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# Explaining and Forecasting Results of The Self-Sufficiency Project

Christopher Ferrall  
Queen's University

Department of Economics  
Queen's University  
94 University Avenue  
Kingston, Ontario, Canada  
K7L 3N6

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# Explaining and Forecasting Results of The Self-Sufficiency Project

Christopher Ferrall  
Department of Economics  
Queen's University

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## Abstract:

This paper models the Self-Sufficiency Project (SSP), a controlled randomized experiment concerning welfare. The model of household behavior includes stochastic labor market skill, job opportunities, and value of non-labor market time. All the variation within and between treatment groups, jurisdictions (provinces), demographic groups, and sub-experiments is derived from four underlying sources: policy variation, endogenous selection into the experimental samples, the SSP treatments themselves, and different mixtures over 4 underlying types. Using the variation within the treatment group is quantitatively important for identifying the complex model: Efficient GMM the parameters are estimated precisely and variation within the treatment group is much more important for identification than either variation within the control group or between treatment and control groups. The model tracks the primary moments well within sample and out-of-sample except for under-estimating the difference in the entry sample. Predictions of the estimated model are computed for different welfare reform experiments. The details of the design are critical for interpretation of the results and it appears that the small SSP+ treatment may have longer lasting impacts than the an in-sample impact analysis would suggest.

**JEL Classification:** I3, C9, J0, C5

## Keywords:

Dynamic Household Behavior, Welfare Policy, Controlled Experiments, GMM

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## I. Introduction

The Self-Sufficiency Project (SSP) was a controlled randomized experiment performed in two Canadian provinces designed to study whether long-term recipients of income assistance (i.e. welfare) respond to earnings subsidies. The main SSP treatment group, single parents on income assistance (IA) for at least one year, was offered a large supplement to earnings if and when a full-time job was acquired within one year (and the parent went off IA). The premise of the SSP, that it would induce sustained full-time employment which would generate skills and thereby substantial wage gains, was partly fulfilled. Approximately 35 percent of the treatment group qualified for the supplement and at the peak that group had a 100 percent increase in full-time work and a 70 percent increase in earnings relative to the controls. However, most of the impact disappeared soon after the supplement expired ([Michalopoulos et al. 2002](#)).<sup>1</sup> The hoped-for self-sufficiency through endogenous wage gains failed to appear. Despite this, the careful and ambitious design of the experiment provides a unique opportunity to study labor market dynamics among low-income households.

This paper describes a forward-looking model of single parent households that predicts labor market outcomes before, during, and after the SSP experiment. The SSP creates exogenous variation in budget constraints and expected income that is used to identify a rich model. The model is estimated using GMM on the first 36 months of the (roughly) 54 months of experimental data. Results from the final 18 months are compared to out-of-sample predictions from the model, and counter-factual experiments are computed. The experiments ask whether the impacts observed in the SSP are robust to modest changes in the design of the experiment. In some dimensions the parameters of the experiment do not have a great effect on the model outcomes. In other dimensions it appears unwarranted to extrapolate the experimental impact to related welfare policies without reference to a

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<sup>1</sup> Most of the work analyzing the SSP is discussed and summarized by the authors themselves in [SRDC 2006](#).

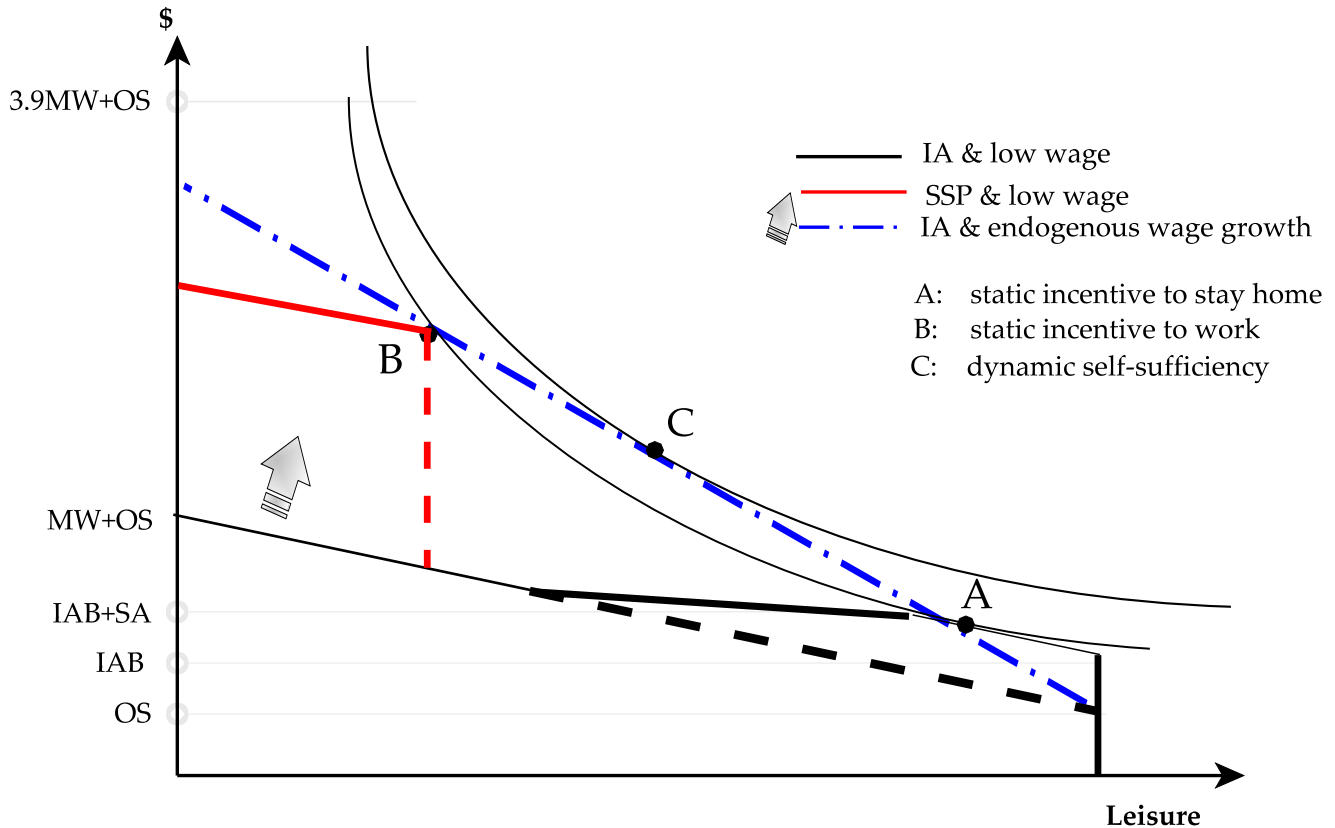
model of behavioral response to the experiment. In addition, the treatment in a small sub-experiment that provided job-finding support is found to be effective for a segment of the population.

To determine the value of using the treatment for estimation, standard errors are computed within experimental groups. For many parameters of the model the standard errors explode when based only on the control samples, quantifying the notion that experimental variation helps identify a richer model than identified by control variation alone. The richer model, if validated in other ways, may then be applicable in environments farther than the sample than a weaker model. Surprisingly, re-scaled standard errors tend to fall when based only on the treatment groups. Thus, without accounting for the direct cost of offering the treatment, it may be more efficient to have a larger treatment group than control group when basing policy predictions on a model estimated from the experiment.

Low-income single parents face a number of constraints when trying to establish a long-term attachment to the labor market. The well-known static tradeoffs between income and leisure created by the welfare are illustrated in [Figure 1](#). A household eligible for an amount IAB is precluded from IA if they take some forms of non-government outside support (denoted OS). Throughout the 1990s earnings up to  $SA = \$200$  per month could be set aside without a reduction in benefits. Thereafter benefits were replaced by earnings (in the figure 1-for-1). An indifference curve with optimal point A illustrates the disincentive to work more than part-time under this budget.

[Figure 1](#) also displays the budget under the main SSP treatment as a solid red line. The parent always has the option of receiving IA, but receipt of the supplement requires they choose not to take IA. Thus, SSP income starts at OS and has a slope equal to the wage until the full-time work requirement is met. At this point, earnings are supplemented to be half the difference between actual earnings and 3.9 times full-time earnings on a minimum wage job. The parent is indifferent between staying on IA at point A and working full time with the supplement at point B.

Figure 1. Earnings, Income, Work Hours under IA and the SSP



If the induced shift to full-time work generates wage growth (through channels such as learning-by-doing) then the untreated budget dynamically shifts up, shown as a mixed blue line in Figure 1. After treatment ends the supplement budget disappears and a new optimal choice is C. In this case the temporary SSP treatment can lessen the welfare trap through employment changes that induce sufficient wage growth.<sup>2</sup>

<sup>2</sup> Keane and Moffitt (1998) use an estimated model of income maintenance programs in the U.S. to predict that reforms without such a full-time work requirement would significantly increase total transfer payments to poor households and shift many away from full-time work.

The depth of the welfare trap is difficult to measure because it depends on how a person's current skill relates to their labor market history. An inference is required on what wages would be now if the parent had (counter-factually) worked more and more steadily in the past. Papers that address dynamic in welfare policy using non-experimental data include [Miller and Sanders \(1997\)](#), [Swan \(1998\)](#), [Kennan and Walker \(2003\)](#), [Keane and Wolpin \(2002\)](#), and [Fang and Silverman \(2003\)](#). This paper provides new evidence on what labor market policies can do to affect welfare dependency by confronting a model that captures many elements of the previous papers with the large and complex variation in static and dynamic incentives created by the SSP. Exploiting this variation yields identification of the time-varying and heterogeneous effects of income and opportunity cost that underly patterns of welfare receipt. The experimental outcomes are used while simultaneously making a household's eligibility for the experiment endogenous to the model. The results generated by the model are therefore applicable quite generally to policies that would fundamentally alter the duration and incidence of welfare receipt.

## II. The Environment

### II.A States and Parameters

In the model a household's situation each month outside the experiment is described by nine endogenous state variables:

$$\theta_{\text{end}} \equiv (l \ p \ n \ x \ b \ s \ h \ d \ k). \quad (1)$$

The state variables are listed in [Table 1](#) and described in more detail in the [Appendix](#). Briefly the variables are indicators or indices for: the parent lost their previous job; the parent worked in the previous month; the earnings offer in the current job; the parent's skill based on previous experience; the upper bound on working hours in the current job; the level of outside non-governmental support; the opportunity cost of time spent outside

the household; the observed demographic group, and the parent's unobserved type ( $\mathbf{k}$ ). The first two variables,  $l$  and  $p$ , do not affect the household's decisions directly and are tracked as state variables in order to match SSP results on job loss and quits.

The endogenous state vector  $\theta_{\text{end}}$  is contained in the overall state vector  $\theta$ , which concatenates five sub-vectors:

$$\theta \equiv \left( \theta_{\text{clock}} \quad \theta_{\text{exp}} \quad \theta_{\text{end}} \quad \theta_{\text{exog}} \quad \theta_{\text{pol}} \right). \quad (2)$$

Each state variable belongs exclusively to one of these sub-vectors. The other sub-vectors are described as needed.

The demographic index  $d$  varies across households but not over time for a given household. Characteristics treated as demographic in the SSP model are indicators for province of residence and whether the parent has two or more children. Each of the  $D = 4$  demographic groups has a vector of policy parameters,

$$\Psi_p[d] \equiv \left( \text{IAB}_d \quad \text{SA}_d \quad \text{CB}_d \quad \text{MW}_d \right). \quad (2)$$

The parameters are the maximum level of income assistance benefits, the income set-aside before benefits are clawed back, the claw-back rate on benefits, and full-time earnings on a minimum wage job. The parameter values were illustrated in [Figure 1](#) and listed below in [Table A.2](#).

The index for unobserved type,  $k$ , is also fixed for a household and determines which of  $K = 4$  vectors of exogenous parameters pertains to the household:

$$\Gamma[k] \equiv \left( \Upsilon_k \quad \Pi_k \quad \delta_k \quad \rho_k \right). \quad (3)$$

Exogenous parameters that shift utility are contained in  $\Upsilon_k$ . Parameters that shift the evolution of state variables are contained in  $\Pi_k$ . The scalar  $\delta_k$  is the discount factor, and  $\rho_k$  controls smoothing of choice probabilities. The elements of  $\Upsilon_k$  and  $\Pi_k$  and the exact roles of  $\delta_k$  and  $\rho_k$  are described in the rest of this section.

Within demographic group  $d$  household types are distributed according to  $\Lambda[d]$ , a vector with elements  $\lambda[d, k]$ . For example,  $\lambda[1, 2]$  is the second element of  $\Lambda[1]$  and equals the proportion of  $k = 2$  households in group  $d = 1$ . Only the proportions and policy parameters  $\Psi_p[d]$  vary with  $d$ . For example, no province dummy appears in the earnings-offer distribution. Instead, inter-provincial differences are generated by a different mixture of types across provinces. Also, the opportunity cost of labor market time does not depend directly on the number of children. Instead, one-child households can be a different mixture across unobserved types than 2+ households.

The exogenous vector contains all the estimated parameters:

$$\theta_{\text{exog}} = (\Lambda[1] \quad \dots \quad \Lambda[4] \quad \Gamma[1] \quad \dots \quad \Gamma[4]). \quad (4)$$

There are  $N = 19$  parameters in  $\Gamma[k]$  leading to a total of  $K(D + N) = 4(4 + 18) = 88$  exogenous parameters. Free parameters are fewer because three parameters in  $\Gamma[k]$  are constrained to be equal across type (on the presumption that they are the least likely to be identified for observed variation). Accounting for this and the fact that elements of  $\Lambda[d]$  sum to 1 results in  $3 + 4(19 - 3) + 3(4) = 79$  parameters estimated from the data.

## II.B Actions

Each month the parent chooses an action vector,

$$\alpha \equiv (m \quad a \quad i) \in \mathbf{A}(\theta), \quad (5)$$

containing three variables: labor market hours, active job search, and acceptance of income assistance. The feasible set  $\mathbf{A}(\theta)$  imposes two restrictions. First, active job search while working is ruled out:  $m > 0$  or  $a = 1$ , but not both. Second, the parent faces an upper bound on work hours:  $m \leq u(b)$  where  $b$  is a state variable specific to the current job. When the parent has no job,  $b = 0$ . With a part-time job they can only work less than  $b = \text{PT} < 1$  hours relative to full-time. PT equals the value in the SSP treatment of 75%. When holding a full-time job ( $b = 1$ ) the parent can chose to work fewer hours. A parent holding a job

who does not work at all effectively quits and loses the option to work until a new job is found and accepted.

## II.C Outcomes and Results

The combination of an action and a state,  $(\alpha, \theta)$ , is referred to as an outcome. The state next month,  $\theta'$ , is randomly determined by the transition  $P\{\theta' | \alpha, \theta\}$  (fully described in the [Appendix](#)). Not all aspects of a household's outcome can be observed by outsiders: some states and some actions are unobserved. Understanding welfare and the incentive to work involves many hidden states, including skill, job quality, and leisure-income tradeoffs. The vector of measurements made from an outcome  $(\alpha, \theta)$  is denoted  $Y(\alpha, \theta)$ . Only some endogenous state variables in the SSP model are directly available in  $Y(\alpha, \theta)$ :  $l$ ,  $p$ , and  $d$ . The action variables  $i$  and  $m$  are observed but active job search ( $a$ ) is not. The moments drawn from the data to estimate the model are based on  $Y(\alpha, \theta)$  and are described below.

## II.D Utility

Utility equals income plus outside support minus the opportunity cost of labor market time:

$$U(\alpha, \theta) = \text{Income}(\alpha, \theta) + \text{OS}(\alpha, \theta) - \text{C}(\alpha, \theta). \quad (6)$$

In turn, income is the sum of earnings, income assistance payments, and SSP payments:

$$\text{Income}(\alpha, \theta) \equiv \text{IA}(\alpha, \theta) + \text{TrueEarn}(\alpha, \theta) + \text{SUP}(\alpha, \theta). \quad (7)$$

Under-reporting of income while on IA is allowed, so measured earnings are a fraction of TrueEarn. The components of (7) are defined in the [Appendix](#). The second term in (6) is the sum of non-government transfers and additional utility (in dollar equivalent) from forgoing IA:

$$\text{OS}(\alpha, \theta) \equiv (1 - i) s \xi \text{IAB}. \quad (8)$$

The transfer component of OS is support that, if accepted, disqualifies the parent for IA. Outside support varies from month to month based on the endogenous variable,  $s$ . When  $s$  changes the parent may go off welfare and rely on other sources of support with or without any change in labor market status. A drop in  $s$  may push the parent back to receiving IA. Because OS includes foregone stigma of the static form in [Moffitt \(1983\)](#), maximum OS is expressed as a factor of  $IAB$ , the maximum amount of IA the household is entitled to. The parameter  $\xi$  is a positive exogenous value dependent on type  $k$ .

Three possible sets of feasible work hours (depending on  $b$ ) are shown in [Figure 2](#) as ranges along the x axis starting from the right at zero work hours ( $m = 0$ ). The x-axis is non-market time expressed as a fraction of full-time employment. The y-axis is dollars per month, and the discrete values of  $m$  are indicated by vertical lines. The value of  $b$  changes from one month to the next for various reasons. A non-working parent finds a job with probability  $p_j(\alpha, \theta)$ , which will have an upper bound on  $m$  of either PT or 1. A working parent loses a job permanently with probability  $\pi_l$  and results in an upper bound of 0 next month. A working parent can quit by setting  $m = 0$ . A parent who quits or is laid-off can immediately engage in job search ( $a = 1$ ), but a job offered that month begins the next month. Thus leaving or losing a job is matched to cases where the parent experiences at least one month not working. Job-to-job transitions are treated as the same job. The model attributes growth in full-time equivalent earnings between contiguous jobs as skill acquisition.

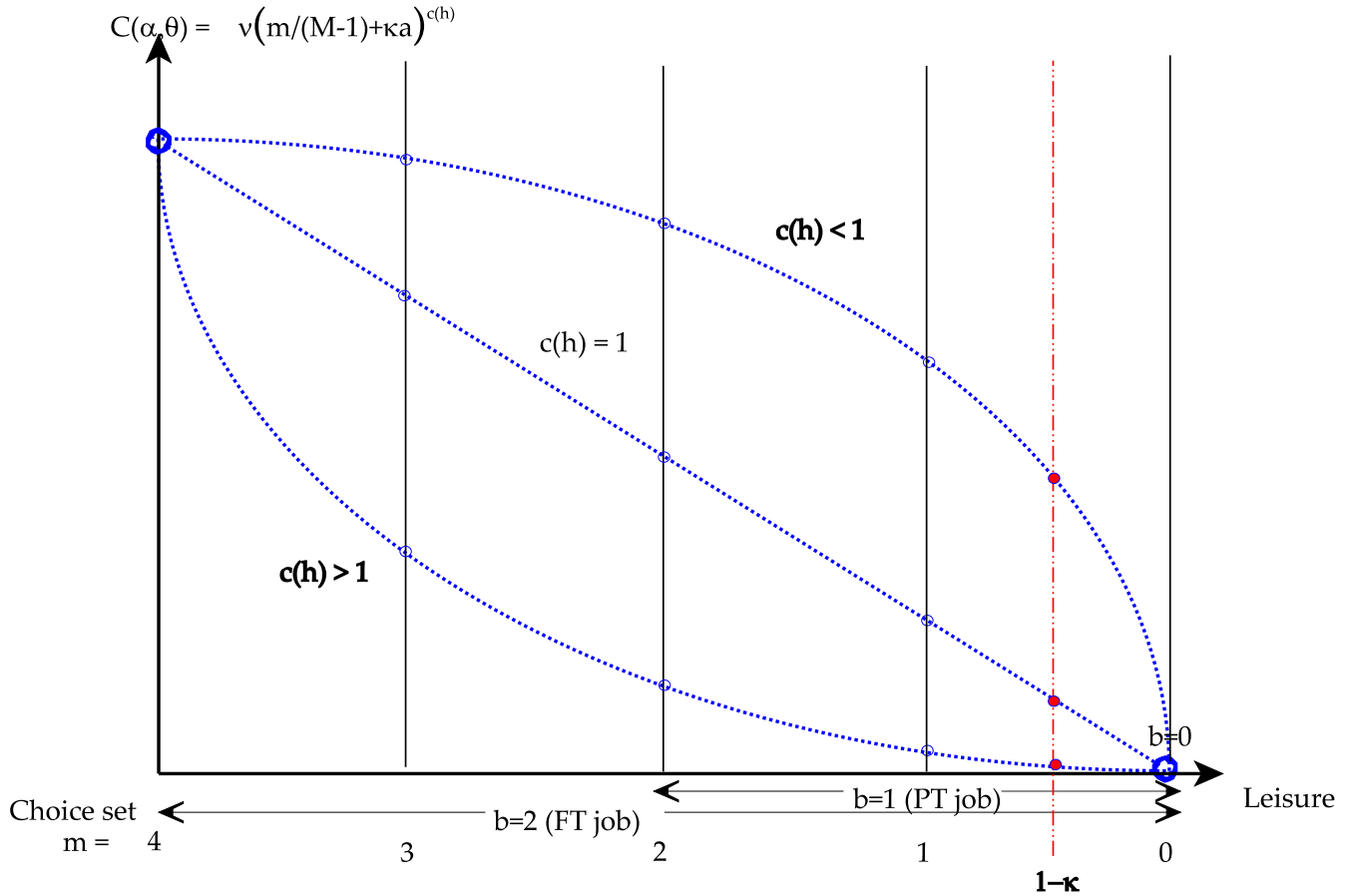
The cost of labor market time,

$$C(\alpha, \theta) = W_{\max} \nu [m + \kappa a]^{c(h)}, \quad (9)$$

is expressed as a fraction of  $W_{\max}$ , maximum possible earnings (defined later). It depends on working hours and search time when not working. Job-search is converted to work time by the exogenous parameter  $\kappa$ . The cost of full-time work is then  $\nu W_{\max}$ .

The curvature of costs is determined by  $c(h)$ . It shifts with  $h$  as described in the [Appendix](#) and as illustrated in [Figure 2](#) by three dotted lines. When not working the

Figure 2. Cost of Work Hours



parent can choose to search actively for a job and incur cost  $\nu \kappa^{c(h)}$ , which is shown on the graph along the mixed (red) line.

A shifting preference for full and part-time work hours is represented by three different costs depending on the state variable  $h$ , which jumps to a new value each month with probability  $\pi_h$ . Costs rise slowly with  $m$  when, for example, children are in school and part-time work has a low opportunity cost. Costs rise quickly with  $m$  when, for example, children are young or sick or part-time care arrangements break down. When the value

of  $h$  jumps to a new value a working parent may change hours or quit and drop out of the labor market. Either change may induce a change in welfare receipt. A non-working parent may respond to a change in  $h$  by beginning or ending active search.

## II.E Skill, Job Search and Wages

Skill is expressed as a fraction of full-skill:  $x \in \{1/4, 1/2, 3/4, 1\}$ . From month to month  $x$  either remains constant or changes by  $1/4$  with a probability that depends on labor market status. While working, skills accumulate with a probability  $m\pi_a$ . While not working, skills decrease with probability  $\pi_d$ . When  $\pi_a = \pi_d = 0$  endogenous skill accumulation and depreciation are eliminated and  $x$  becomes a permanent random effect for the parent.

The Mincer earnings function that relates skill to accumulated labor market experience assumes  $\pi_a = 1 = 1 - \pi_d$ . That is, the stock of skill accumulates linearly with experience and does not depreciate while not working. For other values of  $\pi_a$  and  $\pi_d$  welfare spells caused by transient conditions can last longer than those conditions. The longer a parent is out of work the more likely skill has fallen. Wage offers fall and become less valuable relative to time spent in the household. If a job were taken,  $x$  would eventually increase. But in the presence of IA (even with forward-looking behavior) the rate of endogenous wage growth may be too slow to make work pay.

Wages are expressed as full-time equivalent monthly earnings, denoted  $W(\theta)$ . Jobs have two characteristics,  $b$  described above and an earnings offer  $n$  that takes on 6 values. The offer  $n = 0$  is a "dead-end" job that does not depend on skill and pays MW regardless of skill. Such job offers are a fraction  $\pi_m$  of all jobs. Job offers with  $n > 0$  come from a discretized log-normal distribution with log-mean  $\mu$  and log variance  $\sigma$ .

The wage function allows for an interaction between the minimum wage, skills, the distribution of offers and the subsequent growth of earnings. To explain, start with the simple case of  $MW = 0$ . Then,  $W()$  collapses to a familiar log-linear form:

$$\ln W^0(\theta) \equiv \mu + \sigma\Phi^{-1}(n) + \eta \ln x, \quad n > 0. \quad (10)$$

The offers are percentiles of the distribution with skill shifting the distribution. Each offer is equally likely and the parameter  $\eta$  corresponds to the return to experience. In the MW=0 case  $n = 0$  is not a real offer. When  $MW > 0$  it is assumed that regular offers ( $n > 0$ ) are not each associated with its own level of earnings. Instead, for a given  $x$  let  $\phi_x$  denote the fraction of the underlying distribution below MW:

$$\phi_x = \Phi \left[ \frac{\ln(MW) - \eta \ln(x) - \mu}{\sigma} \right]. \quad (11)$$

For the lowest  $x$  the lowest two regular offers produce a wage of MW. Each offer occurs with probability  $(1 - \pi_m)\phi_x/2$ . For the next skill level ( $x = 2/4$ ) only the  $n = 1/6$  offer is at MW with probability  $(1 - \pi_m)\phi_x$ . For greater skill levels no wages other than  $n = 0$  are at the minimum wage. So  $W(\theta) = MW$  if any of three mutually exclusive indicators are true:

$$M(n, x) = \mathcal{B}[n = 0] + \mathcal{B}[x \in \{1/4, 2/4\} \ \& \ n \in \{1/6, 2/6\}] + \mathcal{B}[x = 2/4 \ \& \ n = 1/6]. \quad (11)$$

For other combinations of  $n$  and  $x$  the wage exceeds the minimum wage. Each offer is equally likely given  $x$ . Let  $\tilde{n}(x)$  be the number of offers above MW,

$$\tilde{n}(x) = 3 + \mathcal{B}[x > 1/4] + \mathcal{B}[x > 2/4]. \quad (11)$$

We arrive at the general expression for full-time earnings:

$$\begin{aligned} W(\theta) &= M(n, x)MW + \\ &\quad (1 - M(n, x)) \left( x^\eta \exp \left\{ \mu + \sigma \Phi^{-1}(\phi_x + (1 - \phi_x)/\tilde{n}(x)) \right\} \right) \\ W_{\max} &\equiv \exp \left\{ \mu + \sigma \Phi^{-1}(\phi_1 + (1 - \phi_1)/5) \right\}. \end{aligned} \quad (12)$$

For a low-skill parent, minimum wage jobs differ in their growth potential. For some offers they will wait for two increases in skill before pay increases. For others only one increase is required.<sup>3</sup> For high-skilled workers only  $n = 0$  offers start at MW. All other

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<sup>3</sup> This can (loosely) be interpreted as the employer over-paying a worker whose productivity is below MW and then eventually under-paying them once their skills increase to recoup the loss. However, no explicit bargaining or contracting model such as [Flinn \(2006\)](#) is included.

offers will increase wages with the first accumulation of skill. Skilled workers may choose to quit a low-offer job to search for a better one (depending on such things as the job offer probability and the risk of losing skill). Even a dead-end job is not really a dead-end since it is assumed skills still accumulate on them. This allows for a pattern in which people respond to the SSP subsidy in terms of employment but may not be on track to self-sufficiency because it does not create a strong incentive to low-wage jobs with growth potential versus those without.

## II.F Value and Choices

To recap, the exogenous parameters that determine utility and transitions are gathered into two vectors,

$$\Upsilon \equiv (\beta \quad \eta \quad \kappa \quad \mu \quad \nu \quad \sigma \quad \zeta \quad \xi) \quad (13)$$

$$\Pi \equiv (\pi_j \quad \pi_m \quad \pi_f \quad \pi_h \quad \pi_i \quad \pi_d \quad \pi_l \quad \pi_s \quad \pi_+).$$

Where:  $\beta$  is the rate of income reporting;  $\eta$  is the curvature in skill;  $\kappa$  converts job search into work time;  $\nu$  is the (scaled) income-equivalent cost of full-time work;  $\mu$  and  $\sigma$  determine the location and spread of wage offers;  $\zeta$  determines the variance in the curvature of time-costs over time; and  $\xi$  is the factor on outside support. The  $\Pi$  vector includes all parameters that enter the transition from one period to the next:  $\pi_j$  is the probability that active job search generates a job offer (in the absence of job-finding support);  $\pi_m$  is the proportion jobs that are true minimum wage jobs;  $\pi_f$  is the proportion of job offers that are full-time jobs;  $\pi_h$  is probability that the curvature in time-costs change;  $\pi_a$  is the probability that skills accumulate while working;  $\pi_d$  is the probability that skills decline while not working;  $\pi_l$  is the probability that a working parent loses their job exogenously;  $\pi_s$  is the probability that outside changes; and  $\pi_+$  is the parameter that determines the effectiveness of the SSP **Plus** treatment described later on.

The value of an outcome and the value of a state satisfy Bellman's equation:

$$v(\alpha, \theta) \equiv U(\alpha, \theta) + \delta E[V(\theta')] = U(\alpha, \theta) + \delta \sum_{\theta'} P\{\theta' | \alpha, \theta\} V(\theta') \quad (14)$$

$$\forall \theta \in \Theta, \quad V(\theta) = \max_{\alpha \in \mathbf{A}(\theta)} v(\alpha, \theta). \quad (15)$$

State-contingent choice probabilities are smoothed with a logistic kernel with parameter  $\rho \geq 0$ :

$$\begin{aligned} \tilde{v}(\alpha, \theta) &\equiv \mathcal{B}[\alpha \in \mathbf{A}(\theta)] \exp\{\rho[v(\alpha, \theta) - V(\theta)]\} \\ P\{\alpha | \theta\} &= \tilde{v}(\alpha, \theta) / \sum_{\alpha'} \tilde{v}(\alpha', \theta). \end{aligned} \quad (16)$$

Given  $\theta$  and the choice probabilities the expected result vector is

$$E[Y | \theta] \equiv \sum_{\alpha \in \mathbf{A}(\theta)} P\{\alpha | \theta\} Y(\alpha, \theta). \quad (17)$$

Combining endogenous choice probabilities with exogenous outcome-to-state transitions generates the state-to-state transition,  $P_s\{\theta' | \theta\}$ . Based on this transition there exists an ergodic (stationary) distribution over the endogenous variables, conditional on the non-ergodic values  $d$  and  $k$  (see [Ferrall 2003](#)). Let  $P_\infty\{\theta\}$  denote this distribution, which is the starting point for modeling the selection into the experiment.

### III. The SSP Experiment

This section provides an overview based on the schematic representation in [Figure 3](#). The [Appendix](#) provides technical details. The oval represents the set of all outcomes  $(\alpha, \theta)$  outside the experiment (the state space). This is phase 0 of the experiment, the real world before random assignment to treatment. The space is partitioned into households which receive IA ( $i = 1$ ) or not ( $i = 0$ ), the only endogenous outcome related to selection into the SSP samples.

#### III.A Experimental Samples and Treatment

Two distinct samples were studied in the SSP: the Recipient Study ( $e = 2$ ) and the Applicant Study ( $e = 1$ ). Each experimental sample had two or more treatment groups ( $g$ ). The control group is denoted  $g = G = 3$ . Within each study there was a main treatment ( $g = 2$ ). In the Recipient Study there was also and a smaller SSP Plus treatment ( $g = 1$ ). The Recipient Study selected single-parent households that had been on IA for 12 out of the last 13 months. This is simplified in the estimation and the figure to 12 consecutive months on IA. Graphically, any sequence of 12 outcomes in the on-IA partition is eligible for the Recipient Study. Households assigned to the control group remain in the real world, and their transition from pre- to post-assignment status is reflected in a change from phase 0 to phase 6, which is the real world after random assignment.

Eligible households assigned to treatment leave the outside world and enter the treatment program. The Recipient Study starts in phase 2. Unlike phase 0/6, the treatment program is non-stationary. This is represented in [Figure 3](#) as rectangular areas with a timeline below it.

The Applicant Study was conducted in British Columbia alone. To be eligible for treatment the household had to apply for IA after being off IA for at least six months. A feasible history for this sample is represented in [Figure 3](#) by six connected points in the off-IA partition followed by a month in the on-IA partition of the outcome space. If assigned to treatment they enter in phase 1.

Phase 1 and phase 2 do not treat utility, only expectations about future utility. Thus, if households were not forward looking these phases would be identical to phase 6 and measurements would be identical (in distribution) in the treatment and control groups. The treatment in phases 2-5 lasted 3 years and was discussed in the introduction and illustrated in [Figure 1](#).<sup>4</sup>

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<sup>4</sup> In the model, treatment phases 3-5 are identical and could be collapsed into a single longer phase. In the experiment they are indeed separate phases, because in at most two months per year the recipient could receive the supplement when hours fell below the full-time requirement. Modeling this facet would require an additional state variable to track months below full-time.

The SSP Plus Sample was a small treatment group selected with the same criteria as the Recipient Study in New Brunswick. Subjects were given the same supplement under the same terms, but in addition they were offered a set of employment services. This additional treatment is not represented in [Figure 1](#) which presumes a single wage is already available. Instead, the potential impact of these services is captured by a change in the job offer probability relative to otherwise identical households. The model assumes that these services enhance active search by raising the probability of a job offer each period:

$$p_j(\alpha, \theta) = a [\pi_j + \mathcal{B}[g = 1] \pi_+ (1 - \pi_j)]. \quad (18)$$

This leads to a change in the decision to search actively for a job and what is an acceptable job offer. Active job search is treated as unobserved, and no attempt is made to measure whether an eligible subject took advantage of the services. Thus,  $\pi_+$  is a measure of how effective the offer of the services is not how effective the services are given they are used.

### III.B Conditioning Variables, Impact and Predictions

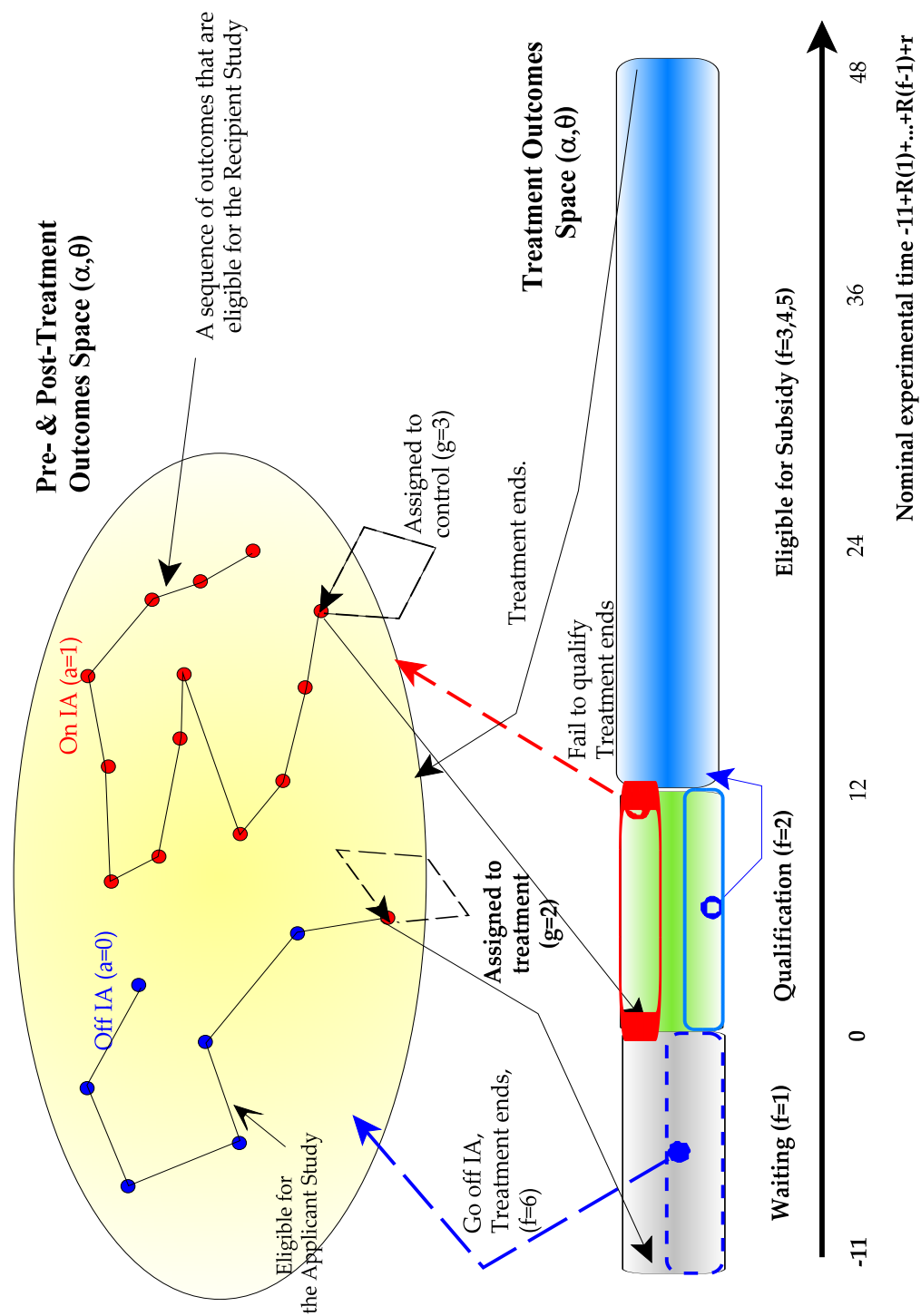
Since  $Y(\alpha, \theta)$  does not include the full outcome, the analysis must condition measurements on less information than households have. A standard impact analysis would condition only on variables that are not functions of past behavior (given eligibility). That is,

$$\theta_{\text{cond}} \equiv (t \quad g \quad e \quad d). \quad (18)$$

The variable  $t$  is experimental time. Usually this would simply be the number of periods since random assignment, but the SSP has two different studies in which subjects enter the same program of treatment at different stages. Thus, to coordinate measurements,  $t$  is set to be 0 at the beginning of phase 2, the later of the two points of entry. The expected measurement is

$$E[Y(\theta_{\text{cond}})] = \sum_{\theta} \lambda^*(k|\theta_{\text{cond}}) \Omega\{\theta \mid k, \theta_{\text{cond}}\} E[Y|\theta], \quad (19)$$

Figure 3. Sample Selection and Progress of Treatment



where (17) defines  $E[Y|\theta]$  as the expected outcome conditional on the subject's information. The Appendix defines  $\Omega(\cdot)$  as the distribution over states observed at  $\theta_{\text{cond}}$  among type  $k$  households and  $\lambda^*(\cdot)$  as the selected proportion of type  $k$ .

Let  $\hat{Y}(\theta_{\text{cond}})$  denote the vector of average observed (empirical) results conditional on the exogenous variables  $\theta_{\text{cond}}$ . Observed impact is the difference in mean results between a treated group and its control:

$$\hat{\Delta}(\theta_{\text{cond}}) \equiv \hat{Y}(\theta_{\text{cond}}) - \hat{Y}(\theta_{\text{cond}}|_G). \quad (20)$$

The notation  $|_G$  means replace  $g$  in  $\theta_{\text{cond}}$  with  $G$  ( $=3$ , the control group). The model's predicted impact is simply

$$\Delta(\theta_{\text{cond}}) \equiv E[Y(\theta_{\text{cond}})] - E[Y(\theta_{\text{cond}}|_G)]. \quad (21)$$

Some insights can be drawn from these expressions without reference to the particular model or experiment. While undergoing treatment the transitions are different from the real world, so the treatment group drifts away from its control group. Selection on unobservables is important if  $\lambda^*(k|\theta_{\text{cond}})$  differs significantly from  $\lambda[k, d]$ . Control groups are drifting as well, but they continue to follow the same transitions as outside the experiment. Their state distribution converges back to  $P_\infty$  but only given the underlying (permanent) type. Based on observables the control group outcomes converge to a different mean than outside the experiment due to selection on unobservables. Ultimately, treatment ends and treated households begin to converge to the same distribution as the controls. So the impact of treatment in a finite-lived experiment is relative to a non-stationary distribution that is converging to the same distribution as the treatment group but at a different rate.

## IV. Experimental Outcomes

### IV.A Measurements

As shown in [Table 2](#), 8,898 people who took part in the SSP experiment are included in the analysis here. Roughly two-thirds were sampled from British Columbia, because the Applicant Study was conducted in BC alone. The SSP Plus Sample includes 292 people in New Brunswick. Roughly one-half of the households had more than one child at the baseline. The results here use 36 post-assignment months of data ( $t = 1$  to  $t = 36$ ) in the Recipient Study and 30 months ( $t = -11$  to  $t = 18$ ) in the Applicant Study. The result vector is computed for each value of demographic, experimental, and treatment group and by experimental time  $t$ .<sup>5</sup>

The 12 contemporaneous variables chosen for study are summarized in [Table 3](#). The monetary variables include mean monthly earnings, mean monthly IA benefits received, and mean monthly SSP supplements received (when applicable). The means of earnings squared and IA squared are also matched because higher moments of these distributions help identify the wage offer distribution.<sup>6</sup> The remaining six results in  $Y(\alpha, \theta)$  are indicators for labor market outcomes. The mean values are therefore proportions of subjects in the given situation, including receiving any IA in the month, earning a wage within \$.10 of the current provincial minimum wage, not working this period due to quitting a job last period; not working this period due to losing a job last period, working full time (according to the SSP minimum), working part-time, and an interaction between receiving IA and working either full- or part-time.

## IV.B Experimental Impact

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<sup>5</sup> Attrition from the sample after the baseline interview is treated as an exogenous result independent of the subject's situation and the SSP treatment. According to this assumption it is valid to use either all individuals reporting results in a given month or use only those individuals who remained in the sample throughout the measurement period. Not all subjects entered the experiment in the same calendar month, so in the 36-month data file there are some observations beyond the 30 and 36 month cut-offs. For a cell's values to be included in this analysis, there had to be at least 50 observations.

<sup>6</sup> There is a lag in receiving SSP supplements and IA benefits. SSP benefits received and recorded in month  $t = 2$  are, for the most part, based on outcomes in month  $t = 1$ . For IA the lag is often two months. For this reason SSP and IA results are forwarded by one and two months so that they are (roughly) contemporaneous with the situation that generated them. This adjustment is not perfect, but it appeared to be the best fixed rule.

[Table 4](#) reports relative impacts  $(\Delta(\theta_{\text{cond}}) / Y(\theta_{\text{cond}}|_G))$  for selected variables at different values of  $t$ . At  $t_0 + 1$  relative impacts are small, as would be expected with random assignment. The only impacts that appear sizeable one month after assignment are 25% responses in earnings and full-time employment in the NB2+ and BC1 groups. By month 13 (one month after the qualification period ends) the earnings impact varies between 32% and 128%. By month 24 relative impacts are generally below the earlier maximum impact, but in many groups is still larger than the initial values. The relative impact on IA receipt is generally smaller than on earnings. By month 24 anywhere between 8% and 32% fewer subjects in the treatment groups are on IA than in the control group. The impact in the Applicant Study at month 13 is in the same range. The relative impact of the SSP treatments on the proportion of jobs at the minimum wage is typically negative and smaller than the other impacts. That is, conditional on working full or part time, a smaller proportion of the treatment groups are working at or near the minimum wage than in the control groups. The differences are small when compared to the impacts on full-time work itself, which range from 52% to 146% in the Recipient Sample.

The impact of the SSP treatment is not limited to mean values of the measured results. The co-relationship between the variables also differs across treatment groups. [Table 5](#) reports the matrix of simple correlations in seven of the results. The SSP Plus Sample was excluded and the four demographic groups were combined, leaving four entry / treatment groups. The main purpose of [Table 5](#) is to compare the same correlation between treatment and control groups. In other words, to compare entries across the diagonal. In each of the four quadrants the signs of the correlations follow similar patterns, which is not surprising given that earnings must be strongly related to work hours and negatively correlated with IA receipt. When comparing correlations across treatments and controls we see only small differences in the Applicant Study. For example, the correlation between earnings and IA benefits among the treated is -.356. Among controls the same correlation is -.360. The difference in the correlations is substantially larger in the Recipient Study, and the number

of observations greater (however they are measured). For example, the same earnings/IA correlations are -0.409 and -0.317, respectively. This is consistent with the model since treatment is milder among applicants than recipients. For a minimum of twelve months there is no direct impact of treatment on utility for recipients. The impact is felt solely through the eventual opportunity to qualify for the supplement, and this forward-looking impact is the same as that felt in the Recipient Study from the start of their post-assignment period. For the applicants the impact is discounted by  $\delta$  and by the uncertainty of finding a job. Thus, the applicant treatment group will on average appear closer to its control group than the recipient treatment group. The one caveat is that the two groups are created by nearly opposite criteria applied to IA receipt. As long as the underlying model exhibits positive correlation in IA receipt, the cross-treatment difference in correlations will indeed be smaller in the Recipient Study. The presence of skill accumulation and depreciation, along with persistence in the other household states and the IA rules themselves combine to ensure some measure of persistence in IA receipt.

Table 5 suggests that analyzing each measured result (and impact) separately is inefficient in a statistical sense. That is, earnings, IA, and full-time employment are not separate outcomes that each requires a separate sequence of impacts. More importantly, the SSP treatment is associated with differences not just in mean results, but also in correlations across contemporaneous results. Even when not using individual-level panel data, the different movements in mean results across variables through experimental time contains important information about the treatment.

## V. The Estimated Model

The model is estimated using Generalized Method of Moments by imposing the conditions that the observed and predicted values of the conditional moment vectors be

equal:

$$\check{\Delta}(\theta_{\text{cond}}) \equiv \hat{E}[Y | \theta_{\text{cond}}] - E[Y | \theta_{\text{cond}}] = 0, \quad (22)$$

for all vectors  $\theta_{\text{cond}}$  post random assignment. The interaction of  $d$ ,  $g$ ,  $e$ , and  $t$  with the twelve contemporaneous results contained in  $Y(\alpha, \theta)$  results in 4884 total moments. The [Appendix](#) describes the estimation procedure including computation of the optimal weighting matrix. It also discusses how variation across samples, treatments, provinces, experimental time and elements of the measurement vector  $Y(\alpha, \theta)$  contribute to the identification of parameters of the model.<sup>7</sup> Some technical aspects of the estimation are show in [Table 6](#).

## V.A Parameter Estimates

[Table 7](#) reports the estimated parameter vector. Since there are no coefficients on observed variables included in the parameters (as in, say, a Mincer earnings function) many of the parameters are difficult to compare with other results. Many of the values are probabilities, but their magnitudes depend on the number of values the state variables take on. For these reasons the discussion of the parameters is short whereas as discussion of the model predictions is extensive.

The estimated mixing probabilities in [Table 7.1](#) show that two types predominate in BC and three types in NB, with NB1 primarily of one of those types. Type proportions vary more across provinces than between numbers of children. The dynamic programming parameters in [Table 7.2](#) indicate that types have very different levels of patience. A period is one month. For only the first two types is  $\delta_k$  close to 1 and place a large amount of weight on the future. The other types make decisions close to a static manner: next year's outcomes have essentially no impact on today. The income reporting parameter  $\beta$

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<sup>7</sup> One can treat a conventional impact analysis as estimating each difference between treatment and control,  $\hat{\Delta}(\theta_{\text{cond}})$ , with a free parameter (the predicted impact is the observed impact). Meanwhile, the estimated model generates impact as the difference between two of the model's predictions without adding new parameters. Thus, the model estimated using GMM can be seen as a nested hypothesis within the unrestricted impact analysis. From this point of view, an impact analysis has as many parameters as moments and has no power to predict out of sample. The estimated model is parsimonious, with only 72 free parameters.

is straightforward to interpret. Three types are estimated to report approximately 40% of their income when on welfare. One type reports 95%.

Wage offer distributions differ across types (Table 7.3) as does the stigma associated with welfare (captured by the coefficient on outside support,  $\chi$ ). Full-time work has a very similar cost across type ( $\nu$ ), but recall that this value is relative to maximal earnings for a given type. This contrasts with the cost of active job search, which is only large and precisely estimated for type 1 (and to lesser extent type 4). Returns to skill and the convexity in household costs are difficult to interpret beyond their effect on predictions.

The last panel of the parameter estimates (Table 7.4) reports the transition shifters. Here we see that type 1 is constrained by a low job offer probability. Most offers are full-time, so the high fraction of part-time work reflects a choice to work fewer hours than the job allows. Between 13% and 53% of job offers are true minimum wage jobs (with no on-the-job growth potential). Estimates of the home environment indicate that outside support is highly persistent ( $\pi_s$  is small) but household costs of work and job search is not ( $\pi_h$  is high). Type 1 workers accumulate skills each period (and have rapid on-the-job wage growth). For other types growth is slower, but still only type 3 has any significant chance of further growth after one year of working. Because average wages do not accumulate in the treatment group, this suggests that the return to skill ( $\eta$ ) reported in the previous table is not large. Thus, parents achieve modest wage growth early in an employment spell but not sizeable long-term growth. Only for type 3 is depreciation of skills rapid while not working. Thus the impression the model gives for the SSP results is that the treatment requires long-term and persistent growth in skills. Skill persistence is much less of an issue than a predominance of jobs with no growth potential and a low wage elasticity to skill accumulation.

## V.B Fit to Selected Moments: Earnings, OnIA, Total Transfers

Figure 5a and Figure 5b present the observed and predicted moments for New

Brunswick and British Columbia, respectively. For both family sizes in New Brunswick the predictions track the data quite well. Selection and the evolution of state variables together generates the upward trend in the control groups as they return to the ergodic distribution. The response to treatment generates an impact that mimics the data. The one aspect of the data that the estimates fail to capture qualitatively is the slope of change in the Applicant groups (Figure 5b). The starting level and impact are accurate but the selection effect in the Applicant study is larger than the model predicts. The fraction of each group on IA is shown in Figure 6a and Figure 6b. The match to the data is similar to that for earnings, although the mismatch in the Applicant study is of a different form. For OnIA the model impact is too large before time 0. From the government budget perspective, the SSP is valuable if additional transfers during treatment result in lower transfers later on. Figure 7a and Figure 7b show total transfers, IA + SSP. Since the impact fades, the policy is a failure in total transfers. In all groups and at each month the impact on transfers is non-negative. The subsidy never induces a substantial move to self-sufficiency. In some groups the model generates a larger impact than the data, but it captures the rise and then near constant impact until month 36.

## V.C Variation from Policy, Selection and Heterogeneity

Figure 8 illustrates the combined effects of all sources of variation. Each panel shows the behavior of a particular unobserved type in all four observed environments. The two most patient types,  $k = 1$  and  $k = 2$ , are shown. Since preferences are held constant, the effect of policy variation is illustrated by comparing the four panels within each type. And since the SSP is based on a selected sample the trends in the control groups capture how distant the selected group is from the population average. The ergodic mean is shown as a triangle. For type 1 we see that all groups are well below the average in earnings. By month 36 the control group has nearly returned to the ergodic distribution. The most striking aspect of the top half of Figure 8 is the large response to SSP+, which is a combination of

a large estimate of effectiveness ( $\pi_+$ ) and a low job offer probability ( $\pi_j$ ). Type 1 is a small fraction of the NB population so the modest additional impact of SSP+ is a combination of a large individual response among a small part of the population. This same group is not particularly responsive to the SSP treatment; its households are constrained by a lack of job offers which the SSP+ alleviates. The bottom half of the Figure shows type 2. For this type the selection effect is more extreme and even after 36 months the control group is still far from the stationary average. The impact under NB policies starts very small and then becomes negative. Apparently this group was induced to accept low wage jobs to qualify for the supplement while their control group counterparts held out for better jobs. Those who qualify tend to keep these jobs until the subsidy ends. This group illustrates one of the difficulties in designing incentive schemes for low-skill parents. The SSP encourages employment but not necessarily patience to wait for employment with high growth potential. The response of type 2 is itself heterogeneous, because the opposite pattern occurs under British Columbia policies. Here the expected impact in earnings occurs and is in fact quite long lasting. However, type 2 is estimated to be a vanishingly small fraction of the population in BC.

#### **V.D Treatment: Identification or Validation?**

The estimated standard errors reported in [Table 7](#) indicate that many parameters are precisely estimated by the variation in moments generated by the experiment. The parameters are identified by restrictions on how the moments can vary across treatment groups, over time within a group, and across demographic groups. An alternative use of the exogenous variation generated by the experiment is to validate a model estimated only on the control group. [Todd and Wolpin \(2006\)](#) and [Lise et al. \(2003\)](#) follow this approach by estimating models of forward-looking agents on control groups within experiments (Progressa and the SSP, respectively) and then using the experimental data for out-of-sample validation. A major advantage of this approach is that behavior under the treatment does

not have to be solved repeatedly while estimating the parameters. The potential cost is that the model that can be estimated from the control group alone may be not be as rich as one that can be estimated using the experimental data. Thus, the parameter estimates may be less applicable outside the sample and less reliable for understanding behavior in populations facing similar but not identical environments.<sup>8</sup>

To quantify this potential cost of not using the experimental variation for estimation, the standard errors for the parameters were re-computed using only the moments within groups. Results were re-scaled to mimic a sample of the original size. [Table 8](#) reports the results. Standard errors based on all the data are compared to those from the control and treatment groups alone.<sup>9</sup> First consider the “Ctrl” column. It is not surprising that throwing out the experimental variation increases the standard errors. However, for nearly all the parameters the estimated standard error is eight times larger than when based on all the data. Included among these are key parameters for understanding dynamic behavior of low-income households: the discount factor ( $\delta$ ), the wage offer parameters ( $\mu$  and  $\sigma$ ), the return to skill ( $\eta$ ) and many probabilities that determine persistence in wages and other states. Thus, if the validation strategy had be used here, a model estimated from the control data alone would have been much simpler in form without the ability to capture some details in the experimental outcomes.

Another result is revealed in [Table 8](#) when the “all” column is compared to the “Treat” column. This counter-factual throws out the variation between treatments and controls and replaces it with more information on the experimental variation. In nearly all the cases the re-scaled standard errors are *smaller* when based on the treatment groups alone and often the increased precision is not trivial. In many cases the standard error is reduced

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<sup>8</sup> [Todd and Wolpin \(2006\)](#) suggest that if the model is validated then one might go ahead and estimate using the experimental data as well to increase efficiency of the estimates. This retains any limits on the identified model created by the limited variation in the control group.

<sup>9</sup> The standard errors in the “all” column are slightly different that in [Table 7](#) because in all calculations the columns for  $\pi_+$  were eliminated. It was eliminated because it is not identified from the “Ctrl” group, an extreme example of limited identification from variation within a group.

by 25% or more. The source of this extra precision is simply the experimental variation in incentives generated by the experimental design. Within the program of treatment the next month is quite different than the current month since one deadline or another is approaching. Within the control group no such deadlines exists.

This result has a somewhat surprising implication. When using social experiments solely to study impact ( $\Delta(\theta_{\text{cond}})$ ) a control observation and a treatment observation have equal weight. Thus, absent other costs, splitting the overall sample evenly is a reasonable design. [Table 8](#) suggests that this logic does not hold when experimental variation will be used to identify an underlying model. In this case impact is not the only outcome of interest and an additional treated observation may be more valuable than an additional control observation.<sup>10</sup> When the fixed costs of running a experiment and it is designed to estimate an economic model then the treatment group should be larger than the control group.

## **V.E Out of Sample Predictions**

[Figure 9a](#) and [Figure 9b](#) compare the model predictions to data on earnings and OnIA by province from the end of the experiment that was not available for estimation.<sup>11</sup> Not surprisingly, the impacts seen to fade in the previous figures continue to fade toward zero as treatment ends and all subjects return to the status quo. The model's prediction are similar in trend but it continue to miss the the level of earnings in the applicant sample. One pattern that is intriguing is that the impact of the SSP+ continues to lie above the regular impact even after treatment ends. The impact decays more in the model, but it also produces a lasting impact of the extra help in the SSP+ program.

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<sup>10</sup> In general, whichever group faces more exogenous variation is the key. Perhaps some experiments actually reduce variation in the subjects' situations and it is the control group that retains. However, most experiments tend to be finite-lived and in all cases they create a 'surprise' that varies across individual states. So typically the experimental group will be the one with more in-sample economic variation than the control group.

<sup>11</sup> Due to a finite research period promised to SSP participants, neither the micro data nor summary statistics for the demographic categories used in estimation are any longer. The model predictions are averaged to compute province-level predictions which are available.

## VI. Policy Experiments and Other Implications

This section conducts experiments that explore the implications of the SSP for counterfactual policy questions. In the figures the results of the hypothetical changes are compared not with the data but with the model predictions based on the SSP experiment.

### VI.A Experiments on Sample Design of the SSP

The SSP+ treatment was available only in New Brunswick and the Applicant study was conducted only in British Columbia. Using the model the missing experiments can be run. [Figure 10](#) shows total transfers for NB 1 Child and BC 2+. We see that an Applicant study in NB would have had a very large short-run impact but once treatment ended those who qualified would quit work and return to IA. The pattern makes it clear that in NB the predominant types can easily find a full-time job before time 0, stay on IA to remain eligible then at time 0 start collecting the subsidy. Since this figure extends to month 60 it also reveals an implication not shown earlier in [Figure 7a](#). Namely, the model does produce a very modest negative impact on government transfers in the NB 1 Child group. It occurs only near month 48 when all supplements are ended.

Another interesting pattern emerges if SSP+ were run in BC where job offers are a major constraint. The extra job-finding help would not only have a major impact, but it is negative almost from the start, meaning that total government transfers are cheaper under the SSP than welfare. In BC good jobs (low supplements) are available but hard to find. Impacts are long-lasting. This prediction is out-of-sample and is possibly an artifact of the model of heterogeneity. Perhaps the impact of SSP+ in job offers would not be so high in BC because with more types the concentrated effect in NB would not be shared by a large fraction of the BC population. This highlights the difficulty of drawing inferences from experiments out of sample whether it is based on a response model or an atheoretic impact analysis. Together with the modest negative impact in NB it also shows that the

hoped-for impact of the SSP and SSP+ are present in the model, but when accounting for all the outcomes the responsive households are not common.

Next, for the Recipient Study consider a sample of single parents who are on IA for exactly 6 months rather than 12 *or more* months. A practical reason for the *or more* clause is that it creates a large population to draw from and it includes long-term welfare recipients. On the other hand, if the SSP were implemented it would not be long until the people qualifying for it would only be on IA for twelve months. The stock of long-term recipients without the benefit of the SSP would no longer exist. Perhaps an experiment on the flow into the long-term recipient pool would more closely reflect results of an SSP policy after an initial transition period. Because the long-term response is so low in the recipient sample an entry condition of just six months on welfare is used. This is an out-of-sample change since many parents meeting this condition would not meet the twelve-month rule. A reverse change is made to the Applicant Study. Parents newly applying to IA after one month or more off as opposed to six months or more are eligible.

The results of this switch in stock versus flow sampling is shown in [Figure 11](#). We see that this slight change in experimental design might have had a very different pattern, at least in NB where the immediate impact is much larger although the impact still disappears rapidly once the supplement ends. The change to six-month flow sampling actually wipes out the small negative impact on total transfers in the NB 1 Child group. The conclusion is that, even if the SSP had encouraged real policy reform it may not have provided accurate guidance for the ultimate response since the stock of long-term IA recipients appears to be much different than the flow, at least in NB. For BC 2+ children households, the difference with the actual SSP sampling scheme is modest, although we see that steeper slope in the data in the Applicant group is similar to the model when "six or more months of IA" is not enforced.

## **VI.B Alternative Treatments**

Finally the model's prediction for total transfers under two alternatives to the SSP treatment are shown. One is a simpler and larger "re-employment bonus." This is a policy in which full-time employment is subsidized for just six months not three years. In addition, the subsidy is up to the full 3.9 times minimum wage earnings rather than "half-way" to that same target wage. Based on the previous result the outcome is not hard to guess, and [Figure 12](#) shows the outcome for the BC 2+ group. This group jumps at the subsidy and drives total transfers way up. But the impact on long-run behavior is even worse than under the SSP. Thus there is some benefit to the longer subsidy period, but as illustrated earlier the SSP was not precise enough to encourage taking only jobs with good wage growth potential. This simple bonus is even worse in these terms.

Finally, consider an experiment that would be difficult to run but may reflect a policy that is ultimately behind most reforms to welfare. Namely, consider offering the SSP treatment while cutting IAB by 20%. Many parents who do not anticipate finding a job will be worse off in this treatment, but real policy changes might likely combine the carrot of the SSP with a stick of reduced IA levels. [Figure 13](#) shows the effect for BC 2+ for OnIA and total transfers. Recipients respond strongly to the cut in benefits. Rates on IA are much lower during the qualifying and eligibility phases. And unlike the actual treatment the impact on total transfers are negative during the qualifying phase. But as implemented, those who failed to qualify leave treatment and return to regular IA benefits. Rates and transfers return to roughly what we see in the experiment.

## VII. Conclusion

Social experiments are designed to guide decisions based on a particular policy (the treatment). As a by-product they create exogenous variation which can be used to infer behavioral responses to other similar policies. That inference depends on a model, of course. This paper has found that results from the SSP can be modeled in a comprehensive

way. During treatment the SSP generated sizeable impacts in key outcomes that the model captures quite well, but it failed to induce any obvious long-run move to self-sufficiency. Out-of-sample prediction of the model are validated on this score as well. The model confirms the difficulty in affecting long-term outcomes for low-income households through lack of job market opportunities, slow transitory skill acquisition, and short decision horizons generated by low discount factors in some parts of the population. Counter-factual experiments confirm that related policies could induce greater short-run response. Only in the case of the SSP+ treatment is there any hint of lasting impacts among a fraction of the population. These are parents who are forward-looking and can acquire skills but have trouble securing employment. With regard to the SSP+ the model has intriguing prediction that stronger results may have been detected if the SSP+ had been run in British Columbia where the population mix contains a higher proportion of this type.

Beyond welfare policies, this paper has explored an alternative approach to combining models and experiments. Estimated standard errors computed after removing groups demonstrate quantitatively that intra-group variation generated by the treatment is critical for identifying a rich and presumably more generally applicable model of household behavior. The literature emphasizes either inter-group variation without any model of behavior or a reliance on variation within the control group for identification. In the case of the SSP either of these strategies is highly inefficient in using the costly exogenous variation generated by the experiment.

## VIII. Technical Appendix

### VIII.A Details of the Model

#### Components of Income

$$\begin{aligned}
 \text{TrueEarn}(\alpha, \theta) &\equiv mW(\alpha, \theta) \\
 \text{Earn}(\alpha, \theta) &\equiv (1 - \beta i) \text{TrueEarn}(\alpha, \theta) \\
 \text{IA}(\alpha, \theta) &\equiv i \max \left\{ \text{IAB} - \beta \text{CB} \min \left\{ \text{Earn}(\alpha, \theta) - \text{SA}, 0 \right\}, 0 \right\} \\
 Q(\alpha, \theta) &= \mathcal{B} [i = 1 \ \& \ 2 \leq f \leq 5 \ \& \ m > \text{PT} \ \& \ W(\theta) \geq \text{MW}] \\
 \text{SUP}(\alpha, \theta) &= Q(\alpha, \theta) \max \{ 0, (1 - \text{TB}) [\text{UL} \times \text{MW} - \text{Earn}(\alpha, \theta)] \}
 \end{aligned} \tag{A23}$$

**Endogenous Variables.** To describe the transition for each variable, let  $q' = q^*(\bar{q}, \{\pi_j\}, \{\mathbf{Q}_j\})$  denote a discrete variable  $q$  that has a default value of  $\bar{q}$  next period and can then jump into one  $j$  different sets of values with probability  $\pi_j$  (not the same as the model parameter). Conditional on jumping into  $\mathbf{Q}_j$  each element of the set is equally likely.

**S1. Unobserved Type:**  $k \in \{1, 2, 3, 4\}$

- ◇ Role: index into  $\Gamma$  and the mixing distribution  $\Lambda$ .
- ◇ Transition:  $k' = k^*(k, 0, \emptyset)$

**S2. Observed Type:**  $d \in \{1, 2, 3, 4\}$

- ◇ Role: index into the policy vector  $\theta_{\text{pol}}$  and the mixing distribution  $\Lambda$ .
- ◇ Transition:  $d' = d^*(k, 0, \emptyset)$

**S3. Household Time Cost:**  $h \in H = \{1/4, 2/4, 3/4\}$

- ◇ Role: determine the curvature of the time-cost function.
- ◇ Auxiliary Equations

$$c(h) = -\zeta \ln(1 - h). \tag{A24}$$

The right hand-side is the inverse exponential distribution with decay rate  $1/\zeta > 0$ . The value of  $c(h)$  determines the convexity of costs for labor market activity less than full-time. For values of  $c(h) < 1$  the cost function is concave for feasible labor market time, creating a tendency to prefer part-time work. On the other hand, costs are convex when  $c > 1$ , which creates a tendency either to stay at home or work full time.

- ◇ Equation in Text: (9)
- ◇ Transition:  $h' = h^*(h, \pi_h, H)$

**S4. Outside Support Opportunities:**  $s \in S = \{0, 1/3, 2/3, 1\}$

- ◇ Role: determines the cash-equivalent amount of support available to the parent that, if accepted, disqualifies the parent from IA.
- ◇ Equations in Text: (8)
- ◇ Transition:  $s' = s^*(s, \pi_s, S)$

**S5. Upper bound on working hours:**  $b \in \{0, \text{PT}, 1\}$

- ◇ Role: constraint on work hours in current job

- ◇ Auxiliary Equations: See feasible actions below
- ◇ Transition:

$$\begin{array}{rcl}
\frac{b'(\alpha, \theta)}{0} & \frac{P\{b'|\alpha, \theta\}}{\mathcal{B}[m > 0] \pi_l} & \\
1 & (1 - p_j(\alpha, \theta)) \mathcal{B}[b < 2] + \mathcal{B}[m = 0] & \\
2 & p_j(\alpha, \theta) \pi_f & \\
3 & p_j(\alpha, \theta) \pi_f & \\
b & \mathcal{B}[m > 0] (1 - \pi_l). & \text{(A25)}
\end{array}$$

**S6. Accumulated Skill:**  $x \in \{1/4, 1/2, 3/4, 1\}$

- ◇ Role: level of earnings and future growth potential
- ◇ Equations in Text: (10), (11), and (12)
- ◇ Transition:

$$x' = x^* \left( x, [m\pi_a + \mathcal{B}[m = 0] \pi_d], [\min\{\max\{1/4, x + \mathcal{B}[m > 0]/4 - \mathcal{B}[m = 0]/4\}, 1\}] \right) \quad (\text{A26})$$

**S7. Wage Offer:**  $n \in \{0\} \cup N = \{1/5, 2/5, 3/5, 4/5\}$

- ◇ Role: search-sensitive component of wages
- ◇ Equations in Text: (10)-(12)
- ◇ Transition: with MW=0,

$$n'(\alpha, \theta) = n^*(n, \{ap_j \pi_m, ap_j(1 - \pi_m)\}, [\{0\} \quad N]).$$

With MW>0

$$\begin{aligned}
n'(\alpha, \theta) = n^* \left( n, [ap_j \pi_m, ap_j(1 - \pi_m) \phi_x \quad ap_j(1 - \pi_m)(1 - \phi_x)], \right. \\
\left. [\{0\} \quad \{1/6, \dots, (5 - \tilde{n}(x'))/6\} \quad \{(6 - \tilde{n}(x'))/6, \dots, 5/6\}] \right). \quad (\text{A27})
\end{aligned}$$

Note that the distribution of  $n'$  depends on the contemporaneous state through the value of  $x'$ . So between periods  $x'$  must be determined before  $n'$ .

**S8. Job Loss:**  $l \in \{0, 1\}$

- ◇ Role: exogenous loss of job.
- ◇ Transition:  $l' = l^*(0, \mathcal{B}[m > 0] \pi_l, \{1\})$

**S9. Employed Previously:**  $p \in \{0, 1\}$

- ◇ Role: tracks whether the person worked last period (with  $l$  can infer the parent quit).
- ◇ Transition:  $p' = l^*(\mathcal{B}[m > 0], 0, \emptyset)$

### Actions.

**A1. Labor market hours:**  $m \in \mathbf{M} = \{0, 1/4, 1/2, 3/4, 1\}$

- ◇ Equations in Text: (9)

**A2. Active Job Search:**  $a \in \{0, 1\}$

- ◇ Equations in Text: (18), (9)

**A3. Accept Income Assistance:**  $i \in \{0, 1\}$

◇ Equations in Text: (8), (23), (23)

## Feasible Actions

$$\mathbf{A}(\theta) \equiv \{(m, a, i) \in \{\mathbf{M} \times \{0, 1\} \times \{0, 1\}\} : m < b \& ma = 0\}. \quad (\text{A28})$$

## VIII.B The SSP Experimental Design

A subject's status in the treatment program is defined by the sub-vector  $\theta_{\text{clock}} = (t, r, f)$ , where  $f$  is the current phase of treatment,  $r$  is the number of periods the subject has resided in that phase, and  $t$  is experimental time, which is defined below. The SSP program of treatment is defined by a vector of parameters,

$$\Psi_t[g] = (R[1] \quad \dots \quad R[5] \quad f_n(y) \quad PT \quad TB \quad UL). \quad (\text{A29})$$

In the SSP experiment there are seven phases, numbered from 0 to  $F = 6$ . Both  $f = 0$  and  $f = 6$  correspond to the real, non-experimental world, before random assignment (0) and after treatment has ended (6). By definition, control groups ( $g = 3$ ) transit immediately from phase 0 to phase 6. The treatment groups transit from phase 0 to the initial phase for their treatment group (listed in Table A.1). Ultimately they reach phase 6 as well. Phase 1 is the entry phase, where a parent must remain on IA for twelve months to get a chance to qualify for the SSP treatment. Phase 2 is the qualification period in which the parent becomes eligible for the SSP supplement if and when they begin a full-time job. They remain eligible for the supplement during phases 3 to 5.  $R[f]$  is the maximum duration of treatment phase  $f$ . Since each phase of the SSP lasts at most 12 months,  $R[f] = 12$  for  $f = 1, 2, \dots, 5$ . The parameter  $f_n(y)$  is shorthand for a set of deterministic transition rules for next period's phase. In other words, it describes how the SSP treatment progresses. Table A summarizes the selection, assignment, and transition rules in the SSP.

The remaining elements of  $\Psi_t$  are parameters that determine the value of the SSP supplement,  $\text{SUP}(\alpha, \theta)$ , which enters utility defined in (A6) through income defined in (A7). The full equation for  $\text{SUP}(\alpha, \theta)$  appears in (A23). The red line in Figure 1 that passes through OS and 2.9MW+OS illustrates the effect of the supplement on the household budget.

The treatment variables  $r$  and  $f$  are not useful for coordinating observations across groups. For example, one parent may take 8 months to leave phase 2 while another may take only 4 months. After seven months the first parent's clock would read (7, 2), the second (3, 3), and for all parents assigned to the control group it would read (1, 6). And the values of  $r$  and  $f$  are meaningless for parents assigned to control groups. To make results generated by the model compatible across groups a separate data clock,  $t$ , tracks the experimental month at which a measurement is taken.

## VIII.C The SSP samples

A subject's treatment group in the SSP is indexed by the sub-vector  $\theta_{\text{exp}} = (e, g)$ , where  $e$  is the experimental sample and  $g$  is the randomly assigned treatment status within samples. The Recipient Study ( $e = 3$ ) includes parents that had been on IA for at least one year. The Applicant Study ( $e = 2$ ) includes parents initiating (applying for) a

Table A.1. Program of Treatment

f	phase name	R(f)	Transition Rules		default at r=R(f)
			f <sub>n</sub> =0	otherwise	
0	pre-random assignment	1			
1	entry	12	stay on welfare (i=1)	f <sub>n</sub> =5	f <sub>n</sub> =0
2	qualification for SSP	12	i=1 or m<=PT	f <sub>n</sub> =1	f <sub>n</sub> =3
3	year 1 of eligibility	12	automatic		f <sub>n</sub> =0
4	year 2 of eligibility	12	automatic		f <sub>n</sub> =0
5	year 3 of eligibility	12	automatic		f <sub>n</sub> =0
6	post-treatment	1			

new spell of receiving IA after a period of at least six months without IA. The treatment variable  $g$  takes on three values. Besides a control group ( $g = 3$ ) and a treatment group ( $g = 2$ ), the separate SSP Plus group ( $g = 1$ ) was offered job-search and employment services in addition to the SSP supplement. Each treatment group has associated with it an initial post-assignment clock setting, a pre-assignment selection period and a sequence of feasible histories.

$$\Psi_x[e] = \left( \bar{\theta}_{\text{clock}} \quad T \quad \mathcal{H}[y; \theta_{\text{cond}}] \right). \quad (\text{A30})$$

The elements of  $\Psi_x$  are listed in Table 2. To make measurements consistent across groups the experimental clock  $t$  must be coordinated. The time  $t_0$  corresponds to the point of random assignment in the group and is normalized to 0 in the group that enters the program of treatment last. Thus  $t = 0$  at the beginning of the qualification phase ( $f = 2$ ) which is when the Recipient Study ( $e = 2$ ) is randomly assigned.

Prior to  $t_0$  is the period of sample selection. For the Recipient Study this period is of length  $T = 12$  and stretches back to  $t_{\min} = -11$ . It requires the parent receive IA each period, so only outcomes with  $i = 1$  are feasible during this time. The Applicant Study ( $e = 1$ ) is randomly assigned at  $t_0 = -11$  and the selection period is  $T = 7$  periods long, extending back to period  $t_{\min} = -17$ . In the first six periods the feasible condition is  $i = 0$ , and the last period is the condition  $i = 1$ , the start of a new spell of receiving IA. One fine point is that after random assignment the Applicant sample has already spent one month on IA and requires only eleven more months to enter phase 2. Therefore the initial clock setting has  $r = 2$ . Formally the selection criteria can be represented several different ways. Table 2 represents them as a 0/1 indicator for a measurement vector  $y$  that survives a period of selection. The indicator is denoted  $\mathcal{H}[y; \theta_{\text{cond}}]$  and it takes on either the  $i$  component of the measurement vector or its complement  $\sim i = 1 - i$  depending on time period and the entry sample.

With all of the policy vectors introduced the policy sub-vector defined as

$$\theta_{\text{pol}} = (\Psi_p \quad \Psi_x \quad \Psi_t) \quad (\text{A31})$$

Table A.2. Policy Vectors Contained in  $\theta_{\text{pol}}$ .

$\Psi_p[d]$					$\Psi_t[g]$			
d	IAB	MW <sup>a</sup>	SA	CB	g	PT	TB	UL
1	712	650	200	100%	3	0%	0%	0.00
2	755				2	75%	50%	3.90
3	982	780			1	75%	50%	3.90
4	1175							

$\Psi_x[e]$					$H[y;\theta_{\text{cond}}]$											
e	Init. r,f	T	t <sub>0</sub>	t <sub>min</sub>	-17	-16	...	-12	-11	-10	-09	...	-03	-02	-01	00
2	1,2	12	0	-11					i	i	i	...	i	i	i	i
1	2,1	7	-11	-17	~i	~i	...	~i	i							

<sup>a</sup>Provincial minimum wages changed during the experiment. These changes are not accounted for in the model but they are accounted for when classifying parents as working at a minimum wage job or not in computing mwgm. i means i=1 (on IA); ~i means I=0 (off IA).

are summarized in Table A.2.

With all transitions defined, the primitive transition function as

$$P\{\theta' | \alpha, \theta\} = \prod_{q \in \theta} \left[ \mathcal{B}[q' = \bar{q}] \left( 1 - \sum_j \pi_j \right) + \sum_j \mathcal{B}[q' \in \mathbf{Q}_j] \frac{\pi_j}{\#\mathbf{Q}_j} \right]. \quad (\text{A32})$$

This notation means to take the product over all state variables  $q$ . Each state contributes the probability that it takes on the value in  $\theta'$ , denoted  $q'$ , conditional on  $P\{\theta' | \alpha, \theta\}$ . This is computed by finding the jump set that  $q'$  is in (if any) and adding the default probability if  $\bar{q} = q'$  at  $P\{\theta' | \alpha, \theta\}$ .

#### VIII.D Conditional Distributions

To compute the selection into sample  $e$  by group  $d$ , begin by setting  $t = t_0 - T + 1$  and  $g = 3$ , which determines the value of the conditioning vector  $\theta_{\text{cond}}$ . Choose an unobserved type  $k$  and use the corresponding ergodic distribution as the starting value:

$$\Omega\{\theta' | k, \theta_{\text{cond}}\} = P_{\infty}\{\theta\}.$$

Initialize the selected weight of type  $k$  to one:

$$\omega(k; \theta_{\text{cond}}) = 1.$$

During selection the feasible choices are imposed on the choice probabilities.

$$P^* \{ \theta' | \theta \} = \sum_{\alpha} P \left\{ \theta' \middle|_{\mathcal{B}[t=t_0] \bar{\theta}_{\text{clock}}} \middle| \alpha, \theta \right\} \mathcal{H} [Y(\alpha, \theta); \theta_{\text{cond}}] P \{ \alpha | \theta \}. \quad (\text{A33})$$

The notation  $\middle|_x$  means to set elements of the state vector to  $x$  holding other elements constant. The condition  $\mathcal{B}[t=t_0]$  means this only happens at time  $t_0$ . In other words, subjects make their last choice before random assignment ignorant of the experiment. Then during the transition to the next month's state, those in a treatment group have their clocks reset to the initial clock for that experimental sample. They 'wake up' in the program treatment with all other states determined by choices before the experiment.

Working recursively forward in time first compute the fraction of type  $k$  households that make it to the next period:

$$\omega \left( k; \theta_{\text{cond}} \middle|_{t+1} \right) = \omega \left( k; \theta_{\text{cond}} \right) \left[ \sum_{\theta'} \sum_{\theta} P^* \{ \theta' | \theta \} \Omega \{ \theta | k, \theta_{\text{cond}} \} \right]. \quad (\text{A34})$$

The proportion of the unselected population that is eligible for assignment may become very small. Thus, the distribution across states is updated and re-normalized to sum to one:

$$\Omega \left\{ \theta' | k, \theta_{\text{cond}} \middle|_{t+1} \right\} = \frac{\omega \left( k; \theta_{\text{cond}} \right)}{\omega \left( k; \theta_{\text{cond}} \middle|_{t+1} \right)} \sum_{\theta} P^* \{ \theta' | \theta \} \Omega \left\{ \theta | k, \theta_{\text{cond}} \middle|_{t-1} \right\}. \quad (\text{A35})$$

Once  $t+1 = t_0$  we have the distribution eligible for random assignment. All of these calculations can be done independently (in parallel) across both  $d$  and  $k$ . But once generating the predictions after random assignment the type-specific distributions must be adjusted:

$$\lambda^*(k | \theta_{\text{cond}}) \equiv \lambda[k, d] \frac{\omega \left( k; \theta_{\text{cond}} \right)}{\sum_{k'=1}^K \omega \left( k'; \theta_{\text{cond}} \right)}. \quad (\text{A36})$$

Since the clock was set properly at  $t_0$ , the updating rules (33)-(35) apply for  $t > t_0$  as well. Since all actions are feasible after random assignment in the SSP,  $\omega \left( k; \theta_{\text{cond}} \middle|_{t+1} \right)$  becomes constant and correction factor on  $\Omega \left\{ \theta' | k, \theta_{\text{cond}} \middle|_{t+1} \right\}$  becomes one. This assumes that attrition is uncorrelated with unobserved types (and unobserved states).

### VIII.E Solving the Model and Computing Predictions

The size of the model and some technical details of the solution are listed in [Table 1](#) and [Table 6](#). The size of the system is notable. Even though each endogenous state variable is restricted to a small set of values, an individual subject can be in one of 2,304 states outside the experiment. The post-treatment infinite horizon problem requires convergence of the value function at these points, although some points in the state space are, from the subject's point of view, redundant and do not require re-solving the maximization problem (A14). For example, the household is not affected by the values of  $l$  and  $p$ , and a currently unemployed worker ( $b = 0$ ) does not care about values of  $n$ .

Since a stationary distribution  $P_\infty$  over states is computed, 16 different linear systems of size 2,304 must be solved on each iteration of the model. The SSP program of treatment adds 60 additional values of  $f$  and  $r$ . With the separate SSP Plus treatment and Applicant sample over 4 phases leads to 51,840 total states for an individual. In keeping track of all states while tracking experimental results in a total of 6,672,384 different combinations are possible. Up to 12 actions are available at each state. When aggregating over all states (including demographic, unobserved, and equivalent variation) the result is an outcome space of size 80,068,608.

The value function (A15) is solved to a level of precision under the infinite horizon. Evaluating the model ‘from scratch’ takes a bit more than an hour using a single processor of a high-end server. The required time is sensitive to the size of the discount factor  $\delta$ . This cost can be cut by roughly  $1/(16)$  through the use of 16 processors to solve in parallel the separate problems defined by  $d$  and  $k$ . Further substantial savings occur when computing numerical gradients by taking account of the limited interactions across parameters implied by a finite mixture model (Ferrall 2005). These savings are essential to making the model feasible to solve. With the computing resources currently available a full iteration of the BFGS algorithm can be completed in approximately an hour.

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### Steps in Computation.

- A0.** Set  $\theta_{\text{exog}} = \theta_{\text{exog}}^0$  and call an optimizer to minimize  $W(\theta_{\text{exog}})$ .
- A1.** To evaluate  $W(\theta_{\text{exog}})$ : Set  $d = D$ .
- A2. Solve completely for one group  $d$ .** Set  $k = K$ .
  - B0. Solve for behavior.** Set  $f = F, r = 1, g = G, e = E$ .
    - C0.** Iterate on  $V(\theta)$  in 15 to convergence.
    - C1.** Once converged, loop one more time over  $\theta_{\text{end}}$  to compute choice probabilities ( $P\{\alpha|\theta\}$  in 16) and  $E[Y|\theta]$ .
    - C2.** Solve the linear system that defines  $P_\infty$  for  $k$  and  $d$ .
    - C3. Solve for the endogenous sample in entry group  $e$ .** Set  $t = t_{\text{min}}$ .
      - D0.** From  $P_\infty$ , compute the first value of  $\omega(k; \theta_{\text{cond}})$  and  $\Omega\{\theta|k, \theta_{\text{cond}}\}$ .
      - D1.** Increase  $t$  by 1. Update  $\Omega$  and  $\omega$  by looping through all transitions.
      - D2.** Repeat previous step until  $t = t_0$ .
      - D3.** Store  $\Omega$  to be used for all  $g$  given  $e, k, d$ .
  - C4. Solve for behavior under treatment.** If  $g = G$  set  $f = 0$  and skip this part.
    - E0.** Decrease  $f$  and set  $r = R[f]$ .
    - E1.** Solve for  $V()$ , choice probabilities, and  $E[Y|\theta]$ .
    - E2.** Decrease  $r$  by 1. Return to E1 until  $r = 0$ .
    - E3.** Repeat the previous two steps until  $f = 0$ .
- C5. Compute expected outcomes given  $k$ .** Set  $t = t_0$  and restore  $\Omega$ .
  - F0.** Loop through  $\theta_{\text{end}}$  and setting the clock to  $\bar{\theta}_{\text{clock}}$ . Compute  $E[Y|k, \theta_{\text{cond}}]$  and update  $\Omega$  for the next period.

**F1.** Increase  $t$  by 1. Repeat previous step until  $t > t_{\max}$ .

**B1.** Decrease  $g$  by 1. If  $g > 0$  set  $f = F$  and return to section E.

**B2.** Decrease  $e$  by 1. If  $e > 0$  then reset  $g = 2$  and return to section D.

**B3.** Decrease  $k$ . If  $k > 0$  return to B0.

**B4. Compute empirical predictions.** Set  $e = E$ ,  $g = G$ ,  $t = t_0$ , and  $k = K$ .

**G0.** Loop over  $k$  to compute the sample-selected mixture for values of  $t$ ,  $e$ , and  $g$  that apply for  $d$ .

**G1.** Compute the contribution  $\check{\Delta}(\theta_{\text{cond}})$  to the econometric objective as defined in (22).

**G2.** Iterate on  $t$  through  $t_{\max}$ , then decrease  $g$  and  $e$  until 0.

**A3.** Accumulate  $W(\theta_{\text{exog}})$ . Decrease  $d$ . If  $d > 0$  return to step A2.

**A4.** Use the optimizer to minimize the objective with respect to  $\theta_{\text{exog}}$ .

**A5.** Iterate on the weighting matrix  $\Sigma$ , return to previous steps to compute  $\hat{\theta}_{\text{exog}}$ .

## VIII.F GMM Estimation Procedure

### VIII.F.1 First Stage

The weighted discrepancy between the data and the model used in the first stage is

$$Z^1(\theta_{\text{exog}}) \equiv \sum_{d=1}^4 \sum_{e=1}^2 \sum_{g=1}^3 \sum_{t=t_0(e)}^{t_{\max}(e)} \frac{n(\theta_{\text{cond}})}{265159} \check{\Delta}(\theta_{\text{cond}})' \Sigma_0 \check{\Delta}(\theta_{\text{cond}}), \quad (\text{A37})$$

where  $\Sigma_0$  is a  $12 \times 12$  diagonal matrix with elements listed in Table 4. For the monetary values the weights are the inverse of the grand mean of the moment over conditioning states. For the binary variables a weight of  $1/.5 = 2.0$  was chosen to avoid putting excessive weight on turnover values which are near 0 and noisy across months. The cell sizes  $n_{\text{cond}}$  (in Table B.11) sum to 265,159 in (A37). The Appendix discusses how variation across samples, treatments, provinces, experimental time and elements of the measurement vector  $Y(\alpha, \theta)$  contribute to the identification of parameters of the model.

Let  $\hat{\theta}_{\text{exog}}^1$  denote the parameters chosen to minimize  $Z^1$ . From these estimates the covariance matrix of the moments is computed. Given the random assignment to groups and the assumption that demographic groups are different (exogenous) mixtures across types, the moments are uncorrelated across groups defined by  $e$ ,  $g$  and  $d$ . That is, the sequence of observed vectors  $Y(\alpha, \theta)$  for an individual is correlated, but across entry, treatment and demographic groups the sequence of individual shocks are independent. The population covariance matrix of moments is block diagonal with non-zero entries only across  $t$ . There are 14 blocks varying in size between 326 and 417. To compute the

covariance matrix, individual paths of  $Y$  are simulated from  $\hat{\theta}_{\text{exog}}^1$  following the model. First, initial states are drawn from the ergodic distribution. Then an action vector is drawn from the choice probabilities and finally a new state is drawn from the primitive transition. Let  $Y_t^r(\theta_{\text{cond}})$  denote the  $r$ th simulated path with vectors concatenated across experimental time  $t$ . Then the deviation of the path from the mean  $E_{\tilde{\tau}}[Y | \theta_{\text{cond}}]$  is computed and weighted by the endogenous type proportion for the type within the sample. The outer product of the vector of deviations is computed and averaged across simulations. The resulting matrix is a consistent estimate of the covariance of the block of moments for the group  $\theta_{\text{cond}}$ . The inverse of the matrix is computed for each block:

$$\Sigma(\theta_{\text{cond}}) = \left( (1/R) \sum_{r=1}^R \sum_{k=1}^4 \lambda^*(\theta_{\text{cond}}) (Y_t^r(\theta_{\text{cond}}) - E_{\tilde{\tau}}[Y | \theta_{\text{cond}}]) (Y_t^r(\theta_{\text{cond}}) - E_{\tilde{\tau}}[Y | \theta_{\text{cond}}])' \right)^{-1}.$$

Based on this new weighting of the moments the parameter and fit changed a great deal. Therefore, the correlation matrices is computed once more from the new parameter values. The final stage objective is:

$$Z^2(\theta_{\text{exog}}) = \sum_{d=1}^4 \sum_{e=1}^2 \sum_{g=1}^3 \check{\Delta}_{\tilde{\tau}}(\theta_{\text{cond}})' \Sigma(\theta_{\text{cond}}) \check{\Delta}_{\tilde{\tau}}(\theta_{\text{cond}}).$$

The GMM estimates are then

$$\hat{\theta}_{\text{exog}} = \arg \min_{\theta_{\text{exog}}} Z^2(\theta_{\text{exog}}). \quad (\text{A38})$$

Let  $D(\theta_{\text{cond}})$  denote the matrix of gradients for the vector  $\Delta_{\tilde{\tau}}$  with respect to the estimated parameters. The estimated variance matrix and standard errors were computed using the standard formula

$$\hat{V}_{ar}[\hat{\theta}_{\text{exog}}] = \left\{ \sum_{d=1}^4 \sum_{e=1}^2 \sum_{g=1}^3 D(\theta_{\text{cond}}) \Sigma(\theta_{\text{cond}}) D(\theta_{\text{cond}})' \right\}^{-1}. \quad (\text{A39})$$

## VIII.G Identification

The estimated parameters are identified from three sources of variation:

- ♦ Controlled and time-varying (path of treatment and assignment to experimental group)
- ♦ Uncontrolled and time-invariant (variation in policy and demographic groups).
- ♦ Uncontrolled and time-varying (unobserved endogenous states and treatment status)

The first two sources are captured in the vector of conditioning variables  $\theta_{\text{cond}} = (t \ g \ e \ d)$ . Different loadings on these three factors will produce different patterns within months (across contemporaneous moments), across months (progress of treatment and initial selection), across studies (differing selection and information), across treatment groups (impact), and across demographic groups (variation in the mixture across exogenous types). It is not possible to prove analytically that the estimated parameters are

identified from data generated by the experiment. Instead, a heuristic argument is given. The sources of variation are appealed to roughly in the order given above.

Begin with the case of no unobserved heterogeneity ( $K = 1$ ) and a simple parameter to identify, the job-loss probability  $\pi_l$ . In the model job loss occurs exogenously and the SSP survey records reasons why a parent stop working. These were grouped into losses and quits as reported in Table 4. Thus the proportion of working parents losing a job each month is available in the data and is directly determined by the value of  $\pi_l$ . Since the observed proportions differ across demographic groups it is feasible to consider unobserved heterogeneity in  $\pi_l$  with different mixtures across groups. Of course, the estimates of  $\pi_l$  enters into all other aspects of the model.

Parents in the control group receiving IA do not quit jobs unless the convexity parameter  $c(h)$  changes value. And some parents go on and off IA with no change in labor market status, which occurs in the model only when the level of outside support changes. The measurement vector includes quits and IA status but not these conditional switch rates. However, the joint movement over time (within control groups) of IA, labor market status, and quits help identify the jump probability for  $h$  the jump probability for outside support,  $\pi_s$ . How the quit rate correlates with labor market earnings helps identify the distribution of  $c(h)$  and thus  $\zeta$ . Mean earnings and the square of mean earnings are included in  $Y(\alpha, \theta)$  so that two moments of the accepted distribution are available to match the mean and variance of the offer distribution. Wage growth and duration dependency in accepted starting wages identify the skill accumulation and depreciation parameters. The correlation between income and welfare benefits helps identify the income reporting rate.

In a stationary model estimated on non-experimental data, the job search parameters (cost of search, offer probability, proportion of full-time jobs) would have to be identified through the reservation wage and the proportion of households working part-time (along with parametric assumptions on the offer distribution already made). It is not guaranteed that they would be identified in such data. The SSP experiment, however, includes exogenous variation in the value of job search and the value of keeping a full-time job. For example, the change in the proportion of people working part-time in the first month of the SSP (relative to the controls) picks up the proportion of accepted jobs that are potentially full-time.

Now consider more subtle variation across the Applicant ( $e = 1$ ) and Recipient ( $e = 2$ ) samples. An impact study focuses on differences between a treatment group and their matched control group. For the Applicant Study, this consists of those who know the SSP subsidy exists and can anticipate becoming eligible for it (i.e. they are in phase  $f = 1$ ), and those in the control group who cannot become eligible ( $f = 6$ ). The model makes clear predictions between the behavior of these two groups. The value of taking a job and/or leaving IA changes with the time spent in phase 1. As  $r$ , the months residing in the phase, approaches  $R(1)$  the higher the value of continued receipt of IA becomes among the treated. The rate at which outcomes diverge across the two groups as  $r$  increases reflects this approach to the change in phase. The change in the value of IA across groups as  $R(1)$  approaches is sensitive to the transition probabilities. For example, high offer probabilities imply the treatment group can afford to reject offers received earlier and/or cease active job search. The pattern of impacts helps identify these probabilities, although there is no one observable difference that can be matched to each parameter.

Treated households in the Applicant and Recipient Studies are in identical situations if and when they reach the qualifying phase of the experiment ( $f = 2$ ). From that point on, any difference between the behavior of the eligible households within the two groups is, within the model, forced to come from the difference in household states conditional upon reaching phase 2. In the Recipient Study reaching phase 2 is exogenous to the SSP and unexpected, whereas for the Applicant Study it is completely endogenous and can be expected and partially controlled up to one year in advance. Thus, the two samples provide experimental variation in *unobserved* household states caused by lagged decisions made while anticipating different future opportunities. Many model parameters affect this cross-sample variation. For example, if job offers are rare then parents in the Applicant Study may not respond strongly to the information they have relative to the Recipient Study before assignment. As argued above, other variation in the data contribute to identifying parameters like job offer rates. For purposes of this discussion, if we treat the other parameters as identified without comparing the entry and applicant treatment groups, then their comparison reveals the discount factor  $\delta$ .

The final parameter to discuss is the smoothing factor  $\rho$ . When  $\rho = 0$  each feasible action has equal probability independent of the household's state. This allows for a conclusion of completely 'irrational' behavior to be drawn from the data. The estimated model avoids this result because it is required to match the overarching patterns across groups and across experimental states that indicate systematic variation in choice probabilities across states. For example, under complete irrationality, the proportion of households receiving IA each month would be the same no matter the assigned treatment group or how long ago random assignment occurred. Since statistically significant differences in choice probabilities exist across groups and experimental time, the estimated parameters will choose  $\rho > 0$ .

The point of the discussion so far is that each of the 19 exogenous parameters interacts with the design of the SSP experiment to affect specific aspects of the 12 matched results. The arguments account for the presence of many unobserved endogenous states, but they do not as yet account for unobserved exogenous parameters. Identification of unobserved heterogeneity in the parameters would be strengthened by applying the model to individual outcomes, because the likelihood or the predicted moments for a single individual would be conditioned on a single type. The computational cost of imposing these additional requirements is, however, prohibitive.

Recall that demographic variation plays a restrictive role in the model. It determines the value of the policy parameters, such as the level of IA benefits, which are pre-determined and not free to explain variation in the data. The behavior of the unobserved types will respond to the differences in the policy parameters but there are no free parameters that directly control the influence of the demographic variables on predictions. That is, there is nothing like a 'provincial coefficient' in the wage offer distribution or a 'number of children' coefficient in the cost of time. Therefore, the model greatly restricts the freedom to calibrate responses in order to match the wide variation in experimental results across demographic groups. The only way for the estimates to gain more leverage in explaining the wide variation across demographic groups is to allow variation in the within-group proportions of each type. Thus it is likely (but not obvious how to demonstrate ahead of time) that the mixture parameters  $\Lambda$  will be identified from the data along with differences in the underlying parameter vectors  $\Gamma[k]$ .

### **VIII.H Supplementary Material**

As of the writing of this draft, computer programs, data and output are available from  
[www.econ.queensu.ca/~ferrall/papers/SSPinformation](http://www.econ.queensu.ca/~ferrall/papers/SSPinformation)

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Table 1. Endogenous Variables and Actions

Item	Variable	Description	Num.	Values / Calculation	Notes
$\theta_{\text{end}}$	1	Lost job entering this month	2	{0,1}	does not affect utility or transitions
	p	worked Previous month	2	{0,1}	does not affect utility or transitions
	n	current earnings offer	6	{0,1/5,...,4/5}	
	x	eXperience level	4	{1/4,1/2,3/4,1}	
	b	upper Bound on hours in job	3	{0,1,2}	Figure 1
	s	Outside Support	4	{0,1/3,2/3,1}	Figure 1
	h	Opp. cost of time outside Household	3	{1/4,2/4,3/4}	Figure 1
Size	d	Demographic group	4	{1,2,3,4}	Table 2
	k	unobserved type	4	{1,2,3,4}	Table 7
	$\Theta_{\text{end}}$	Real states for individual ( $S_1$ )	2,304	$= 2 * 4 * 6 * 4 * 4 * 3$	l & b stored as 1 var. w/ 4 values
	$x\Theta_{\text{clock}}$	All states given assignment ( $S_2$ )	138,240	$= S_1 * 12 * 5$	Cond. on ne & ng (group assignment)
	$x\Theta_{\text{exp}}$	All individual states ( $S_3$ )	417,024	$= S_1 + S_2 * 3$	Control+SSP+ SSP_Plus+Applicant
$\alpha$	$xKxD$	Complete State Space ( $S$ )	6,672,384	$= S_3 * 4 * 4$	$K * D$
	m	labor Market work hours	5	{0,1/4,1/2,3/4,1}	constrained by u(b); see Figure 1
	a	engage in Active job search	2	{0,1}	
	i	accept IA	2	{0,1}	see Figure 1.
Size	$\text{Size}$	Feasible Action Space ( $A$ )	12	$= 6 * 2$	m & a stored as one var. with 6 values
Size	(A, $\Theta$ )	Outcome Space	80,068,608	$= S * A$	

Table 2. Demographic, Treatment, and Experimental Groups

Vector	Index	Description	Subjects	% of Total
$\theta_{end}$	d	Demographic Groups		
	1	New Brunswick, 1 Child	1728	19%
	2	New Brunswick, 2+ Children	1217	14%
	3	British Columbia, 1 Child	3058	34%
	4	British Columbia, 2+ Children	2895	33%
		Total	8898	100%
$\theta_{exp}$	g	Treatment Groups		
	3	Control	4305	48%
	2	SSP Treatment	4300	48%
	1	SSP+ Treatment (NB only)	293	3%
		Total	8898	100%
	e	Experimental Groups		
	2	Recipient Study	5682	63%
	1	Applicant Study (BC only)	3316	37%
		Total	8998	100%

Observations dropped: invalid or missing age, high school attendance or number of children (from IA records).

Table 3. Experimental Results (Moments) Selected for Matching

Var.	Description	Model	Unit	Count	Mean	St.Dev	Note
earn	Rep. Earnings	$(1-\beta i)mW(\alpha, \theta)$	\$100	450	3.439	1.564	
earnsq	Earnings Sq.	$\text{earn}^2$	$\$100^2$	470	62.590	43.470	
ia	IA Received	$IA(\alpha, \theta)$	\$100	466	5.966	1.869	Fwd.2 mth
iasq	IA Recv Sq.	$IA^2$	$\$100^2$	466	57.782	27.593	Fwd.2 mth
gsu	SSP Suppl	$SUP(\alpha, \theta)$	\$100	240	1.530	0.600	Fwd.2 mth
onia	Received IA	$i$	0/1	470	0.708	0.161	
mwg	Worked at MW	$(n^* < 6 - \#n)(m > 0)$	0/1	470	0.777	0.059	hrly w $\leq$ MW+\$0.10
leftjb	Left/quit a job	$p(l=0)(m=0)$	0/1	456	0.003	0.004	Excl. job-to-job
lossjb	Loss a job	$l$	0/1	456	0.004	0.004	Excl. job-to-job
emft	Full Time	$m > PT$	0/1	470	0.223	0.088	
empt	Part-time	$0 < m \leq PT$	0/1	470	0.130	0.023	
onXem	IA & Working	$ia * (m > 0)$	0/1	470	0.161	0.045	
Total				4884			

A summary of the complete data listed in Table B.1-B.10. Count is the number of cells in Table B panel. Mean and standard deviation are across cells not individuals.  $\#n$  denotes the order of  $n$  in the feasible set. For example,  $\#0 = 1$ ,  $\#1/6 = 2$ , etc.

Table 4. Relative Impacts on Selected Moments in Months -11,1,13,25

Var.	t	NB / 1 Child		NB / 2+		BC / 1 Child		BC / 2+	
		Recipients		Recipients		Appl.	Recipients	Appl.	Recipients
		SSP+	SSP	SSP+	SSP	SSP	SSP	SSP	SSP
Earn	-10					(0.15)		0.13	
	1	(0.02)	(0.02)	(0.06)	(0.25)	0.01	(0.25)	(0.61)	0.04
	13	0.51	0.39	1.28	0.89	0.32	0.53	0.35	0.67
	24	0.58	0.30	0.74	0.70		0.20		0.31
OnIA	-11					0.01		(0.01)	
	1	0.00	0.00	0.00	0.00	0.03	0.00	0.05	0.00
	13	(0.24)	(0.19)	(0.29)	(0.19)	(0.21)	(0.09)	(0.18)	(0.08)
	24	(0.32)	(0.18)	(0.20)	(0.18)		(0.11)		(0.08)
Mwgm	-11					0.01		(0.02)	
	1	0.00	(0.01)	(0.04)	0.00	0.00	0.01	(0.05)	(0.03)
	13	(0.13)	(0.11)	(0.15)	(0.13)	(0.12)	(0.13)	(0.12)	(0.11)
	24	(0.06)	(0.05)	(0.12)	(0.07)		(0.04)		(0.05)
Emft	-11					(0.16)		(0.06)	
	1	0.11	(0.07)	(0.09)	(0.25)	0.06	(0.25)	0.12	0.21
	13	0.97	0.76	1.65	1.46	0.36	0.98	0.52	1.17
	24	0.90	0.64	0.86	0.90		0.57		0.68

Difference between SSP and Ctrl columns in Table A divided by Ctrl column. Negative impacts in () and in red. Largest absolute impact within the table shaded for each moment.

Table 5. Contemporaneous Correlations Across Results

<b>Applicants (e=1)</b>						
	earn	ia	onia	mwg	left	emft
earn		<i>-0.360</i>	<i>-0.314</i>	<i>-0.678</i>	<i>-0.029</i>	<i>0.655</i>
ia	<b>-0.356</b>		<i>0.733</i>	<i>0.360</i>	<i>-0.002</i>	<i>-0.375</i>
onia	<b>-0.303</b>	<b>0.720</b>		<i>0.302</i>	<i>-0.011</i>	<i>-0.338</i>
mwg	<b>-0.668</b>	<b>0.379</b>	<b>0.303</b>		<i>0.047</i>	<i>-0.677</i>
left	<b>-0.029</b>	<b>0.008</b>	<b>0.004</b>	<b>0.050</b>		<i>-0.041</i>
emft	<b>0.640</b>	<b>-0.400</b>	<b>-0.353</b>	<b>-0.672</b>	<b>-0.044</b>	
						Group (g) ; Obs.
						(3); 42,056
						(2); 40,875

<b>Recipients (e=2)</b>						
	earn	ia	onia	mwg	left	emft
earn		<i>-0.317</i>	<i>-0.294</i>	<i>-0.550</i>	<i>-0.011</i>	<i>0.564</i>
ia	<b>-0.409</b>		<i>0.692</i>	<i>0.330</i>	<i>-0.018</i>	<i>-0.369</i>
onia	<b>-0.396</b>	<b>0.733</b>		<i>0.267</i>	<i>-0.021</i>	<i>-0.324</i>
mwg	<b>-0.608</b>	<b>0.402</b>	<b>0.376</b>		<i>0.024</i>	<i>-0.576</i>
left	<b>-0.018</b>	<b>-0.009</b>	<b>-0.027</b>	<b>0.031</b>		<i>-0.020</i>
emft	<b>0.638</b>	<b>-0.503</b>	<b>-0.511</b>	<b>-0.611</b>	<b>-0.030</b>	
						Group (g) ; Obs.
						(3); 95,302
						(2); 96,220

Table 6. Summary of the Estimation

Item	Value	Note
Size of linear system to compute ergodic distribution	2,304	See Table 1
Number of Type-Specific Parameters (N)	16	Table 7.1-8.4
Number of Common Parameters (C)	3	
Number of free exogenous parameters	79	$D*(K-1)+K*N+C$
CPU Time to Evaluate Objective (min.)	14	16 X UltraSPARC-III
Value of Objective ( $Z^2$ )	19.426	

Table 7.1.  $\hat{\theta}_{\text{exog}}$  : Estimated Type Proportions ( $\Lambda[d]$ )

d	Description	Type Index (k)			
		1	2	3	4
1	NB, One Child	0.0063 (0.030)	0.8216 (0.006)	0.0911 (0.020)	0.0810 (0.019)
2	NB, Two+ Children	0.000003 (0.860)	0.2072 (0.985)	0.3253 (0.016)	0.4675 (0.016)
3	BC, One Child	0.5228 (0.438)	0.00009 (0.429)	0.4771 (0.014)	0.0000009 (0.014)
4	BC, Two+ Children	0.5736 -	0.00009 -	0.4263 -	0.0000151 -

GMM estimates based on (38). See (4) and (13) for roles of the parameters. Estimated standard errors in parentheses is the square root of diagonal elements of (39).

Table 7.2.  $\hat{\theta}_{\text{exog}}$  : Estimated Dynamic Programming Parameters ( $\delta_k$  and  $\rho_k$ )

Var	Description	Type Index (k)			
		1	2	3	4
$\delta$	Discount Factor	0.9999 (0.0005)	0.934 (0.081)	0.476 (0.416)	0.742 (0.023)
$\beta$	Income Reporting	0.399 (0.004)	0.413 (0.040)	0.955 (0.022)	0.391 (0.019)
$\rho$	Smoothing	36.220 (1.081)	99.192 (143.937)	11.507 (7.440)	6.103 (2.463)

GMM estimates based on (38). See (4) and (13) for roles of the parameters. Estimated standard errors in parentheses is the square root of diagonal elements of (39).

Table 7.3.  $\hat{\theta}_{\text{exog}}$  : Estimated Utility Shifters ( $\Upsilon$ )

Var.	Description	Type Index (k)			
		1	2	3	4
$\mu$	Job Offer Mean	-1.560 (0.045)	-0.072 (0.154)	0.025 (0.020)	0.020 (0.014)
$\sigma$	Job Offer St. Dev.	1.999 (0.014)	1.632 (0.069)	1.825 (0.235)	1.608 (0.018)
$\chi$	Outside Support	1.427 (0.037)	1.080 (3.665)	1.535 (0.023)	0.825 (0.064)
$\nu$	Cost of FT Work	0.346 (0.004)	0.409 (0.767)	0.447 (0.133)	0.487 (0.025)
$\kappa$	Cost of Job Search	0.461 (0.036)	0.0007 (0.350)	0.000002 (0.002)	0.081 (0.036)
$\eta$	Return to Skill	1.355 (0.155)	2.964 (30.406)	31.685 (415.395)	7.070 (2.352)
$\zeta$	1 / Mean Convexity		2.997 (0.069)		

GMM estimates based on (38). See (4) and (13) for roles of the parameters. Estimated standard errors in parentheses is the square root of diagonal elements of (39).

Table 7.4.  $\hat{\theta}_{\text{exog}}$  : Estimated Transition Shifters ( $\Pi$ )

Sub.	Description	Type Index (k)			
		1	2	3	4
<u>Market</u>					
j	Job Offer (b>0)	0.069 (0.003)	0.730 (0.510)	0.99994 (0.060)	0.831 (0.028)
f	Prop. Full Time	0.999996 (0.000)	0.889 (0.080)	0.99955 (0.206)	0.902 (0.066)
m	Prop. MW job (n=0)	0.131 (0.016)	0.439 (0.321)	0.531 (0.512)	0.249 (0.030)
l	Job Loss	0.021 (0.001)	0.0011 (0.007)	0.018 (0.006)	0.002 (0.002)
<u>Home</u>					
s	Support Change		0.029 (0.002)		
h	Prob. Costs Change		0.999989 (0.002)		
<u>Skills</u>					
+	SSP Plus Effect	0.823 (0.015)	0.009 (0.255)	0.641 (1.060)	0.910 (0.036)
a	Accumulation	0.995 (0.038)	0.4787 (0.898)	0.1245 (0.071)	0.2601 (0.016)
d	Depreciation	0.0006 (0.001)	0.0037 (0.021)	0.7654 (1.066)	0.1007 (0.009)

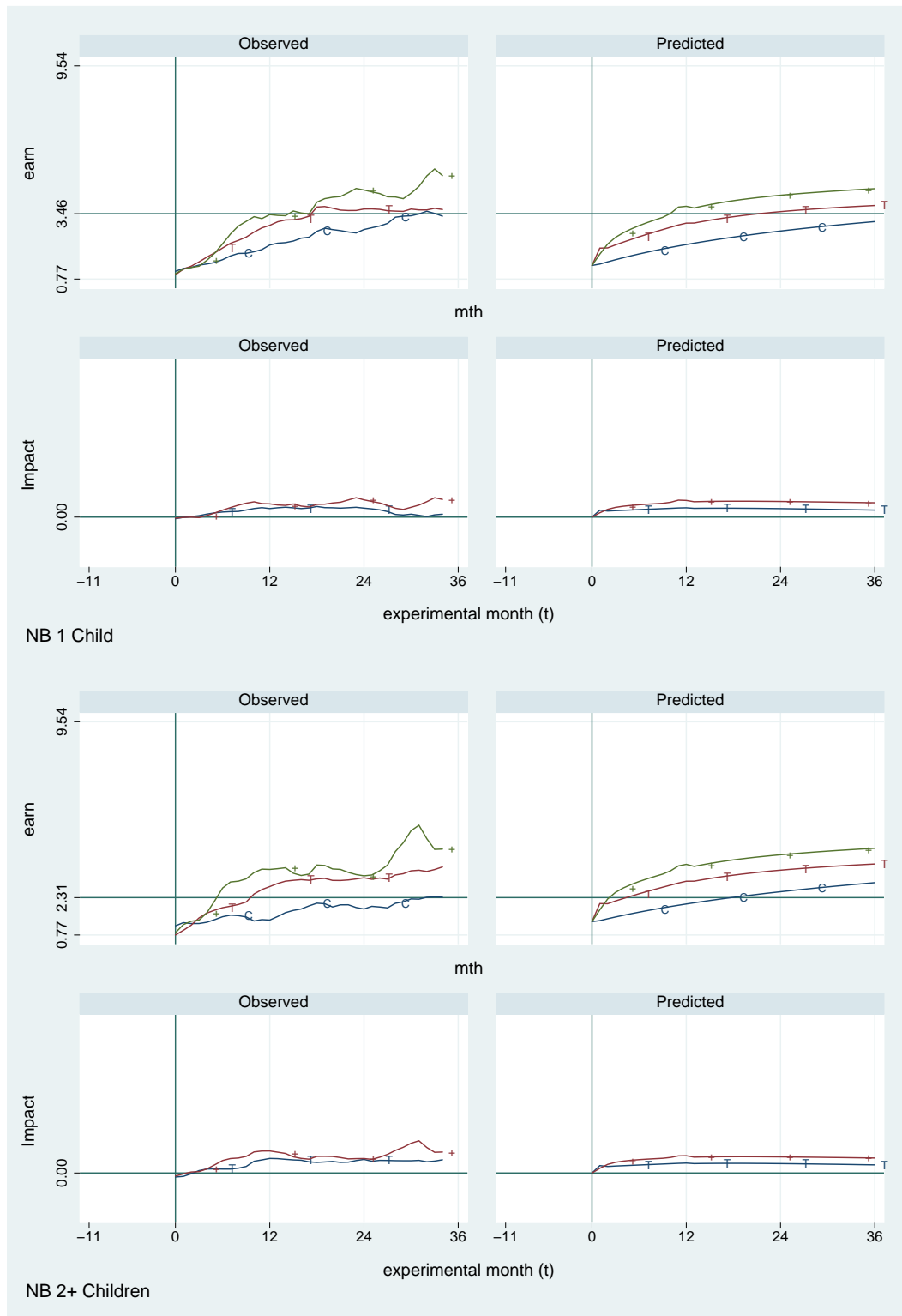
GMM estimates based on (38). See (4) and (13) for roles of the parameters. Estimated standard errors in parentheses is the square root of diagonal elements of (39).

Table 8. Estimated Standard Errors by Group

Par./ Sub.	k=1 / common			k=2			k=3			k=4		
	all	Ctrl	Treat	all	Ctrl	Treat	all	Ctrl	Treat	all	Ctrl	Treat
$\lambda$												
<b>d=1</b>	0.0304	0.0977	0.0315	0.8603	2.6573	0.8945	0.4381	1.2955	0.4556			
<b>d=2</b>	0.0060	<b>0.1480</b>	0.0041	0.9851	<b>4.1870</b>	0.9845	0.4289	<b>1.9720</b>	0.4127			
<b>d=3</b>	0.0203	0.0810	0.0173	0.0161	0.0507	0.0126	0.0137	<b>0.0569</b>	0.0110			
<b>d=4</b>	0.0195	0.0731	0.0165	0.0165	0.0470	0.0135	0.0137	<b>0.0560</b>	0.0112			
$\delta$	0.0005	<b>1.5261</b>	0.0003	0.0811	<b>8.6098</b>	0.0517	0.4163	<b>40.8157</b>	0.3209	0.0226	<b>1.5547</b>	0.0145
$\kappa$	0.0363	<b>41.9370</b>	0.0269	0.3497	<b>524.1280</b>	0.2216	0.0021	<b>0.3221</b>	0.0015	0.0359	<b>0.7234</b>	0.0221
$\mu$	0.0449	<b>11.4133</b>	0.0329	0.1543	<b>673.9400</b>	0.0446	0.0196	<b>0.8958</b>	0.0141	0.0135	<b>0.9132</b>	0.0069
$\sigma$	0.0135	<b>2.2822</b>	0.0098	0.0688	<b>167.6183</b>	0.0682	0.2354	<b>5.2861</b>	0.1520	0.0176	<b>0.2945</b>	0.0127
<b>m</b>	0.0163	<b>32.5299</b>	0.0106	0.3211	<b>1174.5987</b>	0.2210	0.5119	<b>11.1207</b>	0.3855	0.0299	<b>2.2275</b>	0.0209
<b>+</b>	0.0150	-----	-----	0.2552	-----	-----	1.0601	-----	-----	0.0363	-----	-----
<b>a</b>	0.0379	<b>39.1994</b>	0.0273	0.8979	<b>40.6136</b>	0.6789	0.0710	<b>2.7433</b>	0.0556	0.0162	<b>0.5949</b>	0.0121
<b>d</b>	0.0008	<b>0.0192</b>	0.0006	0.0210	<b>0.9998</b>	0.0148	1.0658	<b>9.1376</b>	0.8696	0.0091	<b>0.4074</b>	0.0067
<b>j</b>	0.0027	<b>2.5649</b>	0.0019	0.5096	<b>1520.5518</b>	0.3509	0.0605	<b>0.2741</b>	0.0484	0.0280	<b>2.2569</b>	0.0194
<b>l</b>	0.0010	<b>0.0060</b>	0.0005	0.0065	0.0199	0.0068	0.0065	0.0093	0.0057	0.0017	<b>0.0167</b>	0.0012
$\chi$	0.0371	<b>1.3163</b>	0.0251	3.6653	<b>146.9145</b>	2.5084	0.0231	<b>0.7175</b>	0.0169	0.0635	<b>1.2464</b>	0.0437
<b>f</b>	0.0001	<b>0.0192</b>	0.0001	0.0800	<b>0.6640</b>	0.0750	0.2056	<b>25.7735</b>	0.1458	0.0656	<b>0.7327</b>	0.0425
<b>v</b>	0.0043	<b>0.6478</b>	0.0032	0.7668	<b>192.0367</b>	0.5154	0.1329	<b>3.2502</b>	0.0864	0.0250	<b>0.3219</b>	0.0169
$\eta$	0.1551	<b>371.7602</b>	0.1108	30.4060	<b>2711.5413</b>	18.1565	415.3948	<b>3913.0848</b>	313.3670	2.3517	<b>193.0513</b>	1.2085
$\beta$	1.0812	<b>1114.7384</b>	0.7502	143.9372	<b>6427.0846</b>	89.3818	7.4396	<b>232.9125</b>	5.4599	2.4627	<b>29.4926</b>	1.7980
$\rho$	0.0040	<b>0.0389</b>	0.0029	0.0395	0.0964	0.0444	0.0216	0.0470	0.0187	0.0192	<b>0.1640</b>	0.0150
<b>s</b>	0.0016	0.0040	0.0013									
<b>h</b>	0.0025	<b>0.1558</b>	0.0018									
$\zeta$	0.0691	<b>3.3796</b>	0.0498									

"all" is the standard error for the corresponding parameter as computed for Table 7 "ctrl" is the standard error computed using on moments from the control group (scaled by  $\sqrt{1/2}$ ) to eliminate the sample size effect). "treat" is the re-scaled standard error using only moments from the treatment groups. Bold indicates a standard error 4 times the size of the "all" column. Italic indicates a standard error less than 3/4 of the "all" column.

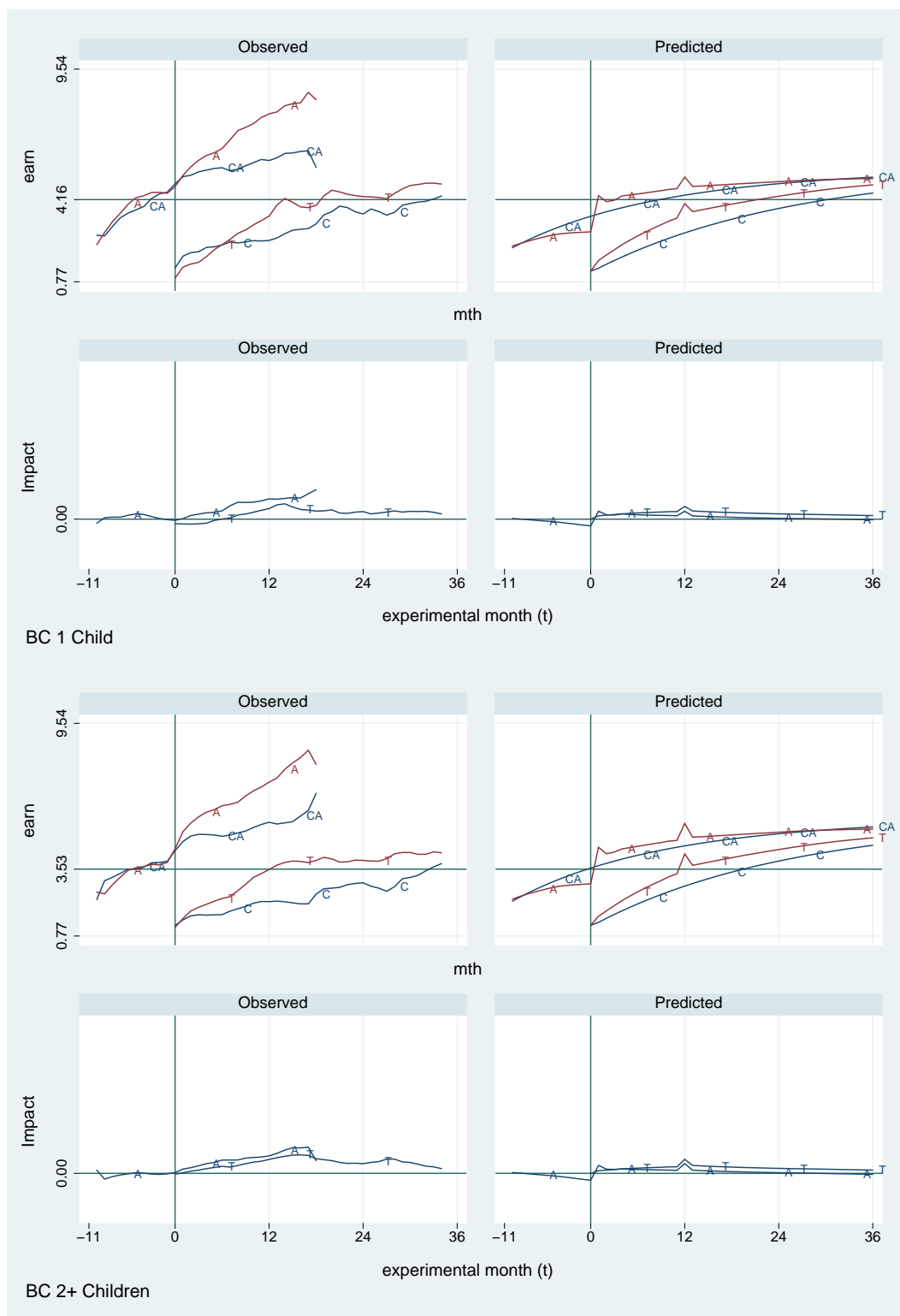
Figure 5a. Result: Earnings, New Brunswick



C: Recipient control; CA: Applicant Control;

T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

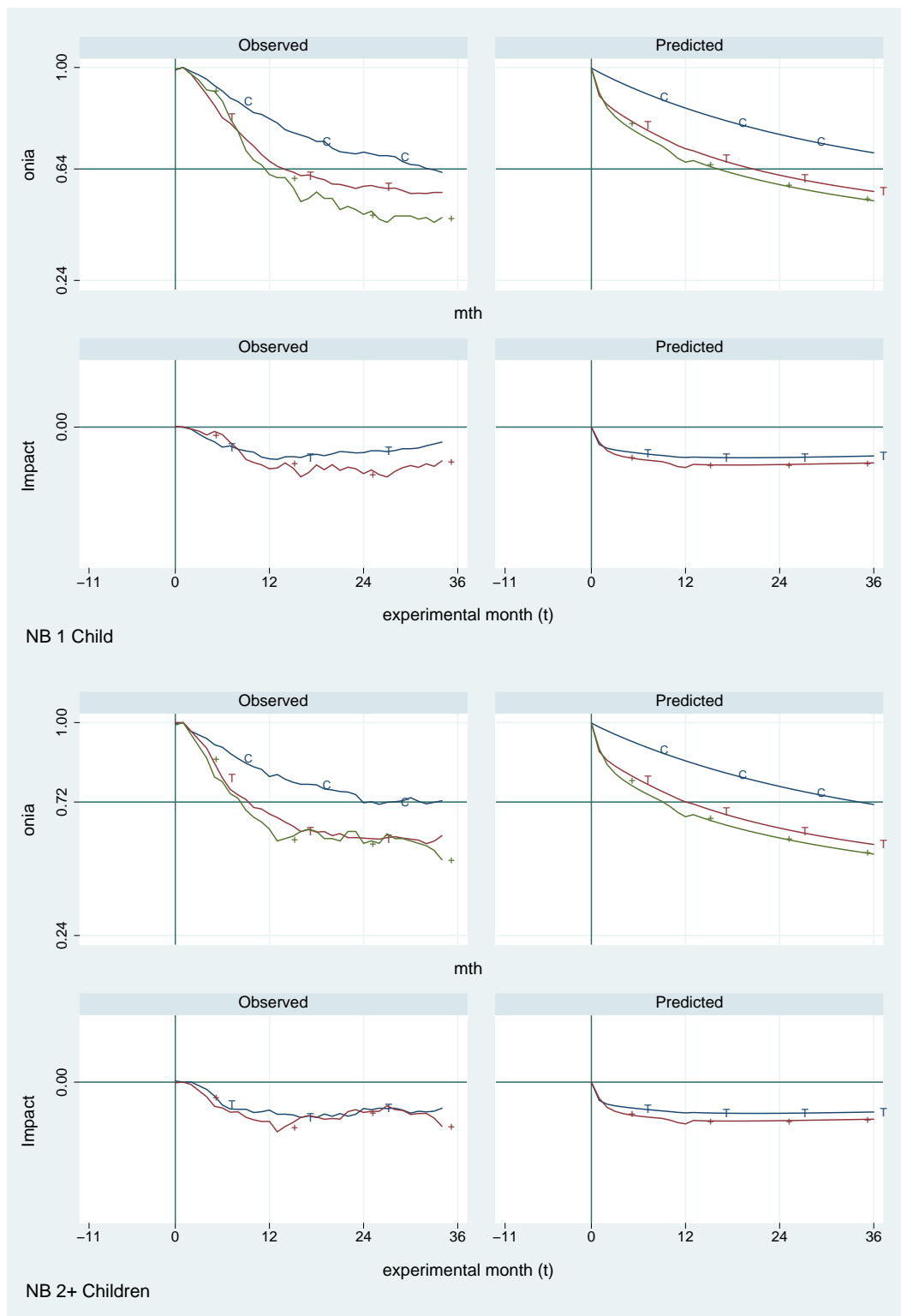
Figure 5b. Result: Earnings, British Columbia



C: Recipient control; CA: Applicant Control;

T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

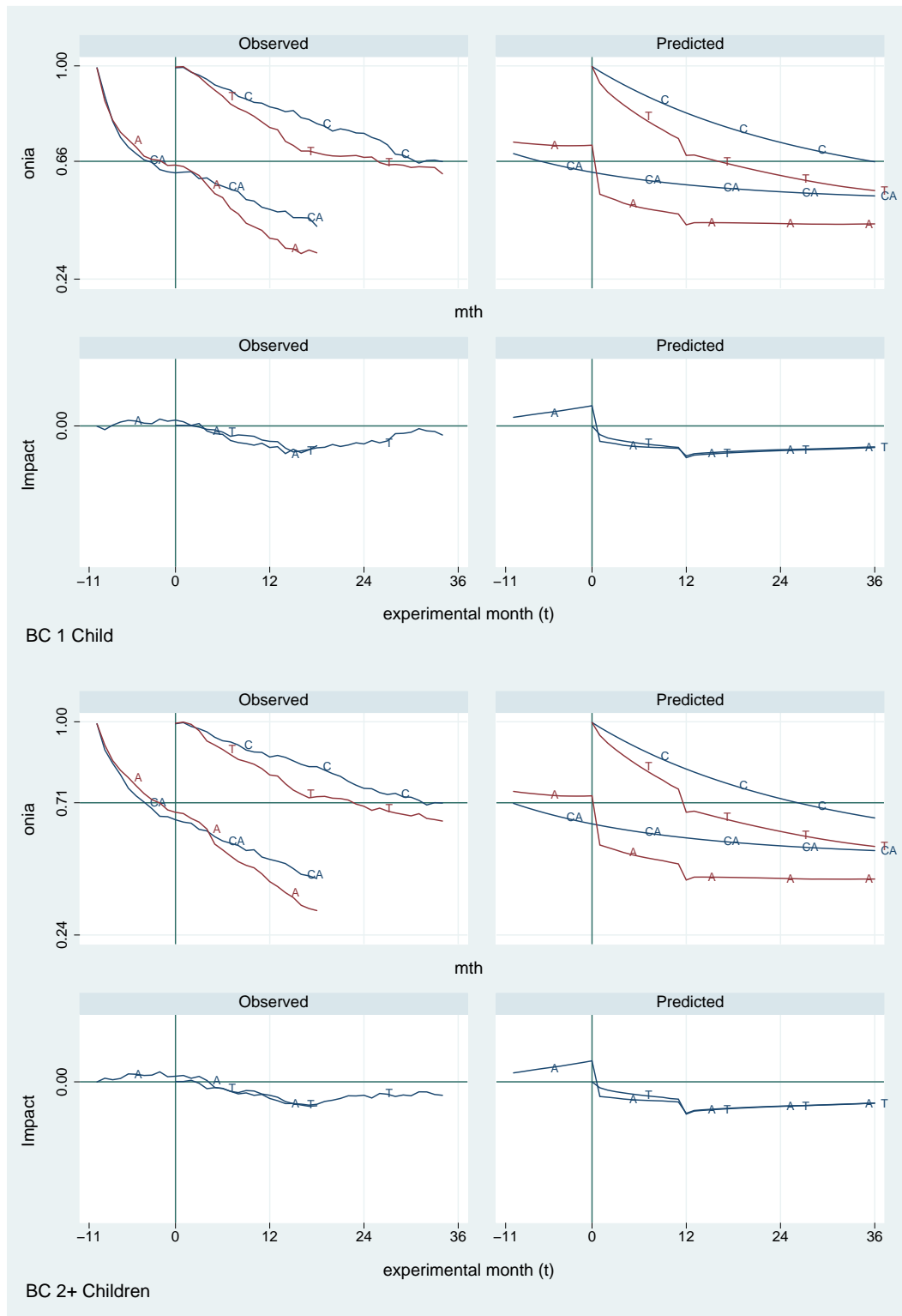
Figure 6a. Result: On IA, New Brunswick



C: Recipient control; CA: Applicant Control;

T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

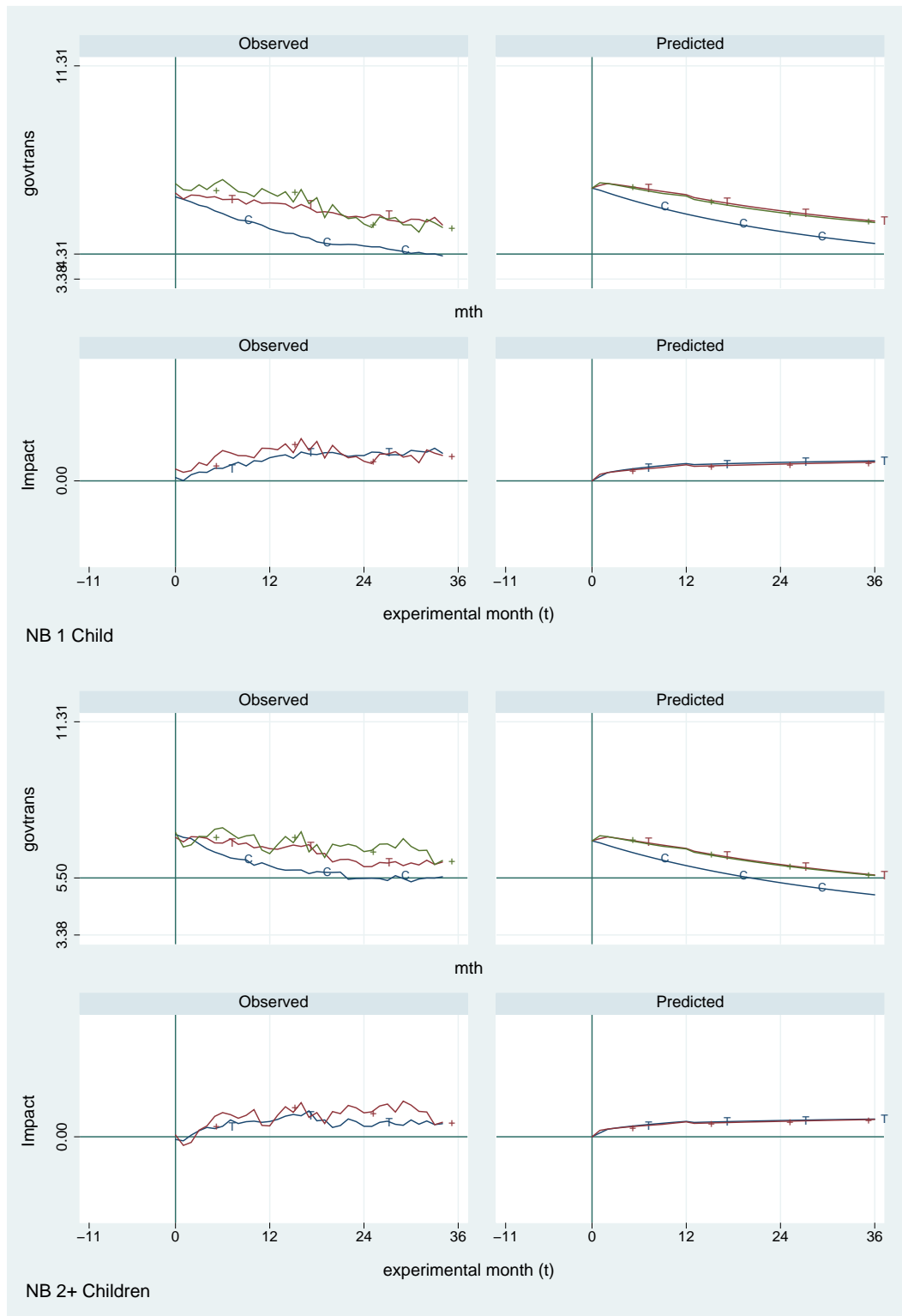
Figure 6b. Result: On IA, British Columbia



C: Recipient control; CA: Applicant Control;

T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

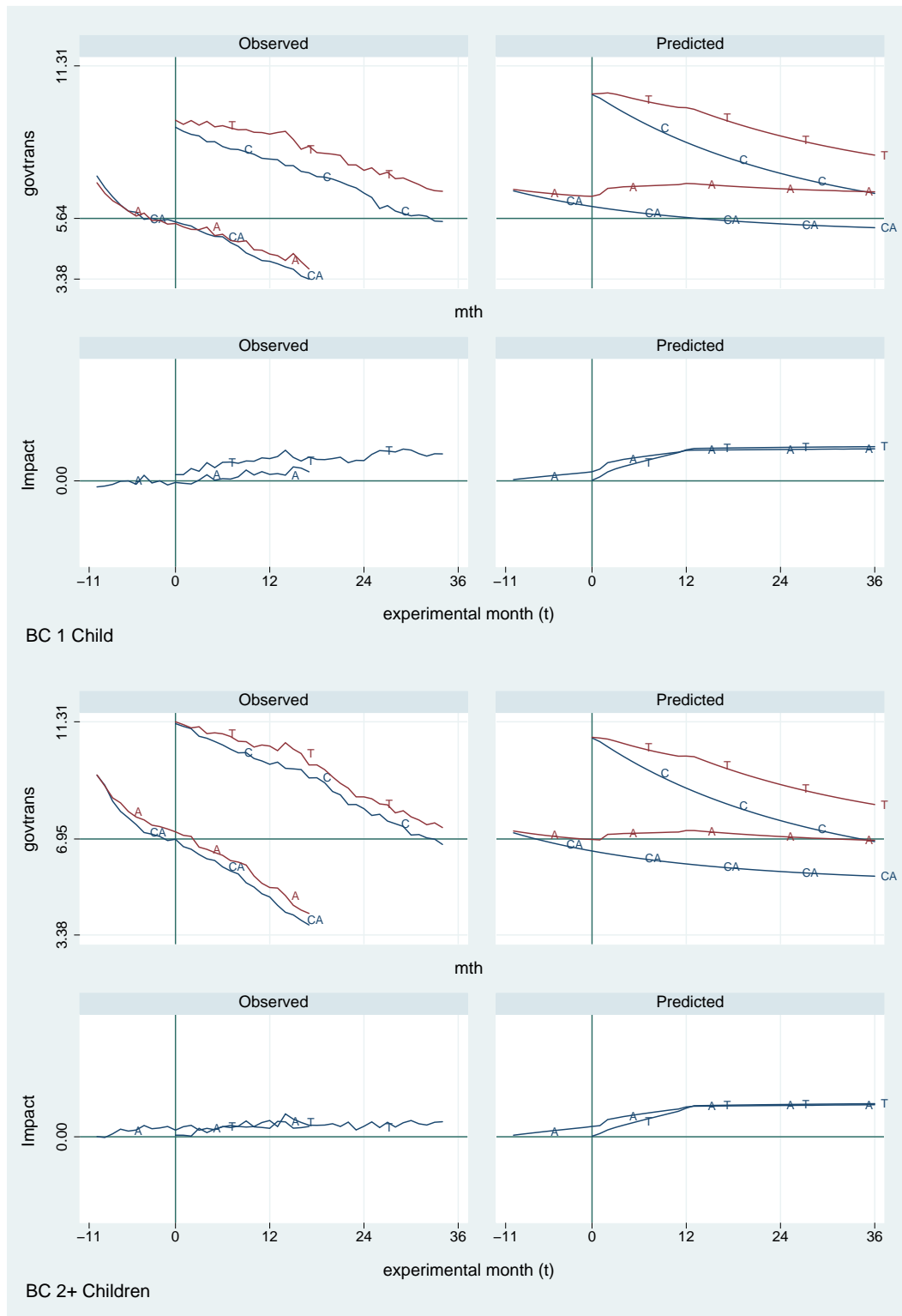
Figure 7a. Result: Total Government Transfers (IA+SSP)



C: Recipient control; CA: Applicant Control;

T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

Figure 7b. Result: Total Government Transfers (IA+SSP)



C: Recipient control; CA: Applicant Control;

T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

Figure 8. Variation from Policy, Selection and Heterogeneity

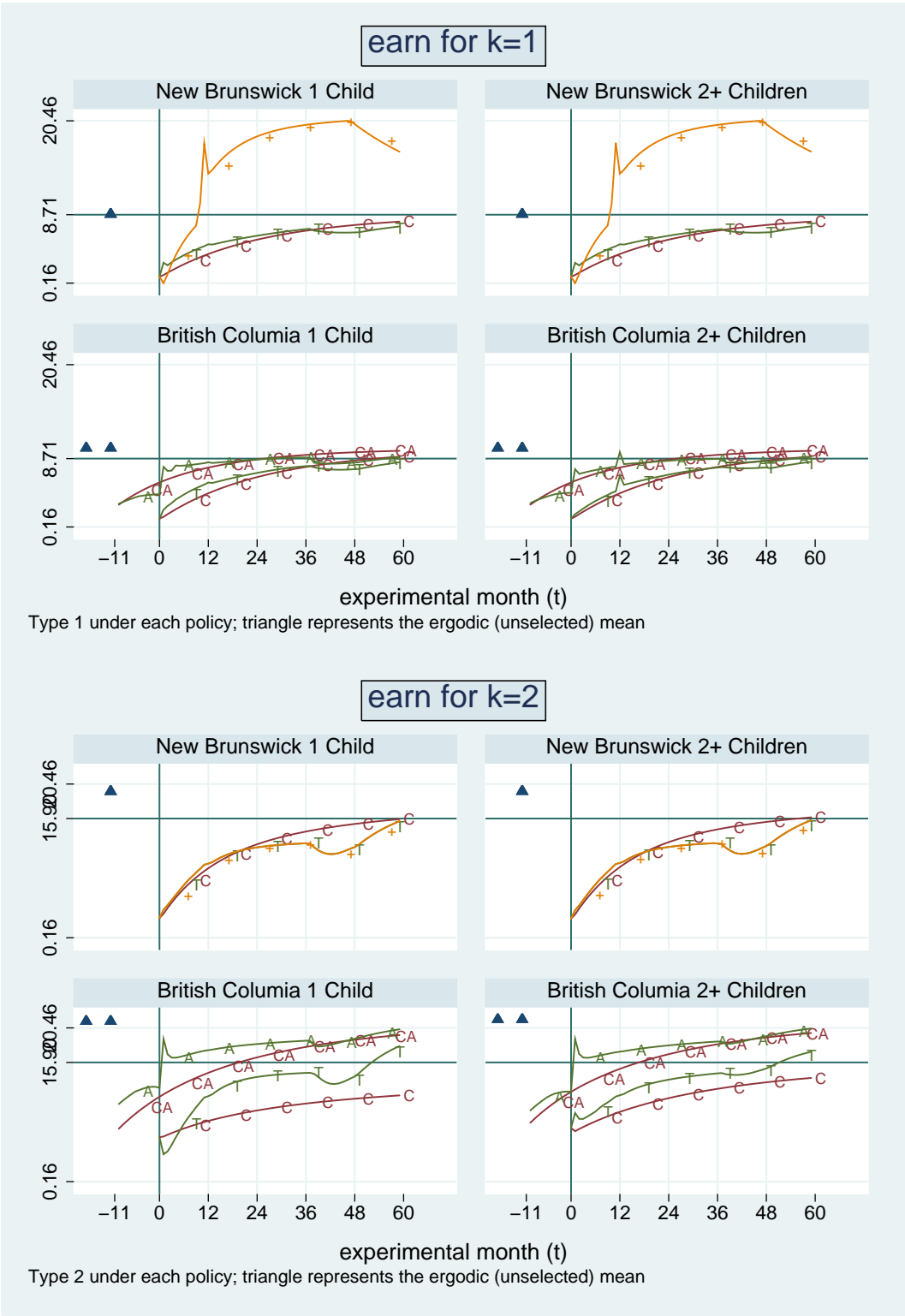
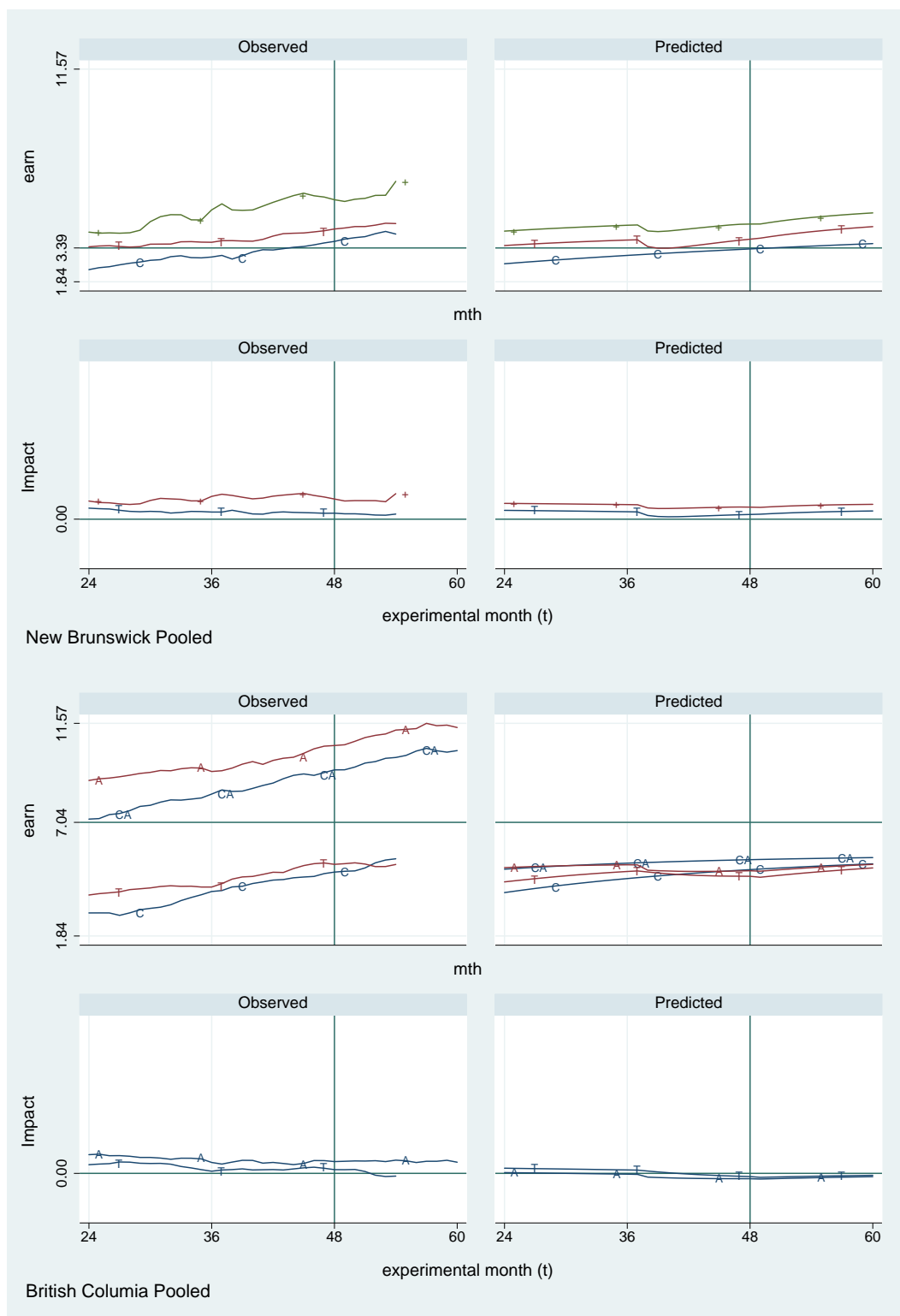


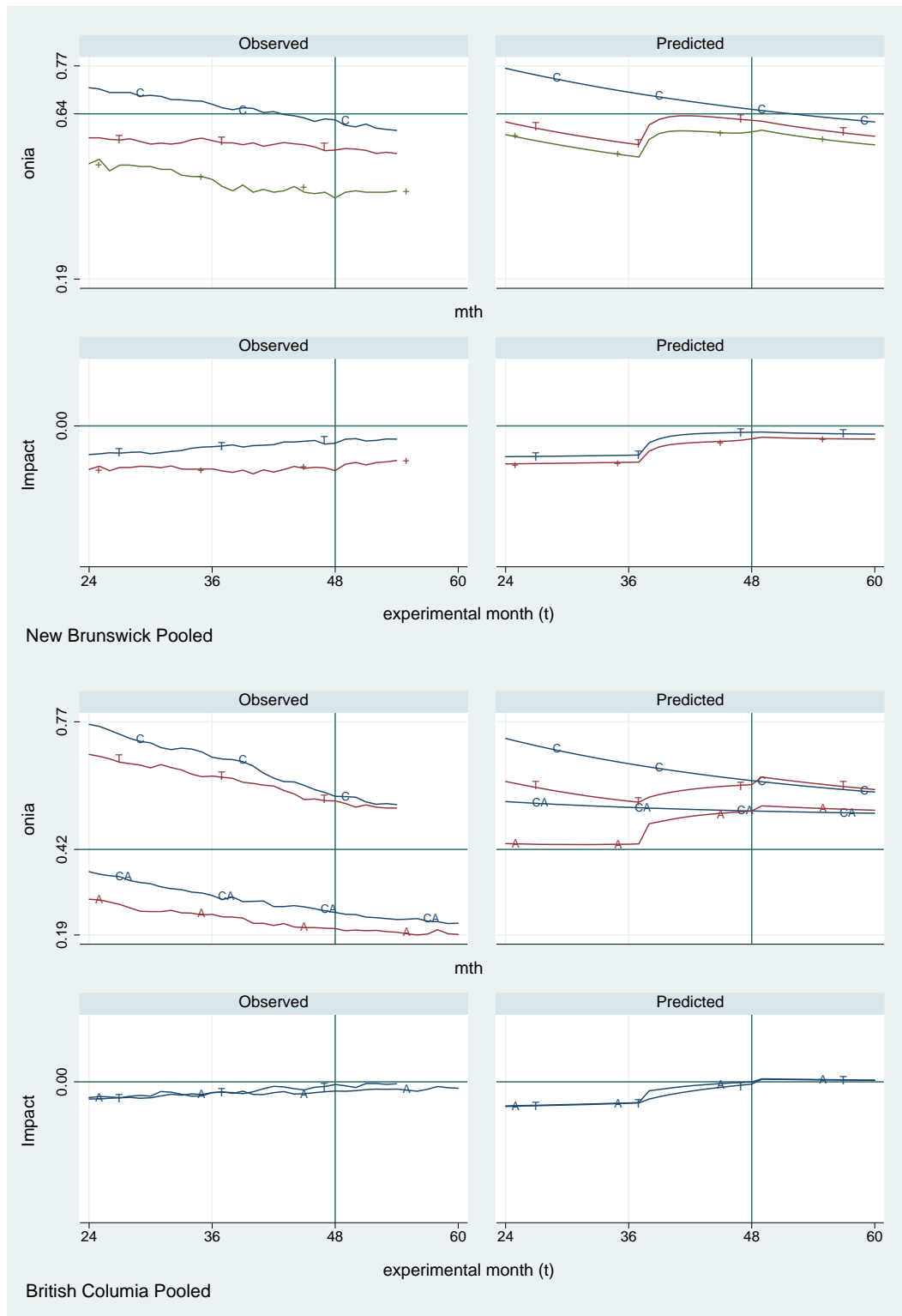
Figure 9a. Forecast: Earnings by Province



C: Recipient control; CA: Applicant Control;

T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

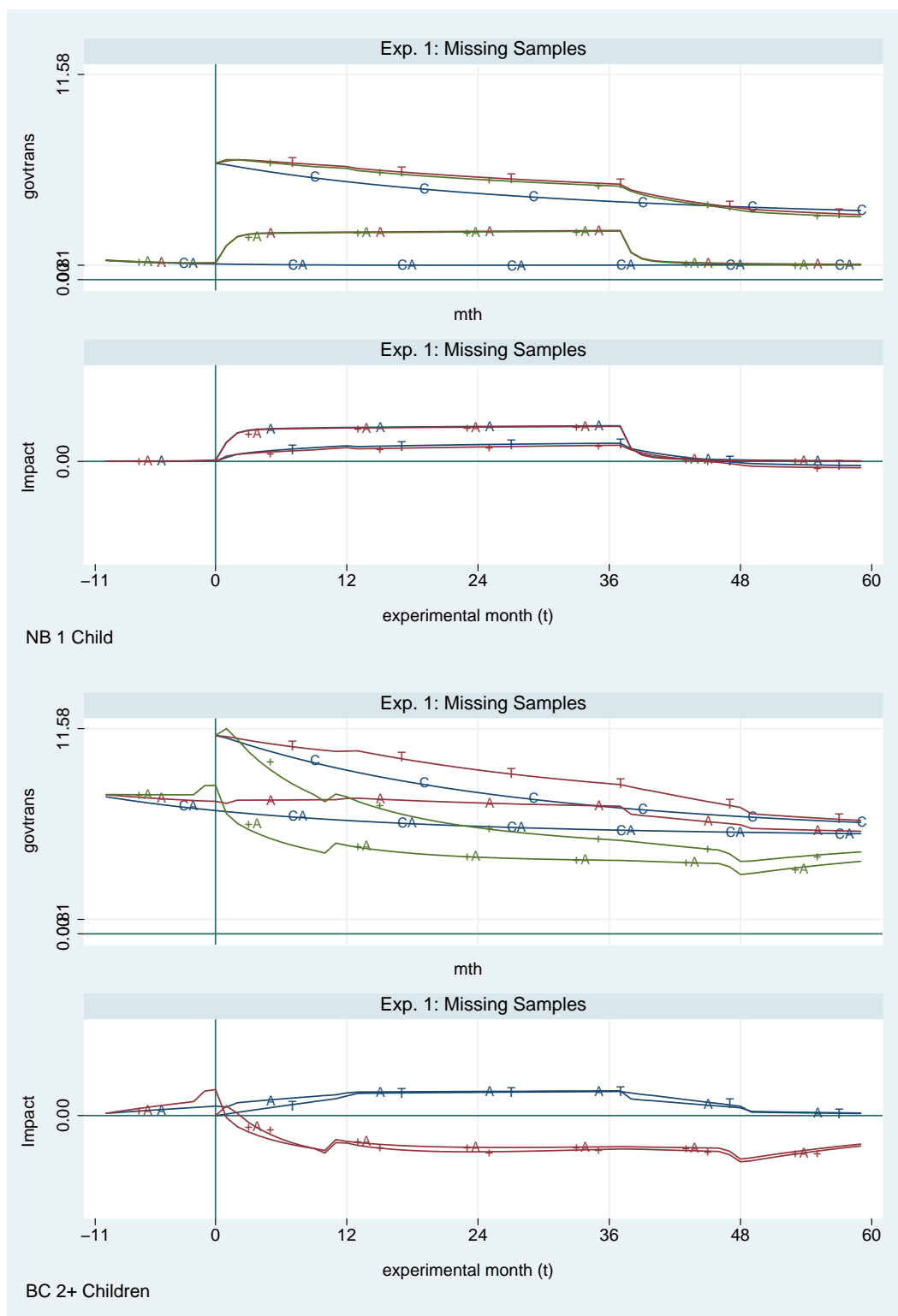
Figure 9b. Forecast: On IA by Province



C: Recipient control; CA: Applicant Control;

T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

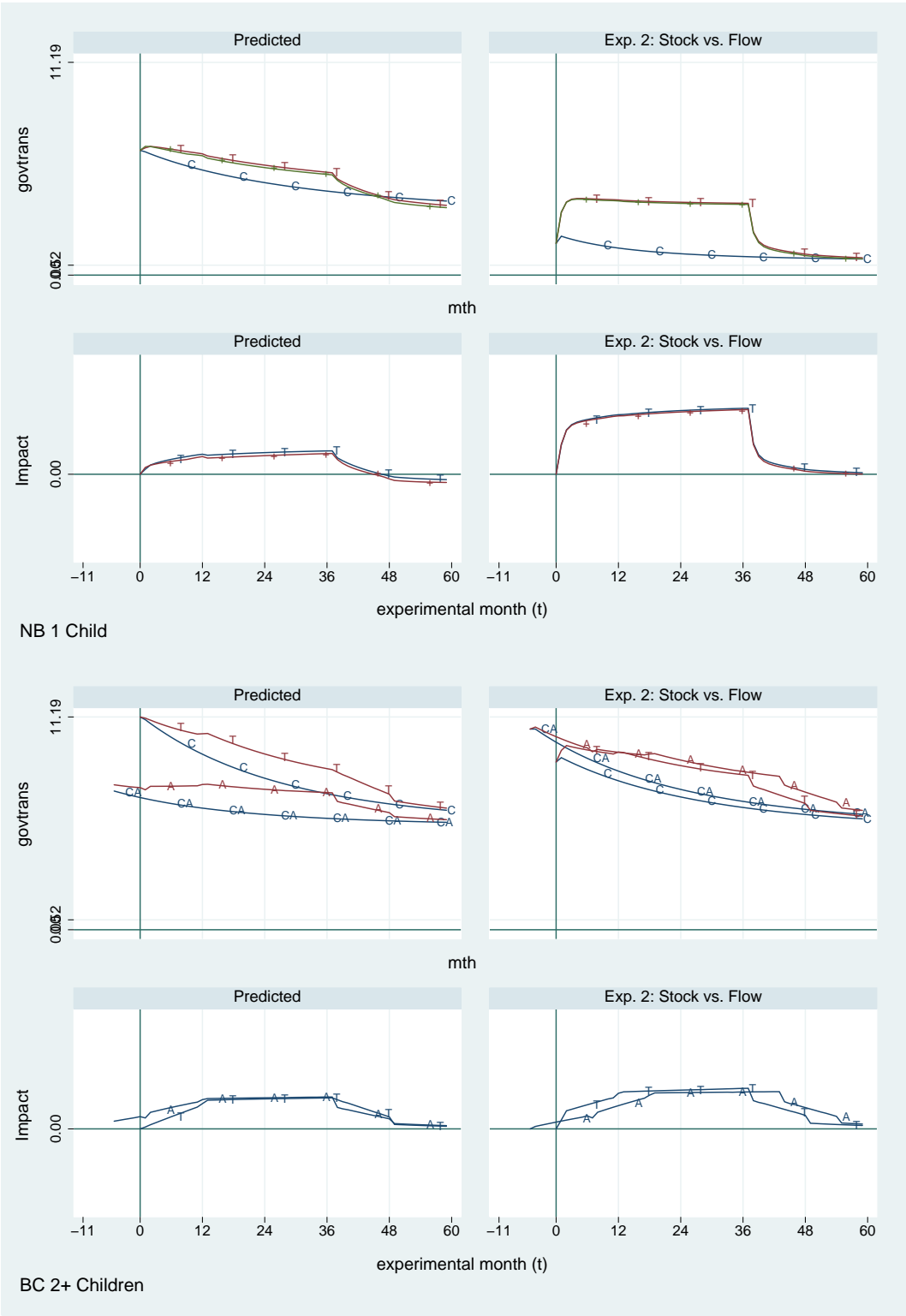
Figure 10. Experiment 1: Total Transfers in Missing Samples



C: Recipient control; CA: Applicant Control;

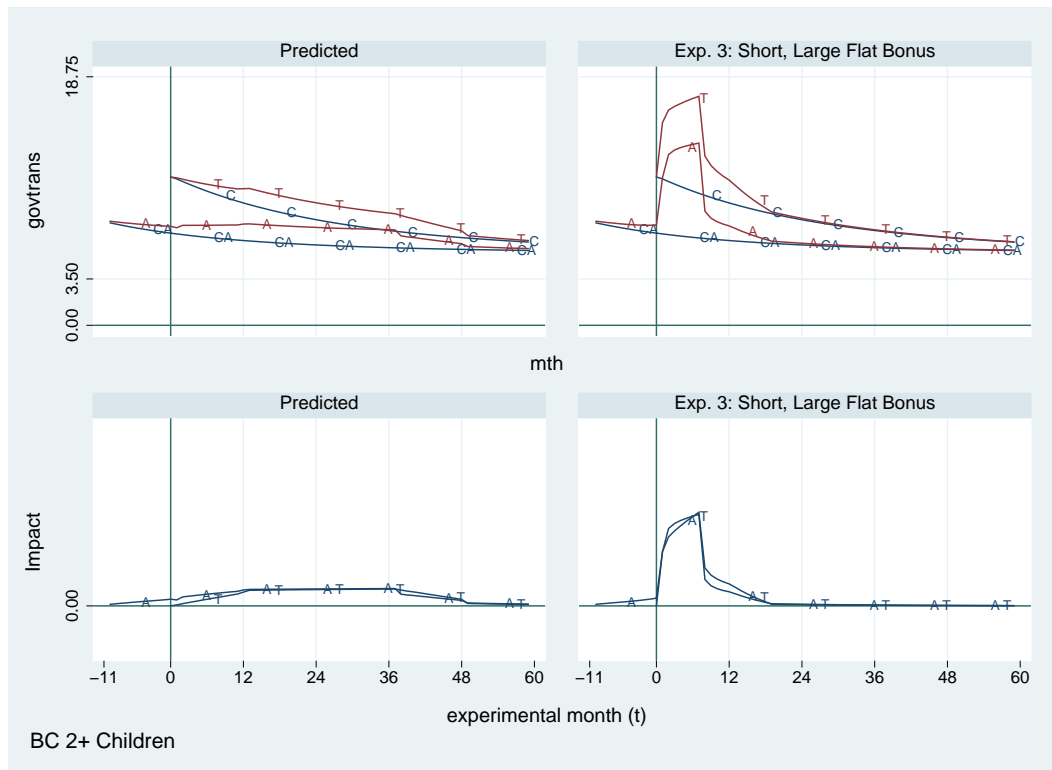
T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

Figure 11. Experiment 2: Total Transfers under Stock/Flow Sampling Reversal



C: Recipient control; CA: Applicant Control;  
T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

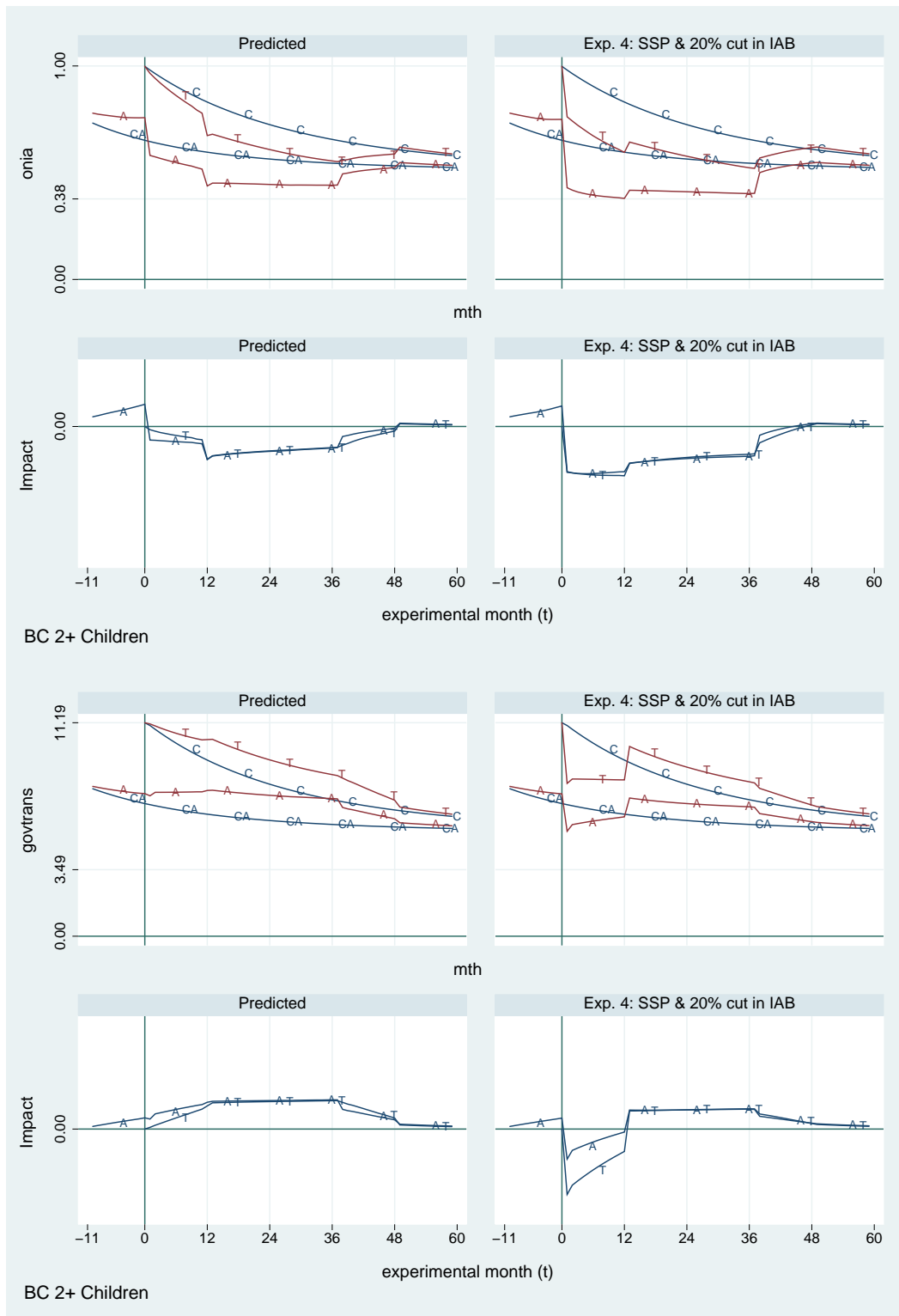
Figure 12. Experiment 3: Total Transfers under Short-Lived, Large, Flat Bonus



C: Recipient control; CA: Applicant Control;

T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

Figure 13. Experiment 4: SSP & 20% cut in IAB, BC 2+ Child



C: Recipient control; CA: Applicant Control;

T: Recipient treatment; A: Applicant treatment; +: Plus Treatment

