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DISTRIBUTIONAL IMPACTS OF THE SELF-SUFFICIENCY PROJECT

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ABSTRACT

A large literature has been concerned with the impacts of recent welfare reforms on income, earnings, transfers, and labor-force attachment. While one strand of this literature relies on observational studies conducted with large survey-sample data sets, a second makes use of data generated by experimental evaluations of changes to means-tested programs. Much of the overall literature has focused on mean impacts. In this paper, we use random-assignment experimental data from Canada's Self-Sufficiency Project (SSP) to look at impacts of this unique reform on the distributions of income, earnings, and transfers. SSP offered members of the treatment group a generous subsidy for working full time. Quantile treatment effect (QTE) estimates show there was considerable heterogeneity in the impacts of SSP on the distributions of earnings, transfers, and total income; heterogeneity that would be missed by looking only at average treatment effects. Moreover, these heterogeneous impacts are consistent with the predictions of labor supply theory. During the period when the subsidy is available, the SSP impact on the earnings distribution is zero for the bottom half of the distribution. The SSP earnings distribution is higher for much of the upper third of the distribution except at the very top, where the earnings distribution is the same under either program or possibly lower under SSP. Further, during the period when SSP receipt was possible, the impacts on the distributions of transfer payments (IA plus the subsidy) and total income (earnings plus transfers) are also different at different points of the distribution. In particular, positive impacts on the transfer distribution are concentrated at the lower end of the transfer distribution while positive impacts on the income distribution are concentrated in the upper end of the income distribution. Impacts of SSP on these distributions were essentially zero after the subsidy was no longer available.

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I. Introduction

As we move into the 21st century, government assistance for poor families has undergone major reform across Europe, Canada and the United States. In some cases, changes have taken place within traditional government welfare programs (e.g., the SSP demonstration in Canada and welfare reform in the United States) to reduce the negative work incentives embodied in programs that taxed away welfare benefits at a high rate with each extra dollar in earnings. In other cases, new programs have been added or expanded providing in-work subsidies for low income workers and families. Prominent examples of these policies are the United States' Earned Income Tax Credit and the United Kingdom's Working Family Tax Credit. Further, a recent report identifies nine OECD countries (including the United States and United Kingdom) that offer in-work subsidies (Owens 2005). A common feature of these program changes is expanding the financial gains to working. In so doing, the goal of the policy changes is to 'make work pay' and increase self sufficiency.

In this dynamic policy environment, a large literature has developed.[1] A common feature of this literature is the focus on the mean impacts of the policy of interest. In this paper, we make an important contribution to the literature by using a simple nonparametric estimator — quantile treatment effects, or QTEs — to estimate the impact of an important policy change on the distribution of earnings and income outcomes.

Many governments have conducted randomized experiments to assess the impacts of generous financial incentives for work on welfare and employment among cash assistance recipients. One such experiment, the Self-Sufficiency Project (SSP), has received an enormous

[1] For example, in the United States, Grogger & Karoly (2005) provide a summary of welfare reform and Hotz & Scholz (2003) and Eissa & Hoynes (Forthcoming) provide reviews of the Earned Income Tax Credit. Blundell & Hoynes (2004) compare the United States' and United Kingdom's in-work policies, and evaluations of the Working Family Tax Credit appear in Blundell et al., (2005), Brewer et al., (2005), Francesconi & van der Klauw (2004), Gregg & Harkness (2003), and Leigh (2004). Bargain & Orsini (2004) and Smith et al., (2003) use cross-country data to evaluate European reforms.

amount of attention due to its ability to increase earnings and income. For example, findings about SSP were cited in numerous U.K. government documents as inspiration for reforms instituted by the Labour government in the late 1990s. SSP was sponsored by Human Resources Development Canada and conducted by the Social Research Demonstration Corporation. SSP was designed to test the impact of a generous earnings subsidy for full-time work on long-term welfare participants.

Between 1992 and 1995, SSP randomly assigned a group of single-parent recipients and applicants for income assistance (IA) in two provinces, New Brunswick and British Columbia, to treatment and control groups. Control group members faced the rules of IA in their home province. Treatment group members who had been on IA for 12 of the previous 13 months were eligible for a generous earnings supplement if they could find full time work (at least 30 hours a week) at or above the minimum wage within a year. The earnings supplement was one-half the difference between their earnings and a benchmark earnings level ($30,000 in New Brunswick and $37,000 in British Columbia) and was available for 36 months. Persons receiving the supplement had to forego their IA payments, although if they gave up the supplement, they could receive IA if they were otherwise eligible.

Several final reports (Michalopoulos et al., 2002 and Ford et al., 2003), and a large number of research papers (e.g., Blank et al. , 2000; Card & Hyslop, 2004; Card et al., 2001; Connolly & Gottschalk, 2003; Foley, 2004; Foley & Schwartz 2002; Harknett & Gennetian, 2003; Kamionka & Lacroix, 2003; Lise et al., 2005; Michalopoulos et al., 2005; Zabel et al., 2004) have looked at the overall impacts of the SSP experiment on income, earnings, labor force attachment, unemployment durations, wages, wage growth, job choice, and marriage. These papers find that the program increased employment, earnings, and income considerably during the years when the supplement was available, while having little or no impacts after the supplement was no longer available.

Existing literature focuses on SSP's mean impacts, in the full sample and in demographic subgroups. Mean impacts, however, may conceal heterogeneous impacts across the distribution of earnings and income. For example, SSP generates an increase in total income and net wages at 30 hours of work and above. The income gains are substantial — most families had after-tax annual incomes $3,000–$7,000 higher with SSP than they would have had if they had worked the same number of hours under IA (Michalopoulos et al., 2002). Static labor supply theory predicts that this increase in nonlabor income would lead some recipients to increase work and leave IA, thereby increasing earnings and income. On the other hand, workers who would work full-time under IA-only assignment receive a windfall payment under SSP, which could lead to much smaller (and possibly negative) earnings effects based on standard labor supply analysis.

In this paper, we move beyond mean impacts and examine the impacts of SSP on the distribution of earnings, transfers, and income using QTE estimation.[2] We estimate the QTEs very simply as the difference in outcomes at various quantiles of the treatment (SSP) and control (IA-only) group distributions. Thus, QTEs tell us how the earnings *distribution* changes when we assign SSP treatment randomly. The QTE is a simple nonparametric estimator that requires only that the treatment is randomly assigned. As we discuss in more detail below, QTEs identify only the impact of treatment on the distribution; this impact is distinct from, and in general not equal to, the distribution of treatment effects (as well as other interesting estimates, such as the treatment effect on people whose control-group outcome would have been the median, etc.).

[2] A host of authors have used QTEs (e.g., Heckman, Smith, & Clements, 1997; Firpo, 2004). Friedlander & Robins (1997) estimate QTEs to evaluate the impact of employment training programs in early welfare-reform experiments. Bitler, Gelbach, & Hoynes (Forthcoming) examine the impact of a welfare-reform experiment in Connecticut in the mid-1990s on AFDC and Food Stamp payments, earnings, and total income.

In this paper, we contribute to the literature on the impacts of financial incentives on labor supply and to the existing experimental literature including our own earlier work (Bitler et al., Forthcoming) in three ways. First, SSP is an important policy that has received a great deal of attention in the international policy arena, influencing adoption of new policies and experiments in Europe. The minimum work requirement and relatively generous benefits make the policy unique and have important implications for the impacts of the policy. For example, SSP moved long term recipients into the labor force much more quickly than they would have under Income Assistance while nearly paying for itself, costing a scant $4000 more per recipient over a 5-year period than Income Assistance (Michalopoulos et al., 2002). Our distributional analysis adds to our understanding of SSP in a fundamental way.[3] Second, the SSP experiment is unusual in that it provides data on wages and hours thereby allowing for a much richer analysis of the labor supply implications relative to our earlier work (Bitler et al., Forthcoming). Finally, to our knowledge we are the first to explore empirically the validity of the *rank preservation* assumption — that is the assumption that one's spot in the distribution of income (or earnings or transfers) is invariant to program assignment. If rank preservation holds, then the QTEs we present are identical to the distribution of treatment effects, and can be used to assess how individuals at various points in a distribution are affected by the program. Understanding the extent to which people move across the distribution is important for interpretation of the results.

Our results show that the SSP program indeed had heterogeneous impacts across the earnings, transfer, and income distributions. During the period when the subsidy is available, the impact of SSP on the earnings distribution is zero for the bottom half of the distribution. The SSP

[3] Michalopoulos et al. (2002, 2005) examine the impact of SSP on poverty rates and on ranges of hours worked and hourly wage rates. While this provides some insight into the distributional analysis, our analysis is more comprehensive in that it also analyzes impacts on earnings, income, and transfers. We also examine the full 54 month follow-up period rather than analyzing data from a few months, making our results more representative of the longer run impacts of SSP. Finally, their analyses focus on wages among the employed. By contrast, we consider the distribution of wages for everyone, which takes account of the changes in employment rates and allows for more clean interpretation of the results.

earnings distribution is higher for much of the upper third of the distribution except at the very top, where the earnings distribution is the same under either program or possibly lower under SSP. Further, during the SSP receipt period, the impacts on the distributions of transfer payments (IA plus the subsidy) and total income (earnings plus transfers) are also different at different points of these distributions. Positive impacts on the transfer distribution are concentrated at the lower end of the transfer distribution. By contrast, positive impacts on the income distribution are concentrated at the upper end of the income distribution, suggesting that within this group of long term welfare recipients, the program benefited the top of the distribution more than the bottom.[4] Impacts of SSP on these distributions were essentially zero or negative after the subsidy was no longer available.[5]

We argue that these findings are consistent with labor supply theory — workers respond to the financial incentives by changing their hours worked and, in some cases, reducing the reservation wages at which they will just be willing to take a job. We can explore these pathways more convincingly in this setting because we have data on wages and hours.

Finally, our tests of rank preservation suggest that there is some evidence of rank reversal (along observable dimensions).

The remainder of this paper is organized as follows. In Section II, we discuss the SSP experiment and the financial incentives in IA and SSP. In Section III, we use theoretical predictions about labor supply to discuss the expected effects of SSP on labor supply, welfare receipt, and income. Section IV discusses the empirical methods and Section V describes our data and presents descriptive statistics and reviews the mean treatment effects. Our main QTE

[4] As we discuss below, a person whose transfer payments are at the bottom of the transfers distribution will not necessarily have income at the bottom of the income distribution (in fact, we would typically expect the opposite to be true).

[5] Note that persons assigned to the supplement group could always obtain Income Assistance (indefinitely) if they were income eligible and not receiving the supplement. This suggests that we might not see large "losers" in the supplement group as IA provided a safety net. This is in contrast to the findings for welfare recipients facing time limits in the post-TANF policy setting in the U.S. (Bitler et al., Forthcoming).

results are presented in Section VI. We explore the validity of the rank preservation assumption in Section VII and we conclude in Section VIII.

II. SSP, Income Assistance, and the SSP Experiment

The SSP experiment randomly assigned welfare recipients to a treatment group — who could obtain SSP — or a control group — who had access only to the existing Income Assistance (IA) program. In this paper, we use data from the SSP Recipient sample which consists of about 6,000 single parents aged 19 or older in British Columbia and New Brunswick who had been on IA for at least 12 month of the last 13 months.[6] Random assignment began in November 1992 and ended in March 1995. We begin by describing the financial incentives in IA and SSP. This will provide background for discussing the expected impact of SSP, which we cover in the next section.

Income assistance was (at the time of the SSP experiment) Canada's universal cash safety net program.[7] The program covers all demographic groups and, in particular, is available to families without children as well as those with children. Benefits are means tested using income and asset tests, and eligibility thresholds and benefit levels vary by province and family size. The IA benefit structure is typical of means-tested transfer programs and is characterized by a guaranteed income (the benefit received if the family has no other income) and a benefit reduction rate or phase-out rate which dictates how the benefit is reduced as earned income increases. The IA program is quite generous — in 1992 the annual guarantee for a single-parent family with one child was $13,752 in British Columbia and $8,964 in New Brunswick. As is

[6] We exclude the SSP Plus sample (293 observations) from our analysis because we wish to focus on the impact of the financial incentives in SSP alone, which may be confounded with the effects of the employment services also offered in SSP Plus (e.g., Michalopoulos et al., 2002). We also ignore the SSP Applicant sample, made up of new applicants at the time of random assignment, who had to stay on welfare for 12 months to become eligible for the supplement.

[7] Information about IA is drawn from Barrett, Doiron, Green, & Riddell, 1996; Michalopoulos et al., 2002; and Ford et al., 2003.

common with welfare programs, the long-run benefit reduction rate was very high, leading to large work disincentives. Specifically, in 1992, IA recipients in New Brunswick faced a 100 per cent benefit reduction rate for every dollar earned over $200 per month. In British Columbia, the disregard was also $200 a month while the benefit reduction rate was 75 per cent for 12 out of each 36 months and 100 per cent for the other 24 months out of 36.[8]

The SSP earnings supplement is a negative-income-tax style transfer payment with a minimum hours of work restriction. In particular, to be eligible for a supplement payment, one had to work full-time (an average of at least 30 hours a week over a four-week period) at one or more jobs paying at least the minimum wage. During the experimental period, the minimum wage in British Columbia started at $5.50 an hour and increased to $7.15 in 1998 and the minimum wage in New Brunswick ranged from $5.00 an hour at the beginning of the period to $5.50 in 1996. Supplement recipients could not receive IA at the same time as they received the supplement, but supplement recipients could return to IA at any time (if income and otherwise eligible) and could also rejoin the supplement group if they met the hours and wage restrictions while the supplement was available. The ongoing IA eligibility suggests that members of the treatment group had a safety net if they could not find work. Lastly, to target the program to long-term welfare recipients, eligibility for the supplement required having been on IA for 12 of the past 13 months.

The supplement is equal to one-half the difference between recipient earnings and a benchmark earnings amount (unearned income was not considered when calculating the

[8] In September 1995, New Brunswick increased the earnings disregard for the first 12 months on aid to a flat amount of $200 or a 35 per cent disregard (whichever was larger) for 6 months and then $200 or 30 per cent for 6 months. In April 1996, the flat disregard of $200 per month was eliminated in British Columbia. In 1996, federal funding for IA was reduced and converted to a block grant. Some provinces — including British Columbia — responded by reducing benefit levels or tightening eligibility requirements. In 1996, British Columbia established sanctions for anyone who quit a job without just cause, barring IA eligibility for 6 months. Later in 1996, British Columbia made its eligibility determination process more stringent. In addition, in 1996, British Columbia created a “Family Bonus” of $103 per child (for all low-income families with children), and reduced IA benefits by the same amount. New Brunswick introduced a similar Child Tax Benefit, but it was much smaller (up to $250 per child per year).

supplement payment). At the beginning of the experiment, the benchmark was $37,000 in British Columbia and $30,000 in New Brunswick.[9] This benefit structure is equivalent to a guaranteed income which begins at an earnings level of the minimum hours point times the minimum wage and is then phased out using a 50 per cent benefit reduction rate with a breakeven point equal to the earnings benchmark. SSP represented a substantial increase in the financial incentives for work compared to the incentives in the IA program.

The SSP experiment gave recipients 12 months to establish full-time work and take up the supplement. If recipients did not take-up the supplement within 12 months, they were ineligible to obtain it at all. Employers were not informed of supplement receipt by the provinces and program participants had to mail in pay-stubs to obtain supplement payments. Takers could receive the supplement for up to three years from the time when it started. Importantly, they could not bank the supplement for later use.

III. Expected Impacts of the SSP Supplement

To motivate our interest in measuring the impact of SSP on the distribution of earnings and income, it is useful to outline the incentives facing welfare recipients in the SSP and IA groups. We should note that this section describes incentives for individuals and does not, therefore, map directly into our QTE measures of impacts of SSP on the distributions.

We examine potentially heterogeneous impacts of SSP on earnings, transfers, and income through two channels. We begin with a static labor supply model where women can freely choose hours of work at the given offered wage and offered wages are constant. (Note that we use women to refer to persons in the experiment for expediency: 96 per cent of those in our final

[9] The earnings benchmarks were adjusted each year for inflation. For more on the annual earnings benchmarks see Michalopoulos et al., (2002).

sample were females.) We then discuss the expected impacts on wages using the dynamic search model in Card & Hyslop (2004).

To guide the discussion, Figure 1 presents a stylized budget constraint for IA and SSP. The figure plots hours of work on the horizontal axis and income (from IA/SSP and earnings) on the vertical axis. The IA portion of the budget set goes from hours of *0* to *H1* (the IA breakeven point): if the woman does not work, she gets the maximum IA benefit. Then, for each additional dollar in earnings, the IA benefit is reduced by one dollar resulting in a slope of *0* for the IA budget constraint.[10] If assigned to the SSP group, a woman is eligible for SSP if she works beyond the hours restriction labeled *H** in Figure 1. At *H*,* income increases by the SSP supplement, which is equal to one-half of the difference between earnings and the benchmark earnings amount, labeled *E2*. Therefore, the slope of the SSP portion of the budget set is one-half of the hourly wage *w*. In this stylized figure, the minimum hours restriction is set below the IA breakeven point ($H^* < H1$). This may not be the case for all families — those with higher wages may have $H^* > H1$.[11]

We begin by considering the static labor supply model with constant wages. The idea is to compare the labor supply incentives for someone facing IA-only to the counterfactual state of the world in which she is assigned to SSP. Consider the case in which a woman would choose not to work when assigned to IA only. Depending on her preferences, assignment to SSP may lead her to enter the labor market and work hours *H*, where $H2 > H > H^*$. Alternatively, she may continue to work zero hours and receive the maximum IA payment. The same qualitative predictions hold for a woman who, when assigned to IA only, chooses to receive IA and work below the hours restriction H^*.

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[10] This stylized budget constraint captures neither the flat earnings disregards in IA nor the lower than 100 per cent tax rate that held in some periods. These features do not alter the qualitative statements that we make here.

[11] In addition, the stylized budget constraint shows that the SSP payment at H* is larger than the IA guarantee which is true for (at least) minimum-wage workers. In general, the maximum SSP payment is inversely related to the wage.

We next consider a woman who, when assigned to the IA-only group, eventually leaves IA and works at hours levels above *H1*. In the counterfactual assignment to SSP, she finds herself in the "windfall" group where she is eligible for SSP and gains income without any change in behavior. Ashenfelter (1983) referred to this as a "mechanical" induced eligibility effect. This effect leads to an ambiguous impact on hours worked depending on whether or not the 30 hour per week requirement (H*) is above or below the IA breakeven point (H1). SSP leads to an increase in nonlabor income and a decrease in the net wage, both of which lead to a decrease in desired hours. However, if H* is above H1 (not as drawn in Figure 1), it is possible that to obtain the SSP supplement, the woman may need to increase her hours. Importantly, for the vast bulk of women in this group, we do not expect the increase in desired hours that is experienced by the nonworking group discussed above. We instead expect hours to decrease for the bulk of the women. Lastly, consider a woman who might have eventually left IA and worked at a high level, say $H>H2$ (yielding income too high for SSP eligibility). She may be induced to decrease her hours, compared to her counterfactual choice under IA only, to become eligible for SSP. Ashenfelter (1983) refers to this group as having a "behavioral" induced eligibility effect.[12]

Now consider the impact of SSP in the context of a dynamic search model. Card & Hyslop (2004) outline such a model and find that SSP should induce women to search more intensely; they might also accept jobs with lower reservation wages than they would under counterfactual IA in order to become eligible for the supplement. Further, Card & Hyslop (2004) find that a woman's reservation wage decreases as she approaches the one-year time limit for establishing eligibility for SSP. These results need not imply that wages will decrease throughout the distribution, however. SSP requires work at the minimum wage or higher — so lower-skill

[12] During months 1–54, only 1.8 per cent of the British Columbia control sample, and 1.8 per cent of the New Brunswick control sample had monthly earnings that would make them ineligible for SSP (under random-assignment eligibility levels), suggesting that a small share of the overall distribution might face such a behavioral induced eligibility effect.

women are unable to reduce their reservation wage below the minimum. Consequently, the reduction in wages will be concentrated at upper end of the wage distribution.

In sum, the expected impacts on earnings are heterogeneous and may be negative, zero, or positive. The static labor supply model predicts no change in earnings at the bottom of the distribution, an increase in earnings in the middle of the distribution, with little change (and possibly a reduction) in earning at the top of the distribution. There might also be reductions somewhat below the top of the earnings distribution, if income effects dominate for those women who would not receive IA when assigned to the control group but would be income-eligible for SSP holding constant their behavior. Further, the dynamic search model implies that earnings may decrease due to a reduction in reservation wages, and this is also likely to be concentrated at the top of the earnings distribution (if high-wage individuals are also high-earnings individuals). Therefore we can assess the contribution of these two channels — hours and wages — to the changes in earnings.

This discussion can also be extended to consider impacts of SSP on transfer income (IA plus SSP if eligible) and total income. The increase in transfers is likely to be concentrated at the bottom of the transfer distribution (among those with lower welfare use) with small or no gains at the top of the transfer distribution. The impact on the distribution of income depends on the relative change in earnings and transfers but is likely to be zero at the bottom of the distribution (where women stay on IA) and higher at the top of the distribution (where high-skill women get the windfall of SSP).

IV. QTE Methodology

The evaluation reports present mean differences between treatments and controls for employment, income, wages, transfers, and children's outcomes at each of the follow-up surveys (e.g., Michalopoulos et al., 2002). Given random assignment to the program, these mean differences are reliable estimates of the true mean impact of the program. The above discussion of the impacts of SSP suggest that mean impacts may conceal heterogeneous impacts across the distribution. Here we outline the quantile treatment effect (QTE) estimator that we use to examine the impact of SSP on the entire distribution of earnings (and transfer payments and total income).

The QTE for quantile q may be estimated very simply as the difference across treatment status in the quantiles of outcomes for the two groups (treatments and controls). To understand QTE, imagine that we take a large group of people — say, $N=N1+N0$ in number — and randomly assign $N1$ of them to SSP and $N0$ of them to IA. For concreteness, suppose we are interested in the effect of SSP on the 25th quantile of the earnings distribution. The 25th quantile of the SSP group's earnings distribution is the smallest level of earnings such that at least 25 per cent of SSP-assigned people have earnings below that level. Similarly, the 25th quantile of the IA group's earnings distribution is the smallest level of earnings such that at least 25 per cent of IA-assigned people have earnings below that level. The QTE at the 25th quantile is the difference in these two earnings levels. Thus, QTEs tell us how the earnings *distribution* changes when we assign SSP treatment randomly. Other quantile treatment effects are estimated analogously, and we evaluate the distributions at 99 centiles.

One important methodological distinction is between quantile treatment effects and quantiles or other features of the treatment effect distribution. To understand the distinction, it will be helpful to briefly introduce a model of causal effects. Let $T_i=1$ if observation i receives the treatment, and 0 otherwise. Let $Y_i(t)$ be i's counterfactual value of the outcome Y if i has $T_i=t$. The fundamental evaluation problem is that for any i, at most one element of the pair $(Y_i(0),$

$Y_i(1))$ can ever be observed: we cannot observe someone who is simultaneously treated and not treated.

Evaluation methodology focuses on inferences concerning various features of the joint distribution of $(Y(0),Y(1))$. In particular, the marginal distributions $F_0(y)$ and $F_1(y)$ are always identified, where $F_t(y)= Pr[Y_i(t)< y]$ for a randomly drawn i. There is an enormous literature concerning this model and the assumptions under which it is useful. See, for example, papers by Heckman et al., (1997) or Imbens & Angrist (1994) for further details.

Quantile treatment effects are features of the marginal distributions $F_0(y)$ and $F_1(y)$. For treatment assignment t, the qth quantile of distribution F_t is defined as $y_q(t) \equiv \inf\{y:F_t(y)\geq q\}$. The quantile treatment effect for quantile q is then simply the difference in the two qth quantiles of the two distributions:

$$\Delta_q = y_q(1) - y_q(0).$$

Our above example concerning the QTE for the 25th quantile involves setting q=0. 25. Thus the estimated quantile treatment effect is the simple difference between quantiles of the distribution for the treatment and control groups

By contrast, for observation i, the treatment effect is $\delta_i= Y_i(1)-Y_i(0)$, and the cumulative distribution of treatment effects may be written as $G(d)= Pr[\delta_i \leq d]$ for randomly chosen i. Thus, unlike quantile treatment effects, quantiles of the distribution of treatment effects cannot be written as features of the marginal distributions. Rather, they require more detailed knowledge of the joint distribution (e.g., further assumptions about it).

Under some conditions, the distribution of treatment effects is recoverable from the quantile treatment effects. For example, if the treatment effect is equal for all observations then the distribution G is degenerate and is fully identified by the mean impact. However, the above discussion of labor supply impacts suggests that such a homogeneity restriction is not valid here.

Second, if people's ranks in the distributions are the same regardless of whether they are assigned to treatment or control group (e.g., there is rank preservation across treatment status), then the QTE at quantile q tells us the treatment effect for the person located at quantile q in the given distribution. Under rank preservation, all features of the distribution of treatment effects that can be associated with an observed characteristic are identified. Rank preservation is a strong assumption and will fail here if, for example, preferences for work do not map one-to-one with rank in the distribution.

In this project we present estimates of the QTE. We do not rely on the rank preservation assumption (although in section VII, we explore empirically the validity of this assumption). We fully recognize that this approach does not identify the distribution of treatment effects, nor does it identify the impact for people at given quantiles. In particular, the discussion of the expected effects of SSP above relies on an individual model of behavior that we cannot, in general, fully identify with only the QTEs. Instead, our method identifies the impact of the SSP treatment on the distributions of earnings, transfers, and income. Identifying these effects does allow one to examine some important issues — for example, how SSP affects the lower end of the earnings distribution compared to its effects on the higher end of the distribution. This knowledge can be very important in policy evaluation — where the distributions of outcomes in two different regimes are compared and social welfare calculations are applied. The advantage of our approach is that it is fully nonparametric and we require no further assumptions beyond random assignment of the treatment. In fact, this is the natural analog to estimating mean impacts in experimental studies by simply differencing means for the treatment and control groups.

As we will show below in Table 1, the SSP treatment and control samples are well balanced and there are few statistical differences in the observables in the two groups. Accordingly, we present simple QTEs and do not adjust for any covariates. Were there clearly significant differences in baseline characteristics between the two groups, we could appropriately adjust for them by using inverse propensity score weighting, as implemented in Bitler et al., (Forthcoming), and formally discussed in Firpo (2004) and Wooldridge (2003).

V. Data, Descriptive Statistics and Mean Impacts

We use data made available by SRDC to outside researchers upon completion of an application process. SRDC obtained administrative data on IA participation and payment amounts from provincial records covering a period of up to 4 years before random assignment and as many as 95 months after. The experiment tracked SSP participation and supplement payment data. Information on monthly employment, earnings, usual hours, usual weeks, wages, and other outcomes come from retrospective surveys conducted at baseline, and at 18, 36, and 54 months after random assignment. This is one distinction between the SSP experiment and many U.S. experiments, where earnings data come from administrative records of the Unemployment Insurance system rather than self reports, while wages and hours are generally unavailable.

Demographic data — including information on the sample members' number of children, educational attainment, age, race and ethnicity, language, nativity, marital status, and work history — were collected at the baseline interview.

The full Recipient sample (excluding the 293 members of the SSP Plus sample) includes a total of 5,685 persons — 2,858 in the treatment (SSP) group and 2,827 in the control (IA-only) group. We limit our analysis to persons with complete data on earnings, hours, and wages for months 1–54.[13] Our final estimation sample includes 3,875 persons — 1,991 in the SSP group and 1,884 in the IA-only group.[14,15]

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[13] Based on personal communication with Douglas Tattrie of SRDC, we have realigned the transfer data so that the months are consistent across the different outcome measures. We have aligned transfers relative to the month when earnings were received. We have replaced IA payments for a given month with those for the following month to reflect the fact that IA payments are issued for the preceding month's earnings. Supplement payments generally

Our unit of observation is the person-month, with 54 months of data leading to a sample of 209,250. We choose to analyze the data at the person-month level because IA and SSP benefits are calculated monthly. We have also estimated models using averages over various time periods and the results are qualitatively similar to those presented here (some of these are discussed below in Section VII).[16] Treatment group members could begin getting the supplement as soon as they began working full-time and gave up IA, but they had only one year to establish eligibility for the supplement. They could receive at most 36 months of supplement payments after the month when they first established eligibility. Thus, we examine two time periods: months 1–48, the period during which persons assigned to the treatment group could have gotten the supplement; and months 49–54, after which all supplement payments should have ended. Our outcome measures include: monthly earnings, average monthly wages (averaged over multiple jobs), usual weekly hours (for that month), total monthly transfers (IA payments plus supplement payments if eligible), and total monthly income (earnings + total transfers).[17]

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correspond to the previous month's earnings or even at times to two months' previous earnings. We adjust the supplement payments to be those of the following month if the first supplement payment was in the first month after the month of random assignment and those of the second month after this month (t+2) if the first supplement payment was the second month after random assignment or after. This adjusts for the fact that there was a delay between processing the pay stubs and issuing the SSP supplements.

[14] The first source for loss of observations is persons who do not complete the 54-month survey (833 observations). In addition, we also drop observations with a 54-month survey that have missing earnings or hours for any month in 1–54 and several hundred cases where the 54 month interview occurred before the 54th month after random assignment (together these result in 977 observations being dropped, of which 336 were interviewed before month 54, 611 were missing an hours or earnings observation in months 1–54, and 30 were missing hours or earnings and were interviewed before month 54).

[15] We examine the selectivity of our sample selection in several ways. First, the IA and SSP payments are available for all observations for all months. Our estimates for total transfers and IA alone using the full sample are virtually identical to those reported here as are estimates for total transfers and IA alone estimated for only the sample in the 54 month survey. Further, the probability of an observation being dropped from the sample does not statistically differ between the treatment and control group.

[16] There are reasonable arguments in favor of using either person-months or person-level averages as the unit of analysis. In general, using person-months will increase the number of zero and outlying values, while using averages will reduce the incidence of zeros and outlying values.

[17] We have also estimated QTEs for the highest wage during a given month and total monthly hours. The results were quite similar to those reported here for average wage and for weekly hours. We settled on presenting QTEs for weekly hours because this made it easier for us to examine the SSP supplement eligibility threshold (30 hours a week), and because the monthly hours (like the monthly earnings) have been standardized to reflect the average number of weeks per month.

Tables 1 and 2 present pre-reform and post-reform mean differences between the treatment and control groups. Standard errors for these tables are calculated using simple T-tests. All other standard error calculations in the paper (including those for QTEs and mean differences across quantile in demographic characteristics) account for within-person statistical dependence using bootstrapping. Our bootstrap procedure uses nonoverlapping person-level blocks (i.e., we resample persons in an *iid* fashion and then use the full profiles of each resampled woman's outcomes). We use 500 nonparametric bootstrap replications and then calculate standard errors using the percentile method. Thus, the endpoints for the 90 per cent confidence intervals for a particular quantile are the smallest bootstrap estimate for that quantile less than or equal to the 5^{th} percentile of the bootstrap estimates for that quantile and the largest bootstrap estimate for that quantile that is greater than or equal to the 95^{th} percentile of the bootstrap estimates for that quantile.

We begin by examining whether the treatment and control groups are well-balanced (as would be expected given random assignment). Table 1 presents means for a wide array of pre-random assignment measures separately for the treatment and control groups. There are several things to note from Table 1. First, as would be expected from the random assignment process, the characteristics of the SSP group are very similar to the characteristics of the IA group. T-tests of the equality of means suggest that for a vast array of pre-random assignment measures (including many more variables than we present in the table), the treatment and control groups do not differ in a statistically significant sense. There are three exceptions: the IA group is 3.0 percentage points less likely to have completed only high school (relative to high school dropout and some post-secondary) with a p-value of 0. 052; 2 percentage point more likely to be unemployed at baseline (p-value of 0.076); and the IA group received welfare for 0.6 fewer months out of the 36 preceding random assignment (p-value of 0.015). A joint test across the 16 pre-random assignment measures listed in Table 1 plus seven others denoting whether various measures are missing fails to reject equality of means (the $\chi^2(23)=29.78$, with a p-value of 0.1559), suggesting that our sample is well balanced across the treatment and control groups.

Table 1 also demonstrates that these women are relatively disadvantaged. About one-half of the group has never been married and half have not completed high school. About half of each group is of Canadian descent (not shown), another 10 per cent of each group is of First Nations ancestry, around 1–2 per cent of each sample is Black, and around 5 per cent are Asian (not shown). Not surprisingly, given that they were all on IA for 12 of the previous 13 months, only about 6 per cent were working full-time and 10–11 per cent were working part-time at random assignment. The groups were fairly evenly split across the two provinces; 52 per cent of the control group was in British Columbia at random assignment versus 53 per cent of the treatment group.

Table 2 presents mean impacts for the full sample and by province. We report means and treatment effects for months 1–48 and for months 49–54. The first four rows present average monthly values of earnings, weekly hours, average monthly wages, IA payments, total government payments (IA+SSP), and total income (earnings + total government transfers). (Note that earnings, hours, and wages are all 0 when the person is not working. This is not standard, especially for wages, but is the only available option if we want to avoid conditioning on working, which is obviously affected by random assignment.) The second and third panels present these same figures for British Columbia and New Brunswick.

The table shows that during the supplement receipt period, SSP led to substantial, statistically significant increases in employment and earnings. For example, in months 1–48 monthly earnings were, on average, \$72 higher per month in the SSP group. Availability of SSP led to a reduction of \$73 per month in IA benefits, but total government transfers were \$58 higher for the SSP group. Overall, these results show that over the four years following random assignment, the impact of SSP on average total monthly income was $130 (an increase of about 14 per cent compared to the estimated IA group baseline monthly income of $922). The last three columns echo the findings widely noted by others that the earnings and income differences decrease substantially (and are no longer significant in the case of income) after the end of the supplement availability period (during months 49–54). The earnings and transfers rows show that the increase in average earnings of $32 is offset by a decline of $33 in average transfers.[18]

The 2nd and 3rd panels show that the mean impacts are larger and longer lasting in New Brunswick compared to British Columbia. For example in the first 48 months, the mean treatment effect on earnings is $59 or 20 per cent of control group earnings in British Columbia compared to $85 or 36 per cent in New Brunswick. After the end of the SSP period, no significant effects remain in British Columbia while the mean earnings effect in New Brunswick remains a statistically significant $57. These differences echo the results in earlier research (Michalopoulos et al., 2002).[19]

VI. Quantile Treatment Effects

<u>Earnings QTEs</u>

Figures 2 and 3 introduce the QTE estimates. Figure 2 plots quantiles of the monthly earnings distribution using person-month observations for the SSP and IA groups for the SSP receipt period — the first 48 months following random assignment. (We also include horizontal lines for the means for the two groups for reference.) The solid line represents the SSP group and the dashed line represents the IA group. The vertical difference between the lines at a given

[18] Note that IA does not quite equal total transfers for months 49–54, the period when, in theory, no one should obtain the supplement. However, our method for aligning the supplement payments with earnings (discussed in footnote 14) is not perfect, and a very small share of persons still report supplement payments in months 49–54 even after realigning transfers.

[19] Differences across provinces have also been found for family structure outcomes (Harknett & Gennetian, 2003).

quantile is an estimate of the SSP treatment effect on the earnings distribution at that quantile — the QTE. These QTEs are plotted in Figure 3. For comparison purposes, the mean treatment effect is plotted as a horizontal (dashed) line, and the 0-line is provided for reference. Dotted lines represent the bootstrapped two-sided 90 per cent confidence intervals.[20] The variation in the impact across the quantiles of the distributions is unmistakably significant, both statistically and substantively. This figure shows that for monthly earnings in the SSP receipt period, the QTEs are zero for about two-thirds of all person-months. This result occurs because monthly earnings are identically 0 for 65 per cent of person-months in the SSP group over the first 48 months and 73 per cent of corresponding IA group person-months. For quantiles 66–94, SSP group earnings quantiles are greater than the control group earnings quantiles, yielding positive QTE estimates. For quantiles 96–99, IA group earnings quantiles exceed SSP group earnings quantiles, yielding negative (though insignificant) QTE estimates (the estimate for quantile 95 is zero).

The range of the QTE point estimates is quite large, from -\$165 to \$470, compared to a mean treatment effect of \$72. Under the null hypothesis of constant treatment effects, all QTE must be equal to the mean. As pointed out by Heckman et al., (1997) in the context of job-training programs, this null can be rejected simply if a large share of the QTE are 0. We can also test whether a constant treatment effect could lead to a range as large as that for our QTE point estimate. We do this as follows, using the bootstrap. We take 250 observations of our bootstrap sample of the control group distribution and add the estimated mean treatment effect to them to create a synthetic null treatment group distribution. We use the other 250 observations of our control group distribution along with our synthetic null treatment group to construct QTE for the

[20] Recall that the confidence intervals (CIs) are constructed by the percentile method, as the lowest bootstrapped quantile treatment effect estimate for the qth quantile at or below the 5th percentile of the bootstrap distribution of quantile treatment effect estimates for that quantile q, and the highest bootstrap estimate for the qth quantile treatment effect at or above the 95th percentile of the bootstrap quantile treatment effect distribution for quantile q. Since we do not assume normality for the standard errors, the CIs need not be (and frequently are not) symmetric around the QTE.

null hypothesis. We can then use the order statistics of the resulting individual distributions for each quantile to generate a confidence interval for testing various features of the null such as the maximum minus minimum range. Such a test compares the distribution for the maximum minus minimum range for the null with our real-data QTE maximum minus minimum range. This comparison suggests that a confidence interval for the null constant treatment range is [54,342] at a confidence level above 99 per cent, while the range estimated using the data is 634. These results clearly show that the mean treatment effect is not sufficient to characterize SSP's effects on earnings.

These results are consistent with the predictions of labor supply theory, discussed above. That is, the QTEs at the low end are zero, they rise, and then they eventually become negative (although not statistically significantly so). To further explore the impacts of SSP on the distribution of earnings, we present QTEs for usual weekly hours (Figure 4) and average wages (Figure 5). The wage measure is an average across all jobs in a given month and zero if the recipient is not working. The structure of the figures is identical to Figure 3 — we present the mean treatment effect, the QTEs and the 90 per cent confidence interval of the QTEs. Both Figures 4 and 5 refer to the SSP receipt period — months 1–48.

Like the QTEs for earnings, the QTEs for hours and wages are zero through the 65th quantile, reflecting the fact that for 65 per cent of person-months in the SSP group, the recipients are not working and earnings are zero.[21] For quantiles 66–91, the QTEs for hours are positive and then the QTEs fall to essentially zero for the top 8 quantiles. This finding is consistent with the "SSP windfall" group having little hours response. It does not suggest a negative hours response among the behaviorally eligible group.

[21] Remember the sample includes nonworkers and workers. We have set wages to zero for nonworkers.

Why might we not see a decline in hours quantiles in the top of the hours distribution? First, SSP requires full time work, so hours cannot fall below 30 hours per week. There is strong evidence of a behavioral response to the full-time requirement — 4.7 per cent of persons in the SSP group have exactly 30 hours per week compared to 1.9 per cent in the IA-only group (and the difference is significant at the 1 per cent level). Second, IA is a relatively generous program — recall that the breakeven earnings point in IA is on the order of $747 (NB) to $1146 (BC) per month for a single mother with one child. Only 12 per cent of the control group in BC and 14 per cent of the control group in NB has earnings in months 1–54 that exceed the IA breakeven point (this is an upper bound for the share of women we expect to reduce their hours either to become behaviorally eligible or because they are mechanically eligible). Thirdly, SSP itself is even more generous than IA. Thus the share of women who would counterfactually be above the SSP breakeven point but could reduce their hours of work to become eligible for SSP is even smaller. Only 4.6 per cent of the control group in BC and 3.2 per cent of the control group in NB ever has earnings in months 1–54 that exceed the benchmark at the time of random assignment (lose SSP eligibility) and only 1.0 per cent of the BC control group and 0.6 per cent of the NB control group exceed the benchmark on average during months 1–54.[22]

By contrast, the QTE estimates for average wages (Figure 5) are negative for the top 9 quantiles, though they are also very imprecisely estimated. Thus the evidence is more consistent with the theory that SSP led to a reduction in the wage distribution at the top of the wage distribution than the theory that SSP led to a reduction in the hours distribution at the top of the hours distribution. It is also consistent with the reductions in wages being concentrated at the top of the wage distribution, where there is scope to reduce wages and not be below the minimum wage. Like Card & Hyslop (2004), we find that the minimum wage is quite important for this

[22] This is quite different from the experiment in Bitler et al., (Forthcoming) where a large fraction of the control group had earnings at or above the "windfall" range and there was no full-time work requirement. In that case, we argued that there was substantial scope for a reduction in labor supply to maintain eligibility for welfare.

group — 4.9 per cent of the SSP group and 3.0 per cent of the IA-only group have average wages equal to the minimum wage during months 1–54. The numbers for workers are more striking, 14.2 per cent of SSP workers and 10.8 per cent of IA-only workers were at the minimum wage during months 1–54.

Figure 6 plots the earnings QTE results in months 49–54, after the three-year SSP receipt period is over for all women. The earnings effects clearly diminish after the completion of the SSP period. However, the basic pattern is still evident: zero impacts at the bottom, (modest) increases in earnings in the middle of the earnings distribution, and reductions in earnings at the top of the earnings distribution.

<u>Transfers QTEs</u>

Figure 7 presents results for total government transfers (IA + SSP) in the first 48 months. There are several observations to make from this figure. First, the QTEs are everywhere nonnegative, which reflects the generous nature of the SSP subsidy. Second, the results show that the impact of SSP on the distribution of transfers is very concentrated. In particular, the QTEs are identically 0 for the bottom 18 quantiles, reflecting the fact that for 18 per cent of person-months, both the treatment and control group have zero transfer income. Between quantiles 19 and 36, the QTE estimates range from $64 to $422 per month. Many of these impacts are quite large compared to the control group mean level of $659 per month. The confidence interval for a null of constant treatment effects is [25, 257] at a confidence level of above 99 per cent, while the estimated range over all quantiles in the real data is 423. For quantiles 37–91, the QTE estimates are relatively small and below the mean treatment effect of $58. This figure provides substantial insight into SSP's effects beyond that afforded by mean treatment effects. Furthermore, the pattern of the QTE estimates is consistent with theoretical predictions. For transfers, the zero-to-small effects in the top two thirds of the transfer distribution is likely to correspond to the bottom of the earnings distribution (where earlier we saw that SSP led to no change in the earnings distribution).

We have also estimated QTEs for IA payments alone (Appendix Figures 1 and 2). Those graphs present a very similar story which is not surprising since anyone getting the supplement must forego IA. The QTEs for IA payments are everywhere nonpositive, and the effects are concentrated in the second quartile of the IA distribution.

Figure 8 shows that the QTEs for total government transfers during months 49–54 (after SSP payments have ended) are much different. The mean treatment effect shows a small decrease in mean transfers (-$33) in this period. For the lowest 39 quantiles, the QTE estimates are zero as are all these quantiles for both the SSP distribution and the IA-only distribution. At quantiles 59 and above, the QTE are close to zero, ranging from -$30 to $8. But for quantiles 40–58, the QTE are negative and sometimes sizable, showing a reduction in the transfer distribution associated with SSP. This last finding is consistent with the results in Figure 6, which imply some positive impacts on labor supply even after SSP ends.

<u>Total Income QTEs</u>

Figure 9 plots the QTE results for total income in months 1–48. These results again suggest a large degree of heterogeneity in the impact of SSP on the distribution of income. The QTEs range from $0 for the bottom 9 quantiles — where total income is $0 for both groups — to a maximum of $495. The mean treatment effect for this period is $130, so again the range of quantile treatment effects is large compared to the mean treatment effect. The confidence interval for a null of constant treatment effects is [48, 467] at a confidence level of above 99 per cent, while the estimated range in the real data is 495. Throughout most of the bottom two thirds of the distribution, the QTEs are relatively small and are below the mean treatment effect. Beginning at quantile 66, however, the QTE estimates suggest that SSP leads to a large increase in income in the upper third of the income distribution (increase above the mean treatment effect). These gains can be compared to the baseline mean for the IA group of $922. This pattern suggests that a generous, work-oriented income supplement can lead to increases in the income distribution — but that most of the gains may be concentrated at the upper ranges of the income distribution.[23] Thus, at least while the supplement was available, it may have led to increased inequality within this group of long-term welfare recipients.

It would be useful to decompose the QTE estimates for income into the impacts on earnings and transfers. But there need not be any particular relationship between QTE for total income and changes in its components, since non-zero points in the income distribution could potentially correspond to any of a variety of non-zero points in the earnings and transfer distributions. However, we can try to describe how the components of total income differ across program assignment at different points in each income distribution.

To do so, we use local nonparametric regression techniques to estimate treatment-control differences in earnings and transfers for each quantile of the income distribution.[24] We plot these differences by quantile for months 1–48 in Figure 10. The results show very small differences up to about the 77th quantile, where positive total income QTE are associated with greater transfer payments among treated women than among those in the control group. Over the next 14 quantiles, the average difference in earnings is positive and generally increasing, while the average difference in transfers is negative. The most striking thing about the figure, however, is that at the top of the income distribution the difference in transfers is positive and large while the

[23] Given the program design and the fact that income- and otherwise-eligible recipients in the supplement group could qualify for Income Assistance if they were not receiving the supplement, it may not be surprising that no one appears to be worse off income-wise (at least not while the supplement is available).

[24] In particular, we use locally weighted regression (LOWESS) techniques to regress earnings (and transfers) on total income at each quantile q of the total income distribution separately for the SSP and IA-only groups. We then construct means for the average treatment and control differences in earnings and transfers (from the LOWESS estimates) at each income quantile by taking the difference between the estimates in the SSP and IA-only regressions. Estimates are constructed similarly for transfers.

difference in earnings is negative and large. These effects are consistent with the interpretation that the positive QTE estimates at the top of the income distribution are associated with a combination of greater transfers and lesser earnings. We emphasize that these differences cannot generally be interpreted as causal, since the total income quantile is not assigned experimentally. In addition, an important drawback to this procedure is that computing average earnings at a given total income quantile implicitly requires varying transfer payments negatively (since transfers plus earnings are held locally fixed in such a regression).

Figure 11 plots the QTE results for the post-SSP time period, months 49–54. Here, the impacts across the distribution are quite homogeneous, showing no change or very small changes in the income distribution after SSP payments cease. Here the mean treatment effect of -$1 provides a fairly complete picture of the impacts during months 49–54 over almost the entire range (with the very top of the distribution being the only real exception).

QTEs by Province

Our results in Table 2 (above) show that the mean impacts are larger and longer lasting in New Brunswick compared to British Columbia. Here we extend that finding by comparing QTE for the NB and BC samples. Figure 12 presents the QTE for earnings in months 1–48. To facilitate ready comparison, we plot both the QTE for the NB sample (solid line, peaks to the left) and the BC sample (dashed line, peaks to the right) on the same figure. For readability, we omit the mean impacts from the figure. Figure 13 plots the difference in the QTE between the two provinces, along with the 90 per cent confidence interval for this estimate.

These figures show that the overall pattern of the impact of SSP on the distribution of earnings in months 1–48 is very similar — the QTEs are zero at the bottom of the distribution, positive in the upper part of the distribution with negative or zero QTEs at the very highest quantiles of the earnings distribution. There are qualitative differences, however. The NB sample has positive QTEs in a larger range of the sample and the largest QTEs are somewhat larger in the BC sample. Figure 13 shows that few of the pairwise differences are statistically significantly different, however.

These differences may reflect differences in the incentives in the two provinces. The guarantee in the IA program is much higher in BC compared to NB. The SSP benchmark, however, was also a more generous \$37,000 in BC relative to \$30,000 in NB. Further, the minimum wage was \$5.50 at the beginning of the experiment in British Columbia but \$5.00 in New Brunswick. In addition, the demographic composition of the welfare population differs in the two provinces. Sample members from British Columbia were more likely to have been born abroad and to have completed some post-secondary education. Women selected in New Brunswick were likely to have had longer welfare spells. One would expect earnings QTEs to be positive first for New Brunswick simply because the minimum wage there is lower, so the lowest earnings level at which one could obtain the supplement is lower.

The QTE results for total income for the two provinces in months 1–48 are presented in Figure 14 (with the difference in the QTE in Figure 15). Again, the QTE for BC and NB look similar: zero at the very bottom, then positive, then close to zero again, then rising at the top and peaking around the 80th quantile. However, the positive QTEs at the bottom for BC are concentrated in a tight band near the 10th quantile, while those for NB are much broader, ranging from about the 10th to about the 25th quantile. At the same time, only the lower peak for BC rises above the mean impact (not shown on the figure).

Notably, Figures 12 and 14 show that the variation in the QTEs within province outweighs that between provinces. This result shows that differences in unobservables can be large compared to observable differences like the demographic, economic, and programmatic differences between NB and BC.

The mean impacts for earnings in post-SSP period (reported in Table 2 and documented more completely in Michalopoulos et al., 2002) suggest that earnings impacts were longer-lasting in NB. However, earnings QTE for months 49–54 (not shown here) show a more nuanced story: while BC does exhibit a larger range of zero QTEs compared to NB, we find that both provinces show positive earnings impacts for a substantial portion of the distribution. The total income QTEs in the post-SSP period (not shown here) are qualitatively similar for the two provinces.

VII. Evidence on Rank Preservation and Rank Reversal

The QTE results provide important evidence that the impact of SSP varied across the distribution of earnings, transfers, and income. As discussed above, the impact of the treatment on the distribution is distinct from and generally not equal to the distribution of treatment effects. If a person's rank in the distribution does not change with the treatment (known as rank preservation), however, then the impact of the treatment on the distribution would be equivalent to the distribution of treatment effects. While we cannot test for rank preservation, we can use the treatment and control distributions of demographic characteristics to see if there is indeed evidence of rank reversal. For example, if the distribution of observable characteristics in some range of the earnings distribution varies significantly between the treatment and control group, this would be evidence against rank preservation. Unfortunately, this sort of test can provide only negative evidence: finding no significant differences in demographics does not imply rank preservation. To the best of our knowledge, we are the first to test for evidence of rank reversal in this fashion.

In particular, we estimate QTEs for person averages of earnings, transfers, and income for months 1–48. These are presented in Appendix Figures 3, 4, and 5, respectively. Note first that the QTEs for the averages are more spread out than the QTEs for the person-months, as one would expect. Also notice that the patterns are qualitatively quite similar to the person-month estimates. We take the 25th, 50th, and 75th cutoffs for the distributions of earnings, transfers and incomes for the treatment and control groups, and calculate mean treatment and control group values for the various demographics within the following ranges: less than or equal to the 25th percentile, greater than the 25th percentile but less than or equal to the 50th percentile, greater than the 50th percentile but less than or equal to the 75th percentile, and greater than the 75th percentile. Since the distributions of average earnings are only positive slightly below the 50th percentile, we only calculate the tests for the 0–50th, 50th–75th, and above the 75th percentiles for earnings. Note that we could calculate similar statistics for the person-month estimates, but find it more transparent to use average QTEs for this purpose, because then each person only falls within a single range. For example, one estimate of the differences is the fraction white among treatment group members in the 25th–50th percentiles of the SSP group's income distribution, minus the fraction white among control group members within the analogous range.

We follow Abadie (2002) in using the bootstrap to estimate the null distribution for these differences; our full bootstrap procedure is as follows:

1. Sampling with replacement from the actual data, draw 500 bootstrap samples, each having the same number of observations as the real data. Index these samples with $b \in \{1,2,\ldots,500\}$.
2. For the bth replication sample, do the following:
    a. Randomly order the observations in the bootstrap sample.
    b. Assign the first 1,991 to a synthetic "treatment" group, and assign the rest to a synthetic "control" group (there are 1,991 treated observations in the actual data).

c. For each mean in which we are interested (for example, the fraction white in the first 25 quantiles of the transfer distribution), calculate its value in the synthetic program groups, and then take the difference of these sample means. Call this difference d_b.

3. Sort all elements of the set $\{d_b\}$, $b=\{1,2,\ldots,500\}$ from lowest to highest.
4. Use d_{25} and d_{475}, respectively, as estimates of the 5th and 95th percentiles of the null sampling distribution of our statistic d; a 90 per cent confidence interval is then constructed as $C=[d_{25,}\ d_{475}]$.

A real-data estimated difference is significantly different from zero if it falls outside the interval C just defined. More generally, following Cameron & Trivedi (2005) we can calculate the p-value for any statistic as follows. Create a vector of values for the statistic, including the real data realization, with the other 500 values being the realizations of the statistic from the 500 bootstrap draws that impose the null. Take the absolute value of the statistic, and sort these values. Let k be the index of the real data observation in the sorted data. (For example, if the real data estimate in the sorted data lies between the 431st and 432nd bootstrap realization, then k = 432.) Then, our p-value is $p = 1- (k-1)/501$.

In Table 3, we present the results for 12 demographic variables for gender, race, ancestry, language, and education. There are three panels in the table; each classifies people by their position (quantile) in the distribution. The top panel uses the earnings distribution, the middle panel uses the transfer distribution, and the bottom panel uses the total income distribution.[25] Each column presents the difference in the demographic variable between the two samples within a given quartile along with their p-value for statistical significance. Of the 132 differences here, 26 are statistically significant at the 10 per cent level or below and 16 are significant at the

[25] There are three columns in the top panel and four columns in the middle and bottom panels. This reflects the fact (noted above) that a larger fraction of each average earnings distribution is 0, requiring us to collapse the first and second quartiles into one group in that case.

5 per cent level or below. This certainly suggests some evidence of rank reversal based on these demographic characteristics. Because we are considering multiple differences in means, simply counting the number of rejections in these individual tests is an inadequate test: each test could have relatively low power, a problem that can be solved with a joint test for the significance of the twelve demographic variables in each quantile range.[26]

When we do joint tests for earnings, transfers, and total income over the entire period, we find that we reject the joint test for earnings in the range $50 < q \leq 75$ (p-value of 0.005), for transfers in the range $0 < q \leq 25$ (p-value of 0.009), and for total income in the ranges $0 < q \leq 25$ (p-value of 0.028) and $q > 75$ (p-value of 0.004). We fail to reject for the other 7 ranges for earnings, transfers, and total income. We also fail to reject in any ranges for IA payments, highest monthly wage, average monthly wage, usual weekly hours, and total monthly hours (not shown in table). While the individual tests suggest some rank reversal may be present, the joint test results convincingly demonstrate that even at the very coarse level on which our demographics test operates, strict rank preservation is rejected. More work — both theoretical and empirical — would be needed to go further in discussing the degree of rank reversal, and this issue is beyond the scope of this paper. Nonetheless, for reasons already discussed, we firmly believe that QTE methodology is informative for evaluating programs like SSP.

[26] To test for the joint significance of these differences within a given quantile range, we will use a simple chi-square test. Let the column vector of sample differences in means for the demographic characteristics be $\overline{d}$. Each difference of means is asymptotically normal. Thus, $\sqrt{n}(\overline{d} - d)$ is distributed $N(0,V)$, where V is the covariance matrix for the random variable $\overline{d}$. Under the null hypothesis that the true vector of differences d equals zero, the statistic $d'V^{-1}d$ will have a chi-square distribution with degrees of freedom equal to the dimension of d, the vector of differences. The practical challenge is to estimate the covariance matrix; we use our realized bootstrap distribution to do so. Letting d_b be the realization of our vector of differences for the bth bootstrap sample, we estimate this matrix with the estimator $V^* = \frac{1}{500} \sum_{b=1}^{500} (d_b - \overline{d}^*) * (d_b - \overline{d}^*)'$, where $\overline{d}^* = \frac{1}{500} \sum_{1}^{500} d_b$ is the bootstrap-sample average of realized differences in means. We then refer our chi-square statistics $d'V^{-1}d$ to a table of Chi-square critical values.

It is important to note, however, that this test is rather weak in two respects. First, for computational ease, we have grouped many quantiles together, and we may have missed differences in demographics within our groupings. Second, even if demographics do not change, rank reversals may have occurred among unobservables, such as preferences for work and fixed costs of work, for example, that are not fully reflected in observables.

Since we have found evidence of rank reversal, it suggests that our QTE estimates are informative about the overall impacts of the program but cannot be used to determine impacts for individuals at specific points in the distribution. Social welfare analysis, of course, makes use of the marginal distributions shown in our QTE. Furthermore, our previous results clearly suggest heterogeneity in the impacts of SSP even if we cannot determine exactly who is impacted by it.

VIII. Conclusions

During the 1990s, a number of governments experimented with changes to their means-tested cash assistance programs to encourage work among low-income women. In this paper, we investigate the impact of a unique experiment—the Canadian Self-Sufficiency Project (SSP). SSP coupled a strict work requirement (full-time work at the minimum wage) with a generous earnings subsidy for a period of up to 3 years for long-term Income Assistance (IA) recipients as an alternative to simply receiving Income Assistance (IA).

Adding to the substantial literature on SSP, we examine the impacts of SSP on the distribution of earnings, transfers (IA plus the SSP supplement), and income for long term IA recipients using quantile treatment effects (QTEs). While mean impact analysis allows one to calculate costs and benefits of new policies, examining impacts on the entire distribution has the potential to uncover effects that may vary systematically across the distribution. For example, we examine whether the benefits of SSP are spread across the distribution or concentrated in particular parts of the distribution. Knowledge of such heterogeneity may be important to policymakers, particularly in the context of programs aimed at poverty alleviation.

Our findings lead to several conclusions. First, we find quite heterogeneous impacts across the various distributions — QTEs for earnings, transfers, and income all show considerable variability that would be missed if we focused on simple mean treatment effects. Moreover, these varied impacts are consistent with predictions of labor supply theory. For example, during the period when the subsidy is available, the impact of SSP on the earnings distribution is zero for the bottom half of the distribution. The SSP earnings distribution is higher than the control group for much of the upper third of the distribution except at the very top, where the earnings distribution is the same under either program or possibly lower under SSP. Further, during the SSP receipt period, positive impacts on the transfer distribution are concentrated at the lower end of the transfer distribution while positive impacts on the income distribution are concentrated in the upper end of the income distribution. Impacts of SSP on these distributions were essentially zero after the subsidy was no longer available. Variation in impacts within province is much larger than variation between provinces.

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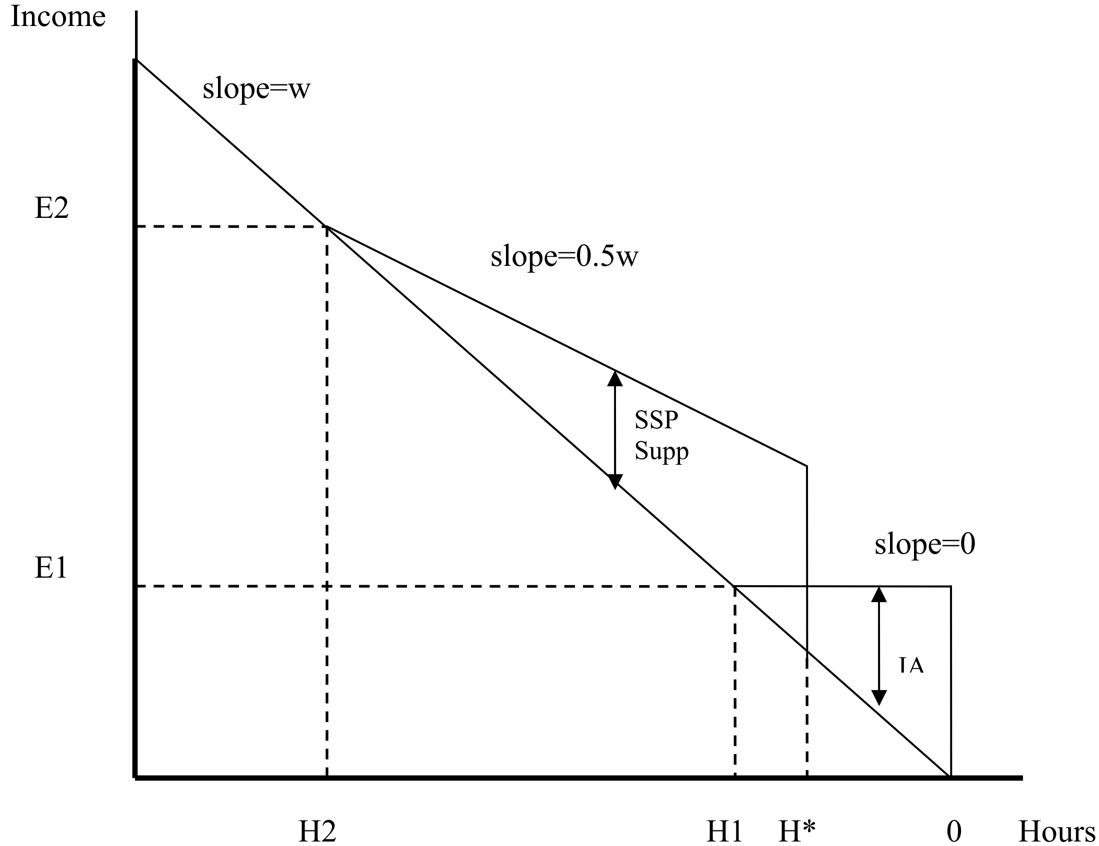
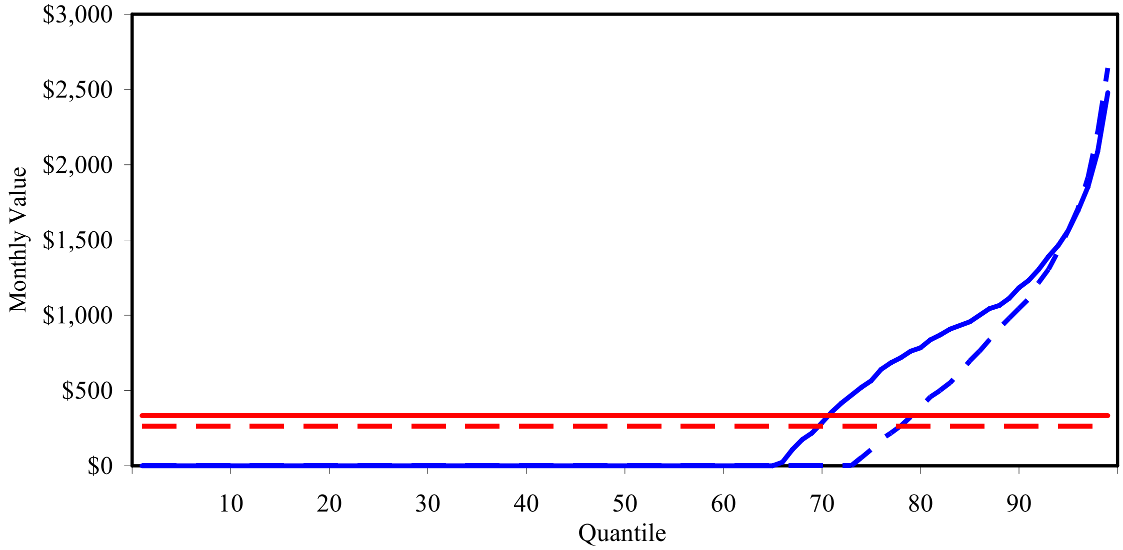
Figure 1: Stylized Budget Constraint for IA and SSP



Notes: Figure depicts stylized budget constraint for IA and SSP, assuming that the wage is such that a recipient's breakeven point for IA is above 30 hours a week of work.

Figure 2: Distribution of Monthly Earnings for SSP and IA-Only Groups, Months 1-48



Notes: Solid lines refer to the treatment (SSP/IA) group and dashed lines refer to the control (IA-only) group. Horizontal lines are means and the other lines are quantiles of the distribution of earnings.

Figure 3: SSP Quantile Treatment Effects on Distribution of Monthly Earnings, Months 1-48

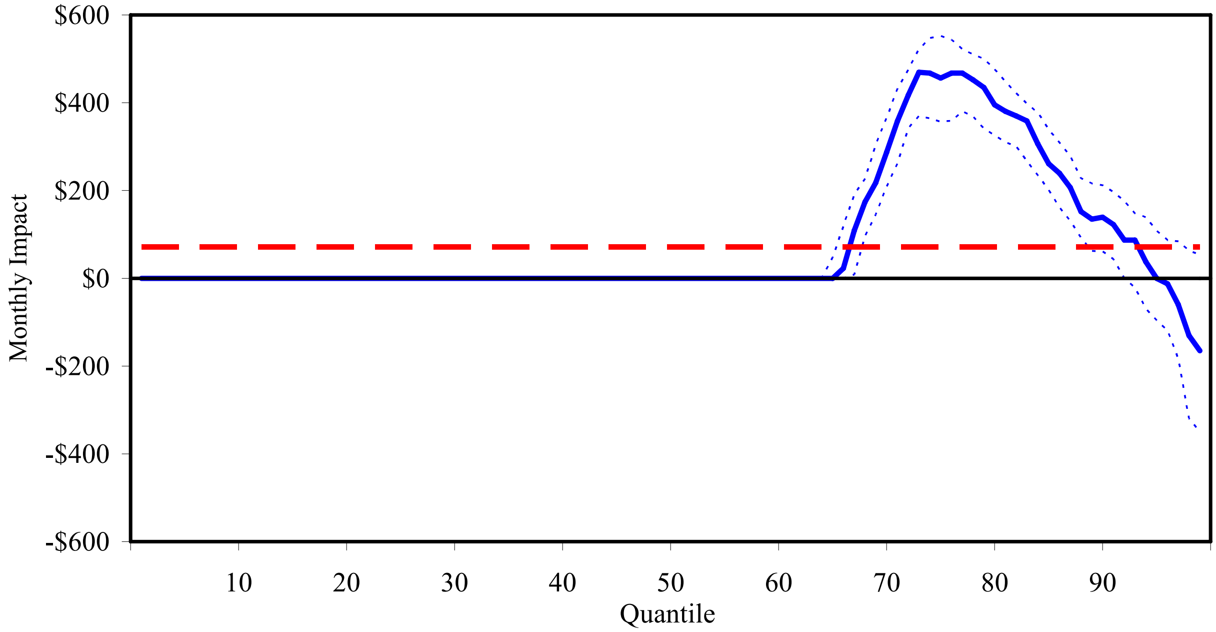
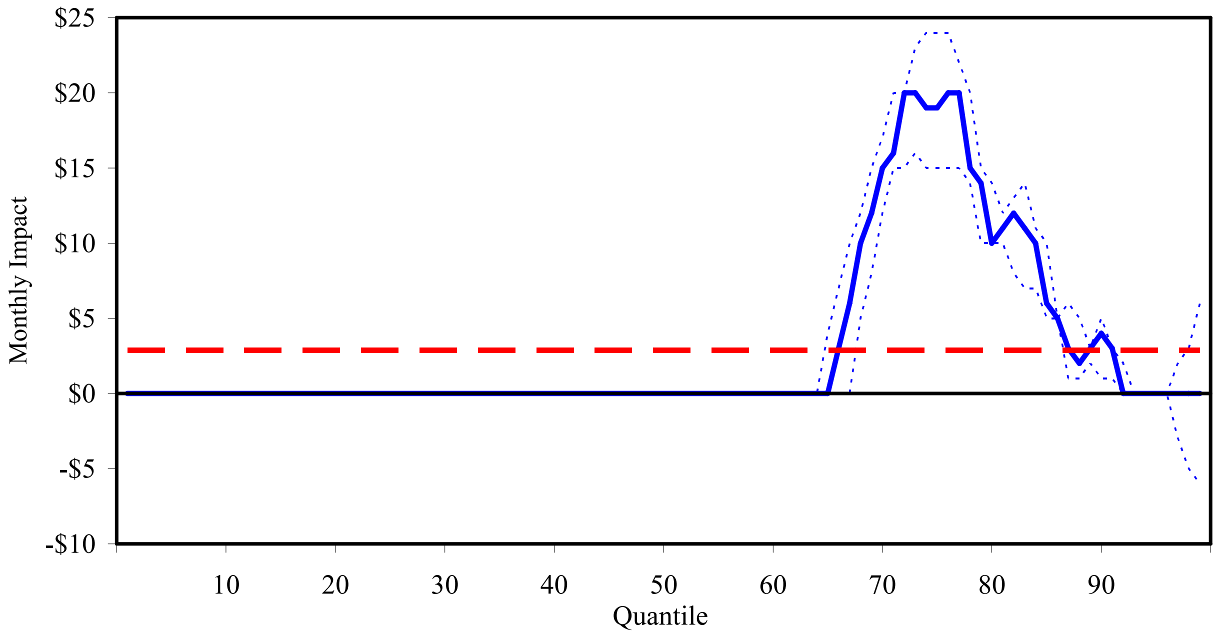


Figure 4: SSP Quantile Treatment Effects on Distribution of Weekly Hours, Months 1-48



Notes: In each figure, the solid lines are QTEs, dotted lines are bootstrapped 90% confidence intervals (accounting for within person dependence), and dashed lines are mean treatment effects.

Figure 5: SSP Quantile Treatment Effects on Distribution of Average Hourly Wages, Months 1-48

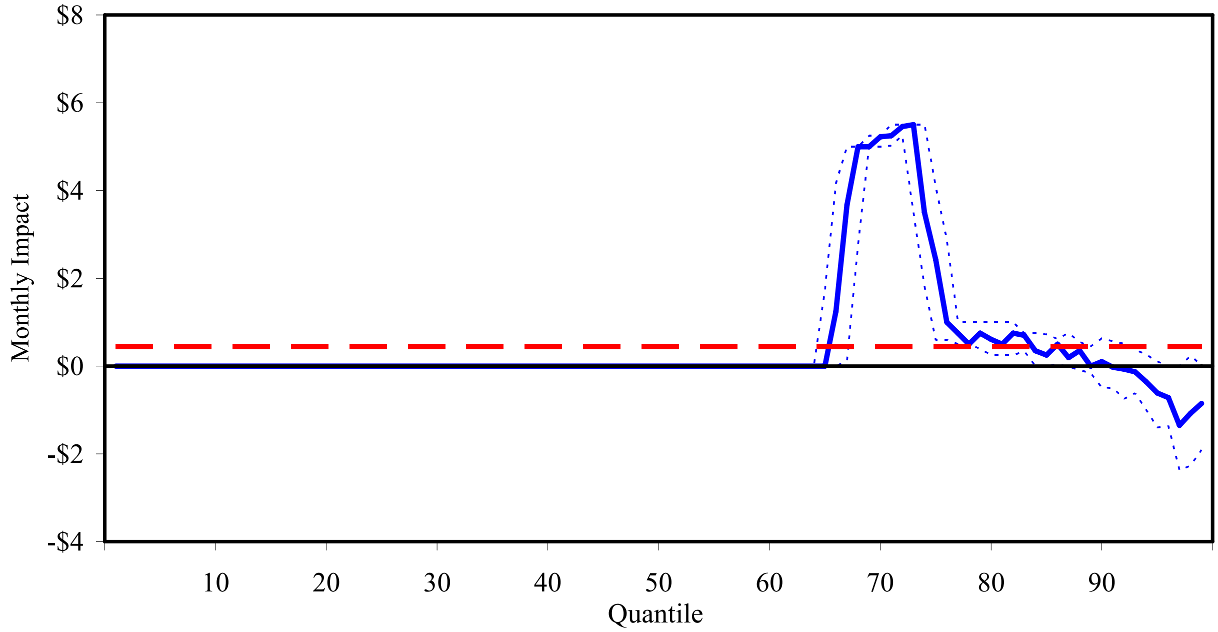
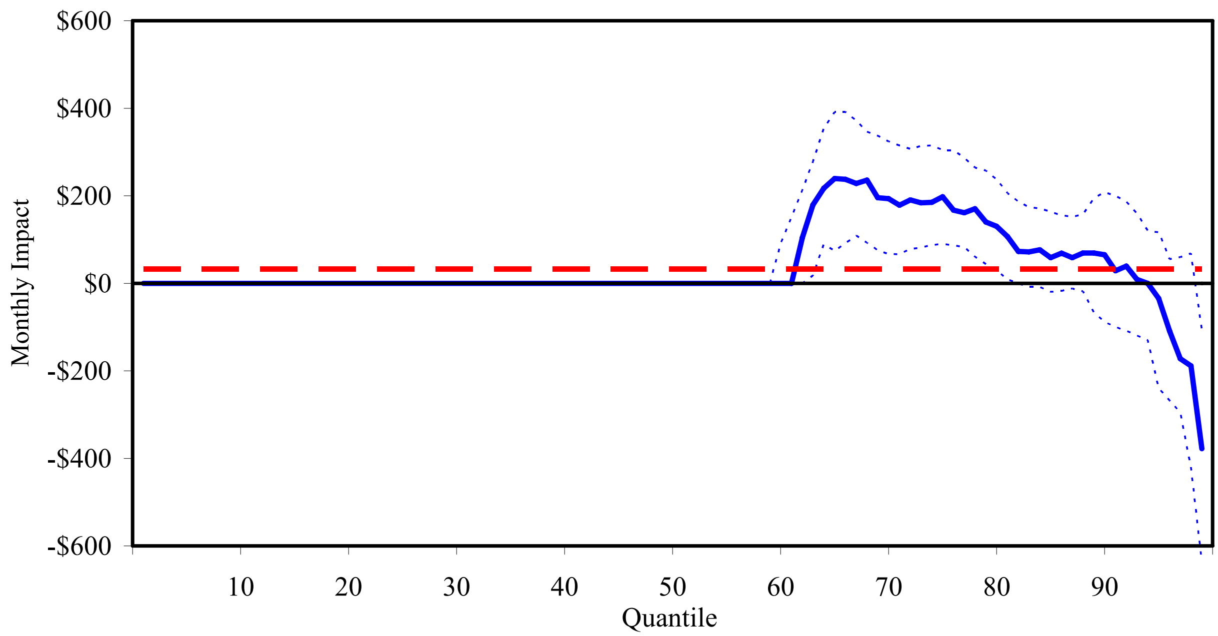


Figure 6: SSP Quantile Treatment Effects on Distribution of Monthly Earnings, Months 49-54



Notes: In each figure, the solid lines are QTEs, dotted lines are bootstrapped 90% confidence intervals (accounting for within person dependence), and dashed lines are mean treatment effects.

Figure 7: SSP Quantile Treatment Effects on Distribution of Transfers (IA + SSP)
Months 1-48

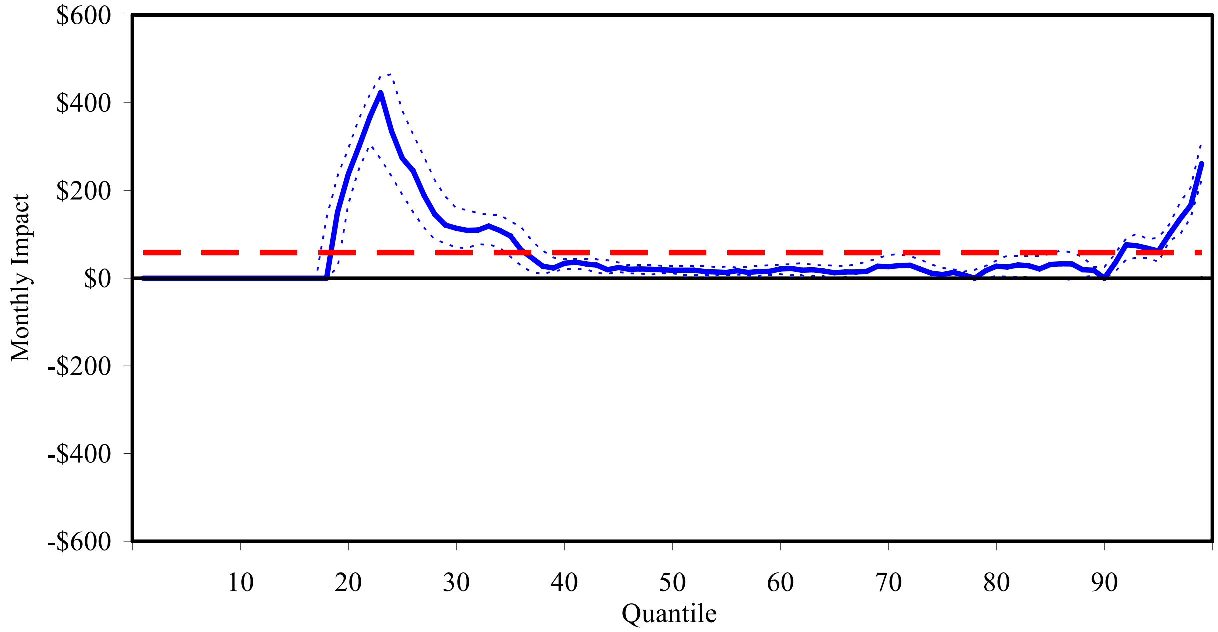
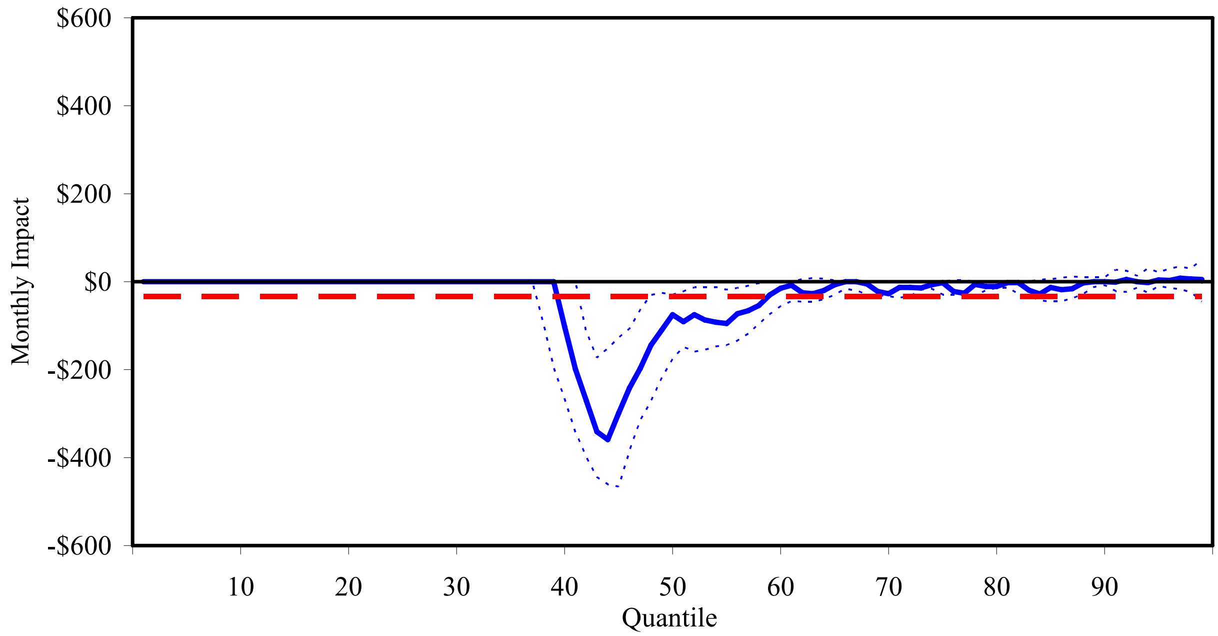
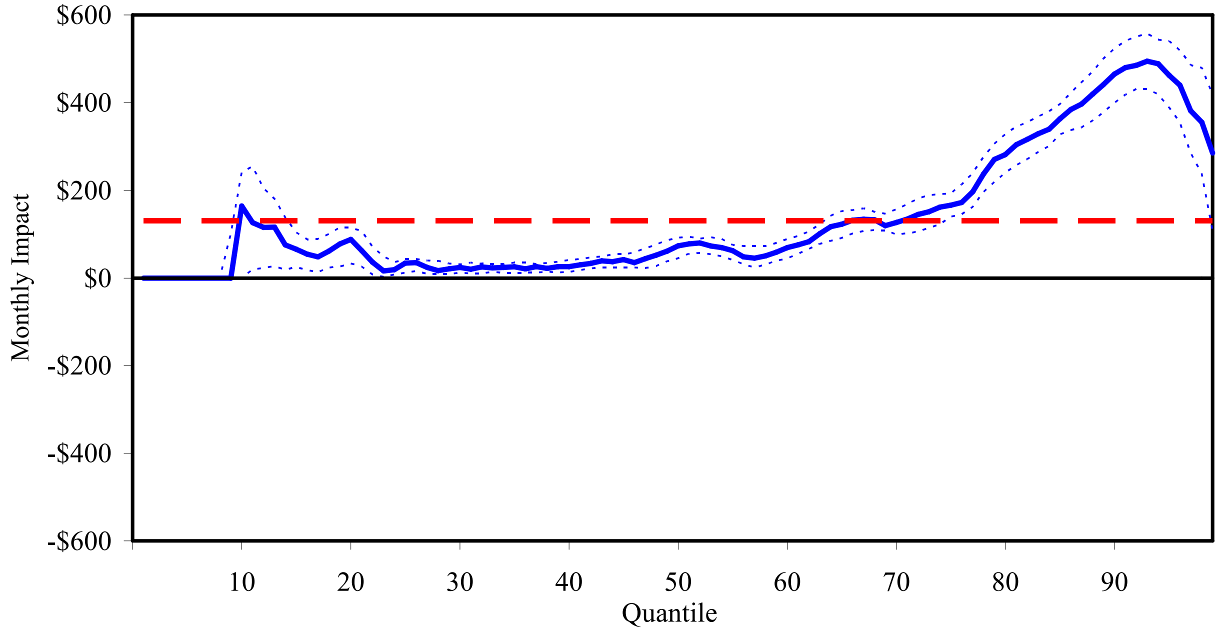


Figure 8: SSP Quantile Treatment Effects on Distribution of Transfers (IA + SSP)
Months 49-54



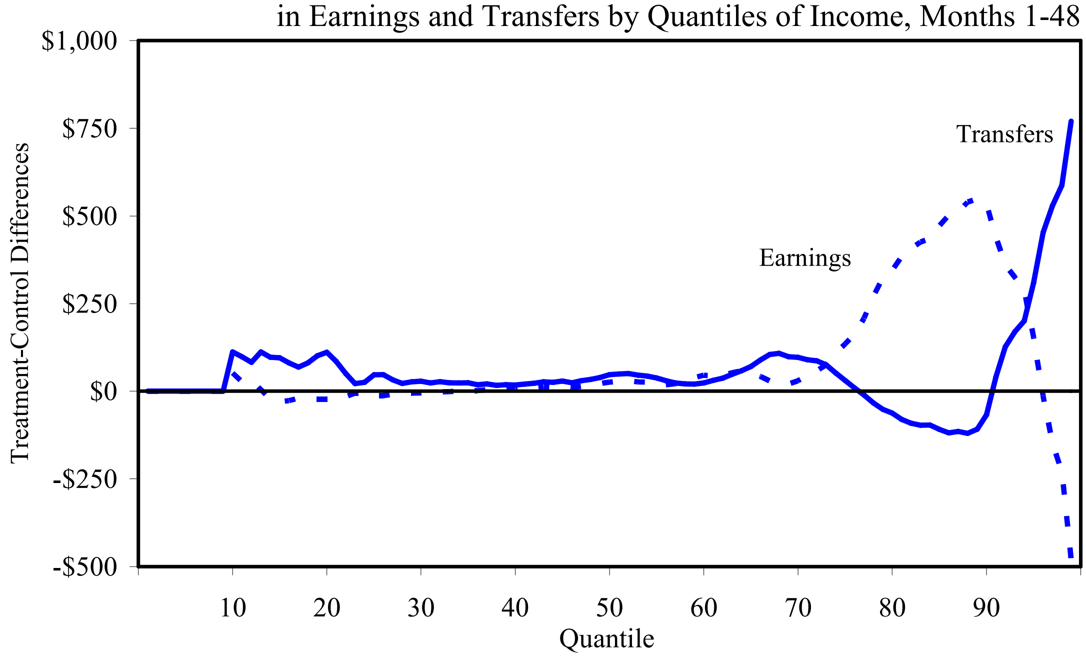
Notes: In each figure, the solid lines are QTEs, dotted lines are bootstrapped 90% confidence intervals (accounting for within person dependence), and dashed lines are mean treatment effects. Transfers include IA and SSP payments (if eligible).

Figure 9: SSP Quantile Treatment Effects on Distribution of Total Income
Months 1-48



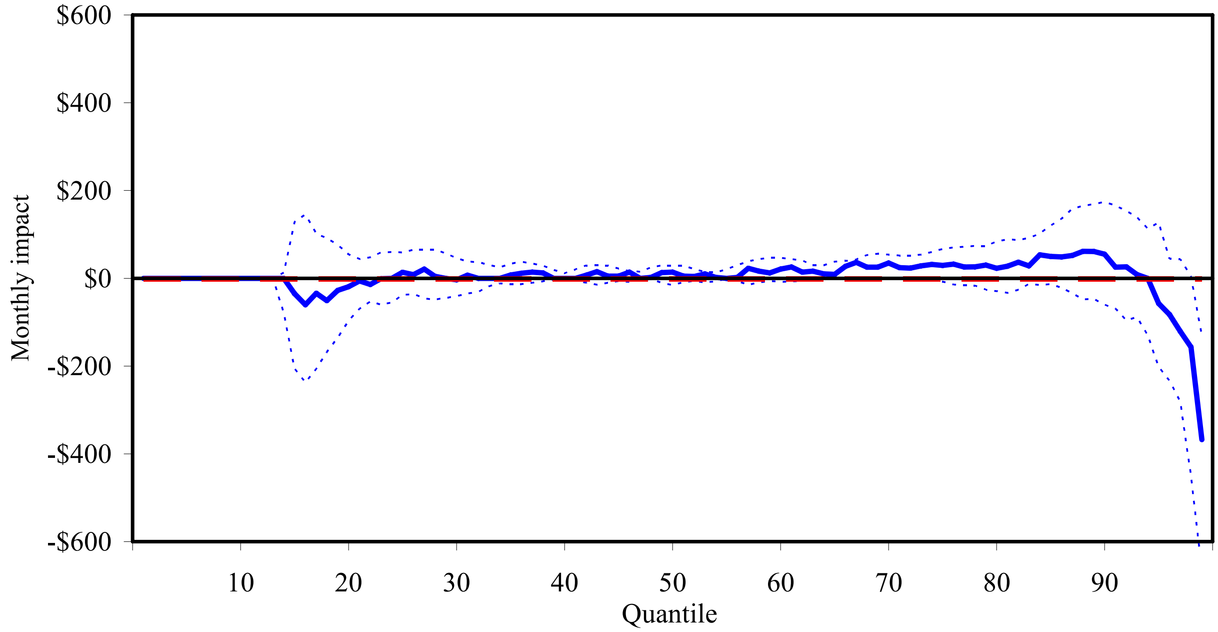
Notes: The solid line shows the QTEs, dotted lines are bootstrapped 90% confidence intervals (accounting for within person dependence), and dashed line shows the mean treatment effect. Total income includes earnings and government transfers (IA+SSP if eligible).

Figure 10: Local Nonparametric Estimates Treatment-Control Differences
in Earnings and Transfers by Quantiles of Income, Months 1-48



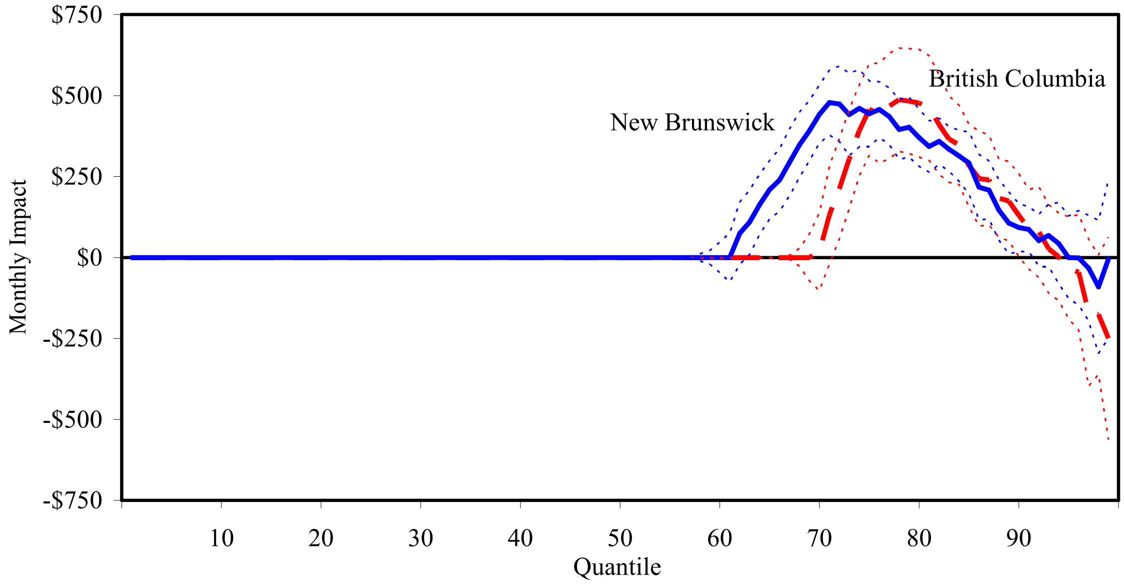
Notes: Solid line refers to the difference in transfers between the treatment and control groups and dashed line refers to the differences in earnings between the treatment and control groups. Differences at each quantile calculated by local regression (LOWESS). See text for details.

Figure 11: SSP Quantile Treatment Effects on the Distribution of Total Income
Months 49-54



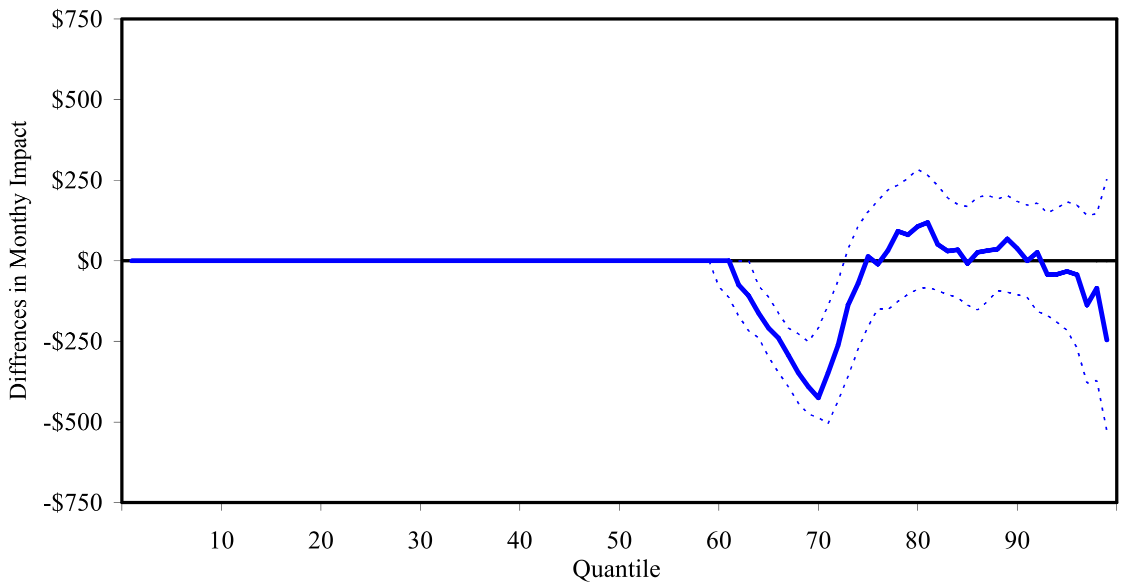
Notes: The solid line shows QTEs, dotted lines are bootstrapped 90% confidence intervals (accounting for within person dependence), and dashed line shows the mean treatment effect. Total income includes earnings and government transfers (IA+SSP if eligible).

Figure 12: SSP Quantile Treatment Effects on the Distribution of Monthly Earnings by Province, Months 1-48



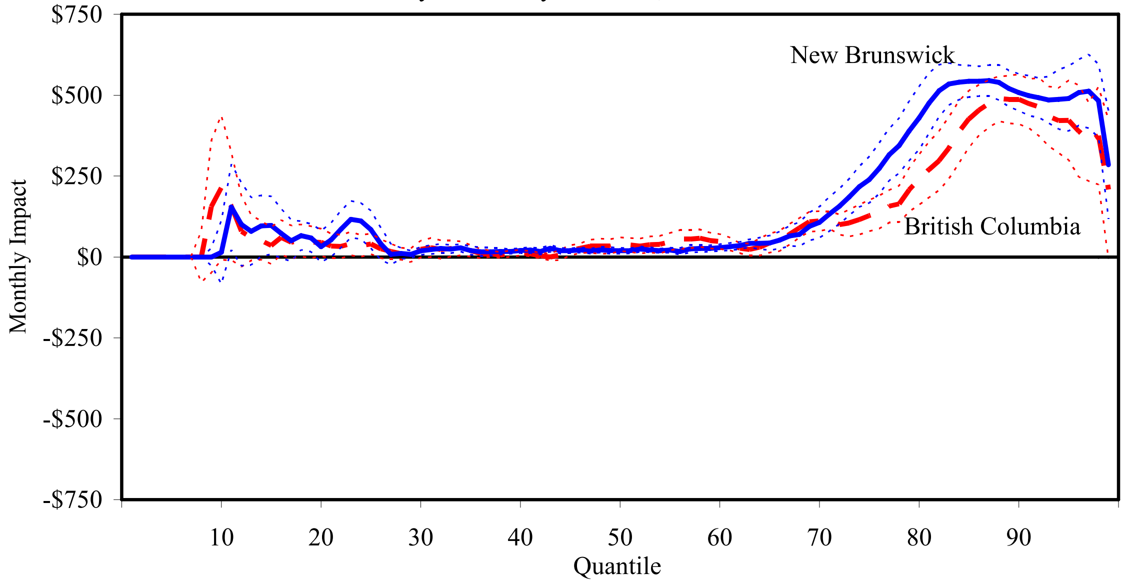
Notes: The solid line (peaks first, on the left) shows the QTEs for New Brunswick, the heavy dashed line (peaks second, on the right) shows the QTEs for British Columbia, and dotted lines are bootstrapped 90% confidence intervals (accounting for within person dependence).

Figure 13: Difference in Monthly Earnings QTEs by Province, Months 1-48



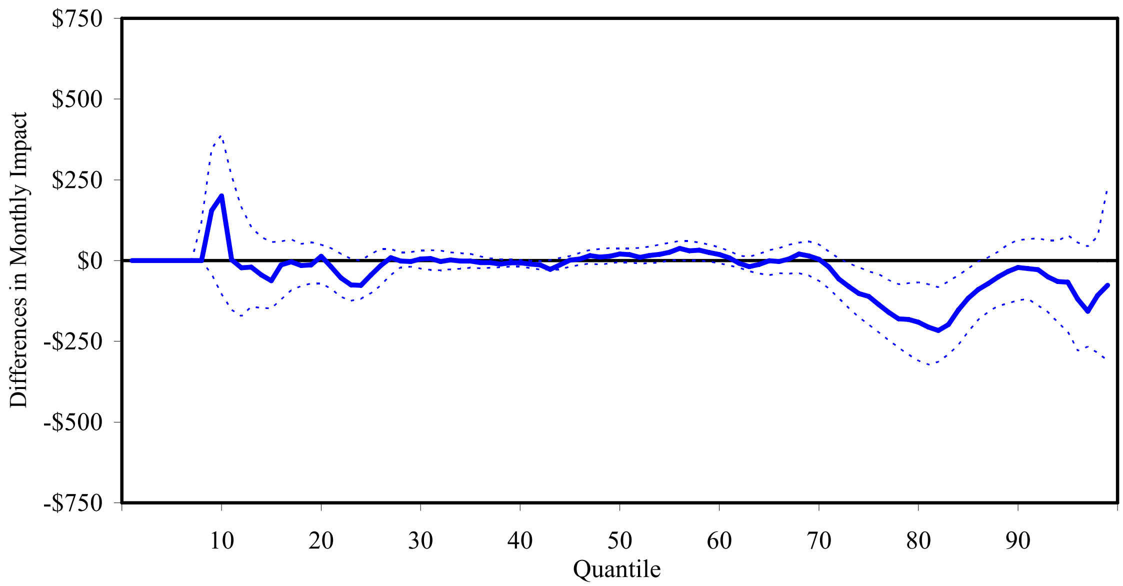
Notes: The solid line shows the difference in QTEs (the QTEs for British Columbia minus the QTEs for New Brunswick) and the dotted lines are bootstrapped 90% confidence intervals for this difference (accounting for within-person dependence).

Figure 14: SSP Quantile Treatment Effects on the Distribution of
Monthly Income by Province, Months 1-48



Notes: The solid line shows the QTE for New Brunswick (peaks second on the left and first on the right), the heavy dashed line shows the QTEs for British Columbia (peaks first on the left, and second on the right), and dotted lines are bootstrapped 90% confidence intervals (accounting for within person dependence).

Figure 15: Difference in Monthly Income QTEs by Province, Months 1-48



Notes: The solid line shows the difference in QTEs (the QTEs for British Columbia minus the QTEs for New Brunswick) and the dotted lines are bootstrapped 90% confidence intervals for this difference (accounting for within-person dependence).

Table 1: Characteristics of SSP Recipient Sample

	Means by group		
	Treatment	Control	Difference
Female Recipient	0.959	0.968	-0.009
Age	31.7	31.6	0.104
Divorced or Separated	0.477	0.473	0.004
Never-Married	0.485	0.481	0.004
First Nations	0.097	0.098	-0.001
Born Abroad	0.136	0.140	-0.005
Speaks French	0.125	0.128	-0.003
# Children under 19 in Case	1.629	1.668	-0.039
High School Dropout	0.541	0.562	-0.022
Completed Only High School	0.363	0.333	0.030*
British Columbia	0.528	0.525	0.003
Currently in School	0.146	0.136	0.009
Working Full-Time at Baseline	0.056	0.058	-0.002
Working Part-Time at Baseline	0.114	0.105	0.009
Unemployed at Baseline	0.212	0.236	-0.024*
Months on IA in 36 Months Pre-RA	30.2	29.6	0.613**
Number of Observations	1,991	1,884	

Notes: Standard errors in parentheses. ***, **, and * indicate statistical significance at the 1 per cent, 5 per cent, and 10 per cent levels, respectively. Baseline data on a small number of observations for some variables are missing (and have been set to zero). Data are for 1,991 recipients assigned to SSP and 1,884 recipients assigned to IA. Rounding done independently and thus may cause slight discrepancies in sums and differences.

Table 2: Outcomes and Mean Impacts in SSP Recipient Sample

	Months 1–48			Months 49–54		
	SSP Mean	IA Mean	Difference	SSP Mean	IA Mean	Difference
Full Sample						
Earnings	334	263	72***	455	423	32***
	(1.9)	(2.0)	(2.8)	(6.7)	(7.2)	(9.9)
Weekly Hours	10.7	7.8	2.9***	12.7	11.3	1.3***
	(0.05)	(0.05)	(0.07)	(0.2)	(0.2)	(0.2)
Average Wage	2.69	2.25	0.44***	3.49	3.28	0.22***
	(0.01)	(0.01)	(0.02)	(0.05)	(0.05)	(0.07)
IA	586	659	-73***	440	474	-34***
	(1.5)	(1.5)	(2.1)	(3.9)	(4.0)	(5.6)
IA+SSP	718	659	58***	441	474	-33***
	(1.4)	(1.5)	(2.0)	(3.9)	(4.0)	(5.6)
Total Income	1,052	922	130***	896	897	-1
	(2.1)	(1.9)	(2.9)	(5.8)	(6.3)	(8.6)
N	95,568	90,432		11,946	11,304	
British Columbia						
Earnings	347	288	59***	476	466	9
	(2.9)	(3.1)	(4.3)	(10.3)	(11.3)	(15.3)
Weekly Hours	9.5	7.2	2.3***	11.1	10.6	0.47
	(0.07)	(0.07)	(0.10)	(0.22)	(0.23)	(0.31)
Average Wage	2.84	2.54	0.30***	3.67	3.64	0.02
	(0.02)	(0.02)	(0.03)	(0.08)	(0.08)	(0.11)
IA	707	768	-62***	483	492	-9
	(2.2)	(2.2)	(3.2)	(5.8)	(6.0)	(8.3)
IA+SSP	826	768	58***	484	492	-8
	(2.2)	(2.2)	(3.1)	(5.8)	(6.0)	(8.3)
Total Income	1,173	1,057	117***	960	958	1
	(3.1)	(2.8)	(4.2)	(8.8)	(9.9)	(13.3)
N	50,448	47,472		6,306	5,934	
New Brunswick						
Earnings	320	235	85***	431	374	57***
	(2.4)	(2.6)	(3.6)	(8.5)	(8.7)	(12.2)
Weekly Hours	12.1	8.6	3.6***	14.4	12.1	2.3***
	(0.08)	(0.08)	(0.11)	(0.25)	(0.24)	(0.35)
Average Wage	2.53	1.93	0.60***	3.30	2.87	0.43***
	(0.02)	(0.02)	(0.02)	(0.06)	(0.06)	(0.08)
IA	451	539	-88***	391	454	-63***
	(1.8)	(1.7)	(2.4)	(5.2)	(5.2)	(7.4)
IA+SSP	597	539	58***	392	454	-62***
	(1.7)	(1.7)	(2.4)	(5.2)	(5.2)	(7.4)
Total Income	917	774	143***	824	828	-4
	(2.7)	(2.4)	(3.7)	(7.4)	(7.5)	(10.5)
N	45,120	42,960		5,640	5,370	

Notes: Standard errors in parentheses. ***, **, and * indicate statistical significance at the 1 per cent, 5 per cent, and 10 per cent levels, respectively (only for differences). Data are for 1,991 recipients assigned to SSP and 1,884 recipients assigned to IA. Rounding done independently and thus may cause slight discrepancies in sums and differences.

Table 3: Tests of Rank Reversal from Distribution of Observables for Ranges in Earnings, Transfers and Income Distributions

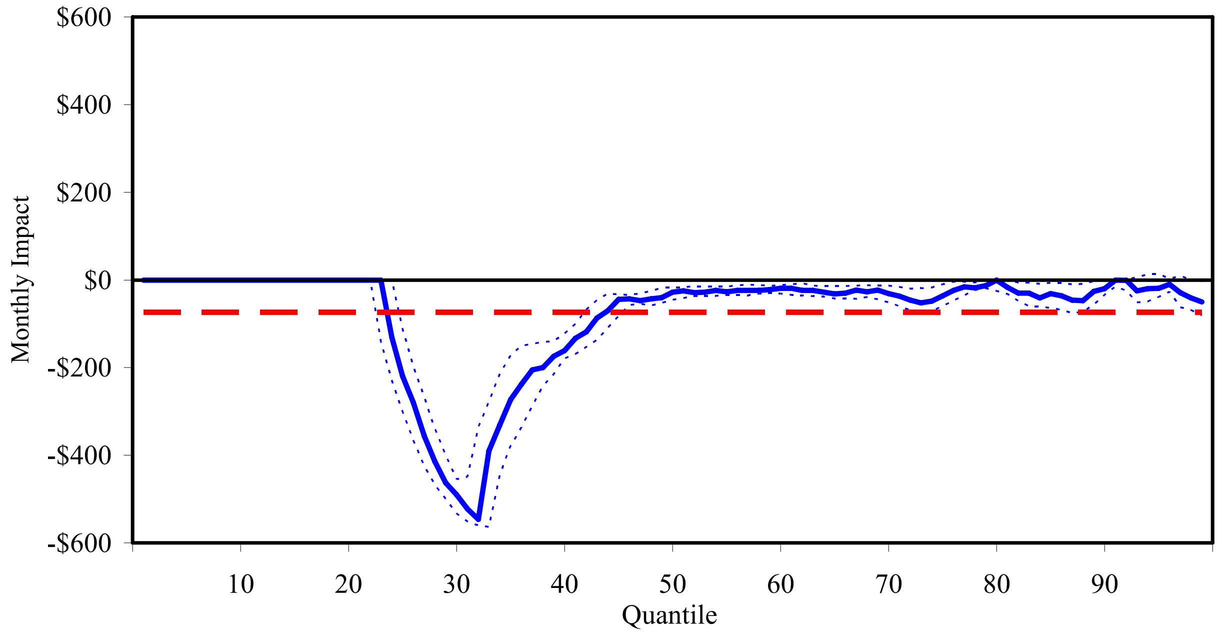
	$q \leq 50$		$50 < q \leq 75$		$q > 75$	
Earnings Distribution Ranges	Mean Diff.	p-value	Mean Diff.	p-value	Mean Diff.	p-value
Female	-0.009	0.353	0.002	0.866	-0.019	0.098
Age	-0.132	0.743	0.296	0.557	0.400	0.425
Employed Full Time	-0.001	0.735	0.040	0.004	-0.046	0.066
Asian Ancestry	-0.009	0.485	0.009	0.327	0.007	0.469
Canadian Ancestry	0.024	0.305	-0.046	0.136	0.016	0.613
Black Ancestry	-0.002	0.731	-0.009	0.244	-0.005	0.567
First Nations Ancestry	-0.009	0.523	0.034	0.060	-0.020	0.232
European Descent	-0.013	0.595	-0.039	0.188	0.026	0.405
French Speaking	0.004	0.800	-0.039	0.126	0.019	0.393
English Speaking	-0.012	0.259	-0.004	0.521	0.003	0.703
High School Dropout	-0.034	0.136	-0.076	0.022	0.055	0.076
High School Graduate, No College	0.042	0.040	0.043	0.186	-0.008	0.818

	$q \leq 25$		$25 < q \leq 50$		$50 < q \leq 75$		$q > 75$	
Transfer Distribution Ranges	Mean Diff.	p-value	Mean Diff.	p-value	Mean Diff.	p-value	Mean Diff.	p-value
Female	-0.021	0.082	-0.004	0.754	0.000	0.996	-0.011	0.345
Age	-0.098	0.850	0.374	0.527	-0.359	0.497	0.502	0.323
Employed Full Time	-0.067	0.004	0.026	0.092	0.017	0.074	0.016	0.096
Asian Ancestry	0.003	0.802	0.005	0.553	-0.010	0.355	0.001	0.946
Canadian Ancestry	0.005	0.900	0.045	0.150	-0.020	0.557	-0.011	0.752
Black Ancestry	-0.009	0.285	-0.003	0.735	0.003	0.685	-0.009	0.277
First Nations Ancestry	0.045	0.012	-0.017	0.325	-0.009	0.641	-0.022	0.277
European Descent	0.023	0.461	-0.058	0.034	0.015	0.649	-0.019	0.609
French Speaking	0.019	0.475	-0.009	0.719	-0.004	0.866	-0.016	0.172
English Speaking	-0.007	0.385	0.003	0.717	-0.007	0.469	-0.016	0.385
High School Dropout	0.027	0.381	0.003	0.928	-0.048	0.126	-0.068	0.032
High School Graduate, No College	-0.010	0.762	0.003	0.938	0.041	0.156	0.086	0.006

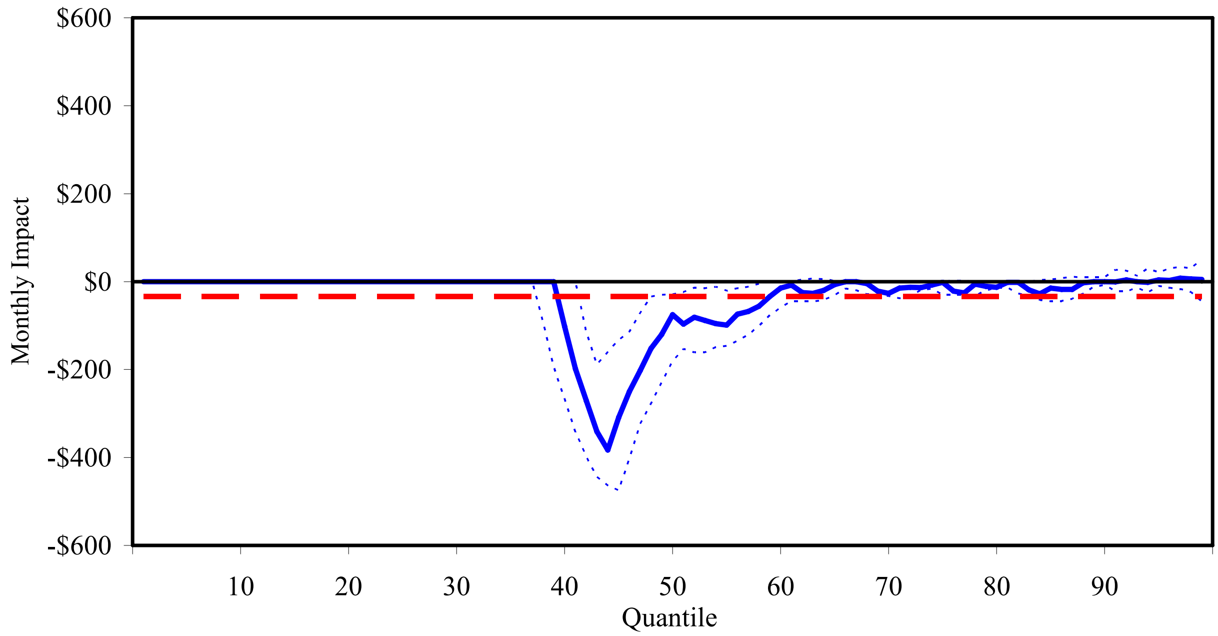
	$q \leq 25$		$25 < q \leq 50$		$50 < q \leq 75$		$q > 75$	
Total Income Distribution Ranges	Mean Diff.	p-value	Mean Diff.	p-value	Mean Diff.	p-value	Mean Diff.	p-value
Female	-0.028	0.032	0.006	0.609	-0.004	0.731	-0.009	0.403
Age	0.675	0.287	-0.254	0.657	-0.453	0.371	0.443	0.359
Employed Full Time	-0.030	0.008	-0.001	0.906	0.012	0.417	0.014	0.503
Asian Ancestry	0.009	0.393	-0.002	0.890	0.020	0.313	-0.028	0.104
Canadian Ancestry	-0.022	0.525	-0.005	0.890	-0.025	0.453	0.073	0.034
Black Ancestry	0.003	0.631	-0.001	0.890	-0.011	0.130	-0.010	0.275
First Nations Ancestry	0.039	0.032	-0.007	0.701	0.014	0.473	-0.049	0.012
European Descent	-0.018	0.559	-0.051	0.090	-0.024	0.443	0.050	0.116
French Speaking	-0.025	0.331	-0.021	0.393	-0.008	0.575	0.044	0.020
English Speaking	-0.015	0.132	-0.007	0.479	-0.024	0.148	0.020	0.076
High School Dropout	0.021	0.507	-0.020	0.587	0.002	0.922	-0.092	0.006
High School Graduate, No College	-0.019	0.585	0.034	0.263	0.027	0.361	0.078	0.018

Notes: Mean treatment-control differences and p-values for tests of individual differences being significant. Null distribution derived using method of Abadie (2002). See text for more information.

Appendix Figure 1: QTE on the Distribution of IA, Months 1-48

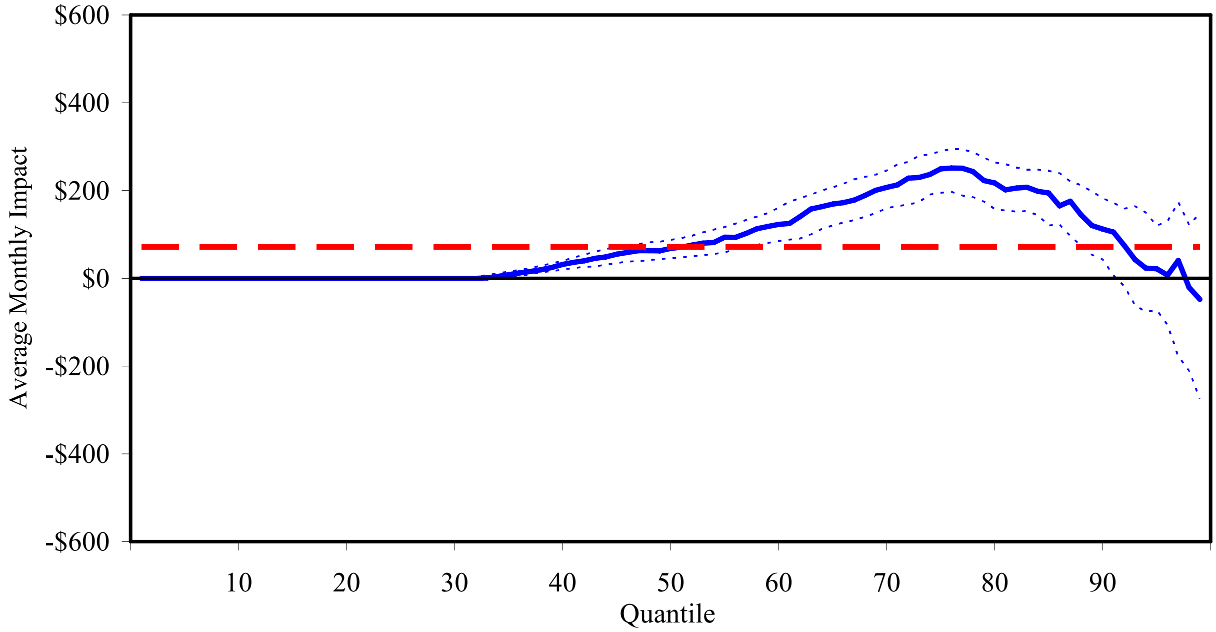


Appendix Figure 2: QTE on the Distribution of IA, Months 49-54

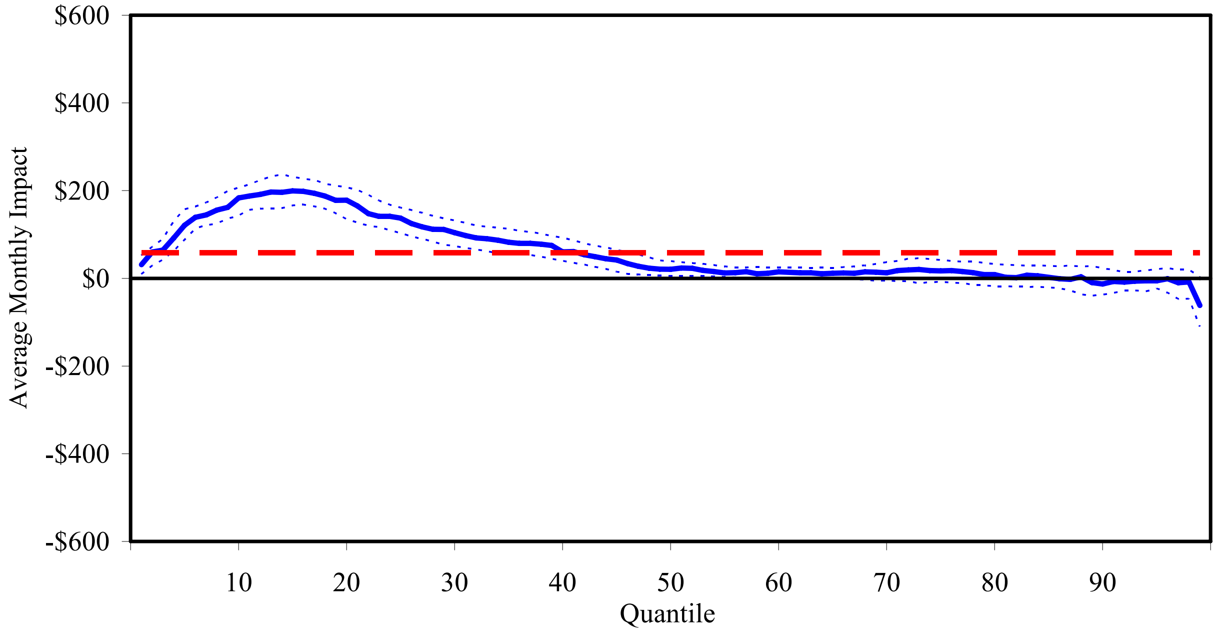


Notes: In each figure, the solid lines are QTEs, dotted lines are bootstrapped 90% confidence intervals (accounting for within person dependence), and dashed lines are mean treatment effects.

Appendix Figure 3: QTE on the Distribution of Average Monthly Earnings, Months 1-48

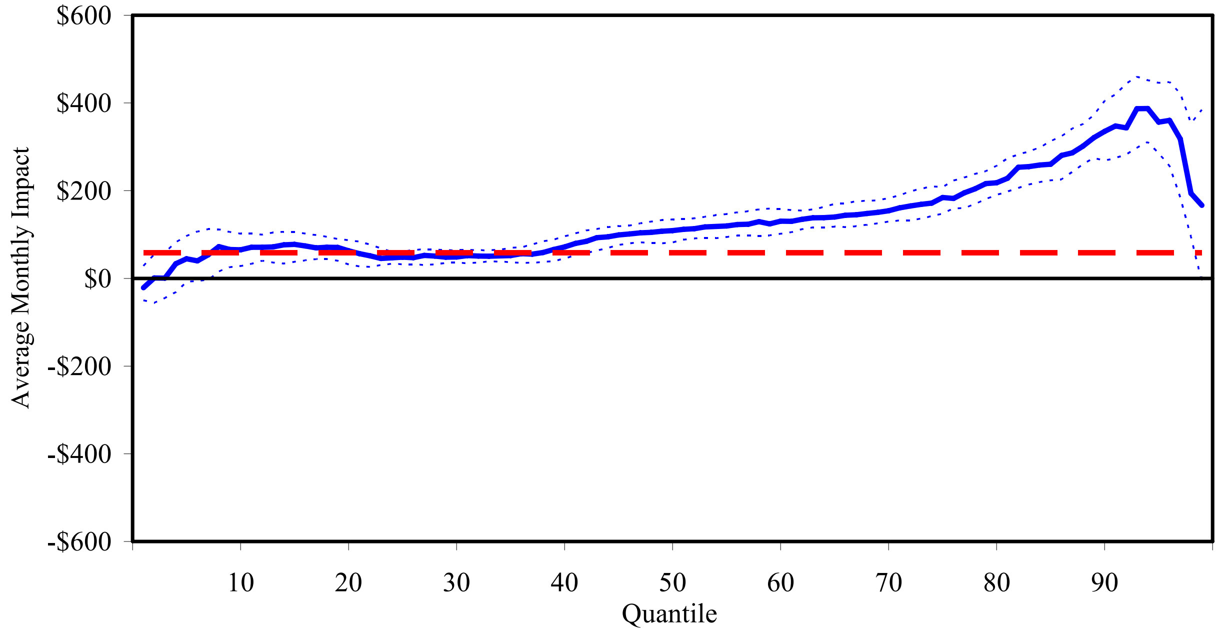


Appendix Figure 4: QTE on the Distribution of Average Monthly Transfers, Months 1-48



Notes: In each figure, the solid lines are QTEs, dotted lines are bootstrapped 90% confidence intervals (accounting for within person dependence), and dashed lines are mean treatment effects. Sample consists of one observation per person with averages over monthly amounts for months 1-48.

Appendix Figure 5: QTE on the Distribution of Average Monthly Income, Months 1-48



Notes: The solid line is the QTE, dotted lines are bootstrapped 90% confidence intervals (accounting for within person dependence), and dashed line is the mean treatment effect. Sample consists of one observation per person with averaged monthly income over months 1-48