can be expressed $Y = q\,(d, x, U_d)$, where $q\,(d, x, \tau)$ is strictly increasing and left-continuous in τ. $A2$. 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. $A4$. Most important, 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.*[10] $A5$. 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

10. 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 very important for achieving rank similarity.

(QR) objective function, which can be written as

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

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 and 2004b) instrumental variable quantile regression (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 (QR) to obtain

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

Then, to find an estimate for $\alpha(\tau)$), seek the value 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, 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

$$\begin{aligned} Q\left(y_{icrt}\,|x(\tau)\right) &= \alpha(\tau) + \beta(\tau)^T T_i + \beta(\tau)^D D_i \\ &\quad + X_i'\lambda(\tau) + \gamma(\tau)_r + \theta(\tau)_t + \left(\gamma(\tau)_r \times \theta(\tau)_t\right) + \epsilon(\tau)_{icrt} \end{aligned} \tag{3}$$

where the subscript i refers to participant, r to randomization district, c to contractor and t to assignment year-quarter. The binary variables T_i and D_i indicate whether the participant obtained a temporary-help job or a direct-hire job, respectively. The vector of covariates (X) includes gender, race, age, as well as the average quarterly UI earnings and the quarters of employment during the eight quarters preceding the assignment. Finally, the model also includes dummies for randomization districts (γ) and year by quarter of assignment (θ).

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.[11] This makes it possible to use the contractor assignment 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.[12] 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, $\hat{P}_{ct}^{T}$ and $\hat{P}_{ct}^{D}$, estimated from linear probability models.

For contractor assignments to serve as a valid instrumental variable for participant job placement types, the estimated placement rates $\hat{P}_{ct}^{T}$ and $\hat{P}_{ct}^{D}$ must be independent of $\epsilon\left(\tau\right)_{icrt}$. 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 appears reasonable. It allows for the possibility that contractors 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.[13]

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

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

12. 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.

13. Formally, this is is shown using an overidentification test.

populations or heterogeneity in the treatments administered with heterogeneity in the *effects* of treatment, which is the empirical object of interest.

These two potential threats to validity—non-comparability of treated participants and non-comparability of treatments—both 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. While this independence assumption is formally untestable—since we do not observe latent ranks—we can use as a rough proxy for participants' earnings ranks their observed earnings in the eight quarters prior to contractor assignment. Not surprisingly, past earnings is highly predictive of future earnings: in an OLS regression of earnings in quarters 2 through 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 ($SE = 0.006$).

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

We implement this test using a seemingly unrelated regression (SUR) model. The intuition of this procedure is readily described using a single equation regression model of the form:

$$D_{icdt}^{k} = \alpha + \gamma_d + \varphi_t + \theta_{dt} + \lambda_{ct} + \omega_{icdt},$$

where D_{icdt}^{k} is a dummy variable equal to one if participant i in prior earnings quartile k assigned to contractor c serving assignment district d in year t received a direct-hire or temporary-help placement during her assignment spell (with separate dichotomous variables for each outcome).[14] The vectors γ and φ contain a complete set of dummies indicating randomization districts and year-by–contractor assignment, respectively, while the vector θ contains all two-way interactions between district and year.

Of interest in this equation is λ, a vector of contractor-by-year of assignment dummies, with one contractor-by-year dummy dropped for each district-year pair. The p-value for the hypothesis that the elements of λ are jointly equal to zero provides an omnibus test for the

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

null hypothesis that the contractor effects on placement rates into direct-hire or temporary-help positions do not differ systematically among participants drawn from different prior earnings terciles assigned to the same contractor. A low p-value corresponds to a rejection of this null.

Table 3 displays the results of this exercise. In most cases, we accept the hypothesis that direct-hire and temporary-help placement probabilities do not differ systematically across the terciles of prior earnings for participants assigned to the same contractor. However, there are a total of 13 of 100 contractor-year cells for which we reject the equality of placement rates 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.[15] We eliminate these cells from the analysis, which reduces the sample size by 6,639 observations, or roughly 17 percent. 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. We restrict our subsequent analysis to this sample, though we note that our findings are essentially unaffected if we instead use the full sample.

The second validity test we perform is a check on covariate balance among participants assigned to contractors within each district and year. We apply the SUR model to test for balance of the following covariates: sex, white race, other (nonwhite) race, age and its square, average employment probability in the eight quarters before program entry, average employment probability with a temporary agency in these prior eight quarters, average quarterly earnings in these prior eight quarters, and average quarterly earnings from temporary agencies in the prior eight quarters. Following our approach above, we perform 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).

As shown in Appendix Table 1, the data accept the null by a comfortable margin in all cases, with p-values in excess of 0.50. It deserves emphasis that neither acceptance of the null for equality of placement rates within contractor-year by prior-earnings tercile nor balance of covariates by earnings tercile across contractor-years proves 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

15. 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

a district-year cell. Acceptance of these nulls is, however, supportive of the plausibility of these 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 2 to 8 quarters following Work First contractor assignment and contrasts estimates obtained from OLS, 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 on 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 a complete set of year-by-quarter dummy variables, assignment district-by-year dummies, as well as controls for age and its square, gender, white and Hispanic race, total UI earnings and total quarters of employment in the eight quarters prior to Work First assignment, and temporary-help earnings and quarters of temporary-help employment in the eight quarters prior to Work First assignment. To facilitate interpretation, we re-center all control variables by subtracting the mean for participants who did not obtain a job during their Work First spell. Thus, by construction, the intercept in the OLS estimates equals the mean of the outcome variable for Work First participants who were not placed into jobs.

V.A. Earnings effects of any job placements

The first panel of the Table 4 presents a descriptive OLS regression of equation (3). Participants who obtain a job placement during their Work First spell earn on average $498 more per quarter over the seven subsequent quarters than participants who obtain no placement. This point estimate corresponds to an earnings gain of more than 50 percent relative to non-placed participants whose quarterly earnings average $935. The OLS model is likely to provide an upward biased estimate of the causal effect of job placements 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 2SLS model in the second panel (B) of the table confirms this expectation. We estimate that job placement raises subsequent quarterly earnings by $299, which is 40 percent smaller than the OLS estimate, though still highly significant.

The OLS and 2SLS estimates describe the conditional mean effect of Work First placements on participant outcomes but are not informative about the distributional impacts of these placements. Panel C presents descriptive (QR) estimates analogous to the earlier OLS estimates. 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 15^{th} percentile to \$953 per quarter at the 85^{th} percentile. Notably, the point estimate and intercept at the 50^{th} percentile are considerably smaller than the OLS analogues, indicating that the distribution of quarterly earnings outcome is significantly right-skewed.

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 placements 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.[16]

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 50^{th} 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 through 90 (accompanied by 95 percent confidence intervals). The causal effects of job placements on subsequent earnings are quite heterogeneous. Below the 35^{th} percentile, the estimated treatment effect is close to zero with a relatively narrow confidence band. From the 35^{th} to 60^{th} 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 run 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 percent level (Panel D of Table 4).

16. 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 and using these 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 sub-sample 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 very slightly more conservative (i.e., larger). The results are reported in Appendix Table 2.

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*, which in our application 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 counterfactually would have had earnings at percentile p' of the conditional distribution of *non-placed* workers had her contractor assignment instead induced that outcome. Though 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.

As a complement to the structural estimation approach, we also fit in Panel E a set of 'reduced form' QR models using a simple two-stage procedure. In the first stage, we calculate average excess job placement rates by contractor year as in our models above. In stage two, we estimate quantile regressions for conditional earnings quantiles at the individual participant level using the excess contractor-year placement rate measure as our key explanatory variable (along with standard covariates). These reduced form QR (RFQR) models make no particular assumptions about the relationships among participant earnings ranks across treatment conditions. Rather, in the spirit of standard QR models, they simply estimate the effect of a treatment on the conditional quantiles of the outcome variable. In our application, the treatment is the exogenous component of contractor placement rates which, as established above, raises or lowers the odds of job placement roughly uniformly across participants assigned to those contractors.

Notably, the RFQR estimates prove quite similar to the IVQR estimates that are based on stronger assumptions. At percentiles 15 through 75, the IVQR and RFQR point estimates all fall within a few dollars of one another, though generally they are not point identical. The comparability of the IVQR and RFQR estimates may be taken as evidence that either the IVQR assumptions are likely to be satisfied or that the IVQR method in this application does not recover an underlying structural relationship. Under either interpretation, these results indicate that our findings are not highly dependent on the IVQR procedure.[17]

17. It is likely that the standard errors reported for the RFQR models are biased downward since the RFQR procedure is akin to estimating 2SLS in two steps without accounting for the fact that the explanatory variable in the second stage is estimated rather than observed. A virtue of the IVQR procedure, by contrast, is that under the identifying assumptions, it will produce asymptotically correct standard errors.

V.B. Distinguishing between direct-hire and temporary-help placements

Table 5 enriches the previous models to separately identify the distinct 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 quarters 2 to 8, while temporary-help placements are associated with a $410 quarterly earnings gain. 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. These OLS results are largely consistent with the earlier empirical literature on temporary-help jobs, whereas those from the 2SLS stand in contrast with the conventional wisdom regarding the assumed positive impacts of any job placements on welfare recipients' labor market outcomes.[18]

We explore the impact of temporary-help and direct-hire placements on the distribution of earnings 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—though one that is consistent with estimates from the 2SLS models. As with these models, only the direct-hire placements retain their positive effect in the IVQR, while the estimates for temporary-help jobs become negative. We estimate that direct-hire-placements have highly heterogeneous, though always non-negative, effects that range from zero at the lowest quantiles to approximately $200 at the median, to $1,026 at the 85^{th} percentile. These quantile treatment effects are generally significant at percentiles 50 to 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 are helped more by direct-hire

18. See e.g. Ferber and Waldfogel (1998), Lane et al. (2003), Corcoran and Chen (2004), Andersson et al. (2005, 2007), and Heinrich et al. (2005 and 2007). These results of course are consistent with Autor-Houseman (2010).

placements and harmed more by temporary-help placements than are those who rank lower in the conditional earnings distribution. 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 80^{th} percentile are significantly negative and large. A Wald test comparing estimates at the 15^{th} and 75^{th} quantiles rejects the null of constant quantile treatment effects at the 1 percent 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 quintiles (Table 5, panel D).

On net, these estimates reveal that the modest overall causal effects of job placements on participant earnings throughout the conditional earnings distribution (Table 4) mask two countervailing effects: relatively large direct-hire placement effects—ranging from \$250 to \$1,000 per quarter over the 50^{th} through 85^{th} 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 plausible interpretation of this result is that temporary-help placements crowd out the earnings that these (relatively) high earnings potential workers would have received if they had not received a placement; that is, temporary-help placements for these workers are worse than no placement at all because they inhibit them from obtaining better positions on their own.

As a reality check on these estimates, panel E presents 'reduced form' QR models analogous to those above, estimated using a standard QR regression with contractor excess placements into direct-hire and temporary-help jobs as the main explanatory variables. The coefficient estimates from the RFQR models are point identical with the corresponding IVQR models for quantiles 25 through 85 and differ only slightly from the IVQR estimates for lower quantiles.

The comparability of the RFQR and IVQR models both provides reassurance on the robustness of our results and suggests caution in their interpretation. On the positive side, the estimates are clearly not sensitive to the estimation procedure (IVQR versus our simple reduced form model). On the negative side, the reduced form QR estimator is clearly only a model for the causal effect of treatment on the conditional *distribution* of earnings and makes no claim to identify the *person-level* causal effect of temporary-help or direct-hire job placement relative to an alternative placement.[19] The fact that RFQR yields near-identical results to the IVQR model raises some doubt as to whether the IVQR model is actually recovering the joint distribution of latent outcomes for participants at each location in the quantile index or whether it is merely estimating the causal effect of treatment on the conditional distribution of wages. Under this latter hypothesis, we would offer a somewhat weaker interpretation of the findings in Table 5: while we are justified in concluding that direct-hire placements have heterogeneous but uniformly positive impacts on earnings and, conversely, that temporary-help placements have heterogeneous but uniformly negative impacts on earnings, we cannot necessarily infer that the participants who most benefit from direct-hire placements are those who are most harmed by temporary-help placements and vice versa.

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's 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.[20] To illuminate these relationships, we re-estimate 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.[21] Alternative estimates that exclude person level covariates are reported in Appendix Table 3, 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 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.

19. Because the IVQR model assumes rank invariance, the implied person-level treatment effect is simply the difference in the causal effects of different treatments at each location in the quantile index.

20. 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 analogue of this technique, however.

21. Chernozhukov and Hansen (2006) 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."

V.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 next two analyses suggest two complementary answers. One is that temporary-help placements do not appear to serve as a stepping stone into direct-hire jobs—nor do placements into direct-hire jobs subsequently promote temporary-help employment. Thus, placements in direct-hire and temporary-help jobs primarily affect earnings in the sectors into which workers are placed, and moreover may crowd out earnings in the alternative sector. Second and quite logically, direct-hire placements appear to yield longer and higher-paying job spells than do temporary-help placements. Putting these facts together implies that direct-hire placements raise earnings because these placements are relatively durable or lead to other, durable direct-hire jobs. Conversely, temporary-help placements do not increase and may lower earnings because these placements end rapidly and do not typically yield subsequent, more durable direct-hire employment. We next explore these points formally.

Table 6 re-estimates the 2SLS and IVQR models separately for earnings in direct-hire and temporary-help employment during post-placement quarters 2 to 8. If placed in any job during the Work First spell, the median participant (i.e. a participant in the 50^{th} quantile of the conditional earnings distribution) increases her subsequent quarterly direct-hire earnings by \$237, with no effect on her subsequent temporary-help earnings. Conversely, participants placed in temporary-help jobs see a small and not significant \$37 increase in direct-hire earnings at the median, and no increase in temporary-help earnings. At higher quantiles, we see larger positive effects of direct-hire and temporary-help placements on earnings in those job types. Simultaneously, crowd-out is also larger at higher quantiles: at high values of the quantile index, participants placed in direct-hire jobs gain the most from those placements and forgo the largest earnings in temporary-help jobs and vice versa. However, because the own-sector placement effects are more consistently large and positive for direct-hire placements, the net effects of direct-hire placements are generally positive while those for temporary-help placements are generally negative. Although Wald tests fail to reject the null hypothesis of constant treatment effects for direct-hire and temporary-help earnings, they do reject the equality of the direct-hire and temporary-help placement effects at the 50th and 75th quantiles.

The final set of tables and figures (Table 7 and Figure 5) re-estimate the models for total wage earnings during the longest post-placement job spell.[22] The results are largely

22. 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 two-year follow-up period comes from state UI wage records. In general we cannot tell whether a

consistent with those discussed above: the estimates in panel B showing separate effects for direct-hire and temporary-help placements exhibit considerable heterogeneity across the conditional earnings distribution. The estimated earnings increases resulting from direct-hire placements in the IVQR range from \$4 to \$930 (at the 15^{th} and 85^{th} 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. Temporary-help placements are not associated with such positive effects, but may in fact 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, it is clear that the patterns are not always monotone, but instead exhibit 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 a result from the lack of support for the instrument at these locations.[23] Chernozhukov et al. (2009) show that it is possible to re-order the quantiles (point estimates and standard errors) to satisfy the monotonicity requirements, and thereby improve upon the original 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.[24]

VI. Conclusions

This paper applies the instrumental variable quantile regression estimator developed by Chernozhukov and Hansen (2004a, 2005, 2006) to job placement and earnings data from Detroit's Work First program, which was previously used by Autor and Houseman (2010) to study the effects of welfare-to-work job placements on subsequent post-placement earnings. We use the rotational assignment of participants to contractors as instruments for direct-hire and temporary-help job placements, which allows us to estimate the causal effects of placements on the distribution of participants' post-program earnings. The quantile treatment effects provide a more nuanced depiction of the effect of welfare-to-work job placements on program participants' long-term labor market outcomes than is feasible using OLS and

job held in the follow-up period is the same as the job obtained through the Work First program.

23. 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.

24. Plots using the rearrangement procedure are available from the authors.

IV methods, which estimate conditional mean effects. In this paper, we are able to estimate the effects of direct-hire and temporary-help placements over the entire distribution of participants earnings.

We document that the effects of job placements on labor market outcomes are quite heterogeneous for both direct-hire and temporary-help placements. Moreover, the treatment effects of direct-hire and temporary-help placements differ qualitatively and quantitatively from one another. Direct-hire placements are estimated to significantly increase subsequent earnings over one to two 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. 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 treatment effects differentials between the top and bottom quartiles of the effects distribution for this group are both economically and statistically significant.

Substantively, these results highlight that the widespread use of temporary-help agencies by public programs may not be an effective strategy for improving earnings in this disadvantaged population. Perhaps more fundamentally, they underscore the possibility that interventions focused solely on job placement may do little to raise the earnings of those in the lower end of the conditional earnings distribution.

Methodologically, a surprising result of our analysis is that a reduced-form quantile IV approach, akin to two-step instrumental variables, produces near-identical point estimates to the structural IVQR approach, which is based on much stronger assumptions. Reassuringly, the comparability of these estimates indicates that our substantive results are not sensitive to the estimation procedure. Nevertheless, we believe these results cast some doubt on whether the IVQR model in our application is actually recovering the joint distribution of latent outcomes for participants at each location in the quantile index or whether it is merely estimating the causal effect of treatment on the marginal distribution of wages. Under either interpretation, our estimates indicate that direct-hire placements have heterogeneous but uniformly positive impacts on earnings and, conversely, that temporary-help placements have heterogeneous but uniformly negative impacts on earnings. Given this methodological uncertainty, however, we are hesitant to conclude that the participants who most benefit from direct-hire placements are those who are most harmed by temporary-help placements and vice versa (as would be implied by the IVQR model). Rather, we reach a more agnostic conclusion, which is that the distribution of outcomes induced by direct-hire placements first order stochastically dominates the corresponding distribution induced by temporary-help placements.

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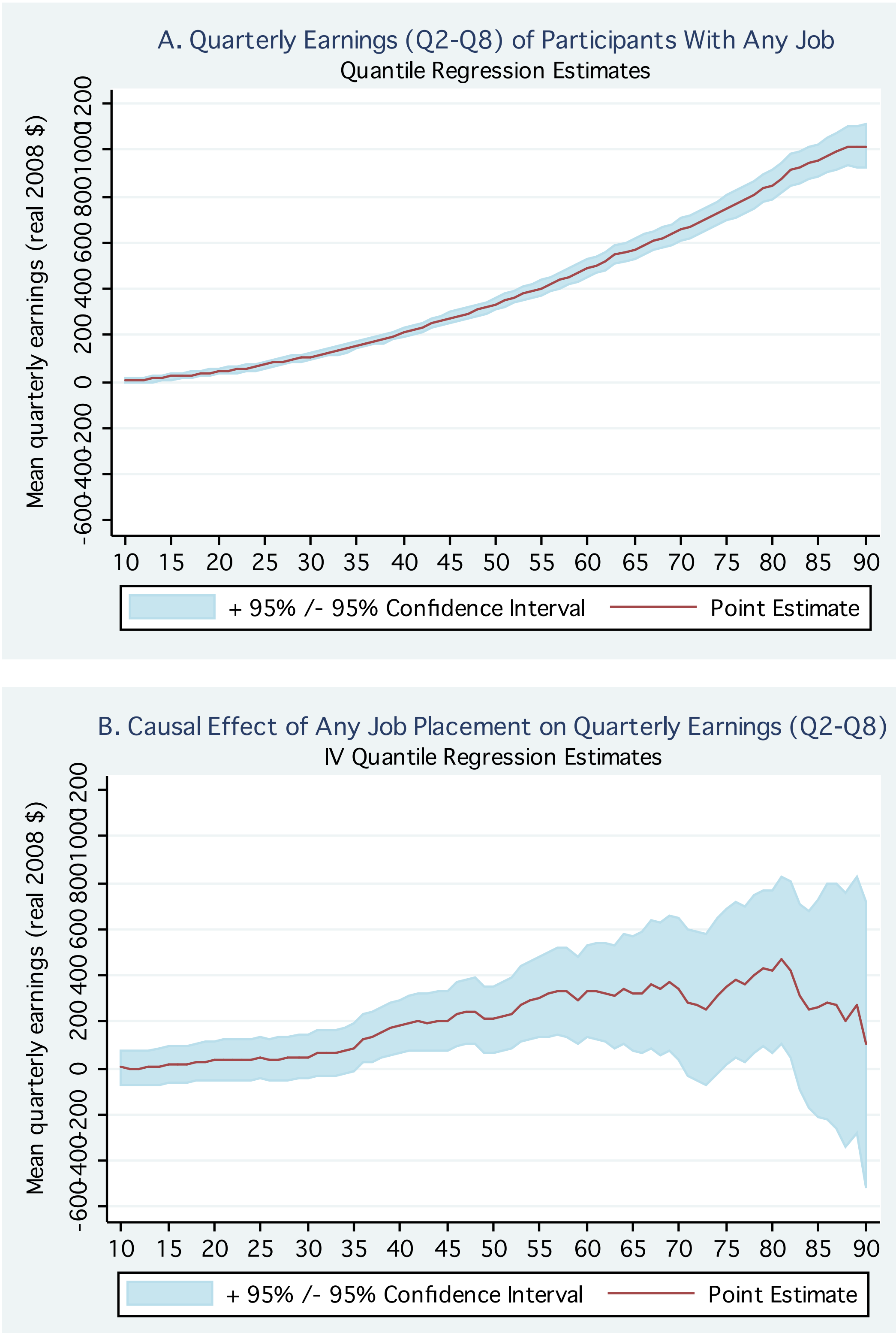
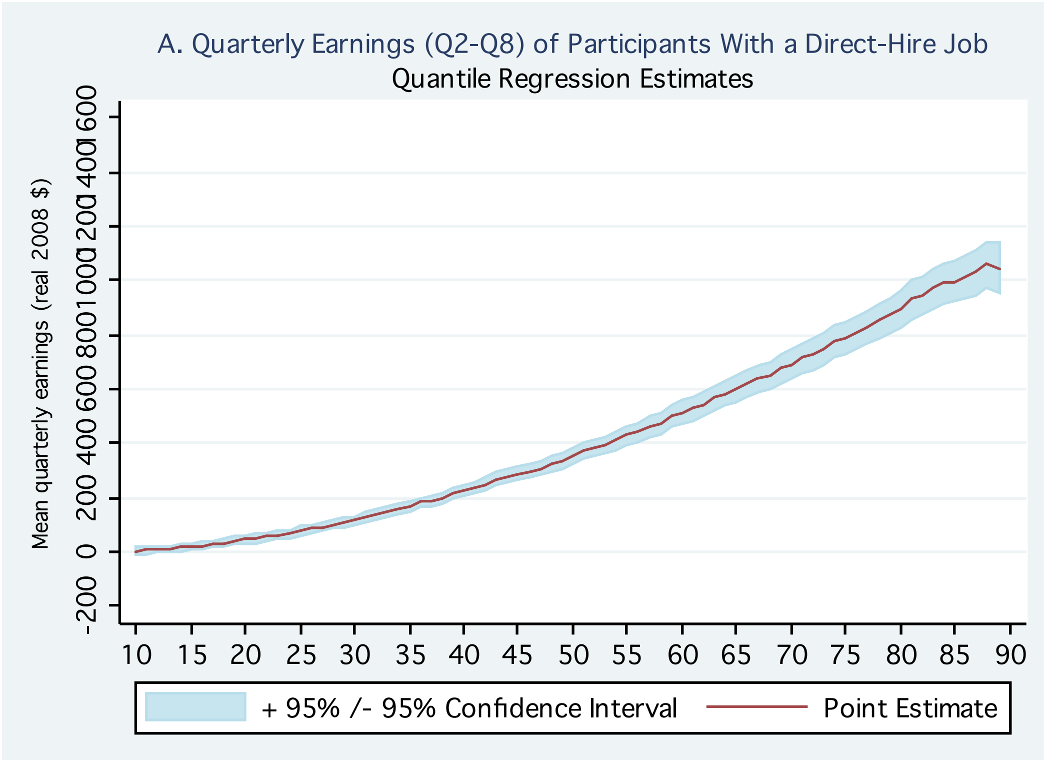


Figure 1: QR and IVQR Estimates for Earnings Quarters 2-8 Following Assignment: Single Endogenous Variable. Coefficient estimates are on the vertical axis and the quantile index on the horizontal axis. The shaded region is the 95% confidence interval.



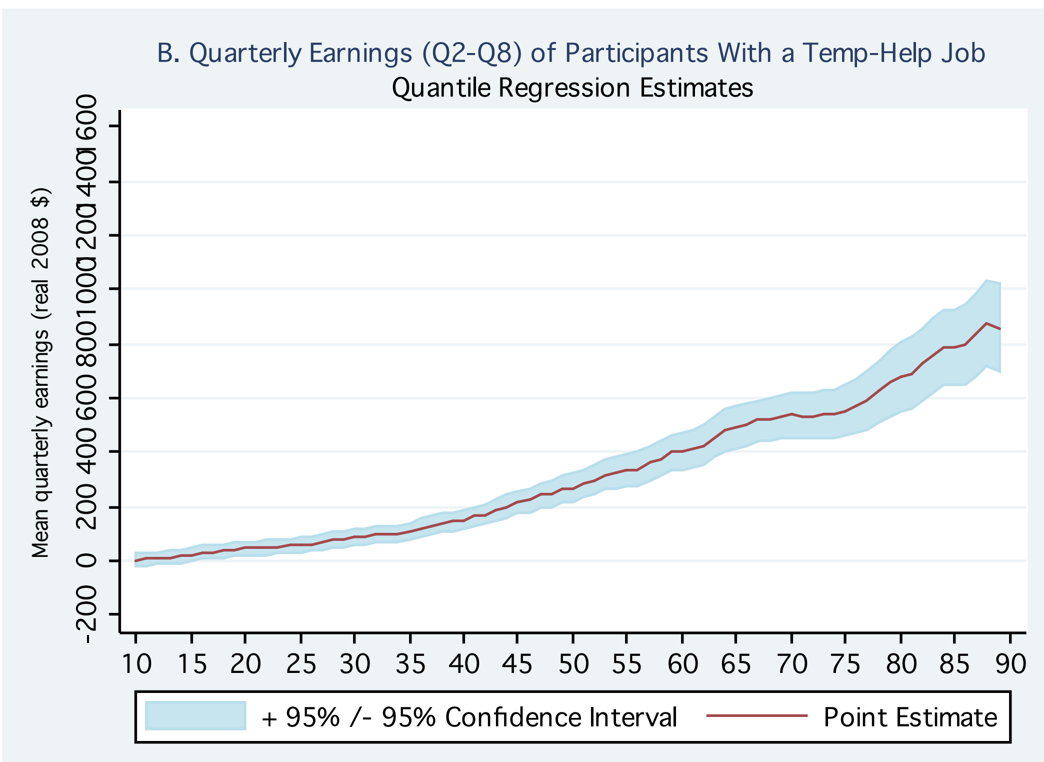
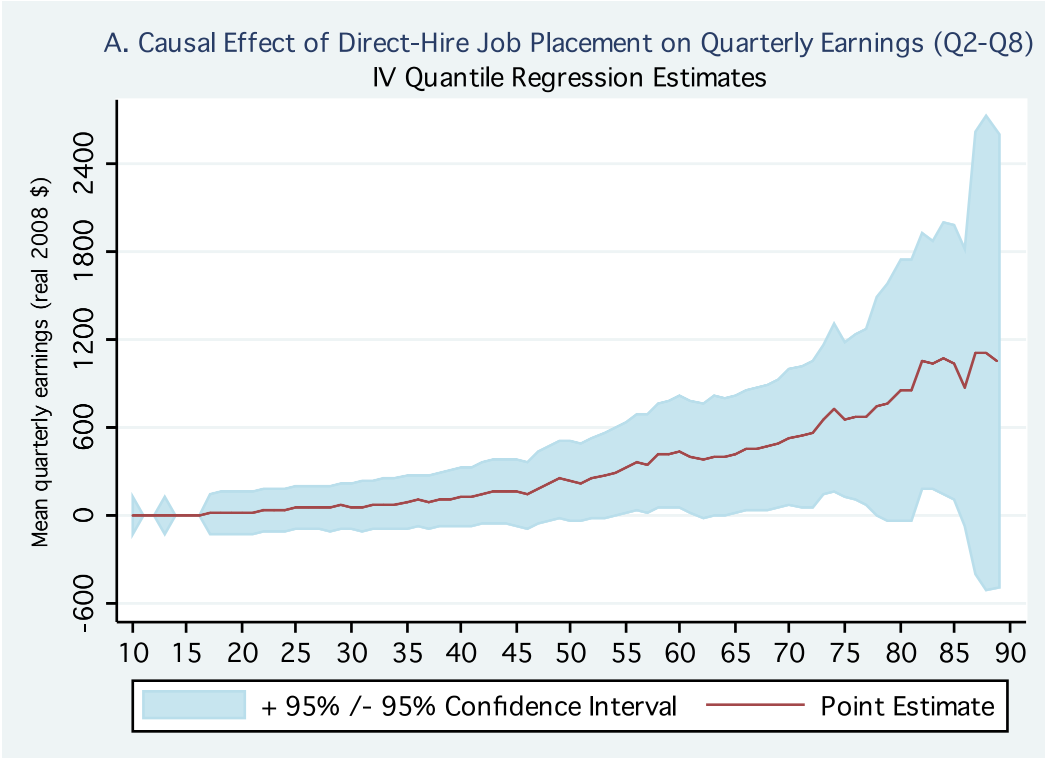


Figure 2: QR Estimates for Earnings Quarters 2-8 Following Assignment: Two Endogenous Variables. Coefficient estimates are on the vertical axis and the quantile index on the horizontal axis. The shaded region is the 95% confidence interval.



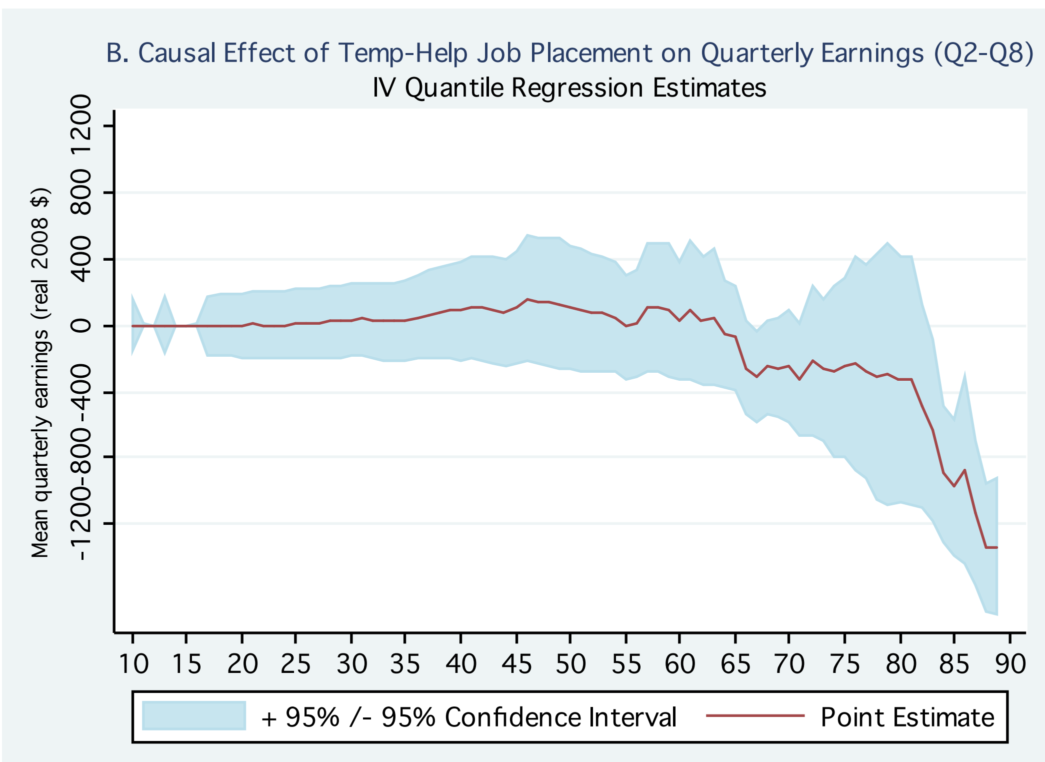


Figure 3: IVQR Estimates for Earnings Quarters 2-8 Following Assignment: Two Endogenous Variables. Coefficient estimates are on the vertical axis and the quantile index on the horizontal axis. The shaded region is the 95% confidence interval.

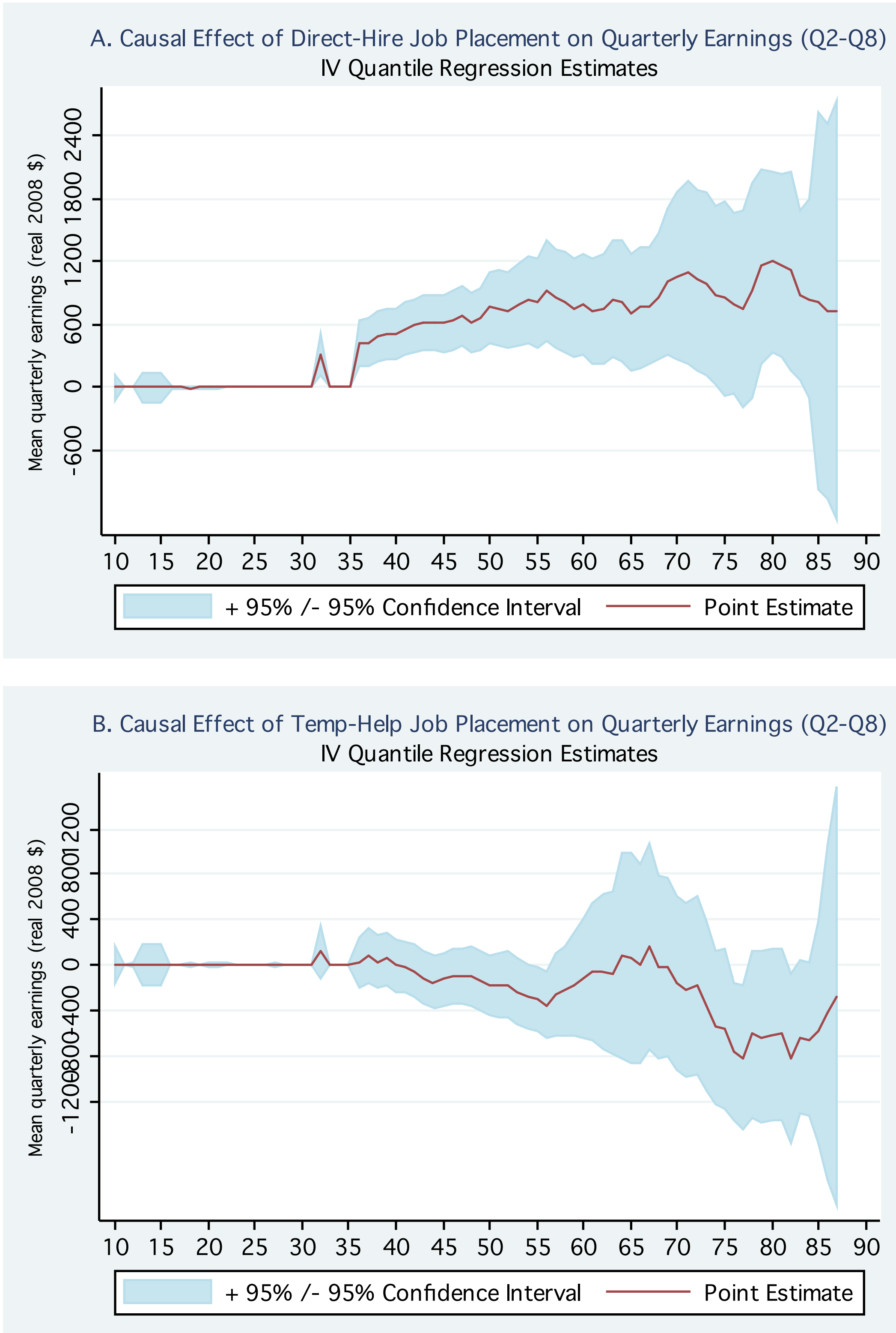
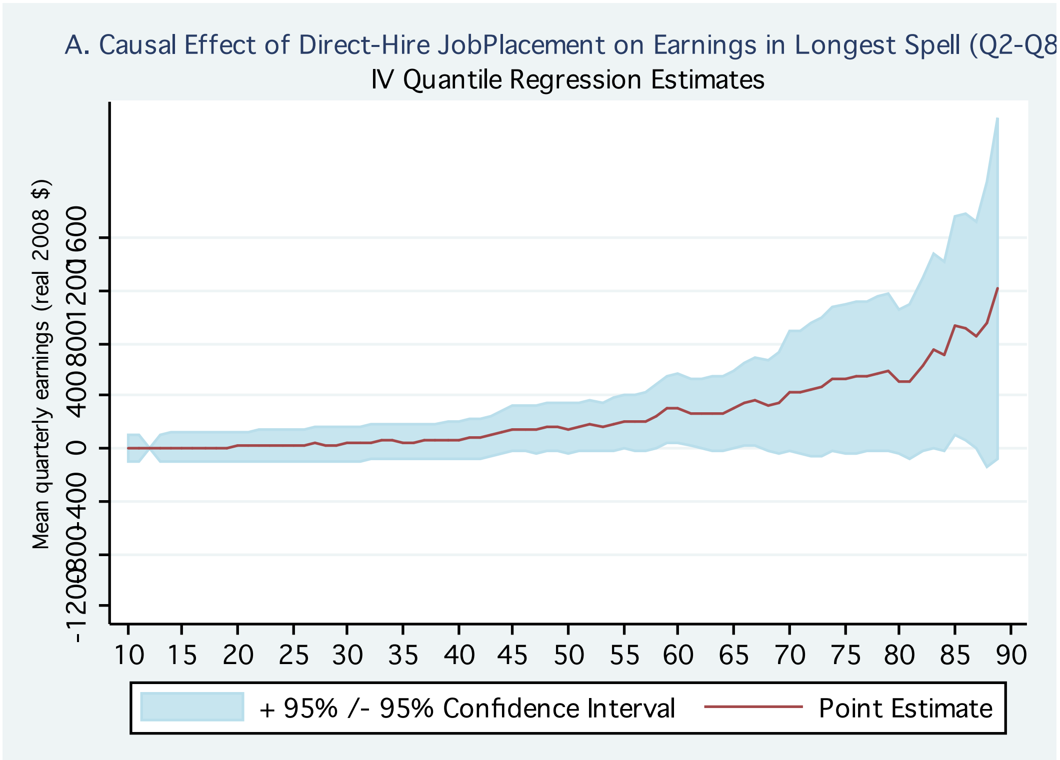


Figure 4: IVQR Estimates for Earnings Quarters 2-8 Following Assignment: Two Endogenous Variables and No Individual Level Covariates. Coefficient estimates are on the vertical axis and the quantile index on the horizontal axis. The shaded region is the 95% confidence interval.



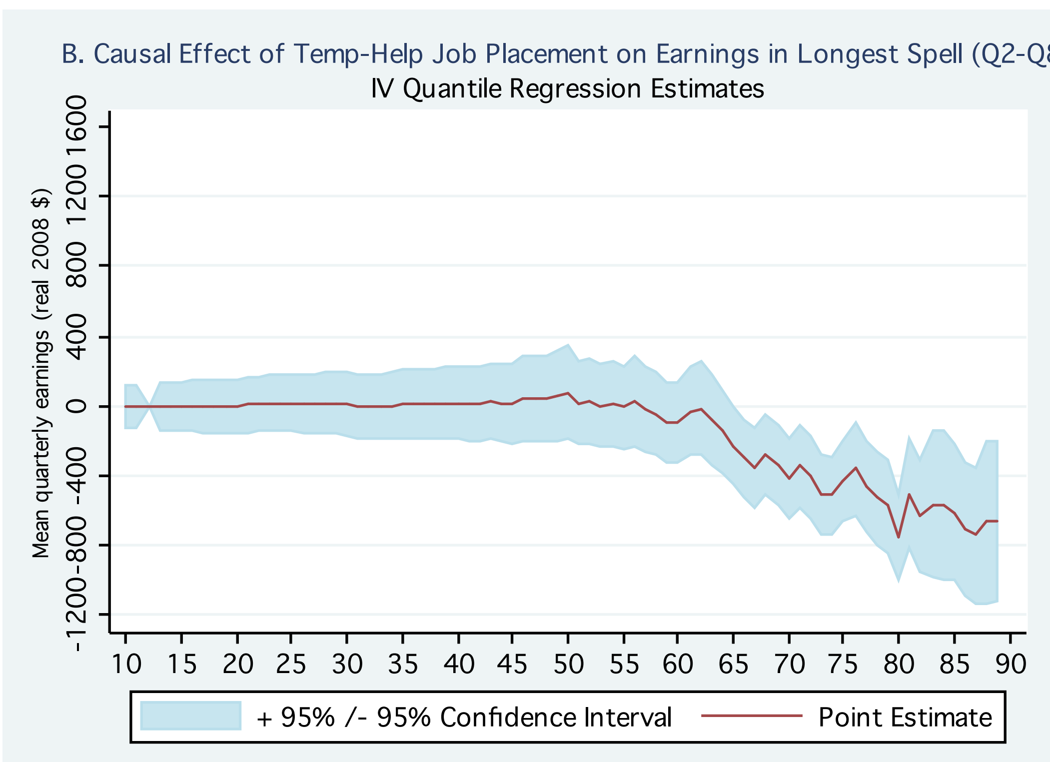


Figure 5: IVQR Estimates for Earnings in Longest Job Spell in Quarters 2-8 Following Assignment: Two Endogenous Variables. Coefficient estimates are on the vertical axis and the quantile index on the horizontal axis. The shaded region is the 95% confidence interval

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
Percent of sample	100.0		53.0		37.9		9.0	
	A. Demographics							
Age	29.6	(0.05)	29.3	(0.06)	29.7	(0.07)	30.4	(0.15)
Female (%)	94.1	(0.13)	94.4	(0.18)	93.9	(0.22)	93.3	(0.48)
Black (%)	97.2	(0.09)	97.1	(0.13)	97	(0.16)	98.3	(0.25)
White (%)	2.1	(0.08)	2.2	(0.11)	2.3	(0.14)	1.2	(0.21)
Other (%)	0.7	(0.05)	0.7	(0.01)	0.7	(0.08)	0.5	(0.14)
< High school (%)	36.9	(0.28)	39.9	(038)	33.3	(0.43)	34.4	(0.90)
High school (%)	36.1	(0.27)	34	(0.37)	38.2	(0.45)	39.6	(0.93)
> High school (%)	7.8	(0.15)	7.2	(0.20)	8.7	(0.26)	8	(0.52)
Unknown (%)	19.1	(0.22)	18.8	(0.31)	19.8	(0.37)	17.9	(0.73)
	B. Work History in Eight Quarters Prior to Contractor Assignment: Quartely 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)
Temp help earnings ($)	139	(2)	124	(3)	136	(4)	237	(10)
	C. Job Placement Outcomes during Work First Assignment for Employed Participants							
Hourly wage ($)	7.53	(0.02)	N/A		7.45	(0.02)	7.89	(0.04)
Hours per week	34.1	(0.06)	N/A		33.5	(0.07)	36.6	(0.12)
Total earnings ($)	260	(0.80)	N/A		253	(0.90)	289	(1.64)
	D. Labor Market Outcomes in Seven Quarters (2-8) Following Contractor Assignment: Quarterly Means							
Employed Q2-4 post-WF (%)	67.5	(0.3)	58.4	(0.4)	77.6	(0.4)	78.2	(0.8)
Employed Q5-8 post-WF (%)	67.5	(0.3)	61.3	(0.4)	74.5	(0.4)	74.6	(0.8)
Total wage earnings ($)	1,229	(9)	935	(12)	1,575	(16)	1,499	(32)
Direct hire earnings ($)	1,078	(9)	817	(11)	1,429	(16)	1,138	(30)
Temp help earnings ($)	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	

Sample: 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. 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 unemployment insurance records industry codes. All earnings inflated to 2003 dollars using the Consumer Price Index (CPI-U).

Table 2. Summary Statistics for Primary Sample of Work First Participants: Post-Placement Earnings Centiles during Quarters 2 to 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	N/A	N/A	76%	64%
Centile 25	68%	N/A	85%	66%
Centile 50	82%	76%	88%	75%
Centile 75	85%	86%	94%	72%
Centile 85	85%	86%	92%	65%

Sample: 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. 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 unemployment insurance records industry codes. All earnings inflated to 2003 dollars using the Consumer Price Index (CPI-U).

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

	Prob. of Direct-Hire Placement		Prob. of Temporary-Help Placement	
	F-value	Prob > F	F-value	Prob > F
Test for equality of contractor dummies across prior earnings terciles	1.51	0.00	1.21	0.06
Full sample N	37,161		37,161	
Test for equality of contractor dummies across prior earnings terciles	0.86	0.83	0.89	0.77
Limited sample N	30,522		30,522	

Sample: 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. 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 unemployment insurance records industry codes.

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

		Conditional Quantile Treatment Effects				
	Mean Effect	0.15	0.25	0.50	0.75	0.85
	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
	(108)	(41)	(46)	(73)	(170)	(239)
Constant	1,026***	40***	187***	637***	1,478	2,256***
	(51)	(15)	(19)	(28)	(74)	(127)
Wald test for constant treatment effects						
Any job placement		Wald statistic (p-value)			10.03	(0.007)
				E. Reduced Form QR		
Any job placement		4*	36***	195***	352**	350
		(2)	(8)	(47)	(180)	(279)
Constant		42***	184***	627***	1,488***	2,235***
		(1)	(4)	(22)	(83)	(129)
				F. IVQR Without Covariates		
Any job placement		0	274***	408***	347	379
		(44)	(52)	(107)	(235)	(368)
Constant		0	5	388***	1,603***	2,542
		(15)	(18)	(34)	(1240)	(214)

N = 30,522. 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, total UI earnings and total quarters of employment in eight quarters prior to Work First assignment, temporary help earnings and quarters of employment in a temporary help job in eight 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.

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

		Quantile Treatment Effects at Quantile				
	Mean Effect	0.15	0.25	0.50	0.75	0.85
	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**
	(178)	(0)	(75)	(138)	(270)	(478)
Temporary-help placement	-57	0	7	106	-254	-977***
	(270)	(1)	(106)	(192)	(277)	(209)
Constant	982***	0	181***	628***	1,452***	2,060***
	(59)	(0)	(21)	(34)	(70)	(135)
Wald test for constant treatment effects						
Direct-hire placement		Wald statistic (p-value)			12.18	(0.002)
Temporary-help placement		Wald statistic (p-value)			1.00	(0.608)
Joint test for the two treatments		Wald statistic (p-value)			14.33	(0.006)
Wald test for equality of direct-hire and temporary-help placement effects						
Wald statistic	2.07	0.39	2.88	10.81	13.98	21.72
(p-value)	(0.150)	(0.824)	(0.237)	(0.005)	(0.000)	(0.000)
				E. Reduced Form QR		
Direct-hire placement		10***	53***	236***	661**	1,046**
		(4)	(14)	(121)	(302)	(441)
Temporary-help placement		-4	7	106	-254	-977
		(5)	(21)	(80)	(457)	(668)
Constant		41***	181***	620***	1,424***	1,975***
		(1)	(5)	(26)	(100)	(136)

N = 30,522. 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, total UI earnings and total quarters of employment in eight quarters prior to Work First assignment, temporary-help earnings and quarters of employment in a temporary-help job in eight 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.

Table 6. The Effect of Work-First Job Placements on Subsequent Direct-Hire and Temporary-Help Earnings Quarters 2-8 Following Work First Assignment

		IVQR				
	2SLS	0.15	0.25	0.50	0.75	0.85
			A. Direct-Hire Earnings			
Direct-hire placement	518***	1	1	237**	594**	1,061**
	(175)	(63)	(79)	(119)	(252)	(441)
Temporary-help placement	-139	-1	-1	37	-424**	-309
	(265)	(68)	(71)	(137)	(152)	(440)
Constant	841***	1	74***	456***	1,288***	1,926***
	(58)	(16)	(19)	(19)	(76)	(123)
Wald test for constant treatment effects						
Direct-hire placement		Wald statistic (p-value)			4.2355	(0.1203)
Temporary-help placement		Wald statistic (p-value)			0.7329	(0.6932)
Joint test for the two treatments		Wald statistic (p-value)			5.6392 (0.2278)	
Wald test for equality of direct-hire and temporary-help placement effects						
Wald statistic	2.960	0.0033	0.0017	5.8876	25.632	3.7798
(p-value)	(0.086)	(0.998)	(0.999)	(0.053)	(0.000)	(0.151)
			B. Temporary-Help Earnings			
Direct-hire placement	-19	0	0	0	-296*	-2,664
	(60)	(5)	(11)	(0)	(163)	(8,505)
Temporary-help placement	97	0	0	0	1,344	2,120
	(92)	(0)	(19)	(3)	(5,141)	(26,570)
Constant	128***	0	0	0	296**	2,664
	(20)	(0)	(4)	(0)	(139)	(8,441)
Wald test for constant treatment effects						
Direct-hire placement		Wald statistic (p-value)			2.58	(0.275)
Temporary-help placement		Wald statistic (p-value)			0.72	(0.698)
Joint test for the two treatments		Wald statistic (p-value)			0.72	(0.949)
Wald test for equality of direct-hire and temporary-help placement effects						
Wald statistic	0.77	0.00	0.00	0.00	2.71	1.01
(p-value)	(0.380)	(1.000)	(1.000)	(1.000)	(0.257)	(0.604)

N = 30,522. 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, total UI earnings and total quarters of employment in eight quarters prior to Work First assignment, temporary-help earnings and quarters of employment in a temporary-help job in eight 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.

Table 7. 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	0.15	0.25	0.50	0.75	0.85
				A. Single Endogenous Variable		
Any job placement	199**	10	23	140***	189	421*
	(96)	(32)	(36)	(50)	(138)	(224)
Constant	814***	28***	115***	411**	1,138***	1,663***
	(45)	(12)	(15)	(20)	(61)	(100)
Wald test for constant treatment effects						
Any job placement		Wald statistic (p-value)			2.3298	(0.3119)
				B. Two Endogenous Variables		
Direct-hire placement	397**	4	28	153	532*	929**
	(158)	(57)	(63)	(95)	(292)	(425)
Temporary-help placement	-146	-1	16	79	-430***	-609***
	(241)	(72)	(84)	(136)	(118)	(198)
Constant	771***	28*	113***	400***	1,072***	1,662***
	(52)	(15)	(18)	(25)	(75)	(112)
Wald test for constant treatment effects						
Direct-hire placement		Wald statistic (p-value)			1.0947	(0.579)
Temporary-help placement		Wald statistic (p-value)			7.5209	(0.023)
Joint test for the two treatments		Wald statistic (p-value)			32.3757	(0.000)
Wald test for equality of direct-hire and temporary-help placement effects						
Wald statistic	2.450	0.0764	3.7251	3.7988	30.4554	4.1716
(p-value)	(0.118)	(0.963)	(0.155)	(0.150)	(0.000)	(0.124)

N = 30,522. 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, total UI earnings and total quarters of employment in eight quarters prior to Work First assignment, temporary-help earnings and quarters of employment in a temporary-help job in eight 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.

Appendix Table 1

P-Values of Tests of Random Assignment of Participant Demographic Characteristics and of Equality of Job Placement Probabilities across Work First Contractors within Randomization Districts: Overall and by

	Randomization District												
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	All
						A. All Prior Earnings Terciles							
1999 - 2000													
P-value	0.52	0.11	0.66	n/a	n/a	0.12	0.80	n/a	n/a	0.90	0.86	n/a	0.59
N	1,863	720	708	0	0	954	807	0	0	794	690	0	6,536
2000 - 2001													
P-value	0.44	0.14	0.01	0.33	n/a	0.85	0.99	n/a	0.69	0.86	n/a	0.26	0.38
N	1,462	1,380	272	1,384	0	516	682	0	145	849	0	1,484	8,174
2001 - 2002													
P-value	0.17	0.12	0.78	0.35	0.37	0.46	n/a	0.37	n/a	0.99	0.63	0.48	0.41
N	2,006	1,589	673	1,423	923	957	0	1,102	0	784	372	1,614	11,443
2002 - 2003													
P-value	0.42	0.79	0.35	0.95	n/a	0.28	0.67	n/a	n/a	0.78	n/a	0.09	0.60
N	717	399	332	715	0	333	476	0	0	419	0	978	4,369
All Years													
P-value	0.30	0.07	0.18	0.65	0.37	0.46	0.98	0.37	0.69	1.00	0.86	0.14	0.51
N	6,048	4,088	1,985	3,522	923	2,760	1,965	1,102	145	2,846	1,062	4,076	30,522
						B. Highest Prior Earnings Tercile							
1999 - 2000													
P-value	0.14	0.40	0.34	n/a	n/a	0.54	0.16	n/a	n/a	0.33	0.93		0.30
N	557	207	186	0	0	253	155	0	0	225	159	0	1,742
2000 - 2001													
P-value	0.80	0.76	0.11	0.06	n/a	0.92	0.98	n/a	1.00	0.88		0.11	0.84
N	562	541	84	443	0	170	166	0	40	266	0	539	2,811
2001 - 2002													
P-value	0.75	0.16	0.10	0.71	0.48	0.14	n/a	0.68	n/a	0.74	0.75	0.81	0.62
N	835	623	245	455	335	319	0	393	0	235	123	592	4,155
2002 - 2003													
P-value	0.40	0.69	0.34	0.64	n/a	0.37	0.88	n/a	n/a	0.69	n/a	0.10	0.57
N	287	145	121	196	0	104	125	0	0	137	0	351	1,466
All Years													
P-value	0.62	0.50	0.07	0.33	0.48	0.54	0.92	0.68	1.00	0.86	0.94	0.16	0.70
N	2,241	1,516	636	1,094	335	846	446	393	40	863	282	1,482	10,174
						C. Middle Prior Earnings Tercile							
1999 - 2000													
P-value	0.16	0.26	0.59	n/a	n/a	0.15	0.85	n/a	n/a	0.98	0.42	n/a	0.47
N	695	245	271	0	0	326	286	0	0	268	230	0	2,321
2000 - 2001													
P-value	1.00	0.37	0.12	1.00	n/a	0.81	0.98	n/a	0.15	0.87	n/a	0.86	0.99
N	472	416	108	537	0	173	205	0	55	312	0	504	2,782
2001 - 2002													
P-value	0.14	0.41	0.79	0.90	0.80	0.13	n/a	0.55	n/a	0.92	0.84	0.75	0.85
N	604	488	213	500	280	335	0	358	0	277	120	511	3,686
2002 - 2003													
P-value	0.03	0.51	0.59	0.64	n/a	0.81	0.33	n/a	n/a	0.29	n/a	0.34	0.22
N	222	138	98	248	0	123	148	0	0	122	0	286	1,385
All Years													
P-value	0.16	0.32	0.54	0.99	0.80	0.40	0.91	0.55	0.15	0.97	0.72	0.79	0.91
N	1,993	1,287	690	1,285	280	957	639	358	55	979	350	1,301	10,174
						D. Lowest Prior Earnings Tercile							
1999 - 2000													
P-value	0.13	0.79	0.64	n/a	n/a	0.03	0.65	n/a	n/a	0.84	1.00	n/a	0.66
N	611	268	251	0	0	375	366	0	0	301	301	0	2,473
2000 - 2001													
P-value	0.07	0.27	0.13	0.66	n/a	0.41	0.57	n/a	0.71	0.40	n/a	0.76	0.22
N	428	423	80	404	0	173	311	0	50	271	0	441	2,581
2001 - 2002													
P-value	0.51	0.54	0.54	0.29	0.35	0.51	n/a	0.22	n/a	1.00	0.92	0.21	0.63
N	567	478	215	468	308	303	0	351	0	272	129	511	3,602
2002 - 2003													
P-value	0.90	0.29	0.75	0.63	n/a	0.67	0.21	n/a	n/a	0.24	n/a	0.87	0.81
N	208	116	113	271	0	106	203	0	0	160	0	341	1,518
All Years													
P-value	0.26	0.52	0.54	0.60	0.35	0.28	0.45	0.22	0.71	0.85	1.00	0.71	0.68
N	1,814	1,285	659	1,143	308	957	880	351	50	1,004	430	1,293	10,174

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

		Quantile Treatment Effects at Quantile 50		
	2 SLS	Reduced Form QR	IVQR Dummy Instruments	IVQR Residualized Instruments
Temporary-Help Placement	-167	-468	-468	-468
	(447)	(296)	(462)	(470)
Direct-Hire Placement	821***	768***	768**	768**
	(171)	(174)	(322)	(325)
Constant	998	547	652	652
	(44)	(57)	(82)	(79)

N = 5,082. 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, total UI earnings and total quarters of employment in eight quarters prior to Work First assignment, temporary-help earnings and quarters of employment in a temporary-help job in eight quarters prior to Work First assignment. Earnings values are inflated to 2003 dollars using the Consumer Price Index (CPI-U).

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 0.15	0.25	0.50	0.75	0.85
	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	623***	0	0	769***	852*	821
	(271)	(75)	(176)	(172)	(476)	(919)
Temporary-help placement	-146	0	0	-181	-566	-588
	(271)	(94)	(1959)	(172)	(360)	(497)
Constant	1,004***	0	0	370***	1,464***	2,458***
	(81)	(19)	(31)	(34)	(151)	(330)
Wald test for constant treatment effects						
Direct-hire placement		Wald statistic (p-value)			0.6970 (0.706)	
Temporary-help placement		Wald statistic (p-value)			0.2966 (0.862)	
Joint test for the two treatments		Wald statistic (p-value)			0.9376 (0.919)	
Wald test for equality of direct-hire and temporary-help placement effects						
		0.0000	0.0000	9.5960	0.8900	0.4628
		(1.000)	(1.000)	(0.008)	(0.641)	(0.793)
		E. Reduced Form QR				
Direct-hire placement		n/a	96***	769***	852**	n/a
			(10)	(213)	(379)	
Temporary-help placement		n/a	99***	-181	-566	n/a
			(15)	(325)	(580)	
Constant		n/a	0	284***	1,521***	n/a
			(3)	(71)	(128)	

N = 30,522. 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).