Can Variation in Subgroups' Average Treatment Effects Explain Treatment Effect Heterogeneity? Evidence from a Social Experiment

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Abstract

In this paper, we assess whether welfare reform affects earnings only through mean impacts that are constant within but vary across subgroups. This is important because researchers interested in treatment effect heterogeneity typically restrict their attention to estimating mean impacts that are only allowed to vary across subgroups. Using a novel approach to simulating treatment group earnings under the constant mean-impacts within subgroup model, we find that this model does a poor job of capturing the treatment effect heterogeneity for Connecticut's Jobs First welfare reform experiment. Notably, ignoring within-group heterogeneity would lead one to miss evidence that the Jobs First experiment's effects are consistent with central predictions of basic labor supply theory.

1 Introduction

In previous work we estimated quantile treatment effects using data from a randomized experiment to evaluate the labor supply impact of Connecticut's welfare reform program Jobs First (Bitler, Gelbach & Hoynes (2006)). In that context, labor supply theory predicts that the reform should cause heterogeneous treatment effects across the earnings distribution. The data revealed a pattern consistent with these predictions, which, roughly speaking, are that there should be mass points at zero earnings in both the treated and control distributions, positive (or negative) earnings effects in the middle of the earnings distribution, and negative earnings effects at the top of the earnings distribution. We found exactly this pattern of results, as Figure 3 of our earlier paper shows, using estimated quantile treatment effects (QTE) of Jobs First on the earnings distribution. Moreover, the range of QTE was quite broad, from -\$300 to \$500, far above the mean impact of \$82.

Our QTE-based approach to measuring treatment effect heterogeneity differs from the conventional one. One common approach involves estimating mean treatment effects but allowing the treatment effects to vary across subgroups based on demographic or other covariates. One then evaluates whether the subgroup-specific differences in treatment effects appear to vary importantly (for a review within the welfare reform literature see Grogger & Karoly (2005)).[1] Because this conventional approach is very simple and is widely followed, it is important to assess whether it is adequate to the task of measuring real-world treatment effect heterogeneity. A natural question is whether the heterogeneity revealed by QTE could somehow be explained using only constant treatment effects allowed to vary across judiciously chosen subgroups that are theorized or known to predict locations in the budget set.[2]

We address this issue in the present paper. In particular, we return to the Jobs First experiment, with its powerful and heterogeneous labor supply predictions, and seek to estimate the earnings distribution that would prevail under experimental treatment under the null hypothesis that a "constant-treatment-effects" model was adequate to characterize Jobs First's effects. While

[1] Kline & Tartari (Forthcoming) use the Jobs First experimental data and apply restrictions implied by labor supply theory to develop bounds on intensive and extensive margin responses to reform. Lehrer, Pohl & Song (2014) also use the Jobs First data and look at where in the distribution there are gains to the program while paying particular attention to issues of multiple testing (across quantiles, across subgroups, etc.).

[2] Our paper is related to work on the effects of changing the distribution of explanatory variables on quantiles of the unconditional distribution (Firpo, Fortin & Lemieux (2009)) or of changing either the distribution of covariates or the conditional distribution of the outcome given covariates on the marginal distribution (Chernozhukov, Fernandez-Val & Melly (2013)).

estimating mean impacts over a finite set of subgroups is a simple parametric problem, constructing this null distribution is not, because only the mean impacts are parametrically specified. All other features of the null earnings distribution under treatment must be left nonparametric.

To deal with this challenge, we construct an estimate of what we term the "simulated earnings distribution under treatment." We construct an estimate of this simulated distribution in a few basic steps. First, we estimate the program's mean impact on earnings for each subgroup of interest; we do so in the usual way, by subtracting the control group's sample mean from the treatment group's sample mean. Second, we estimate each control group woman's "simulated earnings level under treatment" by adding the relevant subgroup-specific mean impact to her actual earnings level. The result is an estimate of the woman's simulated earnings under treatment, given that the constant-treatment-effects model is correct. We use these individual estimates to construct an estimate of the simulated earnings distribution under treatment and compare it to the actual observed earnings distribution under treatment to evaluate the predictive power of the mean impacts approach.

For example, suppose the subgroups are high and low education women. For high and low education women in each time period, we calculate the mean difference in earnings between treatments and controls. We then add this subgroup- and time-specific mean treatment effect to the earnings of each woman in the control group. The empirical distribution of simulated earnings across all control group women is then our estimate of the earnings distribution under treatment that would prevail if the constant-treatment-effects model were correct for subgroups defined by education. We evaluate the performance of various constant-treatment-effects models by comparing earnings QTE estimated using the actual treatment and control earnings distribution to simulated QTE estimated using the simulated earnings under treatment and the actual control group distribution.

We consider three constant-treatment-effects statistical models. In the first, we assume that there is a single mean impact within each subgroup over the entire post-random assignment period we consider. Since the period is relatively long—seven quarters—we then relax the approach by allowing the subgroup-specific mean impact to vary by the quarter after random assignment. We find that the simulated earnings distributions under treatment generated by these constant-treatment-effects models do a very poor job of replicating the pattern of estimated actual QTE. Furthermore, the presence of large and quite different mass points at zero in both the control group distribution and the actual treated distribution are, by themselves, sufficient reason to reject these two constant-treatment-effects models (Heckman, Smith & Clements (1997)). To explore other nulls and avoid rejecting solely on this basis, we consider a third constant-treatment-effects statistical model, which imposes equal mass points at zero in the actual and simulated earnings distributions under treatment (equal probabilities of working in either counterfactual state). The simulated earnings QTE based on this third, "participation-adjusted" constant-treatment-effects model look much closer to the actual QTE, in part by construction. Even so, they fail to exhibit the negative earnings effects at the top of the distribution that we observe in the actual QTE estimates. As we discuss, since these negative effects are a key prediction of labor supply theory, we believe this is an important failure of (even our most flexible) constant-treatment-effects model. Finally, we apply distributional statistical tests. For each family of subgroups, we test a set of joint null hypotheses that treatment effects are constant within subgroups, after adjusting for participation. We adjust for the multiple testing nature of this using the conservative Bonferroni adjustment, and find that for nearly every set of subgroups we consider, we reject this joint null.

In sum, we find compelling evidence against the null hypothesis that any of three "constant-treatment-effects" models can explain the important features of the treatment effect heterogeneity evident using QTE, shown in Bitler et al. (2006). But, importantly, we find that not all subgroups fare equally poorly in generating simulated QTE. We consider a rich set of covariates to assign subgroups including standard demographics (education and marital status of the woman, number and ages of children) as well as variables capturing earnings and welfare participation prior to the experiment. We also consider interactions of these variables, such as education by earnings history. We find that groups defined based on pre-treatment earnings do considerably better than groups based on demographics (as might be expected given Heckman, Ichimura, Smith & Todd (1998) and other papers in the program evaluation literature). Given that, it is important to point out that analyses using standard cross-sectional survey data (such as the Census or Current Population Survey) do not allow for the measurement and use of these most predictive variables. Instead, these cross sectional data sets allow only for differences in treatment effects across the standard demographic variables which we find provide comparatively little ability to capture treatment effect heterogeneity. With the growing use of administrative data, these limitations of survey data may become less important.

In addition to these substantive findings, which we believe are quite important and represent the core contribution of our paper, this paper makes an informal methodological contribution by complementing some important work in the program evaluation literature. For example, Crump, Hotz, Imbens & Mitnik (2008) develop convenient nonparametric tests of the null hypothesis that average treatment effects are zero, or non-zero but constant, across subgroups. By comparison, we test the null hypothesis of constant within-group treatment effects while allowing these treatment effects to vary arbitrarily across subgroups. In addition, one can regard our simulation-based method as an application of Abadie's (2002, p. 289) suggestion that one might be able to test the null hypothesis of a constant treatment effect using distributional equality tests, applied to many subgroups.[3]

To our knowledge, ours is the first paper in the applied literature to construct and test a non-parametric null hypothesis under which all heterogeneity is driven by treatment effects that are constant within, but vary across, a large number of identifiable subgroups.[4] Since many applied researchers use subgroup-specific mean impacts to assess the presence of treatment effect heterogeneity, this is an important addition to the program evaluation testing toolkit.

2 Experimental Setting

Concerns about welfare dependency and low employment rates led many states to reform their Aid to Families with Dependent Children (AFDC) programs during a wave of reform in the 1990s. This movement, which initially involved state-level waivers from federal welfare AFDC rules, culminated in 1996 with the enactment of the Personal Responsibility and Work Opportunity Act (PRWORA). PRWORA eliminated AFDC and replaced it with Temporary Assistance for Needy Families (TANF). Under TANF, welfare recipients face lifetime time limits for welfare receipt, stringent work requirements, and the threat of financial sanctions. PRWORA also allows states substantial flexibility in designing their TANF programs, and some states decided to provide greater financial incentives for participants to combine welfare and work. One such state was Connecticut, which chose to convert its existing, waiver-based Jobs First program into its TANF program. Because Jobs First started out as a demonstration program under federal waiver rules, Connecticut ran a random assignment experiment to evaluate the program. We used MDRC's public use data from this experiment in our earlier paper, Bitler et al. (2006), and we use the same data here.

[3] An interesting direction for future work involves work on program evaluation as a statistical decision problem accounting for effects on distributions, like Manski (2004) and Dehejia (2005), and Bhattacharaya & Dupas (2012). If there is substantial within-group heterogeneity, then it might be possible to improve program assignment decisions more by accounting for this heterogeneity, rather than using only information on within-group mean impacts. Ding, Feller & Miratrix (Forthcoming) consider a similar problem from a statistical perspective.

[4] Importantly, Koenker & Xiao (2002) lay out an approach to such testing for *iid* data, extending Khmaladze's approach to testing in the location scale and other related models.

2.1 The Jobs First Program and Labor Supply Theory

We discuss the Jobs First experiment and its likely incentive effects in considerable detail in Bitler et al. (2006).[5] Here we simply summarize the experiment's main features and explain why it is a good choice for our present analysis. The experimental participants were either assigned to the pre-existing AFDC program (control) or Jobs First (treatment). Jobs First includes a lifetime time limit of 21 months, compared to no time limit under AFDC. The maximum monthly benefit level received by a program participant in a family of 3 was \$543 in 2001 under both programs. Under AFDC assignment, a woman's benefit payment would be reduced by 67 cents for each dollar she earned during her first four months on aid, and by 100 cents thereafter (a 100% implicit tax rate). By comparison, the Jobs First program disregards all earned income below the federal poverty guideline in determining benefit levels. As a result, the implicit marginal tax rate under Jobs First program assignment is 0% for all earnings up to the poverty line, at which point there is a cliff (in principle, another penny of earnings above the federal poverty line would cause the state to terminate the entire benefit payment for women assigned to Jobs First). Thus, the two programs present women with starkly different budget sets.

In this paper, we focus on each experimental subject's first 21 months following random assignment. We do so because the time limit cannot yet bind during this period, so that static labor supply theory makes especially clear predictions concerning Jobs First's effects on earnings. As we discuss in Bitler et al. (2006), these predictions are heterogeneous. First, Jobs First should cause employment to rise, reducing the share of women with zero earnings. Second, by substantially reducing the implicit tax rate on earnings, Jobs First should cause hours worked to rise for women who would have had both welfare income and earnings under AFDC (provided that substitution effects dominate income effects). Since women could receive AFDC only if they had quite low income to begin with, such women will tend to be located low in the earnings distribution in the relevant quarters. Thus, Jobs First should cause an increase in earnings over the lower part of the earnings distribution.

Third, by extending eligibility for cash assistance to women with earnings right below the federal poverty line—which is considerably greater than the level of earnings at which women would lose eligibility for AFDC payments—Jobs First creates incentives for some women to reduce earnings. For women whose earnings would be less than the federal poverty line were they assigned to AFDC,

[5] For a detailed description of the Jobs First experiment and the evaluation results MDRC provided under contract to the state of Connecticut, see Bloom, Scrivener, Michalopoulos, Morris, Hendra, Adams-Ciardullo & Walter (2002).

Jobs First assignment provides a lump-sum transfer of income, which will reduce a woman's optimal earnings in the presence of any income effect. In addition, the cliff nature of the Jobs First budget set creates an incentive to gain Jobs First eligibility by reducing earnings to just under the federal poverty line, among those women whose earnings would not exceed the federal poverty line by more than the maximum benefit payment. Further, even some women who would earn more than the sum of the federal poverty level and the Jobs First benefit payment might choose to reduce earnings to become eligible for Jobs First, due to the disutility of labor supply. Finally, among women whose earnings under AFDC would be sufficiently above the sum of the federal poverty level and the maximum benefit payment, Jobs First assignment will have no effect on earnings, since these women would choose not to receive cash assistance under either program assignment.

In sum, static labor supply theory predicts changes to extensive and intensive margins of labor supply: (i) both the AFDC and Jobs First earnings distributions will have mass points at zero, with the mass being larger among those assigned to AFDC (Job's First should increase extensive margin labor supply); (ii) earnings will be greater under Jobs First over some range of the earnings distribution above zero; (iii) higher up in the distribution, Jobs First may lead to reduced earnings; and (iv) there might be a range in the distribution even further up where there will be no impact of Jobs First assignment.

2.2 The Jobs First Evaluation Data

The Jobs First evaluation was conducted by MDRC, which made public-use data available for outside researchers upon application. The data include information on 4,803 cases; 2,396 were assigned to Jobs First, with 2,407 assigned to AFDC. There are administrative data on quarterly earnings and monthly welfare payments,[6] available for most of the two years preceding program assignment as well as for at least 4 years after assignment. Our outcome variable of interest is quarterly earnings, and as noted above, we restrict attention to the first 21 months, or seven quarters, following each woman's random assignment. In all analyses, we pool the seven quarters of data for each woman, so that there are a total of $4,803$ x $7 = 33,621$ quarterly observations in our estimation sample. In addition to the administrative earnings and welfare data, the public-use data set contains demographics collected at the experiment's baseline, including each woman's number of children, education, age, marital status, race, and ethnicity.

[6]For confidentiality purposes, MDRC rounded all earnings data. Earnings between \$1–\$99 were rounded to \$100, so that there are no false zeros. All other earnings amounts were rounded to the nearest \$100. Welfare payments were also rounded, though with \$50 rather than \$100 increments.

An advantage of the Jobs First data is that it includes pre-random assignment data on earnings and welfare use. These are exactly the variables that the program evaluation literature has suggested one condition on (e.g., Heckman et al. (1998)). These data allow us to construct variables related to pre-experiment earnings and welfare history—variables that are not available in standard survey data such as the Current Population Survey or in many other settings. Using the earnings and welfare history, we can then construct subgroups that are plausibly more likely to map to the theoretical labor supply predictions discussed above. If the constant-treatment-effects model fails here, where we can create unusually well designed subgroups, it is unlikely to succeed elsewhere.

2.3 Defining Subgroups

As discussed above, the Jobs First program is predicted to affect extensive and intensive labor supply. Static labor supply theory implies that variation in the impact of the policy intervention will depend on a woman's earnings opportunities, her preferences for market work versus home time, and her fixed costs of work. Good subgroup choices will proxy for one or more of these elements. Available variables that might proxy for wages include education, earnings and welfare history, age, and marital status. Variables that might proxy for preferences related to market work versus home time include number and ages of children, as well as welfare and earnings history. Age of youngest child is an important predictor of fixed costs of work (child care).

The primary subgroups we use in this paper are based on educational attainment, which is available in standard survey data settings, and earnings and welfare-use history, which are not usually available in such data sets. We also consider other demographics such as age and number of children and marital history as well as interactions of these demographic variables and earnings or welfare history. Our primary interest in choosing subgroups is to find covariates that are useful in separating our samples into women likely to have earnings in the bottom, middle, and top of the earnings distribution when assigned to AFDC. Variables that do a good job of separating the sample this way are more likely to exhibit mean impacts that track the predictions made by labor supply theory, which we discussed above.[7]

First, in online Appendix Figures 1a and 1b, we consider where in the overall control group distribution each of the education or earnings seven quarters before random assignment subgroup members are concentrated. Within a figure, each line shows the share of observations with earnings

[7] We have also used combined demographic variables to estimate a "single index" subgroup measure. We estimated a standard wage equation for a sample of low educated single female heads of household in the CPS. We then used the equation's estimated coefficients to create subgroups based on predicted wages. The qualitative results involving this subgroup were similar to those based on the educational attainment subgroups.

at the qth percentile of the earnings distribution that are in the given subgroup *relative* to the subgroup's overall population share. We use the control group for this analysis.

The online appendix figure shows, unsurprisingly, that high school graduates (relative to high school dropouts) have earnings shifted toward the top of the (control group's) post random assignment earnings distribution. It also shows that high school dropouts have a greater share of quarterly observations with zero earnings. The fact that education leads to "sorting" along the earnings distribution, combined with strong labor supply predictions along the "potential" earnings distribution, suggest that mean impacts calculated using subgroups defined by educational attainment might reflect the treatment effect heterogeneity predicted by static labor supply theory.

We consider other demographically based subgroups in online Appendix Figure 2. These show that subgroups based on age of youngest child (online Appendix Figure 2a) and marital status (online Appendix Figure 2b) do not show much "sorting" along the post-treatment earnings distribution, but instead are fairly evenly (although noisily) spread out across the control group. We found similar results for number of children and age of the woman as well as interactions of these demographic variables. This suggests the mean effects for these other demographic subgroups will not be helpful in uncovering treatment effect heterogeneity.

We also take advantage of our earnings and welfare history data to construct additional subgroups. As discussed above, we observe earnings and welfare participation, at the quarterly level, for seven quarters prior to random assignment. These lagged earnings values and welfare history values are exactly the type of variables the program evaluation literature focuses on for explaining later earnings behavior. Because a large fraction of the sample is in the middle of a welfare spell at random assignment, our main measure uses the earnings for the most distant measure from random assignment—(7th quarters prior)—thus minimizing the influence of "Ashenfelter's dip" (Ashenfelter (1978)). Online Appendix Figure 1b shows the relative shares for this subgroup. In particular, we split the sample into three groups: those with no earnings 7 quarters prior to random assignment (2/3 of the sample), those with earnings at or below the median (among nonzero earnings), and those with earnings above the median (the median being $1600). We label these zero, low, and high earnings history groups. Online Appendix Figure 1b shows that those with high prior earnings are disproportionately likely to be at the top of the earnings distribution post-treatment, while those with no earnings and low earnings 7 quarters prior to random assignment are concentrated in the bottom and middle of the post-random assignment control group earnings distribution.

Online Appendix Figures 2c and 2d show two other similar measures using welfare history and other earnings history data. This shows a similar result to online Appendix Figure 1b—those controls with more earnings history are more likely to exhibit higher earnings in the post-random assignment period while those with no earnings history are more likely to have zero earnings post-random assignment. Finally, in online Appendix Figure 2d we use welfare history to define subgroups, based on whether a woman had any welfare income in the seventh quarter prior to random assignment (47% did, 53% did not). This graph generally shows that women with no welfare history are disproportionately located at the top of the control group earnings distribution post random assignment.

We conclude from this analysis that educational attainment and earnings history represent the most promising candidates for revealing treatment effect heterogeneity, as would be suggested from the previous literature. Women with lower education and less prior earnings should be more likely to have positive mean impacts while those with high education and more earnings history will be the most likely ones to exhibit the negative effects related to program entry and income effects. Welfare history also holds some promise. Other demographics, including marital status and number and ages of children are expected to be less effective. Thus, for the balance of the paper, we will focus primarily on subgroups defined by educational attainment and earnings or welfare-use history (and their interactions).

2.4 Covariate Balance Across Treatment and Control Groups

Exploratory work in Bitler et al. (2006) shows that observed variables are well balanced across the Jobs First treatment and control groups. In that paper, we report means of the baseline characteristics between the two groups and test for statistically significant differences. As described there (and in Bloom et al. (2002)), there were some small but statistically significant treatment-control differences in average values for a small number of these characteristics.[8] However, a test for joint significance of the differences fails to reject the null hypothesis that the vector of covariate means is equal across program assignment ($p = 0.16$). We thus use simple treatment-control differences in this paper (i.e., there are no other controls included in our main results).[9]

[8] The Jobs First group is statistically significantly more likely than the AFDC group to have more than two children and has lower earnings and higher welfare benefits for the period prior to random assignment.

[9] In Bitler et al. (2006), we presented QTE using inverse propensity score weighting (Firpo (2007)) to account for the (small amount of) imbalance in pre-random assignment variables. Weighting does not change our qualitative conclusions in our earlier paper or here. Note that this inverse propensity score weighting can be regarded as semiparametric way of adjusting for many Xs.

3 Results

3.1 Mean Impacts

To begin, we explore whether subgroup-specific mean impacts are consistent with the heterogeneous labor supply predictions discussed above. In Table 1, we report estimated mean treatment effects for the full sample and the subgroups discussed in section 2.3. Each panel presents mean differences for a different set of subgroups, with the estimated mean treatment effects in column 1, their 95 percent confidence intervals in column 2, and the (AFDC) control group means in column 3. Note that we fully stratify the sample and estimate mean impacts within each subgroup. An approach which is probably more common in the broader empirical literature is to estimate regressions which include the treatment dummy as well as interactions of the treatment dummy and subgroup indicators. We view these as alternative models that both fit under our rubric of 'constant-treatment-effects' estimators. The first row in the table shows the overall number of observations in the control (N_C) and treatment (N_T) groups in columns 4 and 5, while the other rows show the share of the control and treatment groups in each subgroup (within the panel) in columns 4 and 5. At the bottom of the panel for each set of subgroups, we present an F-statistic (column 1) and p-value (column 2) for testing the null that the subgroup means are equal (where the standard errors account for correlation within individuals).

The first row of Table 1 shows that overall Jobs First is associated with a statistically insignificant increase in quarterly earnings of \$34, representing a 3% increase over the control group mean of \$1,139. The next four panels of the table present estimates for demographic subgroups defined using the woman's education, number and ages of children, and marital status. The results show some differences in the point estimates across groups, with larger mean impacts for those with lower education levels, those with older children and more children, and for those who had ever been married. These differences in mean impacts are broadly consistent with labor supply theory's predictions of smaller impacts for those likely to have higher wages or high fixed costs of work or lower taste for work.

Notably, however, none of the mean impacts among subgroups defined based on demographic variables exhibit the negative impacts that labor supply theory predicts should occur for at least some women. Moreover, there is no demographic-variable-based subgroup for which the mean impacts vary significantly across subgroups. For example, we cannot reject the equality of the mean treatment effects of \$105 for high school dropouts and \$42 for women with high school graduates

($F = 0.52$, implying a p-value of 0.47). The same is true for the subgroups based on number and ages of children, and marital status (see the table for F-statistics). These small mean impacts on earnings and a lack of heterogeneity across demographic subgroups in the underlying mean impacts for welfare reform is not unique to the Connecticut experiment. In their comprehensive review of the welfare reform literature, Grogger, Karoly & Klerman (2002) conclude that "the effects of reform do not generally appear to be concentrated among any particular group of recipients" (p. 231).[10]

The remainder of Table 1 provides similar analyses for subgroups based on pre-random assignment earnings and welfare history. In contrast to the results for demographic subgroups, the results using earnings history show striking and statistically significant differences across subgroups.

Table 1 shows that for the earnings history subgroupings, the cross-subgroup pattern of mean impacts reflects the labor supply theory predictions we discussed in section 2.1. Among those with no earnings 7 quarters prior to random assignment, the mean impact is \$157, which is a substantial effect by comparison to the mean control group earnings level of \$762. Among those with low earnings 7 quarters prior to random assignment, the mean impacts are positive but smaller (\$35) and statistically insignificant. Strikingly, the mean impacts for women with high earnings 7 quarters prior to random assignment are negative and sizeable (-\$361). A similar pattern is found using the number of quarters of earnings pre-random assignment: the means are \$212 for zero quarters, \$103 for a low number of quarters, -\$137 for a high number of quarters. The F-test results show that for both measures of earnings history, the mean impacts vary statistically significantly across the three subgroup members. These results, together with the patterns in Figure 1b and online Appendix Figure 1c concerning the control-group earnings distribution location of women in different subgroups, suggest that the earnings history subgroups might do a respectable job of reflecting the pattern of effects that basic labor supply theory predicts.

Finally, the results using presence of AFDC income in the 7th quarter before random assignment show an \$83 mean impact for those with AFDC income in the seventh quarter prior to random assignment, compared to a small negative effect (-\$9) for those with no AFDC income in that quarter. However, neither mean impact is significantly different from zero. More tellingly, the

[10] A very small subset of the sample has missing values for these demographic variables. If we include these observations and form separate mean impacts for the "missing data" subgroups, we still fail to reject that the means are equal across any of these sets of subgroups. Note that in constructing the simulated earnings variables used below, we treat women with missing data as a separate category, so that we use the same sample of women for all comparisons.

F-statistic p-value of 0.33 shows that we cannot reject the null hypothesis of equal mean impacts for these two subgroups. In light of online Appendix Figure 2d, this pattern is not surprising.

All in all, these estimates are notable for their consistency with labor supply predictions, given the subgroup-specific patterns of women's locations across the post-random assignment earnings distribution explored above. Subgroups that have a high likelihood of having zero post-random assignment earnings under control group assignment tend to have larger positive mean earnings impacts. Subgroups whose members are concentrated toward the top of the control group earnings distribution are the ones most likely to have negative mean earnings impacts. And subgroup definitions that are not successful in pinpointing women's locations in the post-random assignment control group earnings distribution tend not to have significant differences in, or uniform patterns of, mean earnings impacts.

3.2 Quantile Treatment Effects by Subgroup

In this section we provide another exploration of the adequacy of the constant-treatment-effects model. In particular, we present quantile treatment effects by subgroups (since they are estimated within subgroup, they are termed "conditional" QTE). We adopt the usual potential outcomes model notation. Let $d_i = 1$ if observation i is assigned to the Jobs First rules facing the treatment group and 0 if i is assigned to the AFDC rules facing the control group. To account for multiple quarters of data per individual, we let $Y_{it}(d)$ be the value of Y that i would have in quarter t if i were assigned to program d (Y in our setting is earnings). The treatment effect for person i in period t is equal to the difference between her period-t outcome when treated and untreated: $\delta_{it} \equiv Y_{it}(1) - Y_{it}(0)$. We calculate sample quantiles, within program assignment d, using the pooled sample of observed earnings values, $\{Y_{it}(d)\}$. Let $F_d(y)$ be the population earnings CDF for women when they are assigned to program group d. The q^{th}-quantile of F_d is the smallest value y such that $F_d(y) \geq q$. Then the q^{th} QTE is the simple difference between the q-quantiles of the treatment and control distributions: $\Delta_q = y_{q1} - y_{q0}$. Finally, we can estimate conditional QTE using the cross-program differences in the sample q-quantiles within the subsample of women who belong to the subgroup in question. In the figures below, we plot the QTE and conditional QTE estimates at 99 centiles, i.e., we plot $(\hat{\Delta}_1, \hat{\Delta}_2, \ldots, \hat{\Delta}_{99})$. Note that the QTE (overall or conditional, within subgroup) are not the same as the distribution of treatment effects for individual persons. The distribution of treatment effects is unidentified without strong assumptions such as constant treatment effects for everyone or rank-invariance (where each person is at the same percentile of each potential outcomes distribution given each counterfactual treatment), which involve features of the joint distribution of potential outcomes. See Abadie, Angrist & Imbens (2002) for a discussion of the usefulness of the QTE despite this, and for more on QTE, see Heckman et al. (1997) or Djebbari & Smith (2008). That said, there is still interest in understanding the QTE themselves, and they are useful, for example, for social welfare function analysis of effects of a program, and they are what is frequently estimated in the literature.

We present conditional QTE within education group categories in Figure 1a. These are estimated analogously to the full sample QTE, but each on a sample that is restricted to one of the various education groups. We then plot these on the same X-axis. The solid line represents estimated conditional QTE for high school graduates, while the dashed line is for high school dropouts. For high school graduates, the conditional QTE are zero through quantile 43, although the treatment leads to a positive extensive margin labor supply response.[11] Higher up the distribution, the conditional QTE are positive, then negative. The conditional QTE plot for high school dropouts differs a bit. Note that some part of this difference is driven by the fact that we have plotted the graphs with a common X-axis of centiles, but the values of the q^{th} centiles are not equal across groups.

For both groups, the heterogeneity in Jobs First's impact across the earnings distribution is unmistakable. The pattern of estimated conditional QTE for the high school graduate subgroup mirrors the pattern for the full sample, which we described above and reported in Figure 3 of Bitler et al. (2006): the conditional QTE are zero at the bottom of the distribution, rise in the middle, and then fall in the upper part of the distribution. These results match the labor supply predictions we discussed above. It is very important to note that each education subgroup's conditional QTE profile shows substantial variation in conditional QTE across quantiles. This finding hints strongly that no constant-treatment-effects model is likely to be adequate to explain the pattern of QTE we observe in the overall sample of women.

In Figure 1b, we plot the conditional QTE among earnings history subgroups (earnings 7 quarters prior to random assignment). These figures show substantial within- and across-group heterogeneity in estimated conditional QTE. The dotted line concerns women with no earnings 7 quarters prior to random assignment and for these women, the estimated conditional QTE are zero for more than the half of the earnings distribution, have large positive effects higher in the earnings distribution, and then return to smaller positive or zero values at the very top of the distribution. A reasonable interpretation is that these women would have lower earnings when assigned to AFDC,

[11] To avoid clutter, we omit confidence intervals from the conditional QTE plots.

so that Jobs First is likely to cause them to increase earnings along the extensive and intensive labor supply margins.

The solid line shows estimated conditional QTE for women with high earnings 7 quarters prior to random assignment. These estimated conditional QTE are zero only for the first 30 percentiles of the distribution and are negative for the rest of the distribution. In general, women with high earnings 7 quarters before random assignment would likely have had relatively high earnings even under assignment to the control group: Table 1 shows that average quarterly earnings are $2,524 for members of this subgroup when they are assigned to the control group—nearly twice the level for those with positive but low earnings in the seventh quarter before random assignment, and more than three times the level for those with no earnings in that quarter. Thus, these women are relatively more likely to be located in the part of the control group earnings distribution for which Jobs First will likely cause earnings reductions due to entry and income effects.[12]

4 The Constant-Treatment-Effects Model and Simulated Earnings QTE

Thus far, we have established that the heterogeneity revealed by the QTE is consistent with labor supply predictions and shown that only non-demographic variables such as earnings history are likely to explain the results. Here we develop a method to assess the adequacy of the constant-treatment-effects-within-subgroup model in explaining the QTE. To do so, we construct an estimate of the earnings distribution that would prevail if Jobs First (i) had heterogeneous mean impacts across subgroups, but (ii) had the same effect on each woman within a given subgroup. A bit of notation will help us be more precise. Let δ^{gt} be the population mean impact for subgroup g in period t (t can be either a particular quarter or the whole time period).[13] Let $Y_{igt}(d)$ be woman i's period-t earnings when she is assigned to program group d, given that she is a member of subgroup g. As above, this woman's actual earnings level when she is assigned to the treatment group is thus $Y_{igt}(1) = Y_{it}(1)$. We define her simulated earnings level when assigned to the treatment group, or "simulated earnings under treatment" to be $Y^*_{igt}(1) = Y_{igt}(0) + \delta^{gt}$. If the constant-treatment-

[12]We find qualitatively similar results when we define subgroups based on the share of positive-earnings quarters over the seven quarters preceding random assignment, shown in online Appendix Figure 3c. On the other hand, conditional QTE based on welfare-use history are more similar across subgroups (see online Appendix Figure 3d), perhaps reflecting the fact that the welfare-use subgroup definition does less well in separating women across different parts of the AFDC earnings distribution than do the two earnings history subgroup definitions (see figures discussed in section 2.3).

[13]While in our setting we estimate δ separately for each subgroup (e.g., high education), in many quasi-experimental settings subgroup mean impacts are obtained by pooling subgroups and interacting the key treatment variable with indicators for subgroups. The ideas here carry over to that alternative specification.

effects model is correct, then for each i, t, and g, simulated earnings must equal actual earnings: $Y^*_{igt}(1) = Y_{it}(1)$. This is the null hypothesis we wish to test.

We construct an estimate of the simulated earnings distribution implied by the constant-treatment-effects model as follows:

1. Calculate the sample mean impact, $\widehat{\delta}^{gt}$, for each subgroup g and period t.

2. For each woman actually assigned to the control group, calculate an estimate of her simulated earnings in period t, given that she is a member of group g, as $\widehat{Y}^*_{igt} = Y_{it}(0) + \widehat{\delta}^{gt}$.

3. The estimated simulated earnings distribution under treatment is then given by $\hat{F}^*_1(y) \equiv n_0^{-1} \sum_{i,g,t} 1[\widehat{Y}^*_{igt} \leq y]$, the empirical distribution of simulated earnings.

Under the null hypothesis that the constant-treatment-effects model is correct, this estimated simulated earnings distribution will converge to the true simulated earnings distribution. This convergence is a consequence of the Glivenko-Cantelli Theorem, as extended to deal with estimated parameters (see, e.g., van der Vaart (1998)).

We use our empirical simulated earnings distribution to evaluate the performance of the constant-treatment-effects model. In so doing, it will be here more convenient to work with quantiles, rather than distribution functions, since we have a clear understanding of the predictions labor supply theory makes for the quantiles of the earnings distribution. We thus calculate the sample quantiles of the estimated simulated earnings distribution $\hat{F}^*_1$, or "sample simulated quantiles" for short; we call these $\widehat{y}^*_{q1}$. Our main measure is then the "simulated QTE under treatment" defined as the difference between the sample simulated quantiles and the sample actual quantiles for women in the control group: $\hat{\Delta}^*_q \equiv \widehat{y}^*_{q1} - \widehat{y}_{q0}$. . If the constant-treatment-effects model captures Jobs First's actual effects on the earnings distribution, then the graph of the set of simulated QTE, $\{\hat{\Delta}^*_q\}^{99}_{q=1}$, should look almost identical to the graph of the actual sample QTE, $\{\hat{\Delta}_q\}^{99}_{q=1}$. Note that it is not only the shape but the magnitude of the effects which matters. Further note that we use the control group's sample earnings quantiles in constructing both the simulated and actual QTE. Thus any differences across the QTE reflect differences in the estimated quantiles of the true treatment group and simulated earnings distribution under treatment. Since $\hat{\Delta}_q = \widehat{y}_{q1} - \widehat{y}_{q0}$, then $\hat{\Delta}^*_q - \hat{\Delta}_q = \widehat{y}^*_{q1} - \widehat{y}_{q1}$. In words, the contrast in QTE is the same at any q as the contrast in earnings quantiles at that q.

We begin in Figure 2a, where we plot the simulated QTE generated by the educational attainment subgroups alongside the actual QTE. In this figure, we construct our estimate of the simulated QTE by assuming that Jobs First's mean impacts are constant across all 7 quarters post-random assignment within each education subgroup ($\widehat{\delta}^{gt} = \widehat{\delta}^{g}$); thus it is labeled "Education: Time invariant." We use the two estimated mean impacts for those with at least a high school degree or no high school degree reported in Table 1, plus the estimated mean impact for a third subgroup of women whose educational attainment level is missing. The figure's dashed line presents the simulated QTE, while the solid line presents the actual QTE.[14] The simulated QTE shown in Figure 2a do a very poor job of replicating the actual QTE. They do not exhibit the substantial range of treatment effects, and their pattern bears no resemblance to the theoretical labor supply predictions. For example, there is essentially no range of negative QTE at the top of the distribution. We found qualitatively similar results for subgroups based on the age of youngest child and marital status; we omit these results for brevity.

One candidate explanation for the poor performance of the simulated QTE in Figure 2a is that they were constructed under the assumption that subgroup treatment effects are constant across time. If mean impacts vary not only across education subgroups, but also across time *within subgroups*, then the simulated QTE in Figure 2a will have been based on a mis-specified model. We therefore consider a second version of the constant-treatment-effects model, which allows mean impacts to vary across both quarter and education subgroup (labeled "Education: Time varying" in Figure 2b). In this more flexible model, which we call the time-varying constant-treatment-effects model, we have 21 estimated mean impacts (three education subgroups for each of seven quarters). Figure 2b shows that results for the time-varying mean impacts only model are hardly better than those for the time-constant one. In Figures 2c and 2d we plot simulated QTE from the time-varying constant-treatment-effects model implemented using subgroups based on earnings history (defined using earnings seven quarters prior to random assignment) and welfare history. Simulated QTE based on these subgroup definitions also do poorly in replicating the actual sample QTE.

One striking difference between the actual and simulated QTE involves the mass point at zero earnings. The percentage of person-quarters with zero earnings is 55 percent in the control group and 48 percent in the (actual) treatment group. As a result, sample actual QTE equal zero for all $q \leq 48$. The simulated QTE do not have this feature. The reason why is simple. Estimated mean impacts are nonzero for all subgroups (this is true regardless of whether we use the time-constant or

[14] The full sample QTE included here are directly comparable to Figure 3 in Bitler et al. (2006). The sole difference is that there we adjusted for observables using inverse propensity score weighting but we do not do so here; this adjustment does not substantively affect the results.

time-varying mean impacts). When we construct "simulated earnings under treatment" for the 55 percent of quarterly control group observations that have zero earnings, we therefore add something nonzero to zero. The result is necessarily nonzero, so that the simulated earnings distribution under treatment has no mass at zero. This key problem with focusing only on mean impacts when both the treatment and control groups have mass points is not new (e.g., Heckman et al. (1997)) and leads to interest in evaluating impacts on the extensive margin. It suggests that the constant-treatment-effects model must be modified to allow for mass points at zero if it is to reproduce the Jobs First earnings distribution.

To account for the mass points at zero, we introduce a third version of the constant-treatment-effects model. In this version of the model, we calculate simulated earnings under treatment differently from the first two versions. First, define δ^{gt+} as the treatment-control difference in mean earnings conditional on positive earnings within subgroup g and quarter t. That is, $\delta^{gt+} \equiv E[Y_{igt}(1)|Y_{igt}(1) > 0] - E[Y_{igt}(0)|Y_{igt}(0) > 0]$, and let $\hat{\delta}^{gt+}$ be the sample analog of δ^{gt+}. Second, let p_{0gt} be the probability that a subgroup-g woman would have zero earnings in quarter t when assigned to the control group, and define p_{1gt} analogously for treatment group assignment; and let $\widehat{p}_{0gt}$ and $\widehat{p}_{1gt}$ be the sample analogs. Using this, we calculate estimated simulated earnings under treatment as follows:

1. Calculate $\widehat{\delta}^{gt+}$, $\widehat{p}_{0gt}$, and $\widehat{p}_{1gt}$.

2. For each woman actually assigned to the control group, set simulated earnings in quarter t as follows: $Y^*_{igt}(1) \equiv (1 - Z_{it})[Y_{igt}(0) + \widehat{\delta}^{gt+}]$, where $Z_{it} \equiv 1[Y_{it}(0) = 0]$. This is 0 if $Y_{igt}(0) = 0$ but is $Y_{igt}(0) + \widehat{\delta}^{gt+}$ if $Y_{igt}(0) \neq 0$.

3. Next, reweight each woman in the control group to ensure that the share of zero earners is the same for the treatment group and the simulated earnings distribution under treatment for those in the control group. This weight for control group woman i (who is in subgroup g) in quarter t is $w_{it} \equiv Z_{it} \cdot \widehat{p}_{1gt}/\widehat{p}_{0gt} + (1 - Z_{it}) \cdot (1 - \widehat{p}_{1gt})/(1 - \widehat{p}_{0gt})$.

4. The estimated simulated earnings distribution is then $\hat{F}^*_1(y) \equiv n_0^{-1} \sum_{i,g,t} w_{it} \cdot 1[\widehat{Y}^*_{igt} \leq y]$.

By construction, the share $\widehat{p}_{1gt}$ of subgroup-g, quarter-t observations in the control group in this third constant-treatment-effects model will have simulated earnings equal to zero, as there are $\widehat{p}_{0gt}$ such women, each with a weight of $\widehat{p}_{1gt}/\widehat{p}_{0gt}$. Consequently, the overall share of zero-earnings observations will be the same in the actual and simulated treatment group earnings distributions.

Thus, our third constant-treatment-effects model effectively removes the share of zeros as a reason for the simulated earnings distribution to fail to mimic the actual treatment group's earnings distribution. If this "participation-adjusted" constant-treatment-effects model is correct, then, the conditional actual and simulated earnings distributions must be the same, where the conditioning is on being in the set of person-quarters with positive earnings. We return to this point below when we discuss formal testing of our third model. For the moment, we observe that under the null hypothesis that the participation-adjusted constant-treatment-effects model is correct, the actual and simulated QTE must be the same up to sampling variation.[15]

We report actual QTE and simulated QTE based on the participation-adjusted constant-treatment-effects model in Figure 3. As in previous graphs, we plot the actual QTE using a solid line and the simulated QTE using a dashed line. Figure 3a plots simulated QTE for subgroups defined by education; Figure 3b plots simulated QTE for subgroups defined using earnings in the seventh quarter before random assignment; Figure 3c plots simulated QTE for subgroups defined using the share of pre-random assignment quarters with positive earnings; and Figure 3d plots simulated QTE for subgroups defined using the presence of welfare income in the seventh quarter prior to random assignment.

Overall, these graphs show a much closer resemblance between the simulated and actual QTE than do those presented in Figure 2. But there remain some notable differences. First, there are negative simulated QTE at the <u>bottom</u> of the simulated distributions. These effects occur because some women in the control group have positive but very low earnings and are members of subgroups with negative $\widehat{\delta}^{gt+}$. As a result, these women's simulated treatment group earnings estimates are negative, so they wind up at the very bottom of the simulated treatment group earnings distribution. With the exception of this minor difference, both the actual and simulated QTE equal zero for nearly all of the first 48 quantiles in all panels of Figure 3. Of course, this result follows somewhat mechanically from the participation adjustment (we have set the same share of individuals to be nonworkers in both group).

Over quantiles 50–80 or so, the simulated QTE do a reasonably good job of replicating the general shape of the actual QTE. However, they fail to achieve the amplitude of the actual QTE, which suggests that the constant-treatment-effects model fails to capture some important within-subgroup/within-quarter variation. Moreover, in every case the simulated QTE fail to fully replicate the negative QTE at the <u>top</u> of the earnings distribution. This result is a potentially serious mark

[15] Note also that this process could be done interchanging the treatment and control groups.

against even the participation-adjusted constant-treatment-effects model.

Notably, the subgroups differ in their ability to capture this important result predicted by labor supply theory. The simulated QTE using demographic variables—education (in Figure 3a); age of youngest child (in online Appendix Figure 4a), and marital status (in online Appendix Figure 4b)—show little evidence of negative simulated QTE at the top of the earnings distribution. By contrast, the simulated QTE using earnings history subgroups show more evidence of negative simulated QTE.

5 Testing

The results and discussion above have focused on point estimates and do not address the issue of whether we can statistically reject the null hypothesis that the participation-adjusted constant-treatment-effects model is correct. Perhaps the simulated QTE in Figure 3 differ from the actual ones only because of sampling variation. We thus turn to formal tests of the participation-adjusted constant-treatment-effects model. We do not bother testing the two constant-treatment-effects models that do not account for the mass points at zero earnings (e.g., Figure 2). As Heckman et al. (1997) have pointed out, such models cannot be correct when there are differing mass points (e.g. share with positive earnings) in the two groups. Thus, we regard these two models as already formally rejected.

5.1 Null Hypotheses

We test the null hypotheses that treatment effects are constant for those with positive earnings, within each subgroup g and time period t. That is, within group g, we are testing the null H_{0g} : $F(y|G = g, T = 1, y > 0) = F(y - \alpha|G = g, T = 0, y > 0)$, where T is the treatment indicator, α, the subgroup specific treatment effect is a nuisance parameter, and g is a specific value of a subgroup. Note that a joint test of these across a mutually exclusive set of subgroupings rejects if any one rejects. We then consider whether this null is rejected for any of the subgroups within a family, using a Bonferroni adjustment to deal with the multiple testing issue.

5.2 Dealing with Estimated Parameters

There are many tests developed in the literature for testing equality of distributions. Unfortunately, there are two complications in applying existing tests to our setting. First, our estimate for the empirical simulated earnings distribution depends on estimated nuisance parameters (the vector of estimated subgroup-specific treatment-control differences in mean earnings). The second issue is that our data cannot be treated as *iid*. Women are randomly assigned to the treatment and control groups, but we include seven quarterly earnings observations for each woman, so there is likely to be within-person dependence in earnings across quarters. In the absence of a single test statistic with appropriate critical values that incorporates estimation of multiple nuisance parameters and non-*iid* data, we instead test the null of constant effects *within* each of our subgroups and time periods. Within time period, the data are *iid*, leaving only the estimated parameters problem. We are therefore able to make use of a result in Praestgaard (1995) which is also sufficient to solve the issue of estimated parameters. He shows that the permutation-based critical values for the Kolmogorov-Smirnov test statistic are asymptotically valid even in the presence of estimated parameters, provided a technical condition is satisfied. We have verified that this condition is satisfied in our context (available upon request). A recent paper by Ding et al. (Forthcoming) also considers a setting very much like ours. We are applying what Ding et al. (Forthcoming) term the Fisher-randomization test using the plug-in method. We present the results of these permutation tests carried out within each quarter and subgroup.[16]

We start by testing whether the simulated earnings model is sufficient by allowing for time-varying mean differences. If the constant-treatment-effects model is correct, it is sufficient to test that the actual treatment group earnings distribution and simulated earnings distribution (conditional on positive earnings) are the same within each subgroup and quarter. Thus to reject the null hypothesis that all of these distributions are the same for each subgroup and time period within a *family of subgroups* (e.g., education or 7 quarters pre-random assignment earnings), it is sufficient that we reject the null of equal actual and simulated treatment distributions for *at least* one of the subgroup by quarter combinations. Such an approach, however, runs into a multiple testing issue unless thinking about the full sample (and therefore only estimating one treatment effect and comparing one set of distributions).

5.3 Family Wise Error Rate Control and Permutation Statistics

The approach we use which controls the Family Wise Error Rate[17] is the Bonferroni correction, which adjusts the p-value for multiple testing and is obtained by multiplying the unadjusted p-value by the number of tests (here we multiply by the number of subgroups). The Bonferroni correction

[16]Koenker & Xiao (2002) offer a solution to the incidental parameters problem for testing this location-scale model. Chernozhukov & Fernandez-Val (2005) also prove the validity of a subsampling-based test for distributional equality and Linton, Maasoumi & Whang (2005) have a framework for stochastic dominance tests that allows for estimated parameters. We have also implemented tests of constant-treatment-effects within subgroups and time periods using the approach of Chernozhukov & Fernandez-Val (2005) and Linton et al. (2005), adjusting for the multiple testing as above. Either of latter these alternative approaches leads to similar conclusions to we show here.

[17]The family wise error rate is the probability of making one type 1 error or falsely rejecting one of the family of nulls.

for p-values has limitations, and in particular is known to be conservative.

The KS statistic, for a pair of distributions $\widehat{F}^1$ and $\widehat{F}^0$, is given by: $KS = \sup |\widehat{F}^1 - \widehat{F}^0|$. We calculate the KS statistic for each subgroup and period using the simulated and actual treatment group earnings distribution for that subgroup and period. We then permute the data many times with the null hypothesis imposed, calculating a permutation KS statistic on each iteration. The critical values for our original KS statistic are then based on the permutation distribution. To impose the null hypothesis on each permutation iteration, we do the following procedure. First, we pool the treatment and control groups. Then, we create a random treatment indicator that separates this pooled distribution into two samples, the first having the same number of observations as the true treatment group and the second having the same number of observations as the control group. Using this random treatment indicator, we create simulated treatment group earnings by adding $\widehat{\delta}^{gt+}$ to the "random" control group. Because this treatment indicator is random, the KS statistic for this permutation will be zero up to sampling variation. Next, we calculate the permuted value of the KS statistic 2999 times. Then, we sort the resulting KS statistics. The unadjusted p-value for the null for this subgroup and period is calculated as the rank of the original KS statistic in the overall permutation distribution, divided by 3000 (2999 permutation replicates plus the actual data).[18]

Next, we account for multiple testing within each subgroup and time period. We compare the Bonferroni-adjusted p-value for a family of subgroup tests to the desired significance level. For example, when we use education subgroupings, there are 7 quarters by 3 education groups = 21 test statistics. With 21 test statistics, the Bonferroni-adjusted p-value is obtained by multiplying the unadjusted p-value by 21. If *any* of the 21 education subgroupings has an adjusted p-value below the desired significant level (e.g., 0.05), then we reject the null hypothesis for education.

5.4 Empirical Test Results

Table 2 reports the results for these tests. Each row contains the results from tests for a particular set or family of subgroups. The first column reports the number of test statistics involved (7 for the overall pooled results labeled "Full sample", 21 for the education-quarters). Column 2 reports the smallest unadjusted p-value for the family of tests. Without the necessary adjustment for the multiplicity of tests, one would conclude that the constant treatment effect within subgroup models fail miserably. Columns 3–5 report the results after adjusting for the multiplicity of tests within the families of subgroups using the Bonferroni correction. Column 3 reports the number of

[18] If the original KS statistic is the largest (rank 1), then we can only bound the p-value as $< 1/3000 = 0.0033$.

the permutation tests for each subgrouping that reject the null of equality of the within subgroup-quarter distributions after a Bonferroni adjustment at the 10% level while Column 4 reports the number of adjusted tests that reject at the 5% level. Column 5 reports the minimum Bonferroni adjusted p-value (the level at which the test of equality can be rejected for the test with the lowest p-value).

The first row of Table 2 allows variation only in time period. The results show that 4 of 7 of the full sample permutation tests reject at the 5% level after Bonferroni adjustment, while 5 of 7 reject at the 10% level. Thus, when we do not use any earnings or welfare use history to form simulated earnings and rely on demographics, equality of the actual earnings distribution and the simulated distribution of treated earnings is strongly rejected. The next rows present tests for demographic groups; education, age of youngest child, and marital status; each of which have 21 tests within the "family". For education, one of twenty-one tests rejects at the 5% level after adjustment while 3 of the tests reject at the 10% level. For the 21 subgroup-quarter tests for age of youngest child and marital status, 2 of the 21 tests reject at the 5% level after adjustment. Thus, we easily reject the null of the constant-treatment-effects model for the demographic variables.

Next consider the results when we create subgroups based on a woman's earnings in the 7th quarter before random assignment. Of the 21 subgroup-quarters tests, 1 rejects the null at the 5% level and 2 reject at the 10% level, after adjustment. When we instead use groups based on the number of quarters with any earnings before random assignment, we also reject once at the 5% level after adjustment. Finally, groups based on welfare history reject at the 5% level 3 of 14 times.

Overall, Table 2 shows that we can resoundingly reject the null of equality of distributions within each demographic and earnings/welfare history by time subgroups, even using the relatively conservative Bonferroni adjustment for our permutation test approach. In the remainder of the table we report the results of tests for interactions of the various subgroups (e.g., education group by number of quarters of positive earnings pre-random assignment by quarter after random assignment). Here, the number of groups is much larger, and the Bonferroni correction potentially more restrictive. For example, for the interaction of education with age of youngest child, there are 49 tests. Some of the subgroupings yield as many as 63 tests. Yet despite this, of the 15 "families" of three-way groupings in the table, we reject the null for 6 at the 5% level and 9 at the 10% level, using the adjusted p-values from the permutation tests. Further, as suggested by Figures 2 and 3, the bulk of the subgroupings where we fail to reject include the pre-random assignment earnings measures. Fully 5 of the 6 subgroupings for which we fail to reject at the 10% level, and 5 of 9 at the 5% level include the earnings history measures among the 3-way interactions.

Given how conservative the Bonferroni correction can be, the evidence we have shown in Tables 2 resoundingly rejects the constant mean impacts within subgroup model in this setting, even with participation adjustment and time-varying mean impacts. And note that when we stay within the realm of 2-way interactions between subgroup and time which is the level of much testing of heterogeneity in applied work, we always reject the constant treatment effects within subgroup model.

5.5 Discussion

We note here that we have demonstrated for a specific example about a experiment with strong theoretical predictions about heterogeneity that the type of typical mean impacts or mean impacts within subgroups models common in applied work fail to capture this heterogeneity. Yet this heterogeneity which is consistent with these models is apparent in QTE estimates. We have conducted formal tests of the sufficiency of these constant treatment effects within subgroups models, and they fail, even using conservative adjustments for multiple testing. We note that our testing approach may not be the most useful for the applied researchers we hope to reach, but think this substantive point is important. We aim researchers more interested in the technical issues to the econometrics and statistics literature (e.g., Ding et al. (Forthcoming)).

6 Conclusion

A common approach to explore treatment effect heterogeneity is to estimate mean impacts by subgroups (e.g., Angrist (2004)). These subgroup-mean impacts may come from estimating the mean impact for each subsample of interest (e.g., a fully stratified model) or in a pooled model by adding interactions of the treatment with subgroup indicators (or other parametric models). Another approach is to examine heterogeneity using quantile treatment effects (QTE), which we previously used to examine the effects of welfare reform on earnings (Bitler et al. (2006)). In that setting, we found the QTE revealed striking heterogeneity consistent with labor supply theory. Here we return to that data and setting and explore whether estimating mean impacts by subgroup reveals the observed heterogeneity found in the QTE. The Jobs First experiment and data that we use here are well suited to examine this issue due to the randomization, the substantial changes to labor supply incentives introduced by the treatment, and the access provided to extensive data on pre-random assignment earnings and program participation.

We construct an estimate of the "simulated earnings distribution under treatment" which is the earnings distribution for the control group that would result under the assumption that all heterogeneity is contained in constant treatment effects within subgroup that are differential across subgroups. Under the null of the constant treatment effects within subgroup model, the treatment group distribution and the simulated earnings distribution under treatment are the same. We then evaluate the performance of the constant-treatment-effects model by comparing earnings QTE estimated using the actual treatment and control earnings distribution to the "simulated QTE" estimated using the simulated earnings under treatment and the actual control group distribution. The graphical comparison of the actual and simulated QTE shows that the constant treatment effects within subgroup model does a poor job in capturing the heterogeneity evident using QTE. This is true when we estimate effects across the whole time period within subgroup; when we pool the time periods and estimate one mean effect per subgroup for the entire 21 months of data; when we allow the mean effects to vary across time; or even we allow the heterogeneity to be such that the share of non-participants in the labor market is the same within the treatment and control groups. This finding is confirmed by statistical tests that reject the null hypothesis of equality between the actual treatment earnings and the earnings under treatment simulated under the constant-treatment-effects models.

Importantly we find that not all subgroupings fare equally well or poorly in generating simulated earnings under treatment under the mean impacts only model. We find that groups defined based on earnings history (pre-treatment) do considerably better than groups based on demographics or welfare history. Taken together, these results suggest that even estimating subgroup-specific mean effects for a wide range of subgroups may not reveal all important treatment effect heterogeneity. This is merely one example of such treatment effect heterogeneity, but should raise concerns about relying on mean impact analysis when heterogeneity is of interest.

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Table 1: Mean differences in earnings between treatments and controls by subgroups

Subgroup	Mean $T-C$ Difference	95% CI	Control group Mean	N_C/share in C group	N_T/share in T group
All	34	[-58, 126]	1139	16,849	16,772
By education of case head:					
No high school degree/GED	105	[-16, 225]	662	0.31	0.33
At least high school/GED	42	[-77, 161]	1350	0.62	0.61
F-statistic [p-value]	0.52	[0.47]			
By whether youngest child is ≤ 5:					
Youngest child ≤ 5	48	[-59, 156]	1073	0.63	0.62
Youngest child ≥ 6	88	[-69, 244]	1183	0.33	0.35
F-statistic [p-value]	0.17	[0.68]			
By number of children in case:					
2 or more	101	[-25, 228]	1071	0.47	0.48
1 or pregnant	30	[-95, 154]	1148	0.49	0.48
F-statistic [p-value]	0.63	[0.43]			
By marital status of case head:					
Never married	36	[-65, 137]	1064	0.63	0.62
Ever married	88	[-86, 262]	1224	0.32	0.33
F-statistic [p-value]	0.26	[0.61]			
By level of earnings 7^{th} quarter before random assignment:					
Zero	157	[70, 243]	762	0.67	0.70
Low	35	[-157 , 227]	1332	0.16	0.15
High	-361	[-711, -12]	2524	0.16	0.15
F-statistic [p-value]	4.37	[0.01]			
By number of quarters with any earnings before random assignment:					
Zero	212	[122, 302]	450	0.40	0.44
Low	103	[-36 , 242]	1090	0.32	0.32
High	-137	[-367, 93]	2180	0.28	0.25
F-statistic [p-value]	4.13	[0.02]			
By whether on AFDC 7^{th} quarter before random assignment:					
Yes	83	[-33, 200]	968	0.53	0.55
No	-9	[-154, 136]	1330	0.47	0.4
F-statistic [p-value]	0.95	[0.33]			

This table reports treatment-control differences in quarterly earnings in the full sample and various subgroups during the first 7 quarters after random assignment for the sample of 4803 women. Column 1 shows the mean difference in earnings by subgroup and column 2 the 95 percent CI for this mean difference. For the subgroup member in each row, Columns 3—5 contain the control group mean and the number of observations in the treatment and control groups (top panel) or share of observations in that subgroup member for the treatment and control groups (other panels). At the bottom of each panel, we report the F-statistic and p-value for the test that the mean treatment effects are the same across the subgroups within the panel. The reported F-statistics exclude from the test the coefficients on variables for a small number of observations missing some of the demographic characteristics (not reported here). The F-statistics [p-values] for tests including the missing data categories are 0.84 [0.4320] for education, 1.74 [0.1755] for age of youngest child being less than 5, 1.99 [0.1370] for the number of children in the case, and 0.85 [0.4281] for marital status of the case head. Statistics allow for arbitrary correlation within woman.

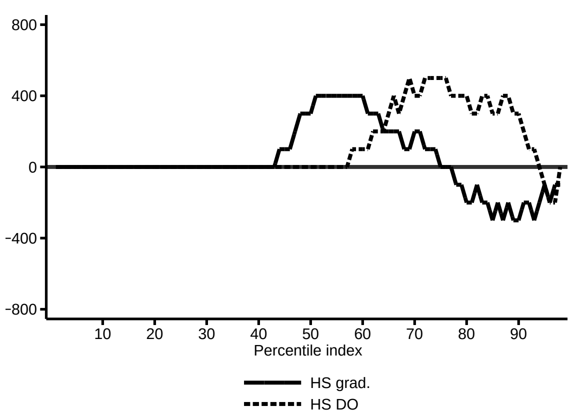
Table 2: Permutation tests of equality of actual and simulated treatment group distributions, time-varying mean treatment effects by subgroup with participation adjustment

		Unadj.	Bonferroni adjusted		
Subgroup	# of Tests	Minimum p-val	# reject at 10%	# reject at 5%	Minimum p-val
Full sample	7	<0.0003***	5	4	<0.0023***
Education	21	0.0007***	3	1	0.0140**
Age of youngest child	21	<0.0003***	2	2	<0.0070***
Marital status	21	0.0003***	4	2	0.0070***
Earnings level 7^{th} Q pre-random assignment	21	0.0017***	2	1	0.0350**
# pre-random assignment Q with earnings	21	0.0007***	1	1	0.0140**
Welfare receipt 7^{th} Q pre-random assignment	14	<0.0003***	3	3	<0.0047***
Education subgroups interacted with:					
Age of youngest child	49	0.0020***	1	0	0.0980*
Marital status	35	<0.0003***	3	3	<0.0117**
Earnings level 7^{th} Q pre-random assignment	63	0.0003***	1	1	0.0210**
# pre-random assignment Q with earnings	63	0.0050***	0	0	0.3150
Welfare receipt 7^{th} Q pre-random assignment	42	0.0033***	0	0	0.1400
Age of youngest child subgroup interacted with:					
Marital status	35	0.0027***	1	0	0.0933*
Earnings level 7^{th} Q pre-random assignment	63	0.0033***	0	0	0.2100
# pre-random assignment Q with earnings	49	0.0010***	1	1	0.0490**
Welfare receipt 7^{th} Q pre-random assignment	42	0.0013***	1	0	0.0560*
Marital status subgroup interacted with:					
Earnings level 7^{th} Q pre-random assignment	63	0.0003***	1	1	0.0210**
# pre-random assignment Q with earnings	63	0.0050***	0	0	0.3150
Welfare receipt 7^{th} Q pre-random assignment	42	<0.0003***	1	1	<0.0140**
Earnings level 7^{th} Q pre-random assignment subgroup interacted with:					
# pre-random assignment Q with earnings	49	0.0003***	0	0	0.1633
Welfare receipt 7^{th} Q pre-random assignment	42	<0.0003***	1	1	<0.0140**
# of quarters any earnings pre-random assignment subgroup interacted with:					
Welfare receipt 7^{th} Q pre-random assignment	42	0.0060***	0	0	0.2520

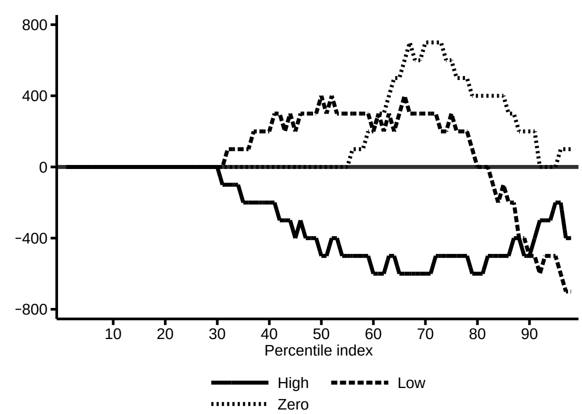
Notes: Table reports the result of tests of the nulls that simulated and actual CDFs are equal for those with positive earnings within each subgroup and quarter for different set of subgroups. p-values adjusted using the Bonferroni adjustment, and represent the upper bound of the p-value. Thus the adjusted p-values for the full sample subgroup where seven tests are carried out (1 for each quarter) are the unadjusted p-values times 7. Test results reported in columns 2–5. are the results of permutation tests of the KS statistic, following Praestgaard (1995). Each family of tests for a number of mutually exclusive subgroups tests the null that the CDF for earnings for workers in the treatment group is equal to the distribution obtained from adding time varying conditional mean earnings for those working in the control group to the control group values, within each period and subgroup. The first column reports the number of tests for each family of subgroups. The second column reports the smallest *unadjusted* p-value for the family of tests. The third and fourth columns report the number of tests where the Bonferroni adjusted p-value for the permutation KS test rejects at the 10% level and 5% level, respectively. The fifth column reports the smallest of the Bonferroni adjusted p-values for this family of tests. The values in column 5 have *** if the smallest *unadjusted* p-value allows rejection at the 1 percent level, ** if the smallest adjusted p-value allows rejection at the 5 percent level, and * if the smallest adjusted p-value allows rejection at the 10 percent level. The values in column 5 have *** if the smallest adjusted p-value allows rejection at the 1 percent level, ** if the smallest adjusted p-value allows rejection at the 5 percent level, and * if the smallest adjusted p-value allows rejection at the 10 percent level. For more details, see text.

Figure 1: Conditional QTE, using education and earnings prior to random assignment to define subgroups

(a) Education: Subgroup QTE



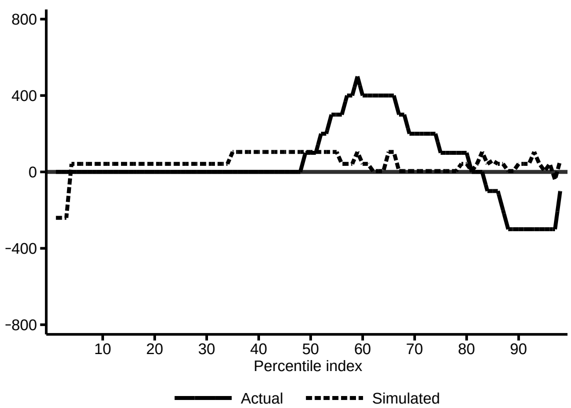
(b) Earnings 7Q pre-random assignment: Subgroup QTE



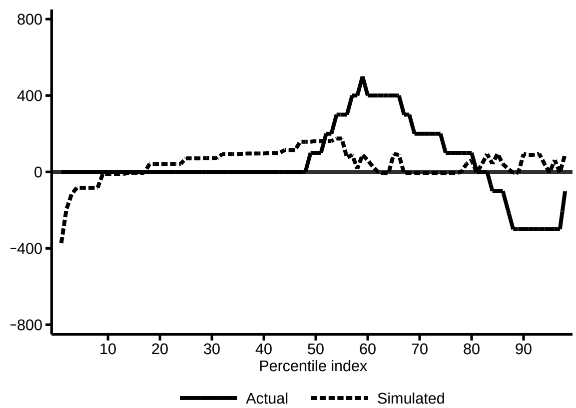
Notes: Figures for subgroups defined by education (left) and subgroups defined by earnings the 7th quarter before random assignment (right). Graphs show conditional QTE within each education subgroup (left graph) or earnings during the 7th quarter before random assignment (right graph).

Figure 2: Actual and simulated QTE, subgroups based on education (top graphs): and earnings and welfare use before random assignment (bottom graphs)

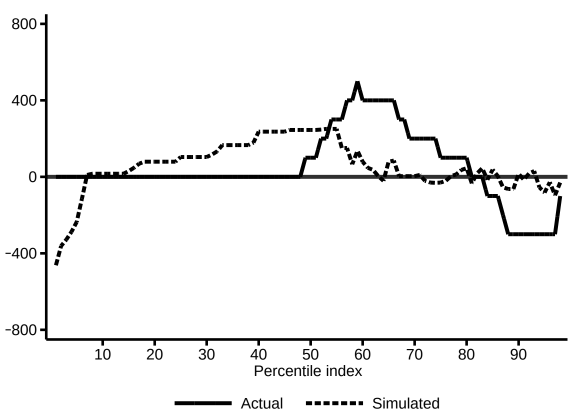
(a) Education: Time invariant



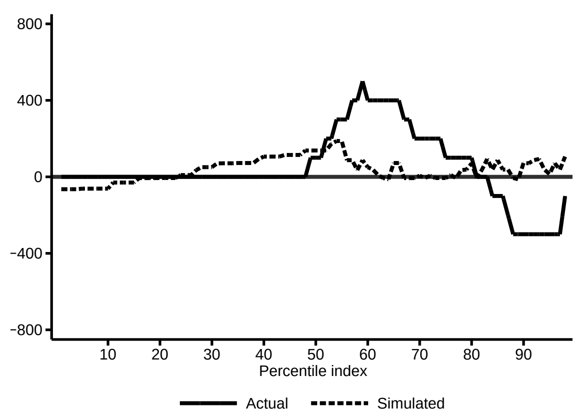
(b) Education: Time varying



(c) Earnings 7Q pre-random assignment: Time varying



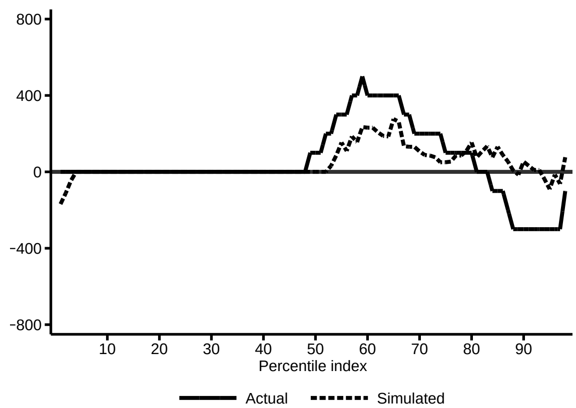
(d) Any welfare 7Q pre-random assignment: Time varying



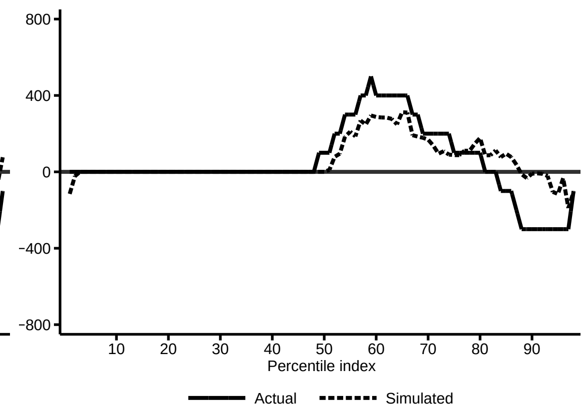
Notes: In each figure, the solid line plots the actual QTE, and the dashed line plots the simulated QTE. In all figures, we allow for either time invariant or time varying program effects on mean earnings within subgroup. -In the top graphs, subgroups are based on education. In the bottom graphs, subgroups are based on earnings 7 quarters before random assignment (left) or welfare history before random assignment (right). In the left top figure, simulated earnings are calculated under the constraint that subgroup-specific treatment effects are constant across quarter. In the right top and bottom two figures, we allow the subgroup-specific treatment effects to vary across quarters.

Figure 3: Actual and simulated QTE with participation adjustment and time varying means, various subgroups

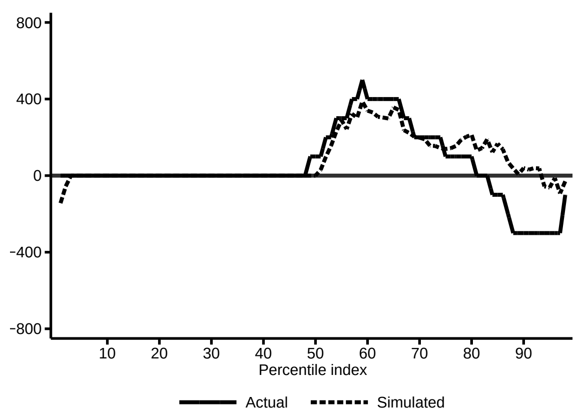
(a) Education



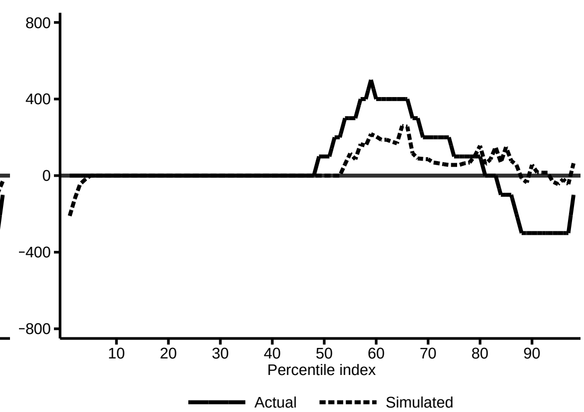
(b) Earnings 7Q pre-random assignment



(c) # pre-random assignment Q with earnings



(d) Any welfare 7Q pre-random assignment



Notes: In each figure, the solid line plots the actual QTE and the dashed line plots the simulated QTE. In all figures, we allow for time-varying program effects on conditional mean earnings within subgroup. Data for simulated QTE constrained to have the share of non-participants equal and the mean treatment-control difference in earnings the same. In the top left graph, subgroups are based on education. In the top right graph, subgroups are based on the level of earnings 7 quarters pre-random assignment. In the bottom left graph, subgroups are based on the number of quarters pre-random assignment with positive earnings. In the bottom right graph, subgroups are based on welfare history pre-random assignment.